Logic, Epistemology, and the Unity of Science 53

Matthias Neuber
Adam Tamas Tuboly *Editors*

Ernest Nagel: Philosophy of Science and the Fight for Clarity

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For my ever supportive brother Christian

Matthias Neuber

To Gabó, Sára, and Róza

Adam Tamas Tuboly

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Chapter 1
Introduction: Ernest Nagel and the Making of Philosophy of Science a Profession

Matthias Neuber and Adam Tamas Tuboly

Abstract This chapter provides an overview of the life (Sect. 1.1) and philosophy (Sect. 1.2) of Ernest Nagel, as well as a summary of the chapters in this volume (Sect. 1.3). Regarding Nagel's philosophy, we focus on his role and activities to stabilize analytic philosophy (of science) and make it a profession, his views about the role and nature of history and sociology of science, naturalism, and socially engaged philosophy, and finally his understanding of the relation between science, society, and philosophy.

Keywords Ernest Nagel · History of philosophy of science · Naturalism · Socially engaged philosophy of science · Analytic philosophy

1.1 Ernest Nagel: Life and Work of an Immigrant

Often forgotten, Ernest Nagel (1901–1985) was among the most influential twentieth-century philosophers of science. Besides publishing important and widely-read works on such major topics as explanation, prediction, reduction, and naturalism, Nagel held many institutional positions that prepared the ground for philosophy of science becoming a well-established discipline within and beyond the American canon of philosophy.

Ernest (originally Ernő) Nagel was born in the Northern part of the Austro-Hungarian Empire (specifically a region inhabited back then mainly by German-speaking Jews—it is now part of Slovakia) and emigrated to the United States when he was ten years old. After receiving a BSc from City College of New York (which

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he attended mainly because of the free tuition), a master's degree in mathematics from Columbia, and finally a Ph.D. in philosophy, also from Columbia in 1931, he spent his entire career at Columbia except for a sabbatical (1959–60) in Palo Alto, CA, and a year (1966–67) at Rockefeller University before his retirement in 1970.

At City College, Nagel was significantly influenced and helped (even financially) by Morris R. Cohen. The latter was a professor of philosophy with a wide range of interests, including history, law, and politics, but mainly focusing on the theory of logic. This influence is quite evident in Nagel's early writings on logic (1929a, b) where he claimed that logic can't be divorced entirely from metaphysics, given that it is connected to "generic traits of existence" and "irreducible traits in every subject-matter of scientific inquiry" (1929b, p. 706). Since logic is part of the world, similar to the inquiring subject, it should be treated as any other worldly scientific affair: "[f]or one who is committed to a whole-hearted naturalism, the continuity between logic and metaphysics can not be broken" (1929b, p. 708). As Cohen defended similar continuity ideas about logic and metaphysics, under a specific Platonist umbrella, it is not surprising that their co-authored textbook, *An Introduction to Logic and Scientific Method* (Cohen & Nagel 1934), contains a similarly loaded approach to logic (Pincock (2017, p. 162) has called this a "modal-realist" approach).

It took many years and a European tour for Nagel to revise his naturalism by incorporating more behaviorist and analytic terms. Even in 1934 (presumably around November, but the letter is without a precise date), Nagel wrote the following words to Sidney Hook, after he had met Rudolf Carnap:

> I am half convinced that our frequent to-do with the 'ontological basis' of formal logic is a mistake, and that the 'necessity' we worry about is a conventional and linguistic one. But I know that for the moment I am much taken with the neatness of formulation with which he gets rid of some skeletons, and sadly easily return to my former rut. (Quoted from Nagel (ms.))

While Nagel remained somewhat skeptical of Carnap's purely linguistic interpretation of logic (and of philosophical debates), he later worked out ways to account for logic in a naturalist setting without metaphysics and ontology (Nagel 1944/1956; see also Sander Verhaegh's chapter in the present volume).

Cohen and Nagel's jointly written volume (though it is contested how the writing and editing tasks were distributed between them) became an influential and significant textbook and remained so for decades. Even in 1950, James R. Newman included the book among the 20 best examples of science popularization (1950, p. 103). The systematic and educational character of the book was recognized and acknowledged even by the logical empiricist Otto Neurath who placed it on a continuum with Jevons's *Principles of Science* and Karl Pearson's *Grammar of Science*—all of which "tried to apply the empirical procedure to all questions [of social and natural sciences] without distinction" (Neurath 1937/1987, p. 133).

But the book was also used and read outside the classroom. In early 1944, Nagel and Cohen received a letter from Dudley Meek on behalf of Harcourt, Brace and Company, Inc., Publishers. Meek claimed that the publisher got a request from the Editorial Staff of the United States Armed Forces Institute to print a special

edition of 15,000 copies of the book "for group study among soldiers in noncombat areas and, later, by soldiers during the period between cessation of hostilities and demobilization." Nagel and Cohen agreed to the special terms, and the book was printed as an "Educational Manual" that "may be useful for meeting requirements for college graduation." In a description of the manual, officers emphasized its "principles of techniques and principles of weighing evidence and establishing soundness of beliefs" (Anonymous 1945, p. 22). It is quite difficult, however, to find copies of this edition, which may be explained by the fact that "part of the understanding is that all of these books will be shipped overseas and, when the soldiers are through with the books, they will be destroyed. Hence the special edition will not get back into the market to compete with the regular book."[1]

During his time at Columbia, Nagel (1930) wrote a well-received doctoral dissertation (published privately) on measurement and got to know the pragmatism of John Dewey at its local base. His relation to Dewey, however, is not without controversies and possibilities for philosophical and historical exploitation. In their fight for a naturalistic philosophy that would serve social goals employing rational standards, they took up the gauntlet against such neo-Thomists as Mortimer J. Adler and Robert Maynard Hutchins (see Hook 1943; Dewey 1943; Nagel 1943/1954). As Nagel claims in his paper, "[t]hose who disparage the application of scientific methods to the evaluation of human goods, on the ground that those methods exclude the exercise of a sympathetic imagination, are not only mistaken in their factual allegations; they are also well on the road to identifying the sheer vividness and the emotional overtones of ideas with their validity" (1943/1954, p. 35).

Two years later, they joined forces even more explicitly and together wrote an article to defend naturalism against the charge that it is a simple mechanistic–materialistic view that relates only to the physical sciences without any practical life guidance (Dewey et al. 1945, cf. Cf.; Reisch 2005, Ch. 3). Nagel was known to many as a leading figure in the second generation of pragmatism; Cheryl Misak (2013, p. 116) has said that he was "perhaps Dewey's best graduate student." Nonetheless, he had the same ambiguous relation to Dewey as many other American intellectuals and philosophers. While they were influenced by Dewey's ideas informally and on a general level (one might say by his worldview), they found the articulation of his views significantly lacking.

Already when he entered Columbia, Nagel had his reservations ("I came to Columbia kind of a scoffer as far as Dewey's philosophy was concerned"), but as time went on, he realized the importance of Dewey's philosophy ("[e]ventually I began to see what he was trying to do and that it wasn't quite as unimportant or as unphilosophical as Cohen sometimes seemed to hold, to believe"). But Nagel's opinion of how Dewey presented his ideas did not change: speaking of Dewey's late book on logic, to which Nagel had written the introduction in the collected works, he noted that "it's not very well written, […] [Dewey] is not a great stylist."[2] It was

[1] Dudley Meek to Ernest Nagel and Morris R. Cohen, January 18, 1944; quoted from Nagel (ms).

[2] The quotations are taken from the interview with Nagel published in this volume.

generally acknowledged that "Dewey's use of language is a stumbling block to the understanding of this thought" (Eastman 1966, p. 110).[3]

Nonetheless, Nagel and Dewey stayed close and Nagel followed the latter's career and publishing activities for many decades. Nagel was thus in a very good position to integrate the views of the two giants of American pragmatism. His philosophical oeuvre is characterized by the (often conflicting) influence of Cohen and Dewey, that is, by a certain form of realist pragmatism that admits the naturalistically conceived place of science and society in the world (Suppes 1994). Furthermore, Nagel successfully combined these approaches with logical empiricism and the Unity of Science movement. Though one might also think of Neurath as a naturalist logical empiricist (Uebel 2007), Nagel had an even more wide-ranging and deeper knowledge of the physical sciences (as shown here in the chapters by David Atkinson/Jeanne Peijnenburg and Marij Van Strien). Nonetheless, he shared with Neurath a growing interest in economics, social sciences, and public activism (for more details see below); and thus Nagel could be seen (as Eric Schliesser's chapter will also demonstrate) as an early (and somewhat timid) representative of a socially engaged philosophy of science.

Speaking of logical empiricism: Nagel became acquainted with leading logical empiricists during his 1934 Guggenheim Fellowship when he travelled throughout Europe to study recent philosophical developments. His interest was presumably sparked by his close friend Sidney Hook, who had studied European philosophy in 1929 and sailed back to the U.S. with a largely pessimistic view of its condition, especially in Germany (Hook 1930). As we will see in the next section, Nagel had a much more positive experience a few years later, by which time logical empiricism had become more mature and its defenders had already gained an international reputation.

Nagel first met the logical empiricists at the Eighth International Congress on Philosophy in Prague (September 1934).[4] After the congress, Nagel went to Poland for a few weeks to visit the members of the Lvov-Warsaw School. In November, he returned to Prague for several meetings with Carnap and Philipp Frank, before going back to Vienna at the end of the year. There he attended—as one of the few international visitors—the Thursday meetings of the Vienna Circle. During the early days of 1935, the discussions centered on C.I. Lewis' recently published paper on verificationism (1934) and Schlick's reply to it (1936), which Nagel reconstructed as "a mistaken form of logical atomism." While Nagel was happy to meet Schlick, Kaufmann, Menger, and Waismann, he was somewhat disappointed. "My own impression of the meeting," as he wrote to Carnap, "is that with the exception of Menger and Kaufmann, it had the air of [a] congregation with the members singing in chorus

[3] Recently, Matthew Brown (2012) has tried to rehabilitate John Dewey's philosophy of science in more accessible terms.

[4] This does not mean, however, that Nagel was unfamiliar with the main trends in logical empiricism. He read some of Carnap's and Reichenbach's writings as early as the late 1920s, and published a paper in *Erkenntnis* about measurement already in 1931. On these early encounters, see Sander Verhaegh's paper in this volume.

with Schlick."[5] Nagel had a similarly low opinion of the next meeting: "There was next to no discussion, there really was no time for that, and I confess I found the session rather dull."[6]

As a short detour, to point out a rarely discussed line of influence, we shall note the following. When Nagel arrived to Vienna in September 1934, his first meeting was with Felix Kaufmann. He wrote to Sidney Hook that "[Kaufmann] interests me very much—you may remember … he lectures on jurisprudence, is interested in theoretical economics, gave his first allegiance to Husserl (still is kind of phenomenologist) and was a pupil of Kelsen" (quoted from Nagel (ms.)). Nagel (1932) reviewed Kaufmann's book on mathematics when it appeared and was also asked to write an introduction (1978) to the English edition of Kaufmann's writings on the philosophy of mathematics. Nagel and Kaufmann stayed in contact, which became more substantial after Kaufmann emigrated to the States in 1938. In his obituary for Kaufmann, Nagel claimed that he "became [Kaufmann's] permanent debtor," and relied much on his thoughts

> on basic issues in scientific method, on problems in the theory of knowledge, on the analysis of probability, on the objectivity of value judgments, and on the character of social and economic theory. Kaufmann's subsequent publications on the logic of the natural and social sciences became for a countless number of his readers both illuminating guides in a notoriously vague subject and also fertile sources of suggestive ideas on important phases of scientific method. (Nagel 1950a, pp. 464–465)

Nagel shared Kaufmann's interests in jurisprudence, social sciences, and economics, and thus it is only natural that Kaufmann 'interested him very much'; nonetheless, this relation is still unexplored in the literature. (One of the reasons might be that Kaufmann's name is not even mentioned in *The Structure of Science*.)

After Vienna, Nagel went to Italy to meet Federigo Enriques and George Santayana who was then living there (on these events, see the interview with Nagel in this volume), and then, through the Netherlands, where he met some intuitionistic philosophers and Neurath, to England. There he attended a joint session of the Mind and Aristotelian Society where he met A.J. Ayer, Max Black, Gilbert Ryle, G.E. Moore, and Susan Stebbing (though he had already known Stebbing for many years). In this context, Nagel's views on Wittgenstein might be of particular interest. He reports some of the trends and developments in Wittgenstein's philosophy, but they are based on a second-hand account as "[Nagel] did not receive his permission to attend his lectures" (1936a, p. 17). We know from Nagel's correspondence that Wittgenstein became outraged when Nagel tried to participate in his lectures:

> I made a vain attempt to be admitted to Wittgenstein's class, and went to see him for the purpose. He tore his hair, said it was impossible, that I couldn't possibly understand anything, that I was a tourist and he didn't like tourists, that he wouldn't be able to lecture with a strange face in the room, and that I should "spare him" that suffering. […] What I have seen and heard is very interesting, but not so novel or revolutionary as the students and dons here

[5] Ernest Nagel to Rudolf Carnap, January 5, 1935.

[6] Ernest Nagel to Rudolf Carnap, March 6, 1935. Karl Menger (1994) has confirmed this impression that he was somewhat of a skeptical outsider concerning the Wittgensteinian choir.

> seem to think. His almost pathological personality has seized the imagination of almost everyone here, and Wittgenstein is the main oracle to which the faithful gather. (Nagel to Morris Cohen, June 4, 1935; see Yvonne Nagel's chapter in the volume)

After Nagel went back to the U.S. and started teaching at Columbia, he remained in constant contact with most members of the logical empiricist camp. In the background, he was working on various emigration arrangements for Carnap, Hans Reichenbach, Olaf Helmer, and others, and welcomed Carl Gustav Hempel in New York, even offering his home to the latter.[7]

After serving as an Instructor of Philosophy (1931–1937), Nagel was appointed as Assistant (1937–1939), Associate (1939–1946), and full Professor (1946) at Columbia. In 1955, a new chair was established to honor John Dewey and Nagel was appointed as the first John Dewey Professor of Philosophy. In 1967, he became a University Professor at Columbia, and then a professor emeritus after his 1970 retirement, though he continued teaching for several years. For his philosophical and educational activities, Nagel received the prestigious Nicholas Murray Butler Medal in Gold in 1980 (he had already earned the silver medal in 1954).

Nagel had a very long and nearly uninterrupted career at Columbia and thus became a widely-known figure in New York's intellectual and philosophical life. Though the two circles overlapped, they can also be discussed separately. Regarding the former, many philosophers and educationalists were naturalists—today they are known as "Columbia naturalists" (Jewett 2011). Believing in the unity and interrelatedness of theory and practice, knowledge and action, they all tried to act as public intellectuals, or, as we might call them nowadays, as "socially engaged thinkers." Dewey, Cohen, Nagel, John Herman Randall, Jr., Herbert W. Schneider, Irwin Edman, Horace L. Friess, and James Gutmann were all liberal-minded democrats with socialist leanings. Their naturalism exemplified and committed them to act and be visible outside the university's walls. Nagel's version of this view will be discussed in the next section.

Philosophically, Nagel was a leading organizer and member of the so-called New York Philosophy Club, which functioned similarly as the Vienna Circle: its members held regular, often weekly, meetings where they discussed a new paper or problem raised by one of the members. For example, Nagel often invited C.G. Hempel to present his latest ideas, thus helping his emigrant friend to gain a foothold in his new environment.

But Nagel was also influential in more formal circumstances. He gave many lectures and seminars on various topics (which, in fact, often caused significant delays to his publications and research) and thus had many students. As Patrick Suppes writes in his memoir of Nagel,

> [t]o many generations of students he was the outstanding spokesman of what philosophy could offer in terms of analysis of the scientific method, as it is practiced in many different sciences, and in the relation between science and perennial problems of philosophy such as those of causality and determinism. What is important about this influence is that it was not

[7] Some of these issues are reconstructed in Yvonne Nagel's and Fons Dewulf's chapters. For further details, see Dewulf (2018).

> simply students of philosophy, but students of many different disciplines whom he influenced in a way that many of them still remember. (Suppes 1994, p. 258)

As Fons Dewulf shows in his chapter in the present volume, Nagel played a seminal role in bringing back the topic of explanation, and thus transforming and determining the main topics of philosophy of science for decades (among others via C.G. Hempel).

It is less well-known that Nagel also held many important institutional positions and thus played a significant organizational role in the development of analytic philosophy in general, and philosophy of science in particular. Early on, he was involved in the executive council of the Association for Symbolic Logic. After the Association established its in-house journal, *The Journal of Symbolic Logic*, it quickly gained an international reputation and became the flagship publication for logically relevant intercultural research. Nagel served as consulting editor for the journal (a role that was done by several other renowned logicians and mathematicians; their main task was to act as referees) and later became its editor (1939–1946). Nagel is less known among logicians (who presumably associate him with philosophy of science, rather than formal logic), but he had a deep knowledge of the field: he reviewed many articles and stayed up to date with the latest developments. Furthermore, Nagel wrote an influential popular book on *Gödel's Proofs*, together with James R. Newman (1958), a former student of his.[8]

Another editorial task deserves mention here:[9] in 1960, Nagel, Alfred Tarski, and Patrick Suppes organized the first International Congress for Logic, Methodology and Philosophy of Science at Stanford. It was held under the auspices of the Division of Logic, Methodology and Philosophy of Science, a subdivision of the International Union of History and Philosophy of Science (which still exists and continues to organize the congress every four years). After the congress, Nagel, Tarski, and Suppes (1962) edited and published the conference proceedings, which became highly influential, containing many classical studies on logic, mathematics, philosophy (of science), and their intersections. This volume was remarkable given the fact that Nagel had his reservations about Tarski. In an exchange of letters dating to 1963 (though it may document trends that go back years), Nagel and Hempel discussed funds for travel to the 1964 congress, which was supposed to take place in Jerusalem. Nagel was asked to advise the organizing committee and to suggest names that could help to arrange the funds. He was not up to the task and suggested Hempel as a replacement. Here is how he relayed the idea to Hempel:

> I continue to think that final decisions in the distribution of travel funds to philosophers of science should remain with the U.S. Committee. I really hate to ask you to do this, and certainly do not want to urge you to take on this chore, for you should not be subjected to unnecessary distractions. However, the names that occur to me of qualified members of the PSA who are residents of the Bay Area are so completely dominated by Tarski's views on

[8] On the influence and history of *Gödel's Proofs*, see Hodges (2008) and Feferman (2009).

[9] While they are not widely discussed among historians of philosophy of science, Nagel did edit other influential volumes, such as a collection of readings on *Meaning and Knowledge* (Nagel 1965) that introduced many classical analytic thinkers to the community of epistemologists.

> what is significant work in the philosophy of science (e.g. Pat Suppes), that designating one of them to advise Tarski would in effect leave the choice of suggested recipients of travel funds entirely up to Tarski himself. (Nagel to Hempel, November 26, 1963. Quoted from Tuboly (ms))

The Bay Area of California refers to Berkeley and Stanford, where the two most influential logicians and philosophers of science were indeed Tarski and Suppes, his previous co-editors. Though he does not state so explicitly, this passage indicates certain reservations on Nagel's part about the role and influence of Tarski among philosophers of science in the 1960s.

Though Nagel is mainly known for his 1961 *The Structure of Science*, he had already published hundreds of reviews, many articles, and several books by that date. Nagel started to write reviews already as a graduate student at the end of the 1920s, and he continued to do so throughout his career. As Yvonne Nagel claims in her paper in this volume,

> [t]hroughout his long career, Ernest read and reviewed many books and papers by philosophers in his field. He started doing this when he was an impoverished student and needed the money the journals paid for reviews. Later, he did it mainly to keep abreast of the literature. His ability to remember everything he read made him a valuable resource for his friends.

The importance of Nagel's reviews is illustrated by the fact that many of them were republished in Nagel's later books (1954a; 1956a).

After privately publishing his dissertation, Nagel's first monograph concerning the *Principles of Theory of Probability* (1939) appeared in Neurath's *International Encyclopedia of Unified Science*. Nagel was interested in probability quite early on (1933, 1936c, d), and defended the truth-frequency interpretation (his views are discussed in detail by Maria Carla Galavotti in this volume).

During the 1950s, he published three books: besides *Gödel's Proof* (1954), he reissued his earlier papers under the titles *Sovereign Reason* (1954a) and *Logic Without Metaphysics* (1956a). The first brings together Nagel's most important philosophical papers from the 1940s, as well as a few substantial and longer reviews (of Russell and Eddington). The papers are important because many of them were published (from an institutional point of view) in unconventional venues, such as *Partisan Review* (one of the most significant journals for socially engaged socialist thinkers around New York), *Modern Review*, *The Nation*, and *Perspectives USA*. These papers articulate a general vision about the relation of science, philosophy, and society, especially within the post-war atmosphere (some of these issues are taken up in Eric Schliesser's chapter in this volume).

The 1956 volume has two parts: the first contains Nagel's philosophical papers about some conventional and fashionable issues relating to naturalism, logic, epistemology, verification, and functionalism, covering the period from the 1930s to the 1950s. The second part contains 20 reviews of sociological, philosophical, historical, and methodological books. Though this presents only a minor and highly selective slice of Nagel's more than 300 reviews, it still clearly conveys to the reader that Nagel's interests were unusually broad and that he was able, for himself and the reader, to competently navigate the latest literature on almost any topic. The reviews,

as noted above, not only enabled Nagel to gain some extra money, but also to stay abreast of the rapidly growing literature on philosophy and the sciences, and in the process motivated others to conduct some much-needed conceptual house cleaning. While Nagel admits, with a nice nod towards the Vienna Circle's manifesto, that practitioners of analytic philosophy often fall back on the "icy slopes of logic" (Carnap, Hahn, and Neurath 1929/1973), he also believes that they had a social function to play (which they could practice, for example, through the publication of critical reviews on accessible platforms):

> Analytic philosophy has thus a double function: it provides quiet green pastures for intellectual analysis, wherein its practitioners can find refuge from a troubled world and cultivate their intellectual games with chess-like indifference to its course; and it is also a keen, shining sword helping to dispel irrational beliefs and to make evident the structure of ideas. It is at once the pastime of a recluse and a terribly serious adventure: it aims to make as clear as possible what it is we really know. (Nagel 1936a, p. 9)

This is indeed what we find, for example, in the Vienna Circle's manifesto, though Nagel does not refer to it specifically. The scientific world-conception of the Vienna Circle and some of its sympathizers claimed that scientific philosophy was fighting against irrationalism, mythology, intuition, and all such purportedly knowledge-producing endeavors that are, in fact, subjective and therefore fail to stand up against the test of intersubjectivity (Uebel 2020).[10]

Less than a decade later, however, Nagel published his magnum opus, which shows a somewhat different picture—or at the very least, *The Structure of Science* can be read as conveying his idea of what can be done on such "quiet green pastures." Though it was written for a "wide audience" (with as little formalism as possible), Nagel's major work[11] shows such a deep knowledge of the special sciences that following him through the 600 pages of philosophical analysis requires the most focused attention from the reader. In this book, which was in the making for more than a decade, Nagel compresses many of the ongoing debates, alternative views, and arguments with wide-ranging applicability.

The book is mainly concerned with a broad approach to logical questions of scientific explanations:[12] their logical structure, their function in inquiries and testing, and their role in the systematization of knowledge, in making predictions, and in simplifying the analysis of experience. While there is no space here to go into the

[10] It might be worth hypothesizing about the idea that Nagel (similarly to Schlick and Feigl) had his manifesto about the role and nature of philosophy of science within society; see Nagel (1943/1954 and 1947/1954).

[11] In the Preface to *Structure*, Nagel noted that further topics in philosophy (such as the evaluation of knowledge claims, concept formation, etc.) are reserved for a volume that is under "active preparation" (1961, p. ix). Although he also confirmed his intentions to Hempel a few years later ("I want to work on the sequel to the Structure of Science"), he had other obligations, and seemingly gave up on the publication of any systematic work that would function as volume two (Nagel to Hempel, June 18, 1964, quoted from Tuboly (ms.)).

[12] A few years before, Nagel formulated this broad approach as follows: "the task of logic is to make explicit the structures of methods and assumptions employed in the search for reliable knowledge in the all fields of inquiry" (1956b, p. ix).

details of the various topics discussed in the book—from scientific explanations to deduction, laws and theories, geometry, causality and determinism, reduction, biology, social sciences, and history—many of these are taken up by the authors of this volume.[13] For the present purposes, it suffices to call attention to how Nagel closes his introduction, and thus sets the tone and aims of his volume. In this context, Nagel claims that science is, after all, a social institution, embedded in a cultural web of values and commitments, an aspect, which, alongside the development of science, has been revealed in much detail by "sociologists, economists, historians, and moralists." But that is not enough. As Nagel goes on,

> [h]owever, if the *nature of the scientific enterprise and its place in contemporary society* are to be properly understood, the types and the articulation of scientific statements, as well as the logic by which scientific conclusions are established, also require *careful analysis*. This is a task—a major if not exclusive task—that the philosophy of science undertakes to execute. (Nagel 1961, p. 14, emphases added)

And even if Nagel was not talking about "fighting for clarity" in the 1960s, his general aims are evident. Even philosophers of science are capable of contributing to a democratic society by wisely and critically analyzing and clarifying many of the concepts that scientists and popularizers often use uncritically.

In 1979, Nagel again collected some of his major papers from the 1960s to 1970s and published them under the title *Teleology Revisited and Other Essays in the Philosophy and History of Science* (1979a). This title is telling, in that all its elements contain specific, substantial references to what is happening in the volume and what Nagel's interests were at that time. After two decades of publishing about functionalism (in the social sciences), Nagel turned to teleology and goal-oriented processes in biology (the topic of his John Dewey Lectures) and also gave impetus to a revision and rehabilitation of these notions for a broader audience (Nagel's views of teleology are discussed by Bohang Chen in this volume). While philosophy of science is well presented in this volume, including standard topics such as induction, determinism, and the methods of science, it also contains two of Nagel's historical essays (their general significance is analyzed in Thomas Mormann's paper in this volume).

Teleology Revisited is important, however, for another reason. When it came out, Nagel was already 78 years old, and he was one of the last surviving logical empiricists (next to Hempel, Herbert Feigl, and Gustav Bergmann). Thus it appears more than appropriate for him to assess the impact and legacy of the movement in general and the situation of philosophy of science as a profession in particular (one that had gained a strong foothold mainly due to the efforts of the logical empiricists and Nagel himself).

In the introduction, Nagel makes a distinction, broadly fashionable in the 1970s, between the old and the new philosophy of science. The former refers (mainly) to the logical empiricism of Carnap, while the latter centers on the views of Kuhn, Feyerabend, Lakatos, and many sociologists. In this account, the old philosophers of science claim the neutrality and formally assessable character of the observation

[13] See also Koslow (2012), Suppes (2012), Chen (2019); reduction is still the most frequently discussed Nagelian topic. See Klein (2009), van Riel (2011), Kaiser (2012), and Schaffner (2012).

language while locating the rationality of science in its conformity to formal rules. The new philosophers of science, however, attack the "alleged ahistorical character of the 'orthodox approach'" (1979b, pp. 2–3), according to Nagel, who argues for a new vision about how science is done. At this point, Nagel aims to build bridges between these two approaches. He not only calls attention to the fact that while the old view indeed covers most of Carnap, but also notes that it ignores the writings of most pragmatists and such logical empiricists as Philipp Frank and Richard von Mises.[14] Furthermore, he claims that new philosophers of science were right in arguing that defenders of the orthodox view

> have failed egregiously to construct a calculus, with the help of which it could be shown that the methods of science are effective instruments for arriving at the truth […]. However it does not follow from this, as some of those proponents have also maintained recently, that such effectiveness of scientific method cannot be shown at all, or that in evaluating the evidence for a theory of formal relations between evidential statements and theory can be completely ignored. (Nagel 1979b, p. 4, original emphasis)

In other words, it does not follow from the historical failure to identify the right formal characteristics of the scientific method that one should "completely ignore" it.[15] While Nagel exaggerates greatly here (given that the new philosophers of science did not motivate their method by the failure of the older generation, but instead brought new substantive arguments to the table), he certainly makes a very valid point about throwing the baby out with the bathwater. Seen from this perspective, he indeed tries to build bridges and to utilize both the old and the new philosophies of science up to a certain point.

Nagel thus always stayed close to the latest developments in philosophy of science. While he noted, in his 1983 interview, that he was less able to follow the recent advances in theoretical physics in the 1970s and early 1980s, his philosophical works matched the general atmosphere. Many of his students and colleagues paid attention to his nuanced analysis and meticulous conceptual clarifications. His activities were honored on many levels: he was elected to the American Academy of Arts and Sciences in 1954, to the American Philosophical Society in 1962, and to the National Academy of Sciences in 1977. He also served as President of The American Philosophical Association, Eastern Division (1954–1955), where he delivered his famous lecture on "Naturalism Reconsidered" (1954/1956). He was a corresponding fellow

[14] It would be highly constructive to consider Nagel's relation to Carnap and Frank in detail, especially because Nagel thanked both Carnap and Frank for their invaluable help in his *The Structure of Science* (1961, p. x) and in the introduction to *Logic Without Metaphysics* (1956b, p. xii). Some of these issues are taken up by Thomas Mormann in this volume.

[15] It should also be borne in mind that three decades earlier, Nagel had distinguished between at least four different meanings of "method of science": (a) methods in the sense of different instruments and specialized techniques; (b) methods as various disciplinary approaches and outlooks; (c) methods extracted from the behavior and anthropology of scientists; and (4) methods as ways of evaluating concerning evidence and principles. Nagel (1950b, p. 20) calls the fourth type the "*logic* of inquiry" (original emphasis) and states that the first three presuppose or come down to *different* or *numerous* scientific methods (in fact, the third approach has shown that "there is apparently no such thing as scientific method," ibid.).

at the British Academy, Fellow and Vice President (1951, 1973) of the American Association for the Advancement of Arts and Sciences, and President of the Philosophy of Science Association (1960–1962). He earned numerous honorary degrees worldwide, and in 1997 a lecture series named after him (the "Ernest Nagel Lectures in Philosophy and Science") was established at Carnegie Mellon University.

Ernest Nagel died on September 20, 1985, in New York.

1.2 The Making of Analytic and Scientific Philosophy

While Nagel is widely ignored among analytic philosophers (a notable exception is a recent special issue of *The Journal of Philosophy* devoted to his ideas), during the twentieth century he was among the most influential American philosophers of science. As noted already in the previous section, there were various reasons for this, connected to his teaching, editorial works, public activities, and seminal writings.

Nagel started his philosophical career not long after the birth of analytic philosophy, at a time when its borders, topics, and tools were not yet settled on either side of the Atlantic Ocean. While a few years before, Hook still claimed that phenomenologists form the "strongest analytical group in Germany" (1930, p. 152), and associated Heidegger and Husserl with the notion and method of analysis, Nagel started to take a somewhat more restricted view and did not care much for Husserl, Heidegger, or any related figures. For him, analysis acquired a more focused meaning and became linked to the people it is usually associated within the standard histories of analytic philosophy. From this perspective, Nagel's two-part paper about the "Impressions and Appraisals of Analytic Philosophy in Europe" (1936a, b) played an inestimable role in constructing analytic philosophy as a continent-wide movement, defending rationality and scientific sobriety against the newest irrational waves of history.

All in all, Hook also called attention to the darkening philosophical situation in Europe where a certain Platonic tendency was taking hold of many philosophers and scientists who had been inhaling idealist leanings since their birth. To counterbalance Hook's pessimistic account, Nagel called attention to the fact that "a student of philosophy interested in analysis need not despair that a romantic irrationalism has completely engulfed Europe" (1936a, p. 5). While it was difficult to disentangle and abstract a precise, commonly shared worldview of the numerous practitioners of analysis, Nagel enumerated the following four characteristics: (a) Impatience with system-building and the pursuit of the clarification and analysis of meanings; (b) reflexivity about the aims and methods of analysis in a cooperative enterprise; (c) an ahistorical view of philosophical problems where only errors are depicted via historical narratives; and (d) a common-sense naturalism.

Nagel, similarly to the logical empiricists, tended to reject system-building. Already in Carnap's 1928 *Der logische Aufbau der Welt* and the above-mentioned manifesto, we read that the new (scientific) philosophy condemns the traditional way of pursuing philosophy, namely that one person, sitting in their study, builds up comprehensive systems of the world. Philosophy, according to these views, should be

a cooperative enterprise, where knowledge (whatever that might be) is to be gathered piece by piece as part of a collaborative and democratic effort of philosophers and scientists. This is partially due to the enormous growth in scientific knowledge, which questioned many details of existing systems and "engendered in us an abiding scepticism concerning the cognitive worth of such grandiose speculative constructions" (Nagel 1956b, p. x).

This is even more so as many previous fields of philosophy became scientific disciplines during the nineteenth century—as Nagel puts it, "[i]n terms of specialized subject matter, ethics and aesthetics seem to be the sole remaining philosophic discipline." Despite their importance, "few if any philosophers would consent to such a whittling down of their fields of legitimate activity" (1938/1939, p. 46). As an alternative conception, to save the status and role of philosophy, Nagel (like many of the logical empiricists) offers the following view: philosophy engages in "reflections upon issues raised by the scientific and social developments of their day" (ibid.). The exact nature of these reflections could be debated (linguistic, naturalistic, practical, sociological), but if philosophy is to reflect on the sciences, especially on the theory and practice of science, then philosophers retain a substantial link to the world and society. (Furthermore, this preserves, if not system-building, then at least "a credible generalized perspective for viewing things in their entirety," 1956b, p. x).

Moreover, in linking philosophy to the instigation of the sciences, a faint and minor, though still respectable shadow of system-building remains. By providing reflections on many, if not all of the sciences (from economics to physics, anthropology, and the arts), philosophy facilitates the creation of systematic accounts. One could even claim that Nagel provided such systematic characterizations in *The Structure of Science.*

Regarding (c), Nagel was, in fact, critical of analytic philosophy's conceived ahistorical approach to philosophical problems. In his report, he claims that

> it seems to me that a better knowledge of the history which they condemn would have saved many of the analytical philosophers from serious error; for, it will be seen, the latter frequently discuss the traditional problems however disguised they may be by a different terminology. Moreover, the historical approach, when wisely cultivated, can frequently produce the same kind of intellectual catharsis and dissolution of pseudo-problems as does the analytic method. (Nagel 1936a, p. 7)

Almost two decades later, Nagel highlighted another, somewhat different type of historical sensitivity. Beyond using history to find parallels with our current issues, he frames it as a vast source of alternatives and forgotten routes that could enrich contemporary discussions. As he puts it,

> [m]any of us are interested in the history of science partly because it provides knowledge about ideas not already familiar to us, and partly because it supplies information about the genesis of ideas and the chains of their influence and use. These interests are not readily satisfied if we pursue historical study with the sole intent of using the materials of the past as illustrations for currently recognized methodological principles. (Nagel 1959/1969, pp. 155–156)[16]

[16] This passage is taken from Nagel's commentary on A.C. Crombie's and Joseph T. Clark's papers about history and philosophy of science, published in Marshall Clagett's *Critical Problems in the*

Although Nagel emphasizes that taking history seriously could be as useful as the rigorous method of logical analysis, he simply disregarded the available historical approaches of the 1920s and 1930s when he set his agenda of focusing "on questions of logic and method" (1936a, p. 5). This suspension of historical, as well as of ethical, aesthetical, and socio-political questions in the characterization and presentation of early analytic philosophy had strange consequences. Presumably, Nagel played an important role with his widely read pieces on the specific professionalism and institutionalism of analytic philosophy.[17]

But while Nagel's ahistorical analytic philosophy became widely shared and practiced, his philosophy was deeply enriched by the history of philosophy and the sciences. In this volume, Thomas Mormann argues that Nagel's pragmatism and historical inclination set him apart from Carnap after his early enthusiasm for the latter's formal and ahistorical philosophy. Suffice it to note here that in Nagel's 1969 Festschrift, besides the usual questions about the method and nature of science, a separate section is dedicated to the history of science (with papers by Marshall Clagett, I. Bernard Cohen, Arnold Koslow, Charles Parsons, Philip P. Wiener, and Sigmund Diamond—see Morgenbesser et al. 1969).

But Nagel also had a special relation to sociology and social approaches towards scientific issues. He is one of the few logical empiricists who take the social sciences as seriously as the natural sciences and also devotes significant attention to their methodological questions. Nagel thus focused on functionalism, especially within anthropology and sociology (1953/1956; 1961, pp. 520–535), the methodology and nature of economics (1963), and finally the value-related character and statistical overtones of sociology (1961, Chs. 13–14). Many of these issues are discussed and analyzed in Matthias Neuber's and Raphael van Riel's chapters in the present volume.

But besides the general methodological level, Nagel is at least somewhat interested in more focused questions of sociology of science. We say "at least" because Nagel is somewhat critical of an extended sociological approach towards science in the introduction of his *Teleology Revisited*. There, as noted above, he summarizes the main tenets and outlook of the old and new philosophies of science (especially by contrasting them) and then goes on to criticize what became known as "the sociology of scientific knowledge" approach. At first, Nagel is sympathetic to approaches that account for scientific changes in terms of "external factors" while also utilizing causal explanations. While he admits that there is "compelling evidence" for the idea that many institutional, economical, and socio-political factors influence and make science possible in the first place,

> if one aim of the sociology of science is to explain, in terms of "external" factors […], the genesis and content of scientific ideas, the direction of their development, and their acceptance or rejection by the scientific community, then I am confident that in this respect

History of Science. Nagel notes that he is not a historian of science with first-hand knowledge, for example, of medieval science, but as some of the major points of the discussion turned on "philosophical interpretations," he is "perhaps not completely miscast as a discussant" (Nagel 1959/1969, p. 153).

[17] On Nagel's role in the formation of analytic philosophy, see Schliesser (2013).

> and at present the sociology of science is at best a program of research, not a corpus of warranted conclusions. (Nagel 1979b, p. 5, original emphases)

Nagel thus identifies a greater potential within the materials of the sociology of science than its actual results. His example is Paul Forman's now-famous thesis about the acceptance of indeterminism and quantum mechanics within the German physicist community after the First World War (Forman 1971). While Nagel thinks that Forman's results (about how German physicists tried to adapt themselves to a hostile and romantic environment by changing their commitments within actual physical research) are both "surprising and entertaining," the evidence was "insufficient" (1979b, p. 5). Nagel makes some general claims about the "little solid evidence" or "real evidence" to support Forman's account, but this may not pose a real problem for the sociologist of science because "solid" and "real" evidence are themselves quite problematic terms.

Elsewhere, Nagel is quite explicit about this issue, which is one of the most important presuppositions of a Marxist-like[18] sociology of science:

> There is, to be sure, scarcely any evidence for the widespread view that systems of philosophy, like other manifestations of the human spirit, are simply the "reflections" of the prevailing economic structure of a society, and that there is therefore a point-to-point correspondence between the history of ideas and the socio-economic changes in the life of a people. (Nagel 1947/1954, p. 51)

Again, sociologists of science are not forced to claim that scientific ideas and relations "are simply 'reflections'" of various external factors. Even certain correlations would be enough to stratify our knowledge of the issue at hand and produce both interesting accounts and empirically confirmable results. In fact, Nagel seems to admit something like that in the next sentence: "But even in its most abstract reaches philosophy is surely a commentary upon experience, so that men steeped in similar cultural traditions and confronted with similar materials for reflections will in general adopt comparable modes of viewing their place in the world" (ibid.).[19] Perhaps, in general, the legitimacy of the program comes down to a difference in degree.

However, Nagel also formulates certain factual issues. He claims that there is little real evidence for the idea that "German scientists accepted the new quantum theory *in order to* accommodate themselves to popular winds of doctrine" (1979b, p. 6, original emphasis). But given the context, Nagel's use of the words "in order

[18] In an interview, Joseph Margolis told an interesting story about Nagel and Marxism: At a discussion, John Sommerville—then a well-known Marxist philosopher at Columbia—talked about Marxism. Sommerville "was rather well-known but not a very strong figure, and his commentator was Ernest Nagel. Nagel said, I remember it to this day, after Summerville gave his account: well, since Professor Summerville hasn't told you what Marxism is, I'll begin by defining it, which he then did very well" (Margolis 2014, p. 306). This story once more confirms Nagel's wide and deep knowledge of history of philosophy.

[19] Nagel is concerned here with the continuity or rebirth of European philosophies (such as idealism, voluntarism, and positivism) in America. As he says, "America shares with western Europe a comparable literary and religious heritage, a similar social and economic structure, and above all an identical science" (1947/1954, p. 51).

to" would presuppose an explicit decision and act on the part of scientists to accept a scientific view matching the *Zeitgeist*. Presumably, however, most social scientists would argue that this is not how our relation to the Zeitgeist works and that the lack of real or explicit evidence (e.g. a report or explicit admission from the scientists) does not invalidate the sociological narrative. Furthermore, Nagel expects another approach from Forman, an internalist explanation, namely that "the problems and the data *internal* to their discipline gave them no viable alternative" than to accept the new indeterminate science (ibid., original emphasis). But again, having internal reasons does not exclude the reality and viability of external factors, not even the fact that the latter could outweigh the former.

Seemingly, Nagel was somewhat lost within the sociology of science and argued from the viewpoint of an unrelated scholar from a superior field.[20] But that is not the case. He was involved in a planned research project on the "Sociology of Science" within the Institute for the Unity of Science led by the logical empiricist Philipp Frank. We know from a 1952 letter of Frank to Thomas Kuhn that the Institute was planning a research group to discuss and investigate problems of technique and theory choice; the relation of experiments and concepts; the resistance of scientists to accept new problems;[21] the role of analogy within science; the role of youth within scientific research; the nature and functioning of professional groups; specialization; and the influence of social status and external factors—that is, the major themes within the sociology of science from the 1940s to the 1970s.

The invitation (which Kuhn declined, without ever sending a reply to Frank) was signed by the three leading scholars of the project, namely Frank, Robert Merton, and Ernest Nagel (though all three were penned by Frank). While in 1952, Robert Merton claimed that "in cooperation with the American Academy of Arts and Sciences, Philipp Frank has lately gathered together a group of scholars to carry forward empirical and theoretical studies in this field" (1952/1973, p. 219), we do not have any evidence that this project ever started doing any actual work within sociology of science under the aegis of the Institute for the Unity of Science.[22] Nevertheless, this shows that Nagel was interested in questions of sociology of science, and that he probably participated in several discussions about such issues and research with

[20] In 1950, when talking about the methods of science, Nagel noted that scientists should also consult the history and sociology of science to become acquainted with all the ways in which things could go astray. Nagel's list contains factors other than cognitive ones—the premature delimitation of variables, the influence of intellectual fashions and tacitly accepted philosophies, the pressure to justify results to a larger community, and the reluctance to abandon favored but doubtful ideas (1950b, p. 22). This may indicate that for Nagel, the sociology and history of science are mainly the sociology and history of errors, and not those of truth (as others would have it). The same could also be said of history of science; at one point, Nagel claims (in the context of education) that history of science is "the history of magnificent victories as well as of tragic defeats for human intelligence in its endless war against native ignorance, childish superstitions, and baseless fears" (1959, p. 57).

[21] Note that only a decade later, Bernard Barber, who was a close colleague of Robert Merton (one of the leaders of this project), and who also had links to Frank, published his famous paper (quoted in *Structure* by Kuhn as well) on "Resistance by Scientists to Scientific Discovery" (Barber 1961).

[22] For further details, see Reisch (2019, Ch. 10), and Reisch and Tuboly (2021).

Frank (who spent every second semester in New York and thus knew Nagel well) and Merton.

Given his participation in matters as distinctive as a research group in sociology of science and the editorial work with Tarski on the methodology of science, Nagel indeed tried to build bridges and utilize both the old and the new philosophies of science up to a certain point. In his case, these bridges are more solidly anchored within the old philosophy of science. Using Thomas Kuhn's famous terminology, one might say that Nagel argues that philosophy of science should investigate the working mechanisms of normal science, while historical and sociological studies focus more on the grand scale and revolutionary changes:

> To be sure, scientific inquiries are often initiated and subsidized by those concerned with problems of commerce and technology, and the manner in which scientific discoveries are assimilated by a society depends on its economic and political organization. But once a department of inquiry establishes its traditions of workmanship, so the history of science seems to indicate, the course of subsequent developments in it is determined by the materials explored, by the talents and skills available, and by the logic of theoretical investigation. (Nagel 1943/1954, p. 17)

This picture corresponds to the general idea that even though logical empiricists such as Nagel accepted as legitimate some of the concerns of the new philosophers (and sociologists) of science, they remained within their field if actual research was on the table.[23]

A note by Nagel himself seems to support this reading. In a paper about the role and place of the scientific method in general education, he claims that "fact" is a tricky notion and that the "complex character of fact-finding" has to be emphasized. This is so because of the "considerable body of assumptions that enter into any determination of what the facts actually are." This could easily be taken as a confession of the theory-ladenness of facts and observations, but Nagel seems to stop just short of admitting all the radical consequences of such a view.

While he says that "facts do not literally announce themselves to the inquirer, and the sheer immediate experience of qualitative situations does not constitute responsible factual study," he goes on to claim that "immediate experience merely sets the problems for knowledge, so that it is one of the central tasks of science to interpret and suitably characterize the content of what is directly presented" (1950b, p. 21). Full-blooded sociologists of science and constructivist new philosophers of science, however, would argue that facts are constructed all the way down—in other words, it is not simply the case, as Nagel puts it, that "one of the central tasks of science [is] to interpret and suitably characterize the content of what is directly presented" (ibid.). In this sense, while Nagel admits that various theoretical and practical assumptions are built into the "facts" and "concepts" of science (at the observational level), these facts are, in the end, directly observed.

It is a difficult question to assess whether Nagel *has* to go down this rabbit hole; *The Structure of Science* contains many considerations about the most important

[23] In this respect, Otto Neurath and Philipp Frank were, of course, exceptions. See Uebel (2000) and Tuboly (2017).

presuppositions of his philosophy, but as one might expect, it is not possible to give a definite answer simply against the background of the consistency and efficiency of his approach. We cannot provide such an answer here either, of course. What we can turn to, however, is one (if not the) basic element of his philosophy, namely naturalism.

A definition of naturalism, in its most general form, goes like this: "a sound generalized account of the world encountered in practice and in critical reflection, and a just perspective upon the human scene" (Nagel 1954/1956, p. 6). This already indicates that for Nagel, naturalism is a comprehensive issue—it is not a theory, however, but rather something akin to a worldview: "it is the inclusive intellectual image of nature and man which naturalism supplies that sets it off from other comprehensive philosophies" (ibid.). That is, naturalism is a view about nature and the place of humanity in it. This view is based on two pillars: namely the ever-changing considerations of the sciences, and "a distillation from knowledge acquired in the usual way in daily encounters with the world" (ibid.).

Both components are of special significance. On the one hand, the use of the plural concerning the sciences is intentional. As Nagel says elsewhere,

> [c]urrent naturalism, like most other standpoints in the philosophical enterprise, is concerned with issues in moral and social theory. But it is also concerned with much else—for example, with questions bearing on the foundations of logic, mathematics, and the natural sciences, and with problems provoked by the structures of physical and organic processes. (Nagel 1953, p. 156)

Nagel's naturalism thus embraces both the natural and the social sciences. From this perspective, his stance is much closer to Neurath's than to Quine's naturalism (see Uebel (2007) for the latter two). As we will see shortly, this is not due to the unity of science thesis per se (though it is not unconnected), but because of the general conclusions that Nagel draws about the ontology of the world.

What does that mean? Based on the second part of the characterization given above, naturalism is concerned with people who live in societies within the natural world. Nagel's naturalism thus has an ontological thesis: he accepts the "existential and causal primacy of organized matter in the executive order of nature" (1954/1956, p. 7). What matters, in the end, are spatiotemporally located bodies "whose internal structures and external relations determine and limit the appearance of everything that happens" (ibid.). This wouldn't amount to much more than a standard form of materialism or the kind of extreme scientist/physicalist naturalism with a minimal ontology that is common today.

Yet Nagel goes further. Despite the primacy of spatiotemporal entities, the world is not exhausted by them. These entities "do not make illusory either the relatively permanent or the comparatively transient characters and forms which special configurations of bodies may possess" (1954/1956, p. 8). It is quite obvious to knowing subjects who go about their daily lives that "many things noted in experience—modes of action, relations of meanings, dreams, joys, plans, aspirations" also play a role. Nagel's naturalism is thus a pluralist enterprise, taking stock of all the things that occur in everyday experience, and then basing his account on both the natural and the social sciences.

Nagel calls his form of naturalism "contextualistic" or "contextual." Its most important characteristics are the above-mentioned non-reductivist and pluralist basic stance, and a reflexive sensibility towards the "limitations of purely formal analysis" (1947/1954, p. 54).[24] But what is contextual in all of this? Nagel tells us that "There is no absolutely privileged context," as all happenings and events in the world belong to a "humanly valuable phase of existence" (1947/1954, p. 55).

But the contextual element and the contextual stratification of ideas and concepts may also be relevant in another form. At one point, Nagel says the following:

> Insight and imagination are undoubtedly necessary conditions for moral wisdom, but they are not sufficient. For insights and visions may differ, and knowledge of the world and human circumstance must be introduced for adjudicating between conflicting moral ideals. (Nagel 1959, p. 58)

The problem is that we often arrive, by generalizations from local and individual insights, to abstract and absolute rules and norms that should also be valid for others and under different circumstances. As there is no privileged or absolute context within experience, there is no privileged or absolute context within morality either. What matters for naturalists are human life and human experiences in all their circumstances, which they study and offer for policymakers for their considerations.

Despite our temporary amazement about how morality comes into the picture, Nagel, in fact, was very concerned about the relations between science, philosophy, society, politics, and morality. As a naturalist, interested in human experience and the human way of things within nature, Nagel was not a "quietist" in socio-political and cultural matters—he was an early practitioner of what later came to be called *socially engaged philosophy of science.* Nagel indeed thinks that science, and thus philosophy of science, could bring about changes in the world; as Eric Schliesser puts it in his chapter in the present volume, 'philosophy of science was a template for a democratic society'. Or as Nagel says,

> [t]he basis for a general outlook on the place of man in nature is supplied by detailed knowledge of the structure of things supplied by the special sciences—an outlook that contemporary philosophy of science has helped to articulate and defend. (Nagel 1954b, p. 307)

Knowledge in science can thus be utilized to understand the structure of mankind: but this is a double-edged sword, as the dissemination of misleading science could put us on morally worrisome paths. Nagel claims that he was dismayed by the rising tide of irrationalism (in the 1930s), which was caused, or at least exacerbated (often unintentionally) by scientists and writers who were exposed to the methods of scientific inquiry during their education but failed to properly absorb it. For him, this means that "it is possible to 'do science' with reasonable proficiency, without possessing a mature and cultivated understanding of what one is doing and of how such doings are warranted" (1950b, pp. 22–23). That is, having a wrong or misleading view of science

[24] One might even wonder how Neurath, given his non-reductive, pluralist naturalism, could disagree to such an extent with Nagel. Moreover, Neurath also repeatedly called attention to the dangers of formalism and systematization throughout his writings in the 1930s and 40s.

could have erroneous and dangerous effects on the moral guidance of society. Good scientists can be bad methodologists and knowledge disseminators in the cultural domain.

Later, however, Nagel went even further in emphasizing the strong connection between science and morality:

> Proposed moral ideals must be congruous with the needs and capacities of human beings, both as biological individuals and as historically conditioned members of cultural groups, if those ideals are to serve as satisfactory guides to a rich and satisfying human life. The adequacy of moral norms, and of proposed resolutions of moral conflicts, must therefore be evaluated on the basis of reliable knowledge acquired through controlled scientific inquiry. It is in consequence simply grotesque to imagine that anyone today can exercise genuine wisdom in human affairs without some mastery of the relevant conclusions of natural and social science. (Nagel 1959, p. 58)

The relevant conclusions and methods of science are the product of a "judiciously skeptical and yet tenaciously reasonable temper" (1950b, p. 23), and that temper is also codified into contextual naturalism. As science has its boundaries, philosophy should lend it a hand by making explicit its presuppositions and logical relations, and by delivering its hidden worldview. It is all the more urgent to take a clear look at science as "[i]t is not an exaggeration to claim that the theoretical understanding that the sciences provide is the foundation for a liberal civilization and a humane culture" (1959, pp. 58–59).

It is not at all obvious that science education or the dissemination of philosophy of science in themselves would suffice to bring about the "moral enlightenment" (1947/1954, p. 57) that Nagel envisions through contextual naturalism. But he is aware of this problem, noting that "since even partial success is an important contribution to the development of a liberal intelligence, the prize is not unworthy of our best efforts" (1950b, p. 23). By means of gradual changes and the consistent use of the "self-corrective method" of science based on "evidence capable of public inspection"[25] (1947/1954, p. 57), the world and humanity's place in it can be altered "almost beyond recognition" (1952, p. 44; cf. 1954/1956, p. 11).

It is no surprise then that Nagel was a well-known figure both within academia and among the public. His engaged and disciplined critical attitude, which comprised not just the mastery of conceptual philosophical argumentation, historical sensitivity, and sober and penetrating scientific knowledge, also provided a moral benchmark. As such, Nagel lived up to his ideals:

> And so I confess I find that being a philosopher interested in only these rather specialized problems is not something that I can take to my bosom. I always thought that philosophy ought to be in a position to dirty its hands in other people's business and offer some sort of a critical evaluation. So, unlike some of my friends and contemporaries, I think of philosophy as a critical occupation and that you can make some sort of a contribution to life even in a specialized society such as ours, provided that you didn't make philosophy a rather narrow discipline—another specialty among other specialties. (From the interview with Nagel published in this volume)

[25] Public inspection, intersubjectivity and accountability are such terms and norms, which also played a significant role within the so-called left-wing of the Vienna Circle; see Uebel (2020).

Nagel's naturalism and philosophy of science is anything but a form of withdrawal from the socio-political, cultural, and moral concerns of citizens. During the heyday of the Cold War, when most logical empiricists put on their new quietist garb (see the account in Reisch 2005), Nagel was one of the few authors who still raised his voice for liberal democracies and the immanent relation between science, philosophy, and morality—thus emphasizing the *responsibility*[26] and *accountability* of scientists and philosophers—without impairing his scholarly reputation one bit. In fact, at a time when even the slightest tinge of socialism would be reason enough to decry someone as a fellow traveler, Nagel was still able to hint at the possible coming of governmental regulations:

> There is finally the fear that an automatic technology will encourage the concentration of political power; that authoritarian controls will be established for all social institutions—in the interest of the smooth operation of industry and of society but to the ruin of democratic freedom. This forecast is given some substance by the recent history of several nations, but the dictatorships differ so greatly from the Western democracies in political traditions and social stratifications that the prediction has dubious validity for us. Nevertheless, one element in this grim conjecture requires attention. Whatever the future of automatic control, governmental regulation of social institutions is certain to increase—population growth alone will make further regulation imperative. It does not necessarily follow that liberal civilizations must therefore disappear. (Nagel 1952, p. 47)[27]

Nagel is quite optimistic about the prospects of liberal democracies; though there are problems to discuss, the emerging difficulties and new technologies would also provide "fresh opportunities for the exercise of creative ingenuity and extraordinary wisdom in dealing with human affairs" (ibid.).

Having argued for decades for the inclusion of scientific methodology and philosophy of science in the educational curricula of liberal societies, the agenda of Nagel, but also of Frank, Neurath, and many other logical empiricists, gained a new foothold during the last decades of the twentieth century. Philosophy of science became a well-established discipline, with many journals (some of the most important ones initially edited by Nagel himself), associations (led by Nagel),[28] and institutions (shaped by Nagel's students), while many universities introduced courses in the philosophy and history of science as part of their curricula in the natural and human sciences.

But as it often happens, life did not live up to our expectations. After so many cultural, political, economic, and—more recently—public health crises, science

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[26] In these contexts, Nagel often uses the term "responsibility." See, for example, his introduction to *Logic Without Metaphysics* where he talks about the "responsible critiques of cognitive claims" (1956b, p. ix).

[27] This paper was an editorial-like introduction to an issue of *Scientific American* about automatization in industry and the workplace. Nagel argues further that automatization, mechanization, the extreme use of quantitative issues and algorithms would not deprive humanity of all qualitative considerations and values immanent to our life and thinking. He even says, "there is no reason why liberation from the unimaginative drudgery which has been the lot of so many men throughout the ages should curtail opportunities for creative thought and for satisfaction in work well done" (1952, p. 47).

[28] Regarding many of these journals and associations and their role in professionalizing the philosophy of science, see Dewulf (2021).

education and philosophy of science proved unable to overcome all the skeptical voices sowing baseless doubts. While in most places, the "substance of a liberal democracy," namely "freedom of inquiry, freedom of communication, and freedom to participate actively in decisions affecting our lives" are preserved and enlarged (1952, p. 47), the "merchants of doubt" that create controversies, dissent, confusion, and divergence continue to be among us, ingeniously using basic values to turn them upside down (Oreskes & Conway 2010). Nagel faced similar issues already back in the 1930s, then in the 1940s, and finally in the 1950s. As he formulates the issue,

> in the midst of actual and impending disaster, men are inclined to listen to any voice speaking with sufficient authority; and during periods of crises, when rational methods of inquiry supply no immediate solutions for pressing problems, spokesmen for institutional and philosophic theologies find a ready audience for a systematic disparagement of the achievements of empirical science. Ideas which the advance of knowledge had partially driven underground during periods of fair social weather, are then insolently proclaimed as panaceas for public and private ills. The assured methods of scientific control and understanding, because they effect no wholesale resolution of problems and because they yield no conclusions beyond the possibility of error and correction, are then declared to be unsuitable guides for rational living. (Nagel 1943/1954, p. 18)

As we noted at the beginning of this chapter, Nagel has often been dropped (in most cases unintentionally) from the histories of analytic philosophy and philosophy of science. Whatever his recognized merits, Nagel tried to do his best not just to promote a resilient union of logical empiricism with pragmatism and naturalism, but to pursue each discipline to its limits to see what philosophy of science has to offer to society. As he put it in 1938, the newly arrived (at least in the American context) movement of logical empiricist and unified science "is an important arm in the interminable warfare against obscurantism and for clarity" (1938/1939, p. 59). That war is not over yet, and Nagel could thus be a useful ally in *our fight for clarity*.

1.3 The Essays in This Volume

In the first part, "Nagel in the History of Philosophy," Yvonne Nagel provides a short overview of Ernest Nagel's life, based on his correspondence with members of the Vienna Circle and other philosophers and scientists. Next, Sander Verhaegh reconstructs Nagel's philosophical development, focusing especially on the philosophical influence of John Dewey, Morris R. Cohen, Rudolf Carnap, and Hans Reichenbach, to shed new light on Nagel's intermediating endeavors by reconstructing his philosophical development in the late 1920s and 1930s. Fons Dewulf traces Ernest Nagel's earliest ideas on explanation by investigating his course notes of the 1930s. Through his teaching of Aristotle, as the paper shows, Nagel inspired his New York colleagues to focus on a philosophical analysis of explanation; Dewulf claims that this resulted in Carl Hempel's earliest work on scientific explanation. Finally, Thomas Mormann explores some aspects of Nagel's changing attitude towards the "new" logical-empiricist philosophy. He shows that Nagel's early enthusiasm for

logical empiricism did not last. Nagel's growing dissatisfaction with the Carnapian version of logical empiricist philosophy (which he considered to be ahistoric and non-pragmatist) was clearly expressed in his criticism of Carnap's inductive logic; as Mormann claims, one of the distinctive features of Nagel's philosophy of science is the emphasis that he places on the role of the history of science in the philosophy of science.

In the second part, "The Method(s) of the Sciences," Raphael van Riel reconstructs Nagel's approach to idealization in the sciences and presents his views as a viable option. David Atkinson and Jeanne Peijnenburg examine Nagel's views on Heisenberg and the uncertainty relations. They show that Nagel's argument is even more substantial than he could have envisaged. In particular, it has become clear that there are, in fact, two uncertainty principles. These findings shed new light on Nagel's criticism, enabling us to see that his intuition was surprisingly apposite, while also elucidating where his argument misses the mark. Marij van Strien shows that Nagel, faced with the claim that determinism fails in quantum mechanics, proposed a notion of determinism that does not rely on a fundamental level of description and can thus play a role in different scientific disciplines, irrespective of their reducibility to physics. Nagel argues that determinism ultimately serves as a guiding principle in scientific research. In this way, according to van Strien, Nagel argues that determinism has an enduring relevance in all domains of science, from quantum physics to the social sciences.

The third part, "Issues in General Philosophy of Science," Maria Carla Galavotti considers Nagel's writings on probability. Her paper illustrates how, according to Nagel, the issue of the meaning of probability should be addressed, how Nagel's criticism of the major interpretations of probability accepted in the literature developed over time, and what are the main traits of his "truth-frequency" theory. Bohang Chen revisits Ernest Nagel's view of teleology in biology, showing that Nagel considered it acceptable to eliminate teleological terms in biological discourse, and that he also treated them as anthropomorphic vestiges. Second, after a defense of Nagel's view, Chen uses it to clarify the current biological function debate. Finally, he attempts to deepen Nagel's view, by pushing it to a further and perhaps its logical conclusion, that is, Kant's view of biological teleology.

In the next part of the volume, "The Social Sciences and the Social Role of Philosophy," Matthias Neuber offers a rational reconstruction of Nagel's account of the social sciences from his famous 1961 book, *The Structure of Science*. He proceeds in three steps: first, Neuber sheds some light on Nagel's discussion of the problem of a clear-cut demarcation between the natural and the social sciences; then he focuses on Nagel's interpretation of the presumptive 'immaturity' of the social sciences; and finally, he provides two case studies in order to illustrate the implications of Nagel's analysis. Eric Schliesser explores Nagel's polemics and reveals that these exhibit a two-fold character: (i) to defend liberal civilization against all kinds of enemies; and (ii) to defend what Nagel calls 'contextual naturalism.' Schliesser shows that both characteristics reinforce each other and undermine alternative political and philosophical programs. The chapter responds to an influential and plausible argument by George Reisch that Nagel's professional stance represents a kind of

disciplinary retreat from politics. Part of the argument traces out how Nagel's views on responsible speech and professionalism reflect a distinct understanding of the political role of philosophers of science.

The final, fifth part of the volume, "Previously Unpublished Papers," contains two papers that might be of general interest to the reader. The first is an interview that Remmel Nunn conducted with Nagel in 1983. It covers the main historical and philosophical events in Nagel's life and reconstructs the principal features of his views, thereby also providing important new insights into the history of analytic philosophy from Nagel's perspective. The second paper, which concludes this volume, is the 1961 talk that Nagel delivered at the famous Delaware Seminar on philosophy of science.

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Part I
Nagel in the History of Philosophy

Chapter 2
Ernest Nagel: A Biography



Yvonne Nagel

Abstract This paper provides a summary and overview of Ernest Nagel's life, based on his archive correspondence with members of the Vienna Circle and other philosophers and scientists. Nagel's main works are noted and discussed against his personal background.

Keywords Ernest Nagel · History of philosophy · Logical empiricism · Logical positivism

2.1 Ernest Nagel: A Biography

This biography is primarily based on a large collection of more than 1000 letters kept by my father-in-law Ernest Nagel. The letter writers included many of the leading philosophers of his time. Among them were Sidney Hook, Rudolf Carnap, Carl G. Hempel, Alfred Tarski, and L. Susan Stebbing. I also drew on information contained in the 1983 Oral History he made with Remmel Nunn and the biography of him written by Patrick Suppes for the National Academy of Sciences.[1] The letters show the many personal and professional connections between these philosophers and the role that Ernest played in helping several of them to move to the United States when their lives and jobs were threatened by the rise of Nazism.

Ernest Nagel was born on November 16, 1901, in Nové Mesto nad Váhom (Vágújhely in Hungarian) which was then in the Hungarian part of the Austro-Hungarian empire. His parents were German-speaking Jews. His father, Isadore Nagel had a small dry goods store that sold things like gloves and hats. His mother, Frida Weiss, was from the nearby town of Vrbove. Young Ernő learned to read and write in Hungarian at the local school. His father was a poor businessman. He fled after his business failed without telling his family where he was going. Months later he sent money for them to join him in New York in 1911.

[1] See (Suppes 1994); the Nunn-interview is included in the present volume.

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Although he spoke no English when he first arrived, Ernest thrived in the public schools of New York. He was forced to leave school twice to earn money to support the family when his father's businesses failed again. Nevertheless, he managed to graduate from DeWitt Clinton High School in 1919 with honors.

He spent four years at City College, where tuition was free, earning a BA in Philosophy. While there he was greatly influenced by the philosopher Morris Raphael Cohen. Ernest then enrolled at Columbia University in 1923 to earn a Ph.D. in Philosophy. He made several lifelong friends while at City College and, later at Columbia. Like him, many of them were the children of Jewish immigrants. Two of them were Sidney Hook and Meyer Shapiro. Sidney later taught philosophy at Washington Square College (which was part of NYU), while Meyer taught Art History at Columbia University.

To support himself and pay his tuition at Columbia, he taught Mathematics during the day at a public elementary school. After he earned a master's degree in Mathematics at Columbia in 1925, he taught in high school and in the evenings at a school for adults. Thus he could only attend classes at Columbia which met in the late afternoon. His financial burdens increased after his father died in 1926 because he also had to support his mother and, for a while, his sister. His progress in graduate school was difficult and slow. His situation improved, though, when his City College mentor, Morris Cohen, raised money for a private scholarship for him of $2000/year for two years. Cohen had heard that during his Oral Preliminary Exam, Ernest picked Plato, Plotinus, Spinoza, and Peirce for the four philosophers he was to be quizzed on. When the committee asked why he picked them, he responded that they all represented a strong, rationalistic outlook on the world, an approach he had learned to respect very highly from his City College teacher Morris Cohen.

After earning his Ph.D. from Columbia in 1930 with a dissertation "On the Logic of Measurement", Ernest worked at several temporary academic jobs while he sought a more permanent position. He had a one-year appointment at City College and then worked as an instructor at Columbia University.

Morris Cohen also proposed that he and Ernest should jointly write an introductory text on Logic. They worked on this for several years, sometimes at Yaddo, a retreat for scholars and artists in Saratoga Springs (Cohen & Nagel 1934).

In the fall semester of 1931, Columbia hired L(izzie) Susan Stebbing, (a philosopher from Bedford College in England with connections to the Vienna Circle) as a visiting professor. Ernest was asked to co-teach a course on Logic with her. She had written *A Modern Introduction to Logic* in 1930 (Stebbing 1930). Although he found her intimidating at first, they became very good friends. They shared many personal and professional interests. After class, Ernest and Susan would continue their philosophical discussions with two of the students: Abraham Edel and Ann Zinkin (later the wife of Sidney Hook).

Cohen and Nagel finished their book *An Introduction to Logic and Scientific Method* near the beginning of 1934. Ernest had applied for a Guggenheim fellowship for the 1934–35 academic year and was eager to use it to travel to Europe and meet the philosophers whose works he had read and admired. Many of them belonged to or were associated with the Vienna Circle. His travel plans were shaped by the political

and academic landscape of the 1930s. His outline of his impressions of philosophy in the 1930s gives a good idea of the scope of his travels and his interests.

The philosophers he sought out were a cosmopolitan group who shared his interests in Analytic Philosophy, Logical Positivism, Mathematics, and Logic. Several of them had traveled or studied in other countries. European examples include Ludwig Wittgenstein, Moritz Schlick, and Friedrich Waismann. Wittgenstein was born in Vienna but moved to Cambridge, England in 1911 to study under Bertrand Russell. He returned to Austria during WWI but came back to England in 1929. He held a Fellowship at Cambridge from 1929 to 1941. Moritz Schlick, the leader of the Vienna Circle, had lectured at Stanford University in 1929.

At least two American philosophers who shared Ernest's philosophical interests traveled to Europe in the 1930s and had similar itineraries. They were W.V.O. Quine (Harvard University) and Charles Morris (University of Chicago). Quine traveled in Europe during 1932–33, meeting Lesniewski and Tarski in Poland, and also met with members of the Vienna Circle (including Carnap who was at that time in Prague). Morris was also in Europe during the academic year 1934–35 and he and Ernest met in Prague in September during the Eighth International Congress of Philosophy and presumably at "The Preliminary Conference of the International Congresses for the Unity of Science". Morris also met with members of the Vienna Circle and with Bertrand Russell in England.

These philosophers also traveled to participate in discussion groups of that time such as the Aristotelian Society and the Moral Sciences Club in England, the Vienna Circle in Austria, and the Berlin Circle in Germany. For example, Tarski visited the Vienna Circle and Otto Neurath and Rudolf Carnap visited England.

Many of them belonged to the Unity of Science Movement, whose chief promoters were Otto Neurath in Europe and Charles Morris in the U.S. This group, whose organizers included Ernest Nagel, Susan Stebbing, Alfred Tarski, Philipp Frank, and many others, organized several Congresses for its members in Paris (1935), Copenhagen (1936), Paris again (1937), Cambridge, England (1938), and Cambridge, Massachusetts (1939).

When Ernest's close friend Sidney Hook traveled to Europe in 1928–29, he spent most of his time in Germany, especially Munich and Berlin (where he met and was impressed by Hans Reichenbach). However, after Hitler came to power in 1933, many academics who were Jewish and/or Marxist were forced to leave. For example, Hans Reichenbach, who was Jewish, lost his position in Berlin and moved to Turkey where he had a five-year contract to teach philosophy in Istanbul. Reichenbach's doctoral students, Carl G. (Peter) Hempel and Otto Helmer stayed another year to complete their dissertations, but they both left Germany in 1934. Helmer went to England where he worked on a second doctorate with Susan Stebbing, while Hempel moved to Belgium to work with (and for) the wealthy industrialist and independent scholar Paul Oppenheim.

Ernest avoided Germany altogether since the philosophers he was interested in had all left. He also noted the strong anti-Semitism he encountered, especially in Poland. He wrote to Morris Cohen on November 28, 1934:

> The saddest thing to me in Poland is the plight of the Jewish students, and it is surprising what a consciousness of being a Jew one develops in that place. They have practically no future in the universities, even though they are frequently the outstanding scholars. Both Lukasiewicz in Warsaw and Chwistek in Lwow commented upon the, to them, hardly explicable zeal of their students, almost all of whom are Jewish, who continue to study in spite of abject poverty and without any hope of social recognition.[2]

Throughout his long career, Ernest read and reviewed many books and papers by philosophers in his field. He started doing this when he was an impoverished student and needed the money the journals paid for reviews. Later, he did it mainly to keep abreast of the literature. His ability to remember everything he read made him a valuable resource for his friends. In several letters, one of his friends might say "I am trying to find an article by so-and-so about such and such" and he would come up with the exact reference. He served as an editor of the *Journal of Philosophy* from 1939 to 1956 and of the *Journal of Symbolic Logic* from 1940 to 1945. He kept a small notebook with a list of all his scholarly writings. It shows that his first published review appeared in the *Journal of Philosophy* in 1927. When he died in 1985 there were 365 entries in the notebook, not counting an article that appeared posthumously.

In preparation for his trip to Europe, he arranged to write a report on the Eighth International Congress on Philosophy for the *Journal of Philosophy* (Nagel 1934). The meeting was held September 1–7, 1934 in Prague. At the Congress, he met many members of the Vienna Circle, whose work greatly interested him. Among them were Moritz Schlick (founder of the Vienna Circle), Friedrich Waismann, Rudolf Carnap, Hans Reichenbach, Alfred Tarski, and Otto Neurath. Reichenbach invited him to visit him in Istanbul and Carnap invited him to visit Prague.

During the fall of 1934, Ernest spent almost two months (September and December) in Vienna where he attended the bi-weekly meetings of the Vienna Circle, which spent much of the time discussing Wittgenstein's work, especially the *Tractatus*. He felt most comfortable in Vienna because he spoke German fluently and had relatives there and in nearby towns. He spent time with Moritz Schlick and Felix Kaufmann. Kaufmann was a Jewish member of the Vienna Circle who had strong interests in both mathematics and Jurisprudence. Unable to secure an academic position, he worked as a Privatdozent at the University but supported himself by working for an oil company.

Ernest spent about a month in Poland, most of that time in Warsaw, where he met with Lukasiewicz, Leśniewski, Kotarbinski, and Tarski. Alfred Tarski (nee Teitelbaum) exemplified Polish anti-Semitism. A brilliant student, he became the youngest person to receive a doctorate at the University of Warsaw (at age 23 in 1924). However, he was unable to secure a university position in Poland even though he converted to Catholicism, married a Catholic, and changed his name to the more Polish sounding Tarski. He and his wife worked as high school math teachers. He continued to meet with his professors at the university and served as Leśniewski's assistant when Ernest met him.

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[2] (Nagel 1936, p. 8) commented on this anti-Semitism also in his philosophical report of his journey.

Ernest also enjoyed meeting a younger couple—Janina Hosiasson and her fiancé the mathematician Adolf Lindenbaum. Hosiasson had received her doctorate in 1926 under Kotarbinski. She had also spent the 1929–30 academic year studying philosophy in Cambridge, England, and was eager to practice her English with Ernest. He was happy to have someone he could speak with as he found Polish baffling. After Warsaw, he spent a week in Lwow which he found more congenial. He was quite impressed by Leon Chwistek, a polymath who was an avant-garde painter and literary critic as well as a philosopher and mathematician.

He then traveled to Prague where he spent nearly a month visiting Rudolf Carnap and his wife Ina. They discussed philosophy and Ernest also helped them translate some notices for a lecture tour which Carnap planned to make in America. Like Quine, Ernest found them to be an utterly charming and hospitable couple. He also met the physicist Philipp Frank who was Einstein's successor in Prague.

Ernest realized that several of the people he met were unhappy with their current positions and would welcome a chance to travel to the U.S. and perhaps even stay there permanently. He wrote to both M. R. Cohen and Sidney Hook about this. By now, Sidney had become the Chair of the Philosophy Department at Washington Square College. He made staffing decisions for the department. He wrote to Ernest in February 1935, that he could hire a senior professor for a one-year appointment and asked whether he should make an offer to Carnap or Reichenbach. They decided to ask Reichenbach because he was very unhappy in Turkey. Reichenbach responded positively and Sidney proceeded to talk to University officials about hiring him. He also solicited letters for Reichenbach, including one from Albert Einstein who was now at the Institute for Advanced Study in Princeton, NJ. After months of work, however, Reichenbach cabled that the Turkish government would not release him from his contract.[3]

In January, Ernest traveled to Nice, France, to meet his fiancée, Edith Haggstrom. They had planned to spend a month there to marry. However, they were unable to satisfy the French requirements for marriage and so continued their travels, claiming they had been married in Nice. Ernest was anxious to complete his ambitious plans to meet philosophers. They traveled the length of Italy, combining tourism with philosophy. The highlight of Ernest's Italian journey was meeting the philosophers (and mathematicians) Federigo Enriques, Gino Fano, and Tullio Levi-Civita in Rome. He also had a memorable luncheon with George Santayana. Santayana had resigned from his position at Harvard in 1912 to spend the rest of his life in Europe.

Edith and Ernest then traveled to Holland, where Ernest wanted to meet the Intuitionist philosophers Mannoury, Brouwer, and Freudenthal in Amsterdam, with Heyting in Enschede and with Otto Neurath in The Hague. Neurath, one of the founders of the Vienna Circle, was a logical positivist and the driving force behind

[3] In a letter of April 16, 1935, Sidney Hook told Ernest that Reichenbach was not released from his contract and thus he "gathered to-gether all the books, reprints and articles of Carnap" to present them to the Dean in order to improve Carnap's chances for a one-year contract at the university. On how Reichenbach was able to go to America, after all, see (Verhaegh 2020).

the Unity of Science Movement. When Ernest visited him, he was promoting *Isotype* an International Picture Language used to graphically display data.

He spent the final months of his trip to Europe in England. Professor Stebbing, who was President of the Aristotelian Society from 1933 to 34 had invited him to attend the Joint meeting of the Society with the Moral Sciences club which would be held July 5–8, 1935 at Bedford College where she taught. They arrived in England in May and went to Cambridge where Ernest hoped to attend some lectures by G.E. Moore and Wittgenstein. He wrote to M.R. Cohen (on June 4, 1935) about his experiences there:

> I made a vain attempt to be admitted to Wittgenstein's class, and went to see him for the purpose. He tore his hair, said it was impossible, that I couldn't possibly understand anything, that I was a tourist and he didn't like tourists, that he wouldn't be able to lecture with a strange face in the room, and that I should "spare him" that suffering. But I have been able to learn something about what he is doing from some of the students and certain mimeographed lectures which he had distributed. He has changed his position enormously since the "Tractatus", and now is interested in "dissolving" philosophic problems by showing that they arise from violating the grammar of language. He thus tries to establish the meaning of words by examining, and artificially constructing, the usages in which they occur; and apparently shows that the "problems" arise because one meaning of the term, as exhibited in one usage, is transferred by analogy to another and different usage. What I have seen and heard is very interesting, but not so novel or revolutionary as the students and dons here seem to think. His almost pathological personality has seized the imagination of almost everyone here, and Wittgenstein is the main oracle to which the faithful gather.

In July, they traveled to London, meeting Susan Stebbing, Max Black, and A.J. Ayer there. Ernest and Edith married in Greenwich, Connecticut after they returned to New York in August 1935. He continued to work as an instructor at Columbia University while she worked on a Ph.D. in Physics at Columbia.

They soon started receiving inquiries about traveling to the U.S. from their European contacts. The Carnaps arrived in January 1936. Charles Morris had arranged for him to visit Chicago for three months (from January through March). He had also been invited to give a course during the six-week summer session at Harvard University. In between, he hoped to travel around giving lectures. Ernest arranged for him to talk at Columbia University. Ernest also attended his summer course at Harvard, staying with the Carnaps in their rented apartment. Edith stayed in New York so that she could attend a summer course taught by Enrico Fermi at Columbia.

Susan Stebbing had introduced them to her student Olaf Helmer. Helmer asked for advice about immigrating and finding a job. He planned to marry his fiancée and bring her with him. He asked Ernest about schools he should apply to and even whether he should try to find work as an au pair.

Susan kept Ernest informed about developments in England and the Continent—especially about the flood of refugees who crossed the channel as the Nazis expanded their territory. She was a member of the Academic Assistance Council (AAC). It was founded in April 1933 by William Beveridge to help the many academics at German universities who lost their positions when Hitler came to power. They offered a small amount of money (£ 100 per year) to refugees and help in finding housing and jobs. On September 3, 1936, she wrote to Ernest

> At the Joint Session in St. Andrews an appeal was made to members of Analysis asking them for help to keep Popper in this country. I promised to contribute if anything could be done. But it seems doubtful. There are so many refugees here and most of us already have committed ourselves to supporting more than it is easy to keep going.

Karl Popper wrote three letters to Ernest in 1936. He had found his position in Vienna untenable because of anti-Semitic remarks from his colleagues. He thought it might be easier to find a position in the U.S. than in England and so planned to come even without a job offer. He asked Ernest for permission to list him as a reference (along with Carnap, Niels Bohr, G.E. Moore, and Bertrand Russell). He thanked Ernest for responding promptly and warmly. Popper had visited England in 1935–1936 and tried to find work there. In 1937 he found a position in New Zealand.

Carl Hempel, whom we shall call Peter as his friends did, wrote to Ernest from Belgium. He wanted to leave Europe because his wife, Eva Ahrends was Jewish. He later became one of Ernest's closest friends.

Carnap was more successful in finding an academic position. In 1936 he received two offers—one from Princeton University and the other from the University of Chicago. He accepted the Chicago offer because it paid a little more but more importantly he had a lighter teaching load and he would only have to teach graduate courses. He was also allowed to hire two assistants. He asked Ernest to recommend some Americans. However, the stipend for these positions was cut from $1500 to $1000 for the academic year, which was not enough to live on. In the end, he hired Helmer and Hempel as the assistants for the academic year 1937–38.

Ernest tried to help Peter Hempel to find a more permanent academic position. For example, he wrote to Eliseo Vivas at the University of Wisconsin. However, Vivas replied that there were no positions there. He did succeed in getting Peter a summer job at City College in 1939, letting the Hempels stay in their New York apartment while the Nagels spent the summer at their home in Wardsboro in southern Vermont. They had purchased it in 1936 after visiting the Hooks who owned the house just up the road from theirs. He also was able to get Peter a more permanent position at Queens College in Flushing, NY in 1939.

A series of events showed how precarious the situation was becoming in Europe: On June 22, 1936, Moritz Schlick was murdered by a former student. This led to the disintegration of the Vienna Circle. Schlick's student Friedrich Waismann lost his position in Vienna and fled to England. The AAC asked Ernest if he could arrange a visiting position for Waismann at Columbia, but he wasn't able to do this. Although Ernest had taught at Columbia since 1931 as an instructor he was not promoted to Assistant Professor until 1937 and then only because other colleges like Oberlin wanted to hire him.

The Anschluss (or Annexation of Austria by Germany) on March 12, 1938, caused many Jews to leave. Ernest received many letters from both academics and relatives pleading for help. Applicants seeking non-quota visas had to obtain an affidavit from a U.S. citizen who pledged to provide $500 in support. One relative from Vrbove asking for Ernest's help even suggested that he might state that his reason for traveling to the U.S. was to exhibit his wares (he was a shopkeeper) at the 1939 World's Fair in Flushing, NY!

There was a lively exchange of letters between Ernest Nagel and Susan Stebbing in London and between Ernest and Ina Carnap (Rudolf Carnap's wife) about the refugee situation. All three were actively involved in helping refugees both academic and non-academic. Susan had enjoyed her 1931 visit to Columbia so much that she kept promising Ernest that she would return "next year". However, it never happened. She was very busy as Principal of The Kingsley Lodge School for Girls (where she also lived) and teaching Philosophy at Bedford College, London (Chapman 2013). In 1933, she had become the first woman to hold a philosophy chair in the United Kingdom. She had a very heavy teaching load. In 1938 she wrote Ernest that she had 17 lectures a week, from 5 to 8 coachings and 12 boards that term. As a member of the AAC she helped refugees like Popper and Neurath (who were forced to flee the Hague when Germany invaded Holland in May 1940). In 1939 she and her friends Vivian Shepherd and Hilda Gavin, co-proprietors of the Kingsley School, bought a large property at Tintagel in Cornwall on the West Coast of England and moved the school there because they felt it was safer than London. They also adopted three orphans.

Susan and Ernest had an interesting exchange of letters about jobs in 1938–1939. Ernest received an offer from City College for a position as Associate Professor at $5800 per year (at the time, he was an Assistant Professor at Columbia). They wanted to hire him to replace Morris Cohen who planned to retire and then teach at the University of Chicago for three years. Ernest was tempted but Columbia countered by offering to promote him to Associate Professor. In the end, he decided to stay at Columbia because he would then be able to teach graduate courses.

In England, G.E. Moore, whom Susan Stebbing greatly admired, planned to retire in 1939 and he urged her to apply for his chair in Cambridge. She did not get the job (which went to Wittgenstein) but she confided to Ernest that one of the electors told her that she and Wittgenstein were the final two candidates.

Ina Carnap and Ernest shared information on refugees as in this letter (dated 11/26/1938):

> We too are swamped with refugee letters and don't seem able to do anything for most of them. Zilsel has escaped to London by now and will wait there for his visa for the States. There are two different points about giving affidavits. In the beginning of the flood the Jewish Charities told me to encourage everybody to sign affidavits for prospective immigrants promising, they would take care for them, once they are in this country. They seemed eager to get in as many Jews as possible. If one could be sure about this status, i.e. be sure that the people coming would not be a financial burden upon you, there might be the way out to borrow money, depositing it in your name, and sign affidavits on the basis of this "property" for your relatives. But I am not sure how much one can depend upon these organizations. I do think, though, that it might be very wise on your part to make an appointment at the Jewish Charities and to talk over with them the cases of your relatives. The Chicago Committee told me it was absolutely wrong that I helped people to come over and then tried to work out plans for them as best as I could. They said, this was their duty and privilege and of course they can do it much more effectively than I ever could. Well, that's that. But I think, it might perhaps ease the financial burden on your shoulders if you send your relatives right away to these organizations and discuss their cases perhaps already before they come as well as the cases of your people whom you had to turn down. The Chicago Committee asked me to deposit with them all the pleas for affidavits which I cannot fill promising that they would try to

> find affiants. I have not heard yet that they have done something, but at least there is some hope. I know of another case in San Francisco, where the Jewish Charities have pledged themselves in advance that they will support a certain family for such and such an amount, if their relatives pledge to contribute so and so much (I don't know the percentage). So you certainly should try to find out before you assume the whole burden.

Ernest also helped several of the Polish philosophers he had met in 1934, especially Alfred Tarski. Tarski had left Warsaw in August 1939 to attend the Fifth International Unity of Science Congress which was held in early September 1939. After he left, Germany invaded Poland and he could not return. As Ernest said in his 1983 Oral Interview with Remmel Nunn:

> Tarski had come over from Poland and for a number of years he was footloose—he couldn't get a job—and so he went the rounds lecturing. And it was suggested that he ought to give a lecture or two to the department of mathematics. They didn't know who Tarski was, but he eventually was invited to give a talk and what he had to say was of no interest to any of them. (See in the present volume.)

Ernest was active in the New York Philosophical Circle,[4] modeled on the Vienna Circle and the British discussion groups. It met in its members' homes. He invited students, colleagues, and visitors like Hempel and Tarski to attend and present a paper. It helped the refugees find kindred spirits and also introduced American scholars to the ideas of the Europeans.

He helped Tarski and Alexander Wundheiler with their applications for non-quota visas. They both asked Ernest to help Janina Hosiasson-Lindenbaum get a visa (by then she had married Adolf Lindenbaum). She had planned to attend the Unity of Science Congress but was unable to leave Poland in time. Ernest had met her in Warsaw in 1934. She fled to Vilnius, Lithuania, and tried to continue her research. She even submitted a paper to *The Journal of Symbolic Logic* (Hosiasson-Lindenbaum 1940) which Peter Hempel reviewed and recommended for publication. At that time, Ernest was an editor of *The Journal of Symbolic Logic*.

Tarski leaned on Ernest for emotional support, confiding in him many details about the hardships his wife and family faced in Poland. Ernest wrote letters to support Tarski's visa application and for academic positions. He gave him a radio and a typewriter and even offered him money. He also had many discussions with Tarski before the latter wrote his 1944 paper on the "Semantic Conception of Truth" (Tarski 1944).

Ernest also helped Otto Neurath's son, Paul Neurath, who was imprisoned in the Concentration camps at Dachau and Buchenwald after the Anschluss but was eventually released (on condition that he leave Austria). Paul Neurath came to New York in 1941 to work on a dissertation under the sociologist Paul Lazarsfeld on the social life in the Dachau and Buchenwald camps. Ernest invited him to join the NY

[4] Susan Stebbing mentioned "your discussion group" in a letter she wrote to Ernest in December 1935. There is also a typed note, dated Feb. 17, 1939, about a meeting at 6 pm Saturday in a private apartment of the NY Philosophical Circle to hear J.C.C. McKinsey discuss a paper on Mathematical systems.

Philosophical Circle. He also introduced him to other people who knew his father such as Meyer Schapiro.

In 1943, Edith Nagel and Eva Hempel were both pregnant. They shared books and notes about infants. Unfortunately, Edith suffered a miscarriage in August. Eva did give birth to a son, Peter Andrew, in January 1944 but died shortly afterward. Peter Hempel was overwhelmed by grief over his wife's death and the responsibilities of caring for an infant. Ernest raised money from his friends so that Peter could hire someone to care for the baby and he could work. Hempel spent the summer working with Paul Oppenheim in Princeton, NJ (with occasional input from Kurt Gödel), then took a vacation, traveling to Saranac Lake with the Oppenheims (where they met Einstein who was sailing his boat). He later traveled to Santa Fe by train to visit the Carnaps at their vacation home overlooking Santa Fe.

In 1936, a young lawyer named James R. Newman took one of Ernest's classes at Columbia. Newman was quite interested in Mathematics. He had an ambitious plan to write a book consisting of the best articles on various topics in mathematics such as geometry, logic, and probability. He had several discussions with Ernest about what he should include. In 1956 he published *World of Mathematics* a four-volume library on the literature of mathematics which contains what he felt were the most important essays in the field (Newman 1956). Newman and Nagel also collaborated on the book *Gödel's Proof* (Nagel & Newman 1958) which explains the main results of Gödel's *Incompleteness Theorem* in a way that is accessible to non-specialists.

Ernest Nagel felt that philosophy allowed him to study a wide range of subjects. His curiosity led him to explore many different fields. In addition to his work on logic, causality, and science, he was interested in and wrote on philosophy as applied to history, law, biology, and sociology. After retiring from Columbia in 1970, he continued to teach. For example, he gave a seminar (already back in the 1950s) with Paul Lazarsfeld on the methodology of the social sciences. He also participated in a graduate seminar at the Columbia Law School.

As he said in the 1983 Interview:

> I've tried to live in accordance with my conviction that philosophy is an important discipline not because it solves problems of its own, but because it deals with problems that really are of general concern to people in all areas. (See the interview in this volume.)

Ernest died in New York City on September 20, 1985, leaving behind two sons, Alexander and Sidney.

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Chapter 3
Nagel's Philosophical Development



Sander Verhaegh

Abstract Ernest Nagel played a key role in bridging the gap between American philosophy and logical empiricism. He introduced European philosophy of science to the American philosophical community but also remained faithful to the naturalism of his teachers. This paper aims to shed new light on Nagel's intermediating endeavors by reconstructing his philosophical development in the late 1920s and 1930s. This is a decisive period in Nagel's career because it is the phase in which he first formulated the principles of his naturalism and spent a year in Europe to visit the key centers of logical empiricism. Building on a range of published and unpublished papers, notes, and correspondence—including hundreds of pages of letters to his close friend Sidney Hook—I reconstruct Nagel's philosophical development, focusing especially on the philosophical influence of John Dewey, Morris R. Cohen, Rudolf Carnap, and Hans Reichenbach.

Keywords Ernest Nagel · Naturalism · Logical Empiricism · Pragmatism · Rudolf Carnap

3.1 Introduction

Ernest Nagel is a crucial figure in the history of analytic philosophy. He played an important role in introducing logical empiricism to the American philosophical community but also remained faithful to the naturalism of his teachers at Columbia University and the City College of New York. In thus mediating between American and European schools of thought, Nagel paved the way for an approach that still pervades academic philosophy today. Not only was he one of the most prominent post-war philosophers of science, his contributions attempt to reconcile the empiricists' focus on "limited and determinate problems" (Nagel, 1954/1956, p. 5) with the naturalists' comprehensive perspective on man, mind, and morality.

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This paper aims to shed new light on Nagel's intermediating endeavors by reconstructing his philosophical development in the late 1920s and 1930s. This is a decisive period in Nagel's career because it is the phase in which he first formulated the principles of his naturalism (Nagel, 1931a; 1954/1956) and spent a year in Europe to visit the key centers of logical empiricism. Building on a range of published and unpublished papers, notes, and correspondence, I reconstruct Nagel's philosophical development, focusing especially on the philosophical influence of John Dewey, Morris R. Cohen, Rudolf Carnap, and Hans Reichenbach.[1]

This paper is structured as follows. The first part provides an overview of Nagel's work in the years before he visited Europe. After an analysis of Nagel's naturalism in the early 1930s (Sects. 3.2 and 3.3), I reconstruct the philosophical influence of Dewey and Cohen, Nagel's most prominent "intellectual mentors" (Sects. 3.4 and 3.5).[2] In the second part of this paper, I examine Nagel's first encounters with logical empiricism. I reconstruct his year in Europe and examine his changing perspective on the views of Carnap, Reichenbach, and the Cambridge school of analysis (Sects. 3.6, 3.7, 3.8). I end the paper with an overview of Nagel's contributions to the American reception of logical empiricism (Sect. 3.9) and I reconstruct his attempts to unite his naturalistic *Weltanschauung* with the empiricists' piecemeal approach (Sect. 3.10).

3.2 Nagel's Naturalism

Nagel was a dedicated naturalist from the start of his academic career. In his dissertation, he describes his philosophy as naturalistic and characterizes it as a position about the nature and the limits of "reflective inquiry". Although naturalists often disagree about the "generic traits" of reality, they are committed to a view about the way in which we acquire knowledge about the world. The "content of what is designated by 'nature'" has changed significantly over the centuries but naturalists share the view that our everyday aposteriori methods, perfected in the sciences, have proven to be the only reliable way to find out what the world is like (Nagel, 1931a, p. ii). More specifically, Nagel commits himself to a holistic picture of inquiry, arguing that we cannot fundamentally question our theory of reality all at once. We can only

[1] In addition to Nagel's early publications, this paper is based on material from the Ernest Nagel Papers at Columbia University's Rare Book and Manuscript Library (hereafter, ENP), the Hans Reichenbach Papers at the Archives of Scientific Philosophy, University of Pittsburgh (hereafter HRP), the Otto Neurath Nachlass at the *Wiener Kreis Archiv* in Haarlem (ONN), the Rudolf Carnap Papers at Pittsburgh's Archives of Scientific Philosophy (RCP), and, most importantly, hundreds of pages of correspondence between Nagel and Sidney Hook (Box 22, Folders 8–9, Sidney Hook Papers, Hoover Institution Library & Archives, Stanford University, hereafter SHP). Transcriptions are mine unless indicated otherwise.

[2] See Suppes (1994, pp. 258–259). In focusing on Cohen and Dewey, I do not want to suggest that they were the only U.S. philosophers to influence Nagel's development. A more complete account would also have discussed the influence of, for example, C. S. Peirce, F. J. E. Woodbridge, and George Santayana. For Nagel's early views on Peirce, see Nagel (1933b). Nagel briefly discusses Santayana's and Woodbridge's influences in Remmel Nunn's interview (this volume).

critically examine our beliefs in a piecemeal fashion because every inquiry presupposes "a large body of knowledge, which in *that* inquiry must remain unquestioned" (original emphasis). For Nagel, inquiry is impossible unless it is framed in a wider realm of accredited knowledge: we can modify theories that have been taken for granted in previous investigations but these revisions can be made only "if other uniformities are used with some confidence" (Nagel, 1931a, pp. 56–57).

Nagel's naturalism has important consequences for his view about the goals and methods of philosophy. He provides a lucid characterization of his position in *American Philosophy Today and Tomorrow*, a volume that primarily aimed to collect "philosophic self-portraits" of the younger generation of American philosophers (Kallen & Hook, 1935, p. v). In his contribution, Nagel argues that philosophy should be an attempt to understand the world as it is found. Our inquiries may reveal that our theories require revision but philosophy can never show that our beliefs rest on an illusion:

> Reflection has at its ultimate point of departure the qualitatively diversified world of common experience. [...] It is this world which man tries to understand by unraveling some threads of its structure, thereby making it familiar to himself. [...] The world which philosophy tries to understand should [also] be the world as it is found; and since what is found includes the familiar things and practices of daily life, I am unable to regard any philosophy as honest or tenable which concludes them to be illusions or unreal. (Nagel, 1935/1956, pp. 39–40)

The converse is also true: if philosophy cannot *question* the reality of our theory of the world all at once, we also cannot *ground* our knowledge by means of single "well-laid foundation". Since no belief is sacrosanct, the road to truth is not a "marble stair" that provides security of position but a "treacherous, unblazed path where security of step is *never* guaranteed" (Nagel, 1929a, p. 171). Or, as Nagel (1935/1956, p. 40) put it in his intellectual self-portrait, philosophy cannot "legislate for the sciences".

Contemporary discussions in metaphilosophy often distinguish between methodological and metaphysical naturalisms. Methodological naturalists emphasize the continuity of common sense, science, and philosophy and argue that our aposteriori methods of inquiry are the only reliable road to knowledge. Metaphysical naturalists, on the other hand, defend a view about the nature of reality, arguing that there is no distinct supernatural world of Gods and spirits, no transcendental Absolute, and no Platonic realm of abstract forms. The natural world we encounter in our everyday lives and our laboratories is self-sufficient; it is governed by its own laws and does not depend on a supernatural realm.[3] *Prima facie*, the two theses are independent. One could hold that the natural world is self-sufficient but believe that there are different methods to acquire knowledge about it; and, conversely, the view that our everyday aposteriori methods are the only reliable source of knowledge seems compatible with the presupposition that our everyday world depends on a supernatural realm of existence. For Nagel, however, metaphysical naturalism is a consequence of his methodological commitments. Due to the advances of science, we have a different view about reality than we did a few hundred years ago. In consequence, our views about what constitutes the 'natural' are tied up with the advances of knowledge. It is

[3] See, for example, De Caro and Macarthur (2004), Papineau (2009), and Verhaegh (2018, Chap. 1).

impossible to spell out what it means to say that the natural world is self-sufficient without reference to our current best theories about nature, and hence to the methods we used in arriving at this picture of reality:

> [N]aturalism is [...] not simply a set of fixed doctrines about the system of events which constitute nature. [...] Indeed, the term 'nature' has meant many things in the history of thought, and professed naturalists have rarely agreed [...] as to what are the specific set of principles descriptive of the order of birth and decay of things. In spite of variations in the content of their views, however, they share a common method by which they support them. If there were a heaven inhabited and operated by disembodied spirits, I can fancy some of them claiming kinship with naturalism in so far as they [...] are at pains constantly to submit their principles to verification. (Nagel, 1935/1956, p. 41)

'Nature', in sum, is whatever we get to know by means of scientific method. To profess naturalism is to subscribe to a "method *as well as* [to] a metaphysics" (1931a, p. ii, emphasis added). Our views about the natural and supernatural would be empty if they would not be based on our best scientific theories.

3.3 The Logic of Inquiry

Since Nagel characterizes naturalism as a view about the nature and limits of *inquiry*, it is perhaps not surprising that he spent much of his career analyzing key methodological tools and concepts. Most of his publications from the late 1920s and early 1930s are analyses of notions like convention (1929a), probability (1933a), verification (1934a), and scientific reduction (1935a). The analysis of scientific method, Nagel maintained, is one of the central tasks of the naturalist:

> naturalism [...] is more certain of the general adequacy of its self-corrective method than it is of the unalterable finality of any particular conclusion. [...] And that is why I think the analysis and practice of scientific method is so essential to a thoroughgoing naturalism.[4] (Nagel, 1935/1956, pp. 41–42)

Even Nagel's dissertation, which deals with "the logic of measurement", is a case in point. The analysis of measurement, Nagel argues, is crucial because it is one of the essential tools by which we delimit and fixate "our ideas to things" (1935/1956, p. 17). Nagel's dissertation develops what is nowadays called a representational theory, the view that measurement is the numerical representation of *empirical* relations between objects (Adams, 1966). Whereas measurement was traditionally defined as the assessment of *quantity* and *magnitude*, Nagel argues that these concepts are theoretically superfluous. Building on the work of Bertrand Russell and Norman Campbell, the Cambridge philosophers who first developed an explicitly representational theory of measurement (Michell, 1992), Nagel appeals to Occam's razor in arguing that we can easily do without these concepts (Nagel 1931b, p. 325).[5]

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[4] In an unpublished radio lecture titled "The Philosophy of Science", Nagel makes a similar point, arguing that it is the "prime concern of the philosopher" to analyze and understand "the logical and physical method" of the sciences (May 1932, ENP, Box 7).

[5] For a detailed analysis of Nagel's theory of measurement, see Michell (2004, Chap. 5).

In addition to analyzing central methodological concepts like measurement and probability, Nagel was also interested in the nature of logic itself. Several of his early publications (Nagel, 1929b, c, 1935/1956) deal with the question whether logic is about "words, thoughts, or objects" (Cohen and Nagel, Chap. 1). The problem, however, is that Nagel did not know how to square his naturalism with a satisfying philosophy of logic. He believed that anyone "who is committed to a whole-hearted naturalism" ought to accept that the validity of logic is ultimately grounded in reality (1929c, 708) but he also felt the "incompatibility" between his empiricism and his view that logical laws can be certified "as being true on the basis of reflecti[on]" alone.[6] He occasionally flirted with a (modal) realist account, arguing that "the norm or correctness of logic is based on the possibilities in the nature of things which are the objects of our discourse" (Cohen & Nagel, 1934, pp. 17–21) but also saw that this account was incompatible with his naturalistic commitments.[7] Ultimately, Nagel (1935/1956, p. 50) came to the conclusion that a naturalistic "analysis of both logic and mathematics […] is still but a hope rather than an achievement." His despair was short-lived, however, for he was about to find a naturalistically acceptable philosophy of logic on his 1934–35 trip to Europe.

3.4 Philosophical Influences (1): John Dewey

It is not a coincidence that Nagel, from the very start of his career, described his worldview as 'naturalistic'. For New York philosophy was dominated by naturalists in the interwar period. Especially at Columbia, the university where Nagel spent almost his entire career, a significant portion of the philosophy faculty (e.g. John Dewey, Irwin Edwin, Horace Leland Friess, John Herman Randall Jr., and F. J. E. Woodbridge) identified as naturalist. These 'Columbia naturalists' defended a variety of approaches to metaphysics and epistemology—ranging from Woodbridge's Aristotelian naturalism to Dewey's pragmatic naturalism—but they were all opposed to "ontological and methodological […] dualisms", whether it be a sharp bifurcation between the natural and the supernatural or a strict distinction between empirical and speculative methods of inquiry (Randall, 1944, p. 357).[8]

Nagel's naturalism was especially influenced by the views of John Dewey, one of his teachers as a graduate student. Dewey, like Nagel, emphasized the *methodological* character of naturalism. In *Experience and Nature*, published just after Nagel entered

[6] Remmel Nunn's interview with Nagel (this volume). See Pincock (2017) for a more detailed reconstruction of Nagel's early views about logic.

[7] One difficulty with interpreting the account defended in the first chapter of Cohen and Nagel's *An Introduction to Logic and Scientific Method* is that it is very unclear to what extent Nagel contributed to the book. Archival evidence does not settle the issue, unfortunately. In a letter to Sidney Hook, Nagel complains that only a few phrases in the book are actually his (March 7, 1935, SHP). Carnap, on the other hand, reports that Nagel told him that he wrote the book almost by himself (November 14, 1934, RCP, 25-75-12).

[8] See Eldridge (2004) and Jewett (2011) for a history of the Columbia naturalists.

graduate school, Dewey argues for a "philosophic method" that is modeled on the aposteriori methods used in everyday and scientific inquiry. Dewey (1925, p. 1) dubs this view "empirical naturalism" and contrasts it with the speculative methods of traditional philosophy:

> [The problem with a] non-empirical method of philosophizing is not that it depends upon theorizing, but that it fails to use [it …] as a path pointing and leading back to something in primary experience. The resulting failure is three-fold. First there is no verification, no effort even to test and check. What is even worse, secondly, is that the things of ordinary experience do not get enlargement and enrichment of meaning as they do when approached through the medium of scientific principles and reasonings. This lack of function reacts, in the third place, back upon the philosophic subject-matter in itself. Not tested by being employed to see what it leads to in ordinary experience [...] this subject-matter becomes arbitrary, aloof [...] something which exclusively occupies a realm of its own without contact with the things of ordinary experience. (Dewey, 1925, p. 6)

Most importantly, Dewey had a similar view about the naturalist's job description. Like Nagel, Dewey believed that it is the primary task of the philosopher to study the logic of inquiry. Although Dewey is rarely classified as a philosopher of science today, he was widely viewed as such in the first half of the twentieth century (Mirowski, 2004; Reichenbach, 1939). Dewey had been a practicing scientist until his early forties (he founded one of the earliest psychological laboratories of the country) and his views about logic and scientific methodology—spelled out in his late-career magnum opus *Logic: The Theory of Inquiry* (1938)—formed the core of his pragmatic naturalism.[9]

Finally, there are strong connections between Nagel's and Dewey's views about ethics and politics. Most Columbia naturalists were socially engaged thinkers who played a role in the so-called "New York intellectuals"—a group of liberal-minded socialist democrats who regularly published in leftist journals like *The Nation*, *New Republic*, and, somewhat later, *Partisan Review*. The philosophical wing of this group was led by Nagel's close friend Sidney Hook, who, until his change of heart in the late 1930s, tried to merge Dewey's philosophy with Marxist theory.[10] Both Dewey and Nagel considered moral and political theory to be integral parts of a naturalistic *Weltanschauung*. They both disliked traditional approaches to ethics and rejected the use of static a priori principles to solve moral and political dilemmas. Rather, they believed that moral inquiry should be guided by the same experimental approach that controls our everyday and scientific investigations. Moral and social problems arise when individual desires and interests are to be adjudicated with socially-defined ends. As such, moral judgments should be viewed as hypotheses, which can be tested by analyzing the interacting interests of the members of a social group. In his philosophical self-portrait, Nagel summarizes the view as follows:

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[9] See Brown (2012) for an excellent overview of Dewey's philosophy of science.

[10] For an overview, see Cooney (1986) and Reisch (2005, Chap. 3). Nagel's own engagement with political and social affairs is evinced by his regular contributions to the above-mentioned periodicals. See, for example, "Dialectical Materialism in Science" (1939/1956), "The Historian as a Moralist" (1940/1956), and "On the Philosophical Battlefront" (1948/1956).

> I find it difficult to understand conceptions of the good which assign values an objective ontological status, independent of organic interests which generate desire. [...] This does not mean that the good or a value is to be identified with the objects of momentary desires. Desires, needs, or their objects are not primarily moral or immoral. Moral considerations arise only when [...] an organization of various interests is attempted. [...] The judgment that anything is a good [...] is a hypothesis which must be explored and evaluated in ways similar to those employed in the sciences. (Nagel, 1935/1956, pp. 50–51)

Considering the strong similarity between their methodological naturalisms, their self-professed job descriptions, and their moral and social views, it is perhaps not surprising that Nagel expressed his deep gratitude to Dewey in his dissertation, arguing that he found in the latter's work "a basis for whatever philosophy I can also call my own" (Nagel, 1931a, p. i).

3.5 Philosophical Influences (2): Morris R. Cohen

Despite the programmatic connections between Nagel's and Dewey's philosophies, the former's day-to-day contributions strongly diverged from those of his teacher. Dewey rarely employed what he called "modern formalistic logic" (1938, p. 182) and he seldom made use of its tools in the analysis of inquiry.[11] Nagel made more adequate use of the recent advances in the formal sciences, especially when they could help illuminate key methodological notions. Whereas Dewey, in *The Quest for Certainty* (1929), provided only informal characterizations of key methodological topics like measurement and probability, for example, Nagel aimed to provide more precise analyses by adopting an axiomatic approach to measurement (1931a) and by developing a truth-frequency interpretation of probability (1933a).

A second major difference between Nagel and Dewey is that the former was better versed in the European literature. Nagel had been born and raised in Nové Mesto, Bohemia (then part of the Austro-Hungarian empire) and had emigrated to the United States at age 10. Whereas Dewey was almost exclusively concerned with American philosophical and societal debates and predominantly responded to discussions that figured in New York-based publications like *The Journal of Philosophy* and *New Republic*, Nagel was also influenced by developments on the continent on which he was born. His analysis of probability, for example, explicitly built on work of German contemporaries like Hans Reichenbach and Richard von Mises, and his theory of measurement was based on Russell's and Campbell's representational theories (see Sect. 3.3). Indeed, Nagel's knowledge of the continental literature is evinced by his numerous reviews of especially Germanophone works in the philosophy of science.[12]

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[11] Dewey's notion of 'logic' was more continuous with the traditional conception of Aristotle, Kant, and Mill. See Brown (2012, Sect. 4) for a reconstruction.

[12] See, for example, Nagel's reviews of *Die Form des Erkennens* by Wilhelm Grebe (1930), *Das Unendliche in der Mathematik und seine Ausschaltung* by Felix Kaufmann, and *Geschichte der Naturphilosophie* by Hugo Dingler (1932).

Nagel's cosmopolitan outlook and formal approach to philosophy of science were likely stimulated by Morris R. Cohen, Nagel's most prominent mentor during his student years. Cohen, who spent most of his career at the City College of New York, was an all-round philosopher who contributed to a variety of disciplines—ranging from the philosophy of history to the philosophy of law—but primarily viewed himself a logician. Like Nagel, Cohen had been born in Eastern Europe and emigrated to the United States in his early teens. In the first decades of the twentieth century, he had played an important role in the American reception of the new logic (e.g. the contributions of Frege, Peano, and Russell) via his reviews and publications.[13] Nagel and Cohen's shared approach is memorialized by their *Introduction to Logic and Scientific Method* (Cohen & Nagel, 1934), one of the first logic textbooks in the United States.

Philosophically, Cohen has been described as one of the most influential naturalists of the first half of the twentieth century (Larrabee, 1944, p. 352 and Randall, 1944, p. 363). Although he was wary of labels and never explicitly identified as a naturalist, he was viewed as such by Nagel, who described him as a "Platonic naturalist" in a 1928 homage to Cohen:

> Professor Cohen has been, ever since we knew him, a Platonic naturalist. He made many of us take for our own his pious attachment to nature as the source of our being, and accept the methods of the physical sciences as fruitful sources for a metaphysics. To be docile to the lessons of experience, but withal to seek blessedness in the shadow of the ideal. (Nagel, 1928, p. 97)

Cohen's 'Platonism' was opposed to Dewey's pragmatic approach, which the former dismissed as 'anthropocentric naturalism' (Cohen, 1940). The distinction between the two is best reflected in their debate about the 'ontological basis of logic', a discussion that likely influenced Nagel, considering the latter's preoccupation with the philosophy of logic (see Sect. 3.3).[14]

Outside philosophy, Cohen probably also influenced Nagel through his role as a public intellectual. Like Dewey, Cohen was one of the prominent public philosophers of New York, as is evinced by his numerous contributions to especially *New Republic*.[15] Throughout the interwar period, Cohen's City College of New York was known for its political radicalism and many leaders of the 'New York Intellectuals' (e.g. Irving Kristol, S. M. Lipset, and Irving Howe) studied at the proverbial 'Harvard of the Proletariat'. Nagel and Hook, too, had been Cohen's students at CCNY, graduating in 1923, and were likely influenced by the latter's political views. Indeed, both Nagel and Hook would later remember him for his genuine 'liberalism' and contributions to the New York intellectual community (Hook, 1976; Nagel, 1957).

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[13] See, for example, Cohen's review of *Principia Mathematica* by Bertrand Russell and A. N. Whitehead (Cohen 1912), as well as his "The Present Situation in the Philosophy of Mathematics" (Cohen 1911) and "The Subject Matter of Formal Logic" (Cohen 1918).

[14] For a reconstruction of the friendly opposition between Dewey and Cohen, see Rosenfield (1962, Chap. 10).

[15] A 1928 bibliography of Cohen lists more than 50 publications in the *New Republic* between 1914 and 1927 (Lazar 1928, pp. x–xi).

3.6 Nagel's First Encounters with Logical Positivism

Considering Nagel's formal approach, his knowledge of the European literature, and his view that it is the philosopher's foremost job to analyze the logic of inquiry, it is perhaps no surprise that he was interested in the ideas of the Vienna Circle (led by Moritz Schlick) and the Berlin Group (led by Hans Reichenbach), which rapidly started to gain influence when Nagel got his first job at Columbia University. The Vienna Circle had recently published its manifesto *Wissenschaftliche Weltauffassung: Der Wiener Kreis* (Hahn, Carnap, and Neurath 1929/1973) and philosophers of science throughout Europe were actively advertising their novel views about meaning, method, and metaphysics via venues like *Erkenntnis*. In the United States, these views came to be known as 'logical positivism' after *The Journal of Philosophy* published the paper "Logical Positivism: A New Movement in European Philosophy", describing the approach as a synthesis of the positivistic-empirical tradition of Mach and Poincaré and the logical tradition of Frege and Russell.[16]

Nagel appears to have been familiar with the new wave in continental philosophy of science even before logical positivism hit American shores. He occasionally cited work of Schlick and Reichenbach in his earliest publications (e.g. Nagel, 1929a; 1930) and he was the first American philosopher to publish an article in *Erkenntnis* (Nagel, 1931b).[17] In one of his memoirs, Nagel remembers that especially Reichenbach's work made a strong impression on him during his student years:

> I [...] studied with enormous profit [Reichenbach's] *Philosophie der Raum-Zeit-Lehre* shortly after the book appeared in 1928. [...] Here is a philosopher after my own heart, I remember thinking, who takes seriously the classical conception of the task of philosophy to provide a solidly based interpretation of science and its logic. (Nagel, 1978, p. 42)

Although it is unclear whether Reichenbach's work directly influenced Nagel's development, there are strong connections between their views and research interests. They were both concerned with the conventionalist aspects of science (e.g. Reichenbach, 1928; Nagel, 1929a) and they both defended a frequentist interpretation of probability (e.g. Reichenbach, 1932; Nagel 1933a).

Most likely, Nagel learned about Reichenbach's views via his friend Sidney Hook, who had spent a year in Germany on a Guggenheim Fellowship in the late 1920s. Hook had participated in meetings of the Berlin Group and had attended Reichenbach's seminars on philosophy of science and the theory of probability (Hook, 1978). These encounters with Reichenbach had been the philosophical highlight of his

[16] The authors of the paper, Albert Blumberg and Herbert Feigl, were both former students of Schlick and had recently adopted positions at American universities. Schlick himself also spent a period in the United States, accepting a visiting professorship at Berkeley in the 1931–32 academic year. The joint efforts of Blumberg, Feigl, and Schlick contributed greatly to the American reception of logical positivism in the early 1930s. For a reconstruction, see Verhaegh (2020a).

[17] Nagel's *Erkenntnis* paper ("Measurement") was a revised version of a chapter in his dissertation. Paul Weiss, also a former student of Morris Cohen, published a paper in the same issue (Weiss 1931).

year in Europe since German philosophers outside the Berlin Group appeared to be unanimously opposed to his naturalism:

> With few exceptions, German philosophers constitute one great idealist family. [...] It is something taken for granted. [...] In a discussion of the implications of a doctrine, when the professor exclaims, '*Aber, meine Herren, das ist Naturalismus*,' he means his hearers to understand that the position has been reduced to absurdity. Naturalism means Locke and an empty mind or John Stuart Mill and the possibility that $2 + 2 = 5$. [...] William James seems to be the only American philosopher who is known—and he is more often 'refuted' than read. (Hook, 1930, pp. 144–146)

Reichenbach, however, was an important exception. Not only had he, in developing a "naturalistic interpretation of the a priori" proven to be a philosophical ally to the American naturalists (Hook, 1930, pp. 159–160), he was also a kindred spirit in ideological terms. In his "Memories of Hans Reichenbach", Hook remembers:

> I [found] Reichenbach's philosophy congenial to my own pragmatic naturalism. [...] We became even friendlier when Reichenbach discovered that I had strong socialist views. He had never met an American socialist before and seemed as surprised to learn that there were American socialists as some proto-Nazi students at Munich had been when I told them that there was a Jewish proletariat in the United States. (Hook, 1978, pp. 33–34)

Hook appears to have described the views of Reichenbach and his group in several letters to Nagel, who immediately started reading up on his work.[18] It is probably also via Hook that Nagel learned about the views of Rudolf Carnap, who rapidly started to become more influential among European philosophers after the publication of *Der Logische Aufbau der Welt* (1928). In his "Personal Impression of Contemporary German Philosophy", Hook briefly mentions the work of Carnap—one of the latter's first mentions in the American literature—but describes him as less naturalistic than Reichenbach, likely because he knew that the Vienna Circle, at the time, disagreed with the latter's views about the aims of scientific philosophy.[19]

Nagel, too, was critical of Carnap's work in the early 1930s. He was especially opposed to (what he perceived to be) some of Carnap's central theses in the *Aufbau*. In "Verifiability, Truth, and Verification" (1934a), Nagel critically examines Carnap's translational reductionism, the view that "the meaning of a proposition is to be obtained by translating it into others until only such appear whose meaning may be directly apprehended in sense" (Nagel, 1934a, p. 146). Nagel's main problem with this thesis is that it is incompatible with his aforementioned holistic perspective on theory testing (see Sect. 3.2). Since any verifying process has "value only *within* a framework of pre-existing knowledge" (Nagel, 1934a, p. 143, original emphasis),

[18] Unfortunately, Hook's letters to Nagel from the late 1920s are lost. Nagel's responses to Hook, however, reveal that the former started reading *Philosophie der Raum-Zeit Lehre* in the fall 1928, shortly after Hook had arrived in Germany (Nagel to Hook, September 4, 1928, SHP). Hook must also have told Reichenbach about Nagel's work. For a few months later, Nagel writes that Reichenbach sent him a reprint of "Ziele und Wege der physikalischen Erkenntnis" (Reichenbach 1929). See Nagel to Hook (July 23, 1929, SHP).

[19] For a reconstruction of the theoretical tensions between the Berlin Group and the Vienna Circle in the early 1930s, see Milkov (2013), Dewulf (2020, Sect. 2), and Verhaegh (2020c).

there can be no such thing as a directly apprehended proposition. Any test presupposes a large body of knowledge:

> [A]tomic propositions [...] require verification in terms of controlled observation, significant only against the background of theoretical assumptions, and therefore inviting further verification. [...] The doctrine of crucial experiments is inadequate, it is well known, because it is the whole body of relevant theory which is brought to an experimental test, and not an isolated proposition. The theory of [...] the final and indubitable apprehension of atomic facts, is inadequate for the same reason. (Nagel,1934a, p. 146)[20]

Just as we can only test the pendulum law for a particular swinging bob by presupposing a host of knowledge about the experimental set up, the measurement tools, and the behavior of free-falling bodies, we can only test an observational proposition like "this is darker than that' by fixating background conditions about brightness, lighting conditions, and spatial coincidence (Nagel, 1934a, pp. 142–144).

Little did Nagel know that Carnap, about a year before, had started to change his views about what he by then called 'protocol sentences'. Spurred by Otto Neurath, *Vienna's* proponent of philosophical naturalism, Carnap now accepted the view that protocol sentences are relative and revisable (Carnap, 1932, p. 469).[21] Nagel was about to find out, however, for he had just been awarded a Guggenheim fellowship that allowed him to spend a year in Europe to familiarize himself with the newest trends in continental philosophy of science. It was this grant that would soon be taking him to Carnap in Prague, where he stayed for three crucial weeks to talk about syntax, science, and socialism.

3.7 Prague

Nagel boarded an ocean liner on August 18, 1934 and arrived in France about a week later. In his first days in Europe, he attended the Eighth International Congress of Philosophy in Prague. Philosophically, the conference was "dull and fatiguing" (Nagel to Hook, September 8, 1934, SHP). In an extensive report for the *Journal of Philosophy*, Nagel complained that there was a "woeful lack of clarity, of analysis, of appeal to logic and empirical findings" and that "a majority of the papers were simply occasions for despair to all those who do not view philosophy as a substitute for music and poetry" (Nagel, 1934b, p. 600). Nagel was particularly worried because several philosophers were using the stage to argue for fascism and *Kultur-intoleranz*. In his report, he explained that the conference was fraught with "notes of national and social conflict", describing plenary sessions in which speakers defended the fascist state or the view that every genuine culture is intolerant toward all others (Nagel, 1934b, pp. 589–596).

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[20] Note that Carnap never talks about 'atomic propositions' in the *Aufbau*. Nagel's choice of words here is probably influenced by Blumberg and Feigl, who described the *Aufbau* as a book in which "all complex propositions are reduced to sets of atomic propositions which are unanalyzable" (Blumberg & Feigl, 1931, p. 287).

[21] For a reconstruction of Carnap's development on this score, see Uebel (2007) and Carus (2007).

Less controversial was the first meeting of the Unity of Science movement, which had taken place a few days before and where the "nazis were not seen nor heard" (Nagel to Hook, September 8, 1934, SHP). Nagel had briefly met Reichenbach, Carnap, and a number of other key European philosophers of science and had used these exchanges to schedule research visits in Vienna, Warsaw, Lvov, Prague, and Istanbul. He had been especially excited to meet Reichenbach, who turned out to be interested in Nagel's work and was curious to learn more about job opportunities in the United States. Reichenbach was "not happy" with his position in Istanbul (ibid.), which he had accepted after the Nazi government had dismissed him from his job in Berlin, and he had asked Nagel to help him find an American publisher for his book *Wahrscheinlichkeitslehre* (Reichenbach, 1935) in order to promote his work in the United States.

Despite his enthusiasm about meeting Reichenbach, it was Carnap who had the biggest impact on Nagel's development during his first months in Europe. After brief visits to Vienna, Warsaw, and Lvov, Nagel returned to Prague in November to spend three weeks with the German professor. He had been reading the latter's *Logische Syntax der Sprache* in his first weeks abroad (Nagel to Hook, October 3, 1934, SHP) but it was in Prague that Nagel learned about Carnap's new views in detail. Not only did the latter's revised ideas about protocol sentences accommodate his worries about Carnap's translational reductionism (see Sect. 3.6), he was especially impressed with his analysis of traditional philosophical problems, such as the question of the ontological status of logic. According to Carnap, philosophical problems are disguised questions about the syntax of language and to interpret them as questions about reality is to be confused about the nature of philosophical questions. Philosophical theses are often posed in what Carnap called the material mode of speech (in which we talk about facts and things) but should actually be phrased in the formal mode of speech (in which we talk about propositions and thing-words). Already after his first few meetings with Carnap, Nagel writes that he is "much taken with the neatness […] with which [Carnap] gets rid of some skeletons", that he is "half convinced that [his own] frequent to-do with the 'ontological basis' of formal logic is a mistake", and that he is "not sure what is left of the problem when it gets formulated in [Carnap's] 'formal mode' of speaking" (Nagel to Hook, November 19, 1934, SHP). Two weeks later, Nagel even describes himself as 'under the influence of the positivists' and wonders whether Carnap's approach does not offer a more solid foundation for naturalism than the 'muddy' philosophizing of his teachers Dewey and Cohen:

> At this distance, and under the influence of the positivists, Dewey's psychologizings and failures to come to grips with the detailed structure of scientific theories seem very serious shortcomings, and I am sure 'our brand' of naturalism will be better served by overcoming them. I have the sense now that in spite of Dewey's attention to language he has never been able to show convincingly how to squeeze out the Platonism and absolutism from the sciences. And in general, the Columbia philos. dept. seems like a home for poets who have missed their vocation. But this is perhaps a passing mood, induced by contact with Carnap. He really has shown me that a man can have a larger vision, without being simply ecstatic or, as in the case of Dewey and Cohen, very muddy. (Nagel to Hook, December 3, 1934)

Nagel was not only impressed by Carnap's *philosophy*. The impact of the Prague weeks was reinforced by the latter's hospitality. Nagel, who was traveling alone because he was afraid to cause a "scandal of some proportions" if he and his then girlfriend Edith Haggstrom were to travel through Europe as an unmarried couple (ibid., November 28, 1934), had been very lonely during his first months in Europe. In a letter to Hook, Nagel had even described his mood as a state of "near-panic" (September 27, 1934, SHP). He had had a few isolated meetings with logicians in Warsaw and Lvov but he had grown increasingly depressed about living in cities where he did not speak the language and where Jews were treated in an "unbelievably shocking" way (ibid., October 17, 1934). His weeks with Rudolf and Ina, on the other hand, felt like a warm blanket. The Carnaps had been "extremely" hospitable, basically opening their house to Nagel in the weeks that he stayed in Prague[22] and they had been very tolerant about his private affairs, encouraging him to bring Edith to Europe and to let them stay with the Carnaps for a while (ibid., November 28, 1934). Most importantly, Nagel and the Carnaps shared the same political views. Both the former's correspondence and the latter's diary reveal that they extensively discussed the political situation in Europe, their views about the disappointing performance of the social democrats, and their sympathies for communism and Trotskyism, even though they both opposed Trotsky's "petty politics" and the dogmatism of the communist party.[23]

3.8 Cambridge

Nagel left Prague in early December. And although he briefly returned to Austria to attend two meetings of the Vienna Circle, he used most of his remaining months to sooth his 'frazzled nerves'. In January, Edith travelled to Europe and they spent the first four months of 1935 in France and Italy. Nagel felt guilty about the lack of philosophical activity in this period[24] but still decided to cancel his plans to visit Reichenbach in Istanbul. Traveling to Turkey would take too much time and there was a big chance that the latter would be coming to New York as a visiting professor in the next academic year.[25]

Only the final two months of Nagel's stay were devoted to serious meetings with European philosophers. After a few days in the Netherlands, where he visited Arend

[22] Carnap's diary shows that he and Nagel met fifteen times between November 10 and December 5, meetings that often started early in the afternoon and lasted until late in the evening (RCP, 25-75-12).

[23] See, for example, Carnap's diary entry for November 18, 1934 (RCP, 25-75-12) and Nagel's letter to Hook from November 19, 1934 (SHP).

[24] Between January and April, Nagel's only seems to have briefly met Federigo Enriques and George Santayana. In a letter to Hook, Nagel writes: "[W]hen I think of other people going off for a year and returning home with well filled note-books or finished manuscripts … I wish I would never have to return. It will take years for me to catch up" (Nagel to Hook, March 14, 1935, SHP).

[25] See Nagel to Reichenbach (July 22, 1935, HRP, 013-51-02). Hook had offered his old friend a one-year position at New York University. For a reconstruction, see Verhaegh (2020c).

Heyting and Otto Neurath, Nagel spent his last weeks in England. In his college days, Nagel had viewed Cambridge as the "holy of holies in philosophy" (Nagel to Hook, May 18, 1935, SHP) since it was the place where Russell and Whitehead had written the *Principia Mathematica* and where Norman Campbell had developed his representational theory of measurement. Once in England, however, Nagel quickly came to the conclusion that Cambridge was "no longer the intellectual Mecca" it had seemed to be when he was a college student (ibid., June 4, 1935). He attended lectures of G. E. Moore, John Wisdom, and D. C. Broad but was disillusioned by the state of the Cambridge school of analysis:

> I [...] must say that on the whole I am disappointed. It is just analysis after analysis, but nothing seems to come out of it, and in the end they are less clear what it is they really mean than in the beginning [...] the Cambridge style is no longer my ideal of what philosophy should be like. (May 26, 1935)[26]

His first meeting with Wittgenstein was rather disenchanting as well:

> I went to see Wittgenstein this week for permission to attend his lectures. He tore his hair, groaned, said it was impossible [...] that he couldn't lecture with a stranger in the room, that I should 'spare him that', etc.… Most of the people around here think he is absolutely the greatest man that lives and ever lived. Maybe it's true, but I wish I could have some evidence for it. I have seen some notes W. had mim[e]ographed last year and distributed; but they are terribly chaotic, and the few flashes of light they contain are not enough to judge by. (ibid.)

Nagel's period in England also had positive sides, however. He enjoyed meeting Susan Stebbing and was enthralled by A. J. Ayer's and Max Black's talks at a meeting of the Aristotelian Society ("better than anything I've seen at our sessions").[27] In addition, Nagel was pleasantly surprised to learn that there was an undercurrent of pragmatic naturalism in Cambridge. He had a chance to get a hold of some unpublished manuscripts from Frank Ramsey and was excited to read "how far Ramsay [*sic*] went in the direction of a Peirce-Deweyan pragmatism […] advocating in part an instrumental theory of thought and a general 'fallibilism'" (ibid., June 23, 1935).[28] When he expressed his surprise in a meeting with R. B. Braithwaite, the latter ensured him that Ramsey was not the only Cantabrigian to be defend such a perspective, explaining that "the published works of the Cambridge men […] should not be taken as indicating of what [is] the accepted doctrine" (ibid.). Just as Carnap had presented him with a model to combine his naturalism with the clarity and the precision of the logical positivists,[29] Ramsey's manuscripts confirmed that it is possible to reconcile pragmatic naturalism with rigorous analysis.

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[26] See also Nagel to Cohen (June 4, 1935): "I am a little disappointed with what I have found in Cambridge […] perhaps in the days when Russell was here it may have been worth the pilgrimage. I still find the atmosphere invigorating. […] But I am hankering for thicker air" (cited in Rosenfield 1962, p. 400).

[27] See Nagel to Hook (ca. May 1935 and July 9, 1935, SHP).

[28] See Misak (2016) for a reconstruction of Cambridge pragmatism in this period.

[29] In a later report for the Guggenheim Foundation, Nagel would write that Carnap represented the wing of the Wiener Kreis "which converged on the ideas familiar from Peirce and Dewey" ("Impressions of European Universities: The Nineteen-Thirties", undated document, ENP, Box 4).

3.9 Impressions and Appraisals

In July 1935, Nagel returned to New York, where he used his newly acquired knowledge to update the American public about developments in Europe. He wrote a popular piece about logical empiricism in a literary magazine (1936c, p. 59) and he published reviews of work by, among others, Carnap, Reichenbach, Tarski, and Black (Nagel, 1935b, 1936c, 1938a, b, d). Most importantly, Nagel published an influential two-part paper "Impressions and Appraisals of Analytic Philosophy in Europe" (1936a, b) in the *Journal of Philosophy*. In this paper, often credited as one of the first publications to use the term 'analytic philosophy' in print (Frost-Arnold 2007), Nagel describes the views of Wittgenstein, Moore, Schlick, Neurath, Carnap, and the Polish logicians, examining in detail their common focus on analysis and clarification:

> In the first place, the men with whom I talked are impatient with philosophic systems built in the traditionally grand manner. Their preoccupation is with philosophy as *analysis*; they take for granted a body of authentic knowledge acquired by the special sciences, and are concerned not with *adding* to it [...] but with *clarifying* its meaning and implications. (Nagel, 1936a, p. 6, original emphases)

Nagel's paper also explicitly thematizes the connection between 'analytic philosophy' and American naturalism. Although Nagel recognized that most of the Europeans he visited are suspicious of comprehensive "philosophic systems", he maintains that they all subscribe to "a common-sense naturalism", accepting as a matter of course "the mechanisms which science progressively discovers" (Nagel, 1936a, p. 7).

Of all the philosophers discussed in "Impressions and Appraisals", Nagel is clearly most sympathetic to Carnap. Whereas the paper is somewhat negative about the Cambridge philosophers and argues that it is not easy to see, "without lapsing into the personal and the impertinent", why philosophers in the U.K. are so fascinated by Moore and Wittgenstein (Nagel, 1936a, p. 10), he introduces Carnap in exceptionally laudatory terms:

> I chanced to hear Carnap take part in a discussion before knowing who the speaker was, and was immediately impressed by the gentle force of a luminous mind. I listened to him subsequently, in classroom and seminar as well as in private, and thought I understood what Emerson meant when he described character as a reserved force acting directly by presence, apparently without means. As a lecturer I found him admirably lucid, as the leader of his seminar incisive and yet sympathetic. In private discussions he was a patient listener to my problems, clarifying them not only by suggesting solutions, but answering many by reformulating the difficulties. He is one of the rare people with whom one does not have to agree in order to be understood by him. I had a sense of immeasurable thoroughness and of untouched intellectual resources by almost everything he said. (Nagel, 1936b , pp. 44-45)

Nagel's description of Carnap as a 'gentle' and 'patient' thinker is likely a response to the largely negative American reception of the latter's early views. In the United States, Carnap was best-known for his staunch solipsism and his ideas had been criticized even by philosophers generally sympathetic to verificationist theories of

meaning (e.g. Lewis, 1934; Morris, 1934).[30] Whether or not the American reading of Carnap's early views was accurate, Nagel's paper seems designed to correct this image: more than seventeen pages of the article are devoted to an extensive analysis of Carnap's 'new' ideas (Nagel, 1936b, pp. 32–49). Nagel introduces the distinction between "left wing" and "right wing" logical positivism and shows that the former faction no longer believes that there is a "theoretically necessary ultimate terminus to the process of verification":

> The dogmatism of indubitable atomic facts is replaced by the view that every protocol is theoretically capable of further verification. From a broad point of view there is no difference in kind between the verification of a theory and the verification of a singular proposition. Both are hypotheses, which commit their asserters to an indefinite series of further propositions. [...] Carnap's wing in the Vienna Circle believes that not only is this revised theory of meaning and verification more faithful than the original one to the actual procedure of science, but that the annoying solipsism of the earlier version is made completely innocuous. (Nagel, 1936b, pp. 34–35)

As this is exactly what Nagel himself had proposed in his own critical analysis of Carnap's views two years before (see Sect. 3.6), it is no surprise that he presents the latter's new position as an improvement. Nagel also sympathetically discusses Carnap's physicalism, which he connects to his own opposition to methodological dualisms (see Sects. 3.2 and 3.3). Nagel explains that Carnap's physicalism should be viewed as a "protest against" the "sharp division between *Natur* and *Geisteswissenschaften*", and interprets the latter's "unity of science" thesis in strongly naturalistic terms, arguing that it "asserts in so many words that the subject-matter for all empirical inquiry is the world of spatio-temporal events" and that "a common logical method is applicable in every department of knowledge" (Nagel, 1936b, pp. 40–41).

3.10 Naturalism Reconsidered

"Impressions and Appraisals" played a major role in the American reception of logical empiricism and its publication likely helped Carnap to find a position in the United States. The paper was published in January 1936, a few weeks before Carnap started an extensive American lecture tour that would take him to, among others, Columbia University and that would result in two job offers. Carnap was thankful for Nagel's support because it helped him in his attempts to correct the American misunderstandings about his philosophy (Verhaegh, 2020b). When the Carnaps arrived in the United States, they were awaited by Ernest and Edith Nagel, now a married couple and eager to be as hospitable as the Carnaps had been a year before.[31]

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[30] In addition, most of Nagel's teachers at Columbia disliked logical positivism. In a letter to Neurath, Nagel explains that "the 'old-timers' distrust it, dislike it, and pretend that it has nothing very interesting to say" (January 2, 1936, ONN, item 275).

[31] See Carnap's diary (RCP 025–82-01) and Nagel's letter to Neurath (January 2, 1936, ONN, item 275).

Reichenbach was less happy with "Impressions and Appraisals", however. Nagel had only mentioned him in a footnote, explaining that exigencies of space made it impossible to include a discussion of the latter's views about induction and probability (1936b, p. 30, n2). In February 1936, Reichenbach sent Nagel a letter in which he explained his disappointment. The paper, Reichenbach maintained, portrays logical empiricism as a movement that is primarily interested in "abstract discussions about meaning". Nagel primarily had eye for views that were either "unfruitfully dogmatic" (Schlick's) or "unfruitfully conventionalistic" (Carnap's) and ignored the more subtle ideas that had been developed by Reichenbach's now-scattered Berlin group. About a month later, Reichenbach published an alternative perspective on "logicistic empiricism" in the *Journal of Philosophy*, introducing the American public to the work of his Berlin group.[32] Reichenbach's strong response to "Impressions and Appraisals" was likely induced by his personal situation. The Turkish government had blocked his visiting position in New York and he was more desperate than ever to find a job in the United States (see Verhaegh 2020c). Still, the letter soured the relation between Nagel and Reichenbach for a few years. Indeed, Nagel's review of *Wahrscheinlichkeitslehre*, published a few months later, was lukewarm at best and argues that Reichenbach merely sketches but fails to work out in detail a convincing theory of probability (Nagel, 1936c; see also Reichenbach, 1938 and Nagel, 1938d).

Nagel played an important role in promoting Carnap's philosophy in the United States. Yet it would be a mistake to view him as a philosophical convert.[33] Nagel incorporated elements of Carnap's program into his own philosophy but he always viewed them as friendly amendments, never as an alternative to his naturalism. The most notable example of such an amendment is Carnap's perspective on the nature of logic. Whereas Nagel had toyed with metaphysical interpretations of logic in the first phase of his career (see Sect. 3.3), he came to view Carnap's position as superior, arguing that logical systems are linguistic proposals aimed at aiding scientists in their everyday inquiries:

> The various systems of formal logic must [...] be viewed not as accounts of the 'true nature' of an antecedently identifiable relation of 'implication', but as alternative *proposals* for specifying usages and for performing inferences. [...] If everyday language requires to be completed and reorganized for the sake of attaining the ends of inquiry, the 'justification' for a proposed set of regulative principles [...] can be given only in terms of the adequacy of the proposed changes as means or instruments for attaining the envisaged ends. (Nagel, 1944/1956, pp. 76–77, my emphasis)

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[32] See Reichenbach to Nagel (February 15, 1936, HRP, 013–51-01) and Reichenbach (1936). Reichenbach's negative portrayal of Carnap's position was not a surprise to Nagel. In Europe, Nagel had already noted that Reichenbach and Carnap respected each other but were far from friends. In a letter to Hook, Nagel wrote: "I don't think [Carnap] and Reichenbach love one another, I had that impression from Reichenbach when I saw him, and get hints of it from mrs. Carnap; he of course is the essence of courtesy and admiration. I think R[eichenbach]'s cock[i]ness gets on the Carnaps' nerves, while Carnap's preciseness must irritate R[eichenbach]" (December 3, 1934, SNP).

[33] Unlike Quine, who, after spending a few weeks in Prague in 1933, came to perceive himself as Carnap's "disciple" for a number of years. See Quine (1970/1976, p. 41).

Nagel first extensively discussed this new perspective in his contribution to *Naturalism and the Human Spirit*, often considered the Columbia naturalists' philosophical manifesto, showing that he viewed his new position as an amendment rather than as a dismissal of naturalism. Indeed, Nagel sets up his paper by discussing his Carnapian account as one of three possible naturalistic perspectives on logic before he goes on to argue that it is the most plausible option.

There are a few reasons as to why Nagel never felt the need to abandon naturalism in the wake of his trip to Europe. First and foremost, Nagel believed the continental philosophers to be (closeted) naturalists themselves. I have mentioned Nagel's attempts to connect analytic philosophy to American naturalism in "Impressions and Appraisals" (Sect. 3.9) but he repeats this assessment in his 1954 presidential address to the APA Eastern Division Meeting. In this address, titled "Naturalism Reconsidered", Nagel argues that although analytic philosophers are wary of comprehensive philosophical perspectives and have directed their attention to the resolution of "limited problems and puzzles", most of them *implicitly* share a naturalistic *Weltanschauung*:

> Some of us [...] have dismissed as utterly trivial most if not all [...] analytical philosophy. I do not share this distress [...]. Concentration on limited and determinate problems has yielded valuable fruits. [...] On the other hand, philosophers like other men conduct their lives within the framework of certain comprehensive if not always explicit assumptions about the world they inhabit. These assumptions color evaluations of major ideals and proposed policies. [...] It is clearly desirable that such basic intellectual commitments [...] be made as explicit as possible [...]. [T]his evening seems to me an appropriate occasion for stating as simply and a succinctly as I can the substance of those intellectual commitments I like to call 'naturalism'. (Nagel, 1954/1956, p. 5)

For Nagel, naturalism and logical empiricism are compatible. The naturalist, like the empiricist, is skeptical of traditional systems of philosophy. Indeed, Nagel himself had defended a contextualist picture of inquiry from the very beginning of his academic career (see Sect. 3.2). Just as Nagel combined detailed analyses of measurement, probability, and verification with a strong commitment to methodological naturalism, he believed that the work of the empiricists, too, was compatible with a naturalistic outlook.

An additional reason as to why Nagel did not dismiss naturalism is that he believed some of Carnap's analyses to be fundamentally misguided. That is, even if Carnap's program *were* a genuine alternative to American naturalism, Nagel would not have adopted it. Especially Carnap's ideas about ethics and esthetics were an eyesore to Nagel.[34] In his popular piece on logical empiricism, Nagel dismissed the positivists' views on ethics as "superficial" and even in "Impressions and Appraisals" he rejects Carnap's position:

> Perhaps the least satisfactory part of Carnap's views is his analysis of the propositions of ethics and esthetics. Ethical norms are declared to be imperatives and not statements capable of truth or falsity. [...] But it is surely not the case that ethical propositions are necessarily

[34] Other examples are Carnap's approach to semantics (Nagel 1942) and inductive logic (Nagel 1963). See Tuboly (2021) and Thomas Mormann's chapter in this volume for a discussion.

> either assertions about some transcendental autonomous realm of values or statements simply about human feelings. They can be conceived as hypotheses concerning ways of organizing or reorganizing the human scene in the interests of the well-being of its members. Ethical statements would still be empirical and would refer to the want and the capacities of living beings; but they would be more than records of private feelings of those who make them. (Nagel 1936b, pp. 48–49)

Although the two were like-minded in their political views (see Sect. 3.7), in other words, Nagel believed that Carnap failed to successfully incorporate them into his worldview.[35]

Second, Nagel believed that Carnap carried his syntactical analyses too far. Nagel, we have seen, was happy to adopt Carnap's suggestion that logical systems are linguistic proposals. He was dissatisfied, however, with the latter's attempts to extend his linguistic approach to philosophical systems more generally. Naturalism, for Nagel, is not a proposal. It is an empirically justified *Weltanschauung*, intellectually superior to the major alternatives American philosophers were offering (most notably, idealism and theism):

> When naturalists give their allegiance to [...] the method of modern empirical science, they do so because that method appears to be the most assured way of achieving reliable knowledge. As judged by that method, the evidence in my opinion is at present conclusive for the truth of naturalism, and it is tempting to suppose that no one familiar with the evidence can fail to acknowledge that philosophy. Indeed, some commentators there are who assert that all philosophies are at bottom only expressions in different idioms [...] so that the strife of philosophic systems is mainly a conflict over essentially linguistic matters. But too many thinkers for whom I have profound respect explicitly reject naturalism, and their espousal of contrary views seems to me incompatible with the irenic claim that we really are in agreement on fundamentals. (Nagel, 1954/1956, p. 12)

Nagel, in sum, remained a committed naturalist. His encounters with European philosophers of science had offered him a way to replace his teachers' 'muddy' theories with rigorous analyses.[36] Still, Nagel never gave up on naturalism itself. Even in 1954, two decades after his year in Europe, when Nagel had the honor to be the first self-identifying analytic philosopher to read a presidential address to the American Philosophical Association, he chose to portray himself a naturalist, arguing that naturalism "embraces a generalized account of the cosmic scheme […] as well as a logic of inquiry" (Nagel, 1954/1956, p. 6).

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[35] This is not to say that Nagel's assessment is correct. Carus (2007), for example, argues that Carnap did incorporate his socio-political views into his philosophy.

[36] Indeed, in one of his rare retrospectives, Nagel writes that that the "vigorous and technically precise" methods of the logical empiricists were "salutary stimuli" to his own development, compelling him to "re-examine assumptions" and "to take a stand on a number of issues on which [his] earlier teachers were […] unclear" (1956, p. xii).

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Chapter 4
The Emergence of Scientific Explanation as a Problem for Philosophy of Science: Aristotle, Nagel, and Hempel



Fons Dewulf

Abstract In this paper I trace Ernest Nagel's earliest ideas on explanation by investigating his course-notes of the 1930s. At Columbia University there was an increasing interest in the study of Aristotle. As I show, Nagel's focus on the explanatory aim of science originated from his reading of Aristotle's *Posterior Analytics*. Through his teaching of Aristotle, Nagel inspired his New York colleagues to focus on a philosophical analysis of explanation. I claim that this resulted in Carl Hempel's earliest work on scientific explanation. Although scientific explanation was not a central topic for philosophers of science in the 1940s or 1950s, Hempel and Nagel's interest in the topic helped to canonize it in a period when the topics and methods in philosophy of science became increasingly standardized.

Keywords Aristotle · Scientific explanation · Carl Hempel · Ernest Nagel · Posterior analytics

4.1 Introduction

Reflections on systematic knowledge (science) have been around, at least, since pre-Socratic philosophers. However, philosophy of science as a domain of inquiry that is institutionally individuated as a subdiscipline of academic philosophy is a much more recent phenomenon. Most of the institutional infrastructure for philosophy of science only arose in the 1960s: standard textbooks, separate research centers for philosophy of science, graduate programs and a professional association with its own conference series (Richardson, 2012). Alongside the institutionalization of philosophy of science in the 1960s, the topics of research and teaching for the domain were also standardized. During this period, the analysis of scientific explanation became a central topic—which it still is to this day. From the 1940s onwards, two philosophers in particular defended that an analysis of scientific explanation should

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take the center stage of teaching and research in philosophy of science: Carl Hempel and Ernest Nagel.

In the influential summary-essay, "Aspects of Scientific Explanation", Hempel assumed that two "enduring concerns" provide the stimulus for humanity's desire for scientific achievement. On the one hand, human beings had always desired to control the surrounding environment. Science was an important means to this end. On the other hand, science was meant to quell "man's deep and persistent desire to know and to understand himself and his world" (Hempel, 1965, p. 333). As a consequence, any understanding of science had to enlighten the logical structure of explanation which enables human beings to fulfill the desire to understand their world. Hempel's covering law model was designed to offer such enlightenment. In a similarly influential philosophy of science book from the 1960s, *The Structure of Science*, Nagel started his inquiry from a similar assumption:

> It is the desire for explanations which are at once systematic and controllable by factual evidence that generates science; and it is the organization and classification of knowledge on the basis of explanatory principles that is the distinctive goal of the sciences. (Nagel, 1961/1979, p. 4)

Because Nagel believed that the quest for explanatory knowledge is an expression of a basic impulse of human nature and represented a "distinct variety of human experience", he argued that science, as the most systematic expression of this human impulse, was a necessary element in any humanist education (Nagel, 1959, p. 57). Today, philosophers of science still eagerly use this anthropological legitimation to argue that scientific explanation is the pre-eminent goal of scientific knowledge. Michael Strevens holds that "when science is pursued as an end rather than as a means, it is for the sake of understanding—the moment when a small, temporary being reaches out to touch the universe and makes contact" (Strevens, 2008, p. 3). Bradford Skow (2016, p. 525) notes that "[t]he natural curiosity that we are all born with, and that most of us retain throughout our lives, is in part a curiosity about why things happen", and answering such why-questions is the most important aim of science.

Since the 1960s philosophers of science have taken for granted that explanation is an aim of science in need of some kind of analysis and they intended to use that analysis for the improvement of scientific practice through philosophical modeling, or for an insight into the nature of science. However, this assumption was not shared by many philosophers from the first half of the twentieth century. As Wesley Salmon (1999, 338) already remarked, logical empiricist philosophers before Hempel and Nagel had little to say about scientific explanation as an aim of science. Both Carnap and Reichenbach never attempted to explicate explanation as a separate aim of scientific inquiry.[1] Neither did they conceive scientific knowledge as an expression of the human desire to understand the world. This should not

[1] In *The Rise of Scientific Philosophy*, Reichenbach (1951, p. 5) states that the essence of scientific knowledge is generalization. Similarly, Carnap (1966, p. 3) believed that science ultimately aimed to capture the regularities in our observations. They both claimed that the subsumption of a singular event under a generalization can be called an *explanation*. However, neither defended that explanation was an aim of scientific inquiry distinct from description. As I will argue below, this distinction

be surprising, since logical empiricist philosophers, like Carnap, Reichenbach or Neurath advanced their analyses of science from an empiricist point of view. This implied that scientific theories should only be evaluated on their potential to account for available empirical phenomena, both past and future. In the philosophical generation before logical empiricism, Pierre Duhem and Ernst Mach had already argued that such an empiricist understanding of science did not distinguish an explanatory aim of science from a descriptive one (Mach, 1893/1974; Duhem, 1906/1991).

As Bas van Fraassen (1980, p. 156) pointed out, this empiricist stance toward the evaluation of scientific theories was abandoned once philosophers of science began to search for an objective model of scientific explanation in general—a model that intended to capture an objective relation between theory and observations that was distinct from the descriptive relation. Arguably, Nagel's and Hempel's most important contribution to philosophy of science as a discipline lay exactly in initiating the shift towards research on such an objective model of scientific explanation. Due to their efforts to promote the topic, explanation became an important subject for research and teaching in philosophy of science at the precise moment when philosophy of science as a domain of inquiry was institutionalized as a subdiscipline in American philosophy.

So far, there is no historical account how Hempel and Nagel, who are often grouped alongside other logical empiricist philosophers, became convinced of the position that explanation is a distinct aim of science—a position that was never defended by earlier logical empiricists and stands in contrast with earlier empiricist interpretations of science. In *Four Decades of Scientific Explanation*, Salmon opened his historical narrative of the debate on scientific explanation with the publication of Hempel and Oppenheim's article "Studies in the Logic of Explanation" (1948). However, this paper already starts from the assumption that "scientific research in its various branches strives to go beyond a mere description of its subject matter by providing an explanation of the phenomena it investigates" (Hempel & Oppenheim, 1948, 135). In the paper, Hempel and Oppenheim already assume that there is "a rather general agreement" on explanation as an aim of science distinct from description.[2] As I will show in Sect. 4.5, this was not the case: until the 1950s philosophers of science had little interest to analyze scientific explanation as a distinct aim of scientific theories.[3]

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is central to the Aristotelian tradition in philosophy of science that Nagel, and indirectly Hempel, pursued.

[2] In the paper Hempel and Oppenheim do not give a proper account of how to understand the distinction between the descriptive and explanatory aim of science. In *Aspects of scientific explanation*, Hempel would relate explanation to the human search for understanding—something that cannot be achieved through mere description (1965, p. 333). When Hempel and Oppenheim wrote their paper in 1948, its thesis was not yet connected to the distinction between a realist and an anti-realist understanding of science.

[3] Hempel's covering law model had several precursors (Hempel and Oppenheim 1948, p. 140), perhaps most notably in the work of John Stuart Mill, Karl Popper and Stanley Jevons. What makes Hempel's covering law model stand out from these precursors is its analytic perspicuity. It is offered as an analysans of the explanatory aim of science. In this regard, it can also be critiqued by showing how examples of the analysandum do not correspond to the analysans (see 4.4 for Further Discussion).

Thus, Hempel and Oppenheim's introduction of a consensus on explanation as an aim of science stands at odds with the contemporary intellectual disinterest to analyze explanation as a distinct aim of scientific theories.

Commentators on the history of the debate about scientific explanation usually refer to Aristotle as the origin of the debate. Salmon (1989, p. xiii) starts *Four Decades* by saying that "[p]hilosophers and scientists, since at least the time of Aristotle, have tried to say in what such scientific understanding consists." James Woodward in his article on scientific explanation in the *Stanford Encyclopedia* claims that "[i]ssues concerning scientific explanation have been a focus of philosophical attention from Presocratic times through the modern period" (Woodward, 2017). Aristotle was the first philosopher to systematically analyze the nature of scientific knowledge, and he too took for granted that "all men desire by nature to know" (Met I, 1, 980a1), which implied a desire not only for knowledge of what happens, but also of why things happen (Met I, 1, 981a27). How Aristotle's understanding of scientific knowledge is historically related to Hempel's model of scientific explanation has so far never been explored.

Salmon (1989, p. 10) simply labeled all philosophical accounts of explanation before Hempel and Oppenheim's paper as prehistory. In this paper, I aim to close the historical gap by showing how Nagel's interest in the Aristotelian conception of science engendered an intellectual context in New York during the 1940s such that the nature of scientific explanation became an intellectual problem for the philosopher of science, despite Nagel's and Hempel's empiricism (Sect. 4.2). I will show that various elements of Hempel's covering law model of explanation were the result of this intellectual context (Sect. 4.3). I will also investigate how the new intellectual problems concerning explanation subsequently expanded from the New York context and became standardized for philosophy of science as a field in the 1960s (Sect. 4.4).

4.2 Ernest Nagel, Aristotle, and New York's Philosophy

After his doctoral defense in 1934 at the University of Berlin, Hempel had no academic position. Fortunately for him, Paul Oppenheim employed him as a personal research assistant in Brussels. In the academic year 1937–1938 Carnap hired Hempel as his research assistant at Chicago University, but could not sustain his employment for more than a year. In the summer of 1938, Hempel returned to Belgium to work under Oppenheim, but was committed to continue his career in the United States. Eventually, he managed to acquire a travel to initiate his return. In January 1939, Hempel arrived in the US without any explicit prospect of employment, but he was warmly welcomed by Ernst Nagel at the docks of the New York Harbor. Nagel immediately integrated Hempel in the flourishing scene of New York philosophy. Searching for an academic position, Hempel was eager to become more familiar with the research of his American colleagues. Nagel welcomed Hempel to his Logic graduate course at Columbia University, and introduced him to many of his New York philosophy colleagues. In his diary entry of 14 February 1939 Hempel noted that, to

his surprise, Nagel's coursework in logic had "many historical perspectives; critical discussion of Aristotelian and neo-Thomistic conception of science".[4] Unlike what Hempel was used to from his experiences with Carnap or Reichenbach, Nagel was not an a-historical philosopher who only focused on contemporary science or formal problems. Nagel's philosophical interests were very different from what Hempel was used to.[5]

Nagel got his Bachelor degree at City College New York, where he was inspired mainly by Morris Cohen. Cohen set him on the path to discover modern, formal logic and the new European logical empiricism in the late 1920s. However, Nagel did not stay at City College, but pursued a graduate program at Columbia University where he received instruction from two philosophy professors. The first was John Dewey. Nagel was not fond of Dewey's teaching style, nor of Dewey's philosophy in particular. At the end of his life, Nagel recollects: "I found Dewey extremely impressive as a human being, but I'm not really sure that I understood the pragmatism that he represented".[6] Nagel's second teacher at Columbia, who also supported his application to the graduate program, was Frederick Woodbridge. He taught the history of philosophy at Columbia and turned out to be much more influential on Nagel than Dewey.

> When I started graduate work in philosophy, Woodbridge influenced me. He was a kind of Aristotelian who in part took a view similar to that of Cohen, believing in the reality of universals. Be he didn't take a Platonistic view, but rather an Aristotelian view on these matters. ("Interview with Ernest Nagel," this volume, p. XY)

Woodbridge had a great interest in Aristotle's philosophy: posthumously, one of his lecture series on Aristotle was edited by John Hermann Randall (Woodbridge, 1966).[7] When Woodbridge retired from Columbia University in 1931, Nagel became an instructor there. At the time, philosophy education at Columbia was mainly oriented towards the history of philosophy. Discussions of contemporary problems in philosophy were largely absent. Nagel was hired to change this and provide courses on the new developments in logic. In his logic course, Nagel combined a discussion of modern logic with a discussion of problems in epistemology and philosophy of science.

> I was hired to do something about the Logic courses. … I thought if you are going to have students who will be prepared to teach all over the lot in philosophy, they ought to be exposed to some discussion in traditional epistemology. So I began giving courses in the theory of

[4] "Viele historische Ausblicke; kritische diskussion Aristotelische und neothomistische Wissenschaftsauffassung." Hempel Diary, 14 February 1939, Carl Hempel Papers (CH) 02-1-1 Archives of Scientific Philosophy (ASP), Hillman Library, University of Pittsburgh, all rights reserved.

[5] On Nagel's historical approach to science and philosophy, see Thomas Mormann's chapter in this volume.

[6] "Interview with Ernest Nagel," in this volume, p. XY.

[7] Randall was Woodbridge's successor. He was specialized in the history of philosophy, played an important role as editor of the Journal of Philosophy and as staunch opponent to formal approaches in philosophy (Jewett, 2011).

> knowledge, both on the college level and graduate level. And the third thing that I think was an innovation—and that was hardly a regular subject when I was a student—was the work in the philosophy of science, and so that, too, became a rather major part of the offering of the department. ("Interview with Ernest Nagel," this volume, p. XY)

In the 1930s there was no standard textbook in philosophy of science, nor any anthology of important literature in the field. Such volumes only became available in the 1950s and 1960s. There were no standard topics, like scientific explanation or standard entry-points into debates, like the deductive-nomological model. Nagel could decide entirely on his own what philosophy of science or the logical investigation of science entailed. Therefore, it is interesting to investigate what Nagel actually taught in his classroom in these early years. In the above quoted recollection from 1983 Nagel separates logic, epistemology and philosophy of science as three distinct domains of inquiry, but in the 1930s, when he started his logic course at Columbia, his actual classes were an interplay of all three.[8] Rather than initiate his students in modern formal logic alone, Nagel decided to introduce them to the different, "warring" conceptions of logic in the history of philosophy, from traditional syllogistic, over mathematical logic, to metaphysical, idealist logic, and transcendental logic which studied "the presuppositions which the mind employs in organizing the sensuous material into significant experience".[9] In this early stage of Nagel's career, he believed that an overview of contemporary logic was ipso facto an overview of the struggle of the various philosophical traditions, of which logical empiricism was only one among many.

The aim of his course was not "primarily historical" as he remarked in his introductory lecture.[10] However, by discussing the history of the logic of knowledge, Nagel wanted to introduce his students to two important aspects of rational inquiry in general. The first was a narrow conception of logic as "a formal and highly technical study of principles of inference and of statements which may be certified simply on the basis of these principles" (ibid.). The second was a broader conception of logic as "a study of the generic features of the methods used in acquiring knowledge, and an account of the nature of scientific systems of knowledge". The latter conception of logic involved a "rational reconstruction" of the operations yielding knowledge, and "to the extent that this is normative, it involved questions as to the source and validity of the norms proposed". Both aspects of logic were related to each other: the technical study of inference was often used to clarify the norms for scientific knowledge.

Nagel introduced this intimate link between the two aspects of logical inquiry through an extensive investigation of Aristotle's philosophy of science (ibid.). Aristotle's completed works was consistently mentioned on the reference list for Nagel's

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[8] The book *An Introduction To Logic And Scientific Method,* co-authored with Morris Cohen, is a good example of the same mix of genres (Cohen and Nagel 1934).

[9] "Logic 101–102, 1933–4", Ernest Nagel Papers (ENP), Box 16, folder "Notebooks Logic", Rare Book and Manuscript Library, Columbia University, New York. Quoted by permission of Columbia University. All rights reserved.

[10] "Teaching Materials Logical Theory", ENP, Box 21, folder "Teaching Materials".

logics course from the 1930s to the 1950s, and it is clear that his students were required to read passages from the *Posterior Analytics* in particular.[11] For Nagel, the *Posterior Analytics* was not an obsolete relic from the history of ideas. It could still provide the starting point to discuss the very nature of scientific knowledge, because in this work Aristotle had captured some of the central norms for scientific knowledge that were still relevant in the modern age, and especially how these norms should be understood from a logical point of view. In his course notes from the 1930s Nagel used Aristotelian philosophy of science to discuss the distinction between common sense and science. According to Nagel, Aristotle made "a sharp contrast between scientific knowledge (which is of the stable and the universal) and common sense knowledge (which is of the particular, the impermanent, being discordant, and based on a variety of different authorities)".[12]

This distinction would also be the starting point of Nagel's own introduction in *The Structure of Science* (1961). There, he argued that common sense knowledge is distinguished from scientific knowledge, because the latter has an interest in systematically explaining the facts (1961/1979, p. 6). Like Nagel in *Structure*, Aristotle too identified the search for explanations as the distinguishing mark of scientific knowledge: science is "not simply knowledge of the fact, but of the reasoned fact."[13] For Aristotle, knowledge of the reason why the fact occurs is the distinctive mark of science. Just like Nagel's own theory of scientific explanation, Aristotle's theory of the demonstrative syllogism was not meant to be used as a guide for scientific inquiry in practice, but as an analysis of "the logical patterns exhibited by explanations in the sciences".[14] Aristotle was engaged in the same project as Nagel, to perform a rational reconstruction of the distinguishing feature of scientific knowledge, i.e. how it systematically offered insight into why phenomena occurred. Although some of Aristotle's norms for explanation were no longer valid, Nagel understood Aristotle's theory as a good stepping stone to an up-to-date normative account of the explanatory nature of scientific knowledge. As Nagel said to his students in the introductory course: "Though historical literature will be employed, the aim is to examine this literature critically, in light of the best available contemporary evidence."[15]

In his course notes, Nagel focuses mainly on the logical, demonstrative structure of Aristotle's model of explanation. Explanations have an inferential character to them. "By demonstration Aristotle means a syllogism productive of scientific knowledge, a syllogism, that is, the grasp of which is eo ipso such knowledge."[16]Crucial to Aristotle's theory was that only those syllogisms were explanatory which exhibit a

[11] "Reading Lists for Philosophy 103, 1944/1946/1948", ENP, Box 21, folder "Teaching Material Logical Theory".

[12] "The aim of the Posterior Analytics", ENP, Box 21, folder "Teaching Material Logical theory".

[13] Ibid. Nagel quoted APo I, 13, 79a15: "It is for the doctor to know the fact that circular wounds heal more slowly, but it is for the geometrician to know the reason for the fact".

[14] Ibid.

[15] Ibid.

[16] Ibid.

middle term that expresses the cause of the phenomenon to be explained.[17] Nagel emphasized that this condition made Aristotelian explanations asymmetrical. He gave the following example of this characteristic. One might prove the fact that the planets are near as follows:

All planets do not twinkle.

All things that do not twinkle are near.

Hence, the planets are near.[18]

But, this syllogism, although formally valid and true, is not explanatory. It merely proves the fact and not the reason why: the planets are not near because they do not twinkle. It is the other way around: the planets do not twinkle in virtue of the fact that they are near.

All planets are near.

All things that are near do not twinkle.

Hence, the planets do not twinkle.

In this syllogism, the middle term ("all things that are near do not twinkle") expresses the cause of the conclusion. The middle term expresses an aspect of the nature of planets (their proximity to the earth) which causes the planets to lack any twinkling. Explanatory arguments should always include information on the inherent nature of things, and the universal statements that make up the middle term should be an expression of that nature.[19] At the end of the section on Aristotle, Nagel compared Aristotle's theory of demonstrations with the norms for contemporary scientific theories: both had an ideal of a systematic classification of natural phenomena—an ideal that can be expressed in logical terms. The greatest difference lay in the norms for this classification. Aristotle demanded that the explanations should be found in the definitions of things, expressing their form or nature, and not in the lawful systematization of their sequential behavior.[20] When Nagel explicated the logical form of scientific explanation in his *Structure*, he abandoned this Aristotelian demand: the scientific laws that have to be used in explanations, are not required to exhibit causal information that express the nature of the things to be explained. As long as the laws express phenomenal regularities they can legitimately operate in explanations, even though some regularities function better because of their capacity to capture more and different kinds of phenomena (Nagel, 1961/1979, p. 78). In *Structure*, Nagel did not explicitly contrast this approach to the Aristotelian one, even though, given his course notes, he was clearly aware of the contrast.

[17] "Teaching Material 1939: Aristotelian Theory of Science", ENP, box 25, folder "Teaching Material: miscellaneous".

[18] "The aim of the Posterior Analytics", ENP, Box 21, folder "Teaching Material Logical theory".

[19] Ibid.

[20] Ibid.

4.3 The Logic of Explanation: Hempel's New Puzzle

Hempel only sat in on Nagel's logic course for the spring term of 1939, since he was unemployed at the time. However, the course could have been more influential on Hempel's larger commitments than Hempel himself may have realized. Nagel introduced Hempel to the topic of scientific explanation as a way to understand the central aim of scientific knowledge. Until that point, Hempel had never been introduced to the idea that scientific knowledge even by contemporary standards could be evaluated on its explanatory power as distinct from its descriptive adequacy. In the summer of 1939, Hempel got a job at City College and subsequently at Queens College New York in 1940. As a consequence, the philosophical debates in New York would be Hempel's direct intellectual context for his first eight years in the United States. During this time, Hempel participated in the Nagel-Hook circle, also known as the New York circle, which was an informal discussion group of New York philosophers organized by Nagel and Sidney Hook (Nollan and Hempel 2000, p. 19).

In 1941, Hempel was invited by the circle to discuss whether historical knowledge could be called science. This talk was eventually published in the *Journal of Philosophy* as the "Function of General Laws in History" (Hempel, 1942). In that paper, Hempel discusses the use of universal sentences to explain and predict historical events. In New York philosophy at the time, the scientific status of historiography and the status of historical laws was a major point of contention: J.H. Randall, Sidney Hook, Arthur Lovejoy and Philip Wiener had been discussing these issues in previous years (Dewulf, 2018, p. 401). Hempel's paper was a continuation of these discussions. Hempel highlighted that historical laws function as systematic representations of lawful relations among historical events. If a historical event is deduced from initial conditions and historical laws, such a deduction can be called an historical explanation or prediction. From this logical vantage point, Hempel assumed historiography was similar to other scientific disciplines (Hempel, 1942, p. 47). However, in the paper Hempel did not yet distinguish explanation from description as two distinct aims of science.

The question whether explanation is a distinct aim of science was part of Hempel's New York context. Philip Wiener, who was a lecturer at City College, had been familiarizing New York philosophers with the French debate between Emile Meyerson and Pierre Duhem on the explanatory aim of science.[21] Duhem claimed that explanation was not the aim of physical theories and Meyerson had argued for exactly the opposite position, that explanation is the foremost objective of science. Wiener reviewed Meyerson's work and books on Duhem's philosophy for the *Journal of Philosophy* (Philip Paul Wiener, 1933, 1934, 1935, 1942). Eventually Wiener would also translate Duhem's *Aim and Structure of Physical Theories* to English in 1954. Wiener was a regular member of the Nagel-Hook circle which Hempel also frequented. Moreover, Nagel regularly listed Duhem's and Meyerson's works in the reading list for his

[21] Within the logical empiricist network this debate also emerged during the second world war. Otto Neurath wrote both Carnap, Hempel and Feigl that an increased focus on the analysis of explanation moved the movement away from the empiricism of Mach and Duhem (Cat and Tuboly 2019, p. 622).

courses throughout the 1940s and 1950s.[22] Nagel's philosophy of science course in the 1950s even began with the question "Do the sciences explain?", for which part of Duhem's and Meyerson's work was the point of reference.[23] Thus, Nagel's students were familiar with the intellectual challenge how to decide whether explanation is an aim of science distinct from description.

In 1941 Ernest Nagel also circulated a list for suggested topics for the Jones Prize, a graduate essay prize for Columbia students. Among the suggested topics, the following were listed: 'the structure of scientific explanation' and 'the logical structure of historical explanation'. In 1943 the topic for the prize was "types of explanation".[24] It is unclear who won the Jones Prizes during this period and what essay they wrote, but, at least, two Columbia graduates worked on these questions during the Second World War: Morton White and John Hospers. White's first paper, published in *Mind*, discussed historical explanation (White, 1943). White argued, similarly to Hempel, that historical events are explained if their occurrence can be deduced from true initial conditions and general laws. Whereas White only analyzed historical explanation in particular, John Hospers discussed explanations in general, not only in science, but also in natural language (Hospers, 1946). From an ordinary language point of view Hospers identified all explanations as answers to why-questions, and argued that any philosophical theory of explanation should address the various ways in which people actually answer such questions. In his paper Hospers came to no definite model or theory, but he left no doubt that scientists seek explanations. Since scientists, just like ordinary people ask and answer why-questions, scientists also offer explanations. However, Hospers did not defend the position that these explanations should be understood as an aim of scientific knowledge that is distinct from description. According to him, it could still be the case that scientists offer explanations by simply describing some state of affairs, and that their explanations can always be reduced to their capacity to describe the world (Hospers, 1946, p. 354).

Questions concerning explanation and its role in scientific knowledge were part of the philosophical context of Hempel during the Second World War: Nagel started his logic course with a discussion of Aristotle's *Posterior Analytics* and Wiener introduced New York philosophers during the 1930s to the debate between Duhem and Meyerson on the explanatory aim of science. The papers of White and Hospers are evidence that other New York philosophers, beside Hempel, were tackling questions concerning explanation. Most likely, this intellectual context was also Hempel's

[22] "Reading List Philosophy of Science 1954", ENP, box 18, folder "Explanatory methods of empirical science"; "Reading List Philosophy 103 1952", ENP, box 18, "Teaching materials Logical theory (1952–53)".

[23] "Reading List Philosophy of Science 1952", ENP, box 24, folder "Teaching Materials Philosophy of science (New School, 1950)".

[24] "Teaching Material 1939: Aristotelian Theory of Science", ENP, box 25, folder "Teaching Material: miscellaneous".

intellectual motivation to work on a complete logical analysis of explanation as an aim of science. This was published in 1948 as "Studies in the Logic of Explanation".[25]

In their paper Hempel and Oppenheim give a set of examples of scientific explanations, like the explanation of the appearance of a broken oar in water. From this set, they abstract four conditions of adequacy for any scientific explanation. First, the explanandum must be a logical consequence of the explanans. Second, the explanans must contain some type of general laws required to infer the explanandum. Third, the explanans must have empirical content and, fourth, it has to be true (Hempel & Oppenheim, 1948, p. 137). These conditions of adequacy share several characteristics with Aristotle's account of the demonstrative syllogism. First, both Aristotle's and Hempel's model state that explanations should be reconstructed as inferences. Second, both believe that some type of universal sentence is a necessary premise of any explanatory argument. In contrast to Aristotle, Hempel did not think that lawful sentences had to express the essential nature of the thing to be explained—the nature because of which the explanandum occurred. Just as Nagel had argued in his course, Hempel and Oppenheim also concluded that contemporary scientific knowledge does not aim to lay bare the causal structure of the world, or the essences because of which things occur.

Just as Aristotle, Hempel and Oppenheim set out to distinguish sound explanations from candidate-explanations by adding a material condition to the formal conditions of their model. They required that the lawlike sentences in the explanans had to be true. Aristotle similarly stipulated that all demonstrations must contain true premises (APo I, 2, 71b25). For Aristotle, this implied that the relation between subject and predicate expressed in the middle term was absolutely necessary. The relation had to be universally valid, and not contingently valid to a limited set of particular instances (APo I, 4, 73a25). For Hempel and Oppenheim, the truth-condition meant that high confirmation of lawlike sentences was not a sufficient condition for their use in sound explanations. They argued that a condition of high confirmation for explanatory laws would lead to cases where, at one point in time, some phenomenon was explained by a well-supported explanans, but would, at a later point in time, be disconfirmed by new evidence. In such cases, one would have to say that the old explanation was sound when the available evidence confirmed the explanans, but turned unsound when disconfirming evidence became at hand. This was an unsatisfactory result.

> This does not appear to accord with sound common usage, which directs us to say that on the basis of the limited initial evidence, the truth of the explanans, and thus the soundness of the explanation, had been quite probable, but that the ampler evidence now available made it highly probable that the explanans was not true, and hence that the account in question was not—and had never been—a correct explanation. (Hempel & Oppenheim, 1948, p. 138)

As a consequence, all premises in the explanans should not only be confirmed by the available evidence. They also have to be true. Hempel was already confronted

[25] Paul Oppenheim is co-author of this paper. In the summer of 1946, Hempel wrote to Carnap that he was working on the manuscript of the paper during his holiday retreat with Oppenheim. However, the extent to which Oppenheim actually contributed to the content of the paper is unclear. Hempel to Carnap, 17 June 1946, Rudolf Carnap Papers, 84–19-25 ASP.

with this problem in 1942. Immediately after his publication of the "The Function of General Laws in History", Hempel received a positive letter from Charles Stevenson, who was a regular visitor to the Nagel-Hook circle. After lengthy discussions with Nelson Goodman, Stevenson pressed Hempel on a problem: the crucial definition of laws in that paper. Hempel had defined a law as a hypothesis of universal form which is well confirmed by the available evidence (Hempel, 1942, p. 35). On Stevenson's understanding, such a definition of law did not withstand scrutiny. "I think we usually expect 'p is a law' to be defined in a way that would imply 'p is true'—and not merely, 'p has some little confirmation'".[26] For Stevenson, laws are true or false, independent of the confirmation that one has of them. Hempel's definition of a law in 1942 did not correspond to this intuition. In his letter to Hempel, Stevenson already gives the argument in favor of a truth-condition which will reappear in the 1948 paper:

> Should a general statement which was at one age well confirmed later become disconfirmed, we should not say 'It was a law, but later ceased to be', but rather, 'It was thought to be a law, but we now have evidence for thinking that it wasn't one.'

Stevenson's philosophical response hinges on an assumed relation between the English language and philosophical concepts. Stevenson purely argues from the use of the concept 'law' in ordinary English sentences and never uses any example of scientific methodology in his reflections. Stevenson's argument counters the core of Hempel's starting point in 1942: laws are not systematizations of experience, their validity is independent of the experience that one has. In Hospers' paper on explanation, the truth-condition for explanatory laws was also defended: "the laws alleged must be true ones, else there is no true explanation" (Hospers, 1946, p. 345). However, from an empiricist point of view, it would make perfect sense to say that certain laws cease to operate as laws relative to a new set of experiences, since these laws could no longer describe the new set. Duhem had explicitly defended that scientific laws are essentially provisional and relative to the accepted experimental information at any point in time. "Any physical law, being approximate, is at the mercy of the progress which, by increasing the precision of experiments, will make the degree of approximation of this law insufficient" (Duhem, 1906/1991, p. 174).

By adding the truth-condition to his model, Hempel sided with Stevenson's and Hospers' concerns for the ordinary use of "law" in English. This also put Hempel at odds with Nagel's ideas concerning the function of laws in explanations.

Ever since Nagel published *The Structure of Science*, his ideas on explanation have been consistently interpreted as similar to Hempel's covering law model (Woodward, 2017; Salmon, 1989, p. 34). However, there are two important differences between their views. First, Nagel added no truth-condition for explanatory laws. Nagel remarked that "in point of fact we do not know whether the unrestrictedly universal premises assumed in the explanations of the empirical sciences are indeed true" (Nagel, 1961/1979, p. 43). Thus, he suggested to replace the truth-condition with a weaker one: "that the explanatory premises be compatible with established empirical facts and be in addition 'adequately supported' by evidence based on data

[26] Stevenson to Hempel, 3 February 1942, CH 38–3 ASP.

other than the observational data upon which the acceptance of the explicandum is based" (Nagel, 1961/1979, p. 43). Interestingly, Nagel explicitly contrasts his "empirical condition" for explanatory laws with Aristotle's condition that the premises of explanatory arguments must be true and known to be true, referring to *Posterior Analytics* I, 2.[27]

The second difference between Nagel and Hempel lay in their use of common sense intuitions concerning answers to why questions as valid evidence for a theory of scientific explanation. Nagel was only interested in capturing how experimental laws and theoretical principles function as the distinguishing characteristic of scientific knowledge in opposition to common sense knowledge. As a consequence, he never used ordinary answers to why questions as evidence for a theory of scientific explanation. Hempel, on the other hand, used common sense intuitions to discuss the nature of scientific explanation. His addition of the truth-condition is a good example of this. Ever since Hempel's covering law model of explanation became influential, many philosophers have assumed that there should be a continuity between an analysis of scientific explanation and ordinary, every-day explanations (Woodward, 2017; Skow, 2016, pp. 524–525). For Nagel, however, an account of scientific explanation had the opposite function: it had to show how scientific knowledge distinguishes itself from the indeterminacy of ordinary language (Nagel, 1961/1979, p. 8). Both differences show how Nagel and Hempel's work differed from a metaphilosophical point of view. Hempel was interested to present a philosophical model that provided a rational account of all possible examples of scientific explanation, including ordinary language examples of explanation. Nagel, on the other hand, wanted to provide an account of scientific knowledge in its opposition to common sense knowledge. The analysandum of their accounts was different, which resulted in interesting distinctions, e.g. concerning the truth-condition.

Hempel's motivation to offer a logical analysis of explanation did not directly originate from his interest in Aristotelian philosophy of science. As far as I can ascertain, Hempel, in contrast to Nagel, never discussed Aristotle in his classroom or read through the *Posterior Analytics* himself. However, his intellectual context changed when he began to work in New York from 1939 onwards: thanks to the intellectual interest of Ernest Nagel for Aristotle's *Posterior Analytics* and the subsequent work on explanation of Columbia graduate students like White and Hospers, Hempel began to reflect on questions concerning the structure of scientific explanation, and he molded various elements from this intellectual context into a new whole: explanation as a distinct aim of science and as a logical inference (from Nagel's discussion of Aristotle), explanation as an answer to ordinary why-questions (from Hospers) and the truth-condition for laws in explanatory arguments (from Stevenson and Hospers). One element of this intellectual context was, however, abandoned by Hempel: the puzzle how to decide whether explanation is a distinct aim of scientific knowledge.

[27] In *Structure*, Nagel holds the position that if a theory is empirically adequate for a domain of inquiry, one can tentatively hold the theory to be true (1961/1979, p. 151). This does not imply that truth is a condition for explanatory theories. Although Nagel thinks instrumentalism and realism are "only a conflict over preferred modes of speech", his refusal to understand truth as a condition for explanations distinguishes him from many contemporary realist-inspired theories of explanation.

In his classroom Nagel discussed the philosophers from the first half of the twentieth century, like Mach, Duhem and Meyerson, who had debated whether explanation was a distinct aim of science. Nagel's students were required to read their texts, and Nagel discussed these texts in his philosophy of science course, even in the 1950s. When Philip Wiener published an anthology to the philosophy of science in 1954, one part of it collected texts from Aristotle, Mach, Duhem, Meyerson and Schlick where they discussed exactly this philosophical puzzle (Wiener, 1953). Hempel never engaged this problem. From 1948 onwards, he took for granted that explanation was the foremost objective of scientific inquiry. He never gave any further reflection to this assumption. All responses to the 1948 paper also take the assumption for granted.

4.4 The Emergence of the Debate on Scientific Explanation

After the publication of Hempel and Oppenheim's paper, it takes a decade before the debate on scientific explanation actually starts. Salmon believed this silence was a sign that Hempel and Oppenheim's analysis of explanation was generally accepted within the philosophy of science community (Salmon, 1989, p. 33). I think that this silence shows the extent to which the topic of explanation was not yet of major significance within philosophy of science as a domain of inquiry during the first decade after the war. Until 1948 the topic had been confined to the philosophers of science in New York and it largely remained confined there for the next ten years.

An investigation of anthologies for philosophy of science of that period, shows that explanation was not yet a settled topic for philosophy of science in the 1950s. In the 1949 anthology *Readings in Philosophical Analysis*, edited by Herbert Feigl and Wilfrid Sellars, "The Function of General Laws in History" is taken up in the section "Problems of Description and Explanation in the Empirical Sciences", which also contained earlier discussions of a regularity account of causation by Schlick (Feigl & Sellars, 1949). In 1953, two anthologies in Philosophy of Science came out, one edited by Feigl and May Brodbeck, and the other was the aforementioned anthology of Wiener. In the Feigl-Brodbeck volume, Hempel & Oppenheim's, 1948 paper was added to the section "The Logic of Scientific Explanation and Theory Construction". The section opened with a passage from Duhem's *The Structure and Aim of Scientific Theories* and contained multiple essays that discussed the aims of theory construction. Hempel and Oppenheim's paper was the only paper directly analyzing the logic of explanation. In the preface Feigl and Brodbeck wrote that their concern had been systematic rather than historical. "Great names have therefore sometimes been sacrificed in the interests of relevance to contemporary issues and a modern idiom that does not in itself present further barriers to an already difficult subject" (Feigl & Brodbeck, 1953, p. v). The addition of the 1948 paper was a good example of this editorial policy to highlight novel and sometimes highly technical developments in the philosophy of science.

Wiener's anthology had an opposite aim. He did not intend to give an overview of the most recent advancements in the technical philosophy of science.

> Philosophy of Science has an educational responsibility in the preparation of future engineers, physicians, lawyers, teachers, journalists, ministers, and public administrators as well as research workers with regard to our cultural problem of keeping apace with rapidly advancing sciences. (Wiener, 1953, p. vi)

Wiener decided to focus on basic concepts and problems, "rather than on the defense of any one school of thought". He emphasized the importance of a historical perspective: "Historical perspectives broaden the scope of logical analyses". Neither Hempel's, 1942 nor his 1948 paper made it into Wiener's anthology. However, by the end of the 1950s Feigl and Brodbeck's reprinting of the 1948 paper had brought attention to Hempel and Oppenheim's paper.

In 1957 Israel Scheffler reacted to Hempel and Oppenheim's basic assumption "that explanation and prediction represent the central purpose of science and are epistemologically basic" (Scheffler, 1957, p. 293). Scheffler aimed to distinguish explanation and prediction from deductive-nomological inferences. To argue for this distinction, the example of an astronomer was introduced, who deduces a configuration of celestial bodies in the past given a current configuration of celestial bodies and a set of laws. Though such deduction fits the logical model of Hempel and Oppenheim, Scheffler believes that it cannot be described as explaining or accounting for the past configuration. Based on Scheffler's intuition about this case, he required that, in order to have an explanation, the event described in the premises has to happen before the event in the conclusion. Scheffler maintained that explanation and prediction were temporally-laden notions, whereas the deductive-nomological model was temporally neutral. The reason for this distinction lay in the fact that "the interpretation of explanation is generally taken as a reflection of causal notions, and that the peculiar temporal asymmetry of explanation is identical to the temporal asymmetry of cause and effect" (Scheffler, 1957, p. 301). Hempel and Oppenheim's model did not account for a causal order and thus Scheffler concluded that they had no reason to view science from the vantage point of notions like 'explanation': "It would seem a better reflection of the full generality of scientific reasoning if we view it as concerned with comprehensive nomological relations among events and abstract from causal explanation entirely" (Scheffler, 1957, p. 302).

Scheffler introduced a dilemma: either Hempel and Oppenheim's assumption that science aims to explain the world, is correct, but then their model is broken, or their model is not broken, but then it shows how science aims to form comprehensive nomological relations, not how science explains. The basis of this dilemma lies in Scheffler's intuitions about explanation which were introduced through the example of the calculating astronomer: inferring past events from current ones cannot be conceived as explaining. Like Aristotle's theory of the demonstrative syllogism, Scheffler believed that explanations had to be asymmetrical. Sylvain Bromberger's well-known flag-pole example that was introduced to Hempel at a conference (Bromberger, 1992, p. 8), would also exploit the same intuitive ambiguity: inferring the length of a flag-pole from the length of its shadow, could be considered a valid explanation of the length of the pole given Hempel's model of explanation—a counter-intuitive result. Scheffler also discussed the truth-condition:

he agreed that explanations should be true and that they should be sound independently from the current state of the evidence. According to Scheffler, this norm does not apply to predictions. Whereas explanations should be true, predictions need not be (Scheffler, 1957, p. 298).

Another early critic was Michael Scriven. He believed that explanations could not simply be equated with the valid answers to why-questions. There were many kinds of questions, like what-questions or how-possibly questions whose answers could also be considered as explanations. Consequently, Scriven introduced an intuition about explanation that stands in opposition to Hempel and Oppenheim's opening statement:

> […] whatever an explanation actually does, in order to be called an explanation at all it must be capable of making clear something not previously clear, that is, of increasing or producing understanding of something. The difference between explaining and 'merely' informing, like the difference between explaining and describing, does not, I shall argue, consist in explaining being something 'more than' or even something intrinsically different from informing or describing, but in its being the appropriate piece of informing or describing, the appropriateness being a matter of its relation to a particular context. (Scriven, 1962, p. 173)

For Scriven, an explanation yields information that makes a phenomenon intelligible to an epistemic agent. As a result of the introduction of this alternative conception of explanations, Scriven was able to argue against the truth-condition for explanations. He gave the following counter-example: we can perfectly talk about two competing explanations for some phenomenon in contemporary physics, since both explanations are capable of offering us understanding, even though only one of them can be true (Scriven, 1962, p. 185). By realigning explanation to human understanding, Scriven reintroduced the relation between explanation and an epistemic agent, which the truth-condition was designed to obstruct. A similar starting point for the analysis of explanation can be found in John Yolton's criticism of Hempel and Oppenheim in 1959 in the *British Journal for the Philosophy of Science*. Yolton emphasized that explanations could not be understood independently from the understanding that they offer to epistemic agents: explanations have to make some phenomena intelligible, even though this intelligibility may require specific training (in physics for instance) (Yolton, 1959, p. 196). "Not deducibility, but intelligibility constitutes the basic feature of the logic of explanation"(Yolton, 1959, p. 207).

Norwood Hanson also argued along these lines in 1959 in the *Philosophical Review*: explanations, like Aristotle's cosmology, aim to make phenomena in the world intelligible, while predictive systems, like Ptolemy's astronomy, aim to predict the position of the heavenly bodies (Hanson, 1959, p. 350). For this distinction, Hanson did not rely on intuitions about what one calls 'explanation'. Instead, Hanson relied on the history of science, from Aristotle to his contemporary age. He argued that predictability and intelligibility had only been perfectly aligned in Newtonian physics, and consequently that Hempel's model only fitted an ideal situation that does not reflect the majority of current scientific theories.

> For [Hempel] puts it that we never really have an explanation of a phenomenon P unless we could, simply by imagining a temporal shift, have predicted P by the same explanatory

> techniques. For Hempel, therefore, explaining P is simply "predicting" P after it has already happened. But if this is meant to be an account of what is actually done in science (not to mention history), where are we to find examples of Hempel's thesis in action? The answer is always the same: in Newtonian mechanics. (Hanson, 1959, p. 357)

Hanson's historical argument was meant to show that Hempel's equation of explanation to subsumption under a lawful generalization was based on a mistaken ideal of science: many actual scientific theories—Hanson gave the example of quantum mechanics - could explain phenomena, without having predictive capacity.

Hanson, Scheffler, Scriven and Yolton all criticized the introduced conditions of adequacy for scientific explanation that had been introduced by Hempel and Oppenheim. The effect of this criticism was two-fold. First, it focused part of the attention in philosophy of science on Hempel and Oppenheim's paper, and as a result also on the concept "explanation" and on the examples and intuitions which Hempel and Oppenheim had introduced. "Studies in the Logic of Explanation" became an important point of reference, a paper that exemplified a particular image of science. Because several critics at the end of 1950s identified the 1948 paper as a reference point, the paper became the "received view" on a question that had not been a matter of intellectual concern for philosophers of science before the paper was written. The question what the structure of scientific explanation is, emerged as a standard question in philosophy of science through the debate that resulted from the paper. Second, the early critics quickly expanded the possible examples and intuitions relevant to the analysis of scientific explanation. Scheffler's paper added the causal counterexample, Scriven's criticism added the intuition of explanation as intelligibility, and Hanson added the statistics of quantum mechanics. At the beginning of the 1940s it was not important to call specific examples of science "explanations", but by the end of the 1950s there were a whole collection of examples of "explanations in science" that had to be accounted for in the philosophical reasoning about science. None of the early critics questioned Hempel's central assumption, that explanation is an aim of science. As a result, this assumption, which originated from Nagel's coursework on Aristotle, silently shaped all further discussion.

Hempel and Oppenheim's formal analysis of scientific explanation also received critical attention. The first paper that responded to Hempel and Oppenheim's formal characterization of explanation was published in *Philosophy of Science* in 1955. It was written by Samuel Gluck, who had recently received his PhD at Columbia University under Ernest Nagel. Gluck indicated in his first note to the paper that Nagel had encouraged him to work on scientific explanation (Gluck, 1955, p. 34). Gluck accepted Hempel and Oppenheim's general conditions of adequacy for a model of explanation, but attempted to formally characterize explanations that used statistical laws. Through the introduction of statistical laws the non-occurrence of the explanandum became consistent with the explanans. This was formally excluded by the original analysis of 1948. However, Gluck saw no issue: "*Practically speaking*, C&Tb [the conjunction of conditions and statistical laws] really suffice to explain E, an event which has already occurred" (Gluck, 1955, p. 36). Thus, Gluck's short paper prepares the expansion of Hempel's formal covering law model with statistical

laws.[28] Another six years passed before the next reaction to the formal conditions of the model. In 1961 Rolf Eberle, David Kaplan and Richard Montague proved that the formal conditions of the 1948 paper allowed the sentence 'the Eiffel Tower is a good conductor of heat' to be explained by 'all mermaids are good conductors of heat'. They believed that this example diverged from the "customary notion of explainability" and, thus, served as a counterexample (Eberle et al., 1961, p. 419) Soon after the exposure of this formal problem, it was again taken up by Kaplan and, Hempel's own PhD student, Jaegwon Kim, who offered possible solutions (Kaplan, 1961; Kim, 1963). The early reactions to the formal aspects of the 1948 paper had the same two-fold effect: they referred to the 1948 paper as a pursuit-worthy analysis of science that discussed something which was at stake in philosophy of science, and they expanded or solidified the examples of "explanations in science" that could be accounted for in philosophy.

In 1960 a new set of anthologies for Philosophy of Science appeared. Now, Hempel and Oppenheim's paper was taken up consistently. In Sidney Morgenbesser and Arthur Danto's anthology, there was a section on laws and theory which contained an excerpt from Hempel & Oppenheim's, 1948 paper and Scheffler's criticism of that paper. In their introduction to the section they note that "the logic of explanation is very much under discussion these days" (Danto & Morgenbesser, 1960, p. 180). Edward Madden's anthology *Introduction to Philosophy of Science* also contained the first part of the 1948 paper (Madden, 1960, p. 3). At the end of the 1960s "Studies in the Logic of Explanation" had become an undeniable classic. In the 1968 *Oxford Readings in the Philosophy of Science* the editor, Peter Nidditch, judged that "explanation is widely regarded as the primary explicandum for the philosophy of science, while the best known explications offered mold scientific explanations along deductive lines" (Nidditch, 1968, 3). Consequently, a summary of the covering law model written by Hempel in 1962, "Explanation in Science and in History", was taken up in the readings. Baruch Brody's anthology for philosophy of science from 1970 also contained Hempel & Oppenheim's, 1948 paper (Brody, 1970). A count of papers discussing the covering law model in *Philosophy of Science* during the first two decades after its publication similarly shows that the topic of explanation solidified only during the 1960s. Between 1948 and 1958 only four papers discussed Hempel and Oppenheim's paper, whereas 25 papers got written about explanation and the covering law model between 1959 and 1968. By the end of the 1960s explanation had grown into a central research topic within the early discipline of philosophy of science. Now, a whole generation of philosophers was introduced to philosophy of science partly through a discussion of scientific explanation. What is the covering law model, what does it account for, what are some counterexamples? All these questions had become standard.

[28] Hempel already explored these ideas in (Hempel and Oppenheim 1948, p. 141).

4.5 Conclusion

Scientific explanation has existed as a topic for the systematic reflection on science ever since Aristotle's *Posterior Analytics*. Aristotle identified the search for explanations as the main aim of scientific knowledge. When Ernest Nagel began teaching contemporary logic at Columbia University in the 1930s, this Aristotelian gloss on the aim of science was part of his course. Nagel did not want to reduce logic to the formal study of inferences, and included an investigation of the formal structure of science into his logics course. The *Posterior Analytics* was Nagel's preferred paradigm to introduce his students to a broader understanding of logic. Unlike German speaking logical empiricists, Nagel used the history of philosophy as a valid source for insight into the nature of science. For Nagel, a study of logic also implied a critical reflection on the source and validity of the norms proposed for knowledge by logic. This critical reflection required a historical insight into the reason why intellectuals in the past had defended certain norms for knowledge and why some older norms were subsequently abandoned. Because of Nagel's interest in Aristotle from this historico-critical point of view, the intellectual problem how to analyze the structure of scientific explanation became part of the philosophical context of New York during the Second World War.

Although Hempel has often been credited for bringing explanation at the center stage of research in post-war philosophy of science, it was Nagel's historico-critical approach to logic and philosophy of science that brought the topic to the attention of Hempel and, indirectly, to the discipline at large. This genealogical narrative about the re-emergence of scientific explanation shows that a historical approach to the philosophy of science is an enrichment to the discipline. The concepts and methods that philosophers of science use to approach science, or that scientists use to understand their own reasoning, are the result of intellectual decisions from the past. Consequently, a critical reflection of scientific reasoning should scrutinize these historical decisions. Nagel's Logic course and its important effect on philosophy of science as a field is a good example of this.

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Chapter 5
From Cautious Enthusiasm to Profound Disenchantment: Ernest Nagel and Carnapian Logical Empiricism

Thomas Mormann

Abstract In this paper I'd like to study the complex relation between logical empiricism and American pragmatism examining the case of Ernest Nagel. More precisely, I want to explore some aspects of Nagel's changing attitude towards the "new" logical-empiricist philosophy that arrived in the US in the 1930s. In the beginning, Nagel welcomed logical empiricism almost wholeheartedly. This early enthusiasm did not last. Nagel's growing dissatisfaction with the Carnapian version of logical empiricist philosophy was clearly expressed in his criticism of Carnap's inductive logic and more generally in his last book, *Teleology Revisited and Other Essays in the History and Philosophy of Science,* where he criticizes Carnap's philosophy of science in general as ahistoric and non-pragmatist. It is argued that Carnap and Nagel represented opposed possibilities for how the profession of a philosopher of science could be understood: Carnap, as a "conceptual engineer", was engaged in the task of inventing conceptual tools for a better theoretical understanding of science, while Nagel is better characterized as a "public intellectual" engaged in the more general practical project of realizing a more rational and enlightened society.

Keywords Logical empiricism · American pragmatism · History of science · Rudolf Carnap · Ernest Nagel

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5.1 Introduction

The relation between logical empiricism and American pragmatism is one of the more difficult problems in the history of philosophy.[1] This relation cannot be described as a point-like event; rather, it was a process that evolved for various decades. For some time, a variety of contradicting narratives about this difficult and complicated issue have been espoused.

In this paper, I do not want to tell another global story about this issue. Rather, I would like to take a more local perspective and concentrate on the details that concern the vicissitudes of a philosopher who played an important role in the encounter between logical empiricism and American pragmatism, namely, the American philosopher Ernest Nagel (1901–1985). Although Nagel was one of the most influential American philosophers of science in the middle of the twentieth century, he has been unduly ignored in recent debates on the relation between logical empiricism and pragmatism.

In this paper, I want to explore some aspects of Nagel's changing attitude towards the "new" logical-empiricist philosophy arriving from Europe at the shores of the New World in the 1930s. Like many other scientifically minded American philosophers in the beginning, Nagel welcomed the logical empiricists whole-heartedly as allies in the project of the "cooperative, intensive cultivation of the methods of the sciences with the help of the most advanced tools of modern logic" (Nagel, 1940, p. 69).[2] He was one of the younger American philosophers who had visited with the proponents of European logical empiricism in the early 1930s before they were forced to emigrate to the US or elsewhere. Moreover, together with Charles Morris and others, he hosted many European emigrants when they had to build up a new life in US. As I want to show, this early cautious enthusiasm did not last. At the end of his philosophical career in the late 1970s, Nagel's early positive attitude towards logical empiricism (essentially characterized by Carnap's philosophy) had been replaced by a much more reserved attitude to put it mildly.

Thus, instead of a global narrative about the general relation between two multifaceted philosophical currents, I propose to pursue a kind of longitudinal analysis concentrating on one individual philosopher who played an important role in the encounter of the two movements. This is enough to refute some of the sweeping narratives on this issue presently propagated in the literature, or so I want to argue.

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[1] For the sake of simplicity let us assume in the following that there were only two parties of the encounter—the logical empiricists and the American pragmatists. Actually, this is a simplified description of the situation: Reality was more complicated. Science-oriented philosophy in US comprised more than pragmatism, there was an important philosophical current called "(Columbia) naturalism", "contextualistic naturalism", or "realism", which was closely related to but different from genuine pragmatism; cf. Jewett (2011, 2012) and Kuklick (2001). Perhaps it may even be expedient to speak of various different "pragmatisms" at that time; cf. Tuboly (2021).

[2] Certainly not all, however. For instance, according to Jewett (2011, p. 91), in 1948, J.H. Randall, one of the proponents of Columbia naturalism, still described Carnap as "a Prussian systematizer" who was "relatively insulated from the main currents of American experience and thought". Indeed, in the 1940s many pragmatists opposed Carnap-style semantics.

European logical empiricism was present on the American scene, in one way or another, for approximately 50 years, from approximately the 1930s to the 1980s.[3] Nagel's career as a professional philosopher comprised roughly the same period. This fact may be taken as evidence that he may well serve as a (more or less reliable) individual witness of the events that took place in this historical period.

The organization of the paper is as follows. To set the stage, in the next section, I briefly recall some of the main contemporary narratives that are en vogue to describe the complex relationship between logical empiricists and American pragmatists in the second third of the twentieth century. All these narratives are of a global character—they all paint the relation between the two philosophical currents with a broad brush, hardly taking into account the vicissitudes of individual philosophers. As I want to show, they all have difficulties to deal adequately with the case of Nagel. Indeed, he turns out to be an interesting challenge for all existing narratives of the relation between logical empiricism and American pragmatism.

In the third section, I will deal with Nagel's early work that exhibits what may be characterized as a cautious enthusiasm towards the "new logical empiricist philosophy". In particular, Nagel's contribution, "Charles S. Peirce: Pioneer of Empiricism" (Nagel, 1940), at the *5th International Congress for the Unity of Science* in Harvard is considered here.

The topic of Sect. 5.4 is Nagel's growing dissatisfaction with Carnap's version of logical empiricist philosophy in the following decades. This dissatisfaction was clearly expressed in Nagel's criticism of Carnap's inductive logic (Nagel, 1963) and more generally and more explicitly in his last book, *Teleology Revisited and Other Essays in the History and Philosophy of Science* (Nagel, 1979).[4] There, he criticized very harshly Carnap's philosophy of science in general as ahistoric and outdated.

One of the distinctive features of Nagel's philosophy of science is the emphasis that he put on the role of the history of science for philosophy of science. Compelling evidence for this attitude is seen in his works on the history of geometry and algebra that, to the present day, are considered valuable contributions to the history of ideas. This aspect of Nagel's philosophy of science is treated in Sect. 5.5. Finally, in Sect. 5.6, we briefly discuss the question of who the audience of philosophy of science is. One may say that Carnap and Nagel represented opposed possibilities for how the profession of a philosopher of science may be understood: As will be discussed Carnap as a "conceptual engineer" was engaged in the task of inventing the conceptual tools for a better understanding of science as a complex endeavor in itself, while Nagel was to be considered more of a "public intellectual" engaged in the project of realizing a more rational and enlightened society.

[3] Herbert Feigl came to America in 1931, Carnap passed away in 1970. Feigl and Hempel, often characterized as the "last logical empiricists", lived until 1988 and 1997, respectively.

[4] This book contains (among some other earlier texts of Nagel) his *Dewey Lectures*, which he delivered at Columbia University in 1977. These may be considered a kind of *summa* of Nagel's philosophy and history of science.

5.2 The Encounter of Logical Empiricism and American Pragmatism: A Potpourri of Narratives

The encounter between European logical empiricism and American pragmatism was a complex and multifaceted event of the history of philosophy in the twentieth century. Perhaps the simplest narrative of this event is Richard Rorty's version of the (temporal) replacement of home-spun American pragmatism by logical empiricism. It goes like this:

> Along about 1945, American philosophers were [...] *bored* with Dewey, and thus with pragmatism. They were sick of being told that pragmatism was the philosophy of American democracy, that Dewey was the great American intellectual figure of their century, and the like. They wanted something new [...] What showed up [...] was logical empiricism, an early version of what we now call "analytic philosophy".
>
> The incursion of this kind of philosophy was [...] a mixed blessing. [...] [I]t represented a temporarily fruitful confusion of a *very* good idea (that language was a more fruitful topic for philosophical reelection than experience) with a couple of rather bad ones (that there was something worth preserving in empiricism; [...]). (Rorty, 1995, p. 70)

Fortunately, according to Rorty, the eclipse of pragmatism did not last for long. With Rorty himself actively engaged in the project, the eclipse was overcome in the next decades. At least, this is the story that Rorty wanted to make his audience believe:

> The narrative I have tried to construct in my books tells how the bad ideas gradually, in the course of the 1950s and 1960s, got filtered out and thus made it possible for pragmatism to get a new lease on life by undergoing linguistification. (Rorty, 1995, p. 70)

A different version of this eclipse narrative has been offered by Scott Soames. He skips the second half of Rorty's story, i.e., for him the alleged resuscitation of classical pragmatism in the form of (Rortyan) *neo*-pragmatism the heroes of which were Dewey, Wittgenstein, and Heidegger, does not exist. In his narrative, even Dewey does not occur (to say nothing about Wittgenstein and Heidegger). According to Soames, the only contribution that American pragmatism had to offer to the new analytical wave was the logical achievements of Peirce and C.I. Lewis. As minor figures in the transition from the pre-analytic to the analytic period in American philosophy, Morris Cohen and Nagel are briefly mentioned (Soames, 2008, pp. 451–452).

Not all people agree with the stories told by Rorty and Soames. Cheryl Misak, for instance, in her book *The American Pragmatists* (Misak, 2013), completely discards the replacement or eclipse narrative. According to her, the logical empiricist invaders were assimilated by American pragmatism in such a way that they hardly left any trace:

> One thing, however, should be clear from my account of the fortunes of pragmatism. Those who would argue that pragmatism was bullied into the backwaters by the logical empiricists … have their intellectual history wrong. Not only were there strong connections between pragmatism and logical empiricism, but *the logical empiricists drifted closer and closer to their pragmatist cousins* until the views were almost indistinguishable. (Misak, 2013, p. 254; original emphasis)

These incompatible narratives may leave the reader somewhat perplexed, since even the simple question "At the end of the day, who replaced whom?" does not find a unanimous answer. Even less satisfying is what this potpourri of narratives has to offer for the task of determining Nagel's position in this changing conceptual landscape. As usual, Rorty's overall general pastiche is of no use for discovering any detail. Soames mentions Cohen's and Nagel's early contributions to a (broadly understood) naturalist and empiricist philosophy of science but ignores Nagel's later criticism of Carnapian logical empiricism. Misak's assimilation narrative entails that Nagel should have recognized the later Carnap as a fellow pragmatist. However, as will be shown in the next section, this was not the case. Rorty, Soames, and Misak offer competing global narratives about the question of how the evolving relation of logical empiricism and American pragmatism may be understood.

The following two proposals of Alan Richardson and Thomas Uebel are of a somewhat different nature. These authors deal with some more specific aspects of the encounter of the two movements that mainly concern not logical empiricism and American pragmatism in general but concentrate their attention on a small group of individuals. Nevertheless, also for them Nagel's case is a challenge.

Richardson's focus is on Carnap, Lewis, and Dewey. According to Richardson (2007), the emigration to America and the contact with American pragmatists led Carnap to adopt a kind of *sui generis* pragmatism. As Richardson points out, Carnapian pragmatism must be distinguished from "genuine" American pragmatism of, say, Dewey or Lewis. That Nagel did not recognize the later Carnap's philosophy as pragmatism may be read as a confirmation that "Carnapian pragmatism" was not "genuine" or "ordinary" American pragmatism. Indeed, according to Richardson, the two versions of pragmatisms were essentially different:

> [T]he key features of Carnap's version of pragmatism are not features his pragmatism shared with the leading American pragmatists of his era. In particular, both Dewey and Lewis wanted to ground their pragmatism in an account of human nature, … [A]t the end of the day, it is the difference over methodological naturalism that undergirds the difference between Carnapian pragmatism and the pragmatism of Lewis and Dewey: Lewis and Dewey both need and think they can have a place in philosophy to speak of the proper nature and interests of humanity; Carnap has no such need and no such place. Richardson (2007, p. 312)

Thus, if Richardson is right (I believe he is), the case of Carnap refutes Misak's just mentioned sweeping thesis that "the logical empiricists drifted closer and closer to their pragmatist cousins until the views were almost indistinguishable." As will be shown in more detail in Sect. 5.4, also Nagel did not see Carnap as someone who had drifted towards pragmatism. Even more, he denied the later Carnap's philosophy the predicate of pragmatism. In sharp contrast, other European empiricists, such as Frank, Neurath, and Hempel, were considered by him as fellow pragmatists.

Another proposal for understanding the evolution of logical empiricism in America has been proposed by Uebel. His thesis is

> that we can ascribe to [the members of the left wing of the Vienna Circle] the conception of a [...] "bipartite metatheory", a conception of philosophy of science as comprising both formal-logical and empirical investigations. [...] This conception constitutes the "unified science"

> alternative to Moritz Schlick's Wittgensteinian conception as meaning determination. (Uebel, 2012, p. 117)

The bipartite metatheory seems to offer an elegant and ecumenical ("tolerant") way of doing a philosophy of science that everybody might feel comfortable with. Indeed, Carnap may be characterized as a partisan of such a theory. In a letter to Robert S. Cohen, he described the task of such a bipartite metatheory as follows:

> For a total (not only logical) theory and analysis of knowledge and science, it is certainly very important to take into account also activities, including (1) the practical behavior of scientists in their research work (this may include pragmatics but goes far beyond it), and (2) the ways in which science is of help in all fields of practical life. I have myself not made any investigations of these kinds; but this does not mean that I regard them as less important. (Cohen, 1963, p. 150; quoting from a letter of Carnap written to Cohen dated 12. August 1954)

Cohen was less than fully convinced by Carnap's answer and objected: "But what is the status of a purely logical analysis of knowledge in a total theory of scientific knowledge, once pure syntactic and pure (formal) semantic reconstructions are left behind?" (ibid.)

Carnap seems to have assumed that the logical and the non-logical ingredients of a "total theory" of knowledge and science can be juxtaposed and put together in such a way that they form a conceptual whole. How the purely logical and the non-logical parts of the total theory fit together, how they interacted with each other (if at all), was not a problem for him. Not all of his fellow philosophers were able to conceive of the issue in such a relaxed manner. For instance, Nagel was unable to see the conceptual unity allegedly underlying the two components of the bipartite metatheory. He came to the conclusion that Carnap's version of philosophy of science was a deadlock, while he considered philosophers such as Frank, Hempel and others to be kindred spirits.

More generally, Nagel is a problem for all existing narratives that aim to explicate the complex relation of logical empiricism and pragmatism: Nagel is a problem for Misak's drifting thesis, since he would have vigorously denied that Carnap had drifted towards genuine pragmatism. Nagel has been a challenge for Rorty's version of the eclipse narrative because, until the end of his life, he stuck to the thesis that science is the basis for a humane and liberal civilization (Nagel, 1979, p. 10). Very probably, Nagel would not have subscribed to Richardson's thesis that Carnap's later philosophy of science can be characterized as a kind of pragmatism. Of course, one may contend that Nagel simply got it wrong. However, perhaps this is a solution to the problem that is slightly too simple to be right.

5.3 Cautious Enthusiasm

In 1939, the *Fifth International Congress for the Unity of Science* took place in Harvard/Massachusetts. Nagel was one of the congress participants. He took this

opportunity to remind participants of the centennial anniversary 1839 of Peirce, presenting Peirce as a "pioneer of empiricism", who had anticipated many of the insights that contemporary American pragmatism and the empiricism of the Vienna Circle had obtained independently from each other. For Nagel, this kind of convergence (which often occurred in science) was to be considered evidence that some measure of truth had been attained:

> It is therefore a happy sign that so many of the central ideas of the present movement have been independently developed on both sides of the Atlantic. One is not minimizing the contributions of the Vienna Circle in pointing out that many of its recent views have been taken for granted for some time by American colleagues, largely because the latter have come to intellectual maturity under the influence of Peirce (Nagel, 1940, pp. 69–70).
>
> Were [Peirce] still among us he would surely have endorsed the happy marriage of the cultivation of logic and the empirical temper which distinguishes this movement, and he would have joined hands with us in furthering the quest for and the understanding of progressively more adequate tools of inquiry (Nagel, 1940, p. 80)

Presenting Peirce as the founding father of empiricism required, of course, a considerable amount of philosophical surgery on the body of Peirce's philosophy.[5] The young Nagel had no qualms about doing just this. Moreover, according to Nagel, the Vienna Circle's logical empiricism was an expedient tool for improving the central notion of Peirce's pragmatism:

> "Peirce's own formulation of the pragmatic maxim leaves much to be desired in the way of explicitness and clarity; and more recent formulations, such as those by Professor Carnap and others, have the same general intent but superior precision. (Nagel, 1940, p. 73)

Regrettably, Nagel never pointed out more precisely where Carnap had achieved such a remarkable deed, and Carnap never boasted to have done it. Thus, it remained unclear exactly where Carnap had proposed an improved version of Peirce's pragmatic maxim, to say nothing about the issue of whether he had faithfully followed such a maxim in his own philosophical work.

Although Nagel's thesis that Carnap had the merit of having improved Peirce's pragmatic maxim may sound slightly overstated, to put it mildly, Nagel was not the only one who claimed a profound affinity between logical empiricism and American

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[5] To render plausible this claim, one does not need a subtle interpretation of Peirce's thought nor a detailed elucidation of what exactly the logical empiricists of the Vienna Circle understood by "metaphysics". It should be enough to read the following passage from the later Peirce's *The New Elements of Mathematics* (1904) and to imagine how the arch-logical empiricist Neurath would have reacted to it: "What we call a 'fact' is something having the structure of a proposition, but supposed to be an element of the very universe itself. The purpose of every sign is to express 'fact,' and by being joined with other signs, to approach as nearly as possible an element of the very universe itself. The purpose of every sign is to express 'fact,' to determining an interpretant which would be the *perfect Truth,* the absolute Truth, and as such […] would be the very Universe. […] We may adopt the word *entelechy* [from Aristotle] to mean the very fact, that is, the ideal sign which should be quite perfect, and so identical, – in such identity as a sign may have, with the very matter denoted united with the very form signified by it. The entelechy of the Universe of being, then, the Universe *qua* fact, will be that Universe it its aspect as a sign, the 'Truth' of being. The 'Truth,' the fact that is not abstracted but complete, is the ultimate interpretant of every sign" (Peirce 1976, pp. 239–240).

pragmatism. Much later, a fellow empiricist of Carnap, Philip Frank, confessed that he had had a similar lightning recognition long ago even before the two had arrived in America:

> When I read [Carnap's *Aufbau*] it reminded me strongly of William James's pragmatic requirement, that the meaning of any statement is given by its "cash value," that is, by what it means for human behavior. I wrote immediately to Carnap, "What you advocate is pragmatism. This was as astonishing to him as it had been to me. We noticed that our group [...] had reached conclusions by which we could find kindred spirits beyond the Atlantic in the United States. (Frank, 1949, p. 33)

In recent years, many different and quite sophisticated interpretations of *Aufbau* have been proffered; none of them, however, confirms Frank's reading of Carnap's *opus magnum*. Be this as it may, there can be no doubt that in the 1930s, members of both groups were strongly engaged in the project of bringing together the two philosophical movements. Even Carnap may have appeared as someone who had a keen interest in the project of t logical empiricism and American pragmatism joining forces. In *Testability and Meaning* (Carnap, 1936–37), he jettisoned unnecessary philosophical ballast that hindered a closer alliance with pragmatism: First, he pointed out that methodological solipsism should not be considered the only possible solipsism and not even the best interpretation of the *Aufbau*. *A fortiori*, in no way should the logical empiricist philosophy of science be considered as being committed to methodological solipsism in general. Second, he abandoned the overly strong and unrealistic concept of (complete) verification, replacing it with (gradual) confirmation. *Testability and Meaning* is often taken as evidence for the emergence of a "new" flexible Carnapian logical empiricism that subscribed to some essential philosophical theses of American pragmatism. A closer look reveals that this apparent assimilation was far from complete.

Carnap complied with the pragmatist doctrine only on one point, namely, that the absolute verification of synthetic assertions was impossible, and thus, one had to be content always with a more or less complete confirmation. Other essential, possibly anti-pragmatic, ingredients of Carnap's logical empiricism did not change after he came into contact with American pragmatism.

From early on, Nagel was well aware of certain possibly non-pragmatic aspects of Carnap's philosophizing. This was already evidenced in Nagel in 1940. On the one hand, he praised the Viennese philosopher as a kind of contemporary reincarnation of Peirce who had formulated Peirce's "pragmatic maxim" in a more precise and better way than Peirce himself; on the other hand, he criticized Carnap—without explicitly mentioning his name—for not complying with the standards that Peirce had already set forth for a good pragmatist. Thus, from the very beginning, Nagel's enthusiasm for Carnap's logical empiricism must be characterized as a reserved or cautious enthusiasm. More precisely, in the early 1940s, Nagel's reservations concerned the question of whether Carnap's emerging semantics were fully compatible with a truly empiricist philosophy of science:

> Some have suspected, perhaps unjustly, that the recently inaugurated discipline of semantics will open wide the door for the rehabilitation of Bolzano's Saetze-an-Sich, Meinong's

> objectives, Russell's subsistents, and allied conceptions of the referends of signs. […] I think it would be a retrograde step if modern logical empiricism were to revive them in a new form; for the great strength and promise of the movement has been its interpretation of the abstract in terms of the concrete, and its resolute turning from speculations which have no ascertainable consequences in issues of observable fact. I can think of no better way to still these suspicions than by placing the study of semantics into a behavioral context, and by instituting an analysis of such key semantic terms as "designation" and "truth" as used in specific contexts, in order to reveal the modes of action they signify. (Nagel, 1940, pp. 76–77)

Obviously, Nagel considered himself to be one of those "who suspected…".[6] His suspicion was confirmed a few years later when Carnap published his groundbreaking *Introduction to Semantics* (Carnap, 1942). This is evident by Nagel's review of this work in Nagel (1942). Carnap was at pains to dispel the concerns that his "empiricist friends" had with respect to semantics. He wanted to convince them that semantics were empiristically innocent. For this purpose, he published *Empiricism, Semantics, and Ontology* (ESO) in 1950 (Carnap, 1950). He argued that semantics are empiristically harmless. Now, the interesting point is the following: In 1954 (i.e., four years after the publication of ESO), Nagel considered it appropriate to republish the paper of 1940 in which he had voiced empiricist concerns with respect to semantics. At that time, Nagel certainly had taken notice of ESO, which had appeared in 1950. Nevertheless, he stuck to his 1940 paper. This evidence indicates that he did not accept Carnap's defense of semantics in ESO. Rather, he stuck to his original criticism that Carnapian semantics was suspicious from an empirical point of view.

In the sequel, Nagel felt no inclination to revise his verdict. This is seen by the fact that he published his criticism of Carnapian semantics twice without any change, although Carnap had seriously attempted to dissolve the empiristically and pragmatically founded doubts concerning his semantics.

Let us return to Nagel and his approximation of the new logical empiricist wave. In 1939, he published one of the early successful contributions to Neurath's *International Encyclopedia of Unified Science*, namely, the monograph *The Principles of Probability* (Nagel, 1939). *The Principles of Probability* became a well-recognized standard work and could be considered an example of a successful collaboration between logical empiricism and pragmatism in the context of Neurath's *Encyclopedia of Unified Science*. Nagel intended to take Peirce's pragmatism as a framework for a genuinely pragmatist approach to probability. In the course of time, it turned out that Nagel's conception was essentially different from that of Carnap and Reichenbach.[7]

Nagel's *Principles of Probability* intended to satisfy all requirements that a good unifying item of the *International Encyclopedia of Unified Science* should satisfy.

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[6] Another prominent logical empiricist who suspected that Carnap's "semantic turn" was a move away from true empiricism was Neurath, who severely criticized Carnap in their correspondence in the 1940s (cf. Mormann 1991). The quarrel concerning semantics led Carnap and Neurath almost to the breaking of their friendship. For some details, see the recently published letters of Carnap and Neurath in Cat and Tuboly (2019).

[7] On a detailed presentation of Nagel's view on probability and his monograph in the *Foundations of the Unity of Science*, see Galavotti's contribution to the present volume.

Nagel based his considerations on the founding fathers of American Pragmatism, Peirce and Dewey, but also mentioned Carnap's then quite recently published *Testability and Meaning* and Frank's *Das Kausalgesetz und seine Grenzen.* Moreover, he brought Morris' trisection of syntax, semantics, and pragmatics into play, characterizing *Principles* as a contribution that was relevant for its semantics and pragmatics of the concept of probability and not for its syntax.

Thus, at least on the surface, *The International Encyclopedia of Unified Science* presented itself as a successful model of collaboration between logical empiricism and American pragmatism. In the official press announcement of the *International Encyclopedia* that was published in the volumes of all monographs of the first volume *Foundations of the Unity of Science*, the editors pointed out that the collaborators of the encyclopedia might have different points of view but that all agreed in considering the unity of science as the ideal aim of their efforts:

> They agree that any form of speculation other than that recognized in science has to be eliminated they stress the importance of logical analysis in various fields and in taking into account the historical development of scientific concepts and regulative principles. Such collaborators include, for instance, persons stemming from the Vienna Circle, from the Berlin group of scientific philosophers, from the Polish school of logicians, from the group centering around Scientia and the Centre de Synthèse, as well as representatives of American pragmatists, the English analytical school, French conventionalism, [...].
>
> (Foundations of the Unity of Science)

This description of a peaceful and harmonious collaboration of the various groups was—of course—a highly idealized picture of what truly happened. Behind the curtain, the activity of the proponents often could not be described as very harmonious (cf. Reisch, 2005; Dahms, 1999). In particular, there were heavy quarrels over who should be the author for the piece on probability and induction. Originally, Reichenbach was chosen as the author for this item, but for several reasons, this was not realized (see Dahms (1999) for details). The clash between Nagel's and Carnap's conceptions of probability broke out only later in the 1950s and 1960s, since in the 1940s, Carnap's ideas on probability and induction were still in an embryonic state. Hence, Nagel's empiricist-pragmatist piece seems to have pleased everybody (with the possible exception of Reichenbach). It may have even been considered as evidence that Charles W. Morris's program of a synthesis of formalism, pragmatism, and traditional empiricism that combined the virtues of these accounts while avoided their shortcomings was feasible.[8] The idyllic picture of a fruitful collaboration of European logical empiricism and American pragmatism that the *Encyclopedia*

[8] Morris had sketched his program of a comprehensive, practice-oriented scientific philosophy already at the *International Congress of Unified Science* in Paris 1935. He published a more elaborated version two years later in his booklet *Logical Positivism, Pragmatism and Scientific Empiricism* (Morris, 1937). Against the overly narrow logical empiricist understanding of philosophy as the syntax of the language of science (that Carnap had propagated) Morris argued for a "scientific pragmatism" that comprised four levels: (1) Philosophy as Logic of Science, (2) Philosophy as Clarification of Meaning (Peirce), (3) Philosophy as Empirical Axiology (Dewey), and (4) Philosophy as Empirical Cosmology (Whitehead).

project (and more generally, Morris's *Paris program*) might have offered to a superficial observer in the 1940s did not last; with respect to the issue of probability and induction, it would turn out to be an illusion only a few years later.

5.4 Alienation and Disenchantment

When exactly Nagel's disenchantment with orthodox logical empiricism began is hard to say. Although in his contribution to the *Fifth International Congress for the Unity of Science* 1939 in Harvard, he described American pragmatism as being in full harmony with logical empiricism, even though at that time, Nagel was not a dyed-in-the-wool logical empiricist. This is shown by his work on the philosophy of mathematics around the same time the logical empiricists arrived in the New World (Nagel, 1935/1979, 1939/1979). In this work, Nagel ascribed to the history of science a much more prominent role for the philosophy of science than orthodox logical empiricists would have been prepared to swallow. This issue will be treated in more detail in the next section.

In any case, a profound alienation between the philosophical outlooks of Carnap and Nagel is clearly documented in Nagel's contribution to the Schilpp volume dedicated to Carnap (Schilpp, 1963) and Carnap's rejoinder to it. Nagel's criticism of Carnap's inductive logic is to be considered much more than a disagreement between fellow philosophers who considered each other to belong to the same movement.[9] Moreover, Nagel's "most ungracious essay" (Nagel, 1963, p. 825) cannot be dismissed as an insulated, perhaps only temporal disagreement concerning some technical details. Nagel republished it in his last book, apparently considering it an important piece of his philosophical legacy.[10] Nagel rejected Carnap's conception of probability for fundamental reasons. According to him, Carnap's account of probability and induction had little to do with the way these concepts were used in common-day life and the sciences. As he put it:

> [...] if the major criticisms advanced in [this essay] hold water, it shows that despite the remarkable constructive power and ingenuity Carnap has brought to the reconstruction of inductive logic, he has not resolved the outstanding issues in the philosophy of induction, and his general approach to the problems is not a promising one. (Nagel, 1963, p. 825)

Carnap's answer to Nagel's politely formulated, but radical, critique was unmistakable:

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[9] Schilpp (1963) was published with a long delay. Hence, it may seem plausible to assume that Nagel's contribution is the result of work that can be traced back well into the mid-1950s.

[10] Nagel's alienation and disappointment with orthodox logical empiricism as evidenced by Nagel (1979) has been ignored in the secondary literature: Limbeck-Lilienau (2012) and Misak (2008, 2013) do not mention Nagel (1979) at all. This holds true, of course, for Uebel (2016), since this paper only deals with the early years of the relation between the Vienna Circle and American pragmatism.

> I am sorry that my overall reaction to the essay by my dear old friend Ernest Nagel could not be more positive. My convictions on the possibility and the nature of inductive logic, acquired in many years' work and vindicated by constant reexamination, can only be shaken by strong arguments (Carnap, 1963, p. 995)

In plain English, then, for Carnap, Nagel had failed to bring forward "strong arguments" against the former's inductive logic.[11] Nagel was not impressed by Carnap's harsh criticism. On the contrary, he flatly ignored it and stuck to his position until the end of his life. Almost ten years after Carnap's death, he re-published his verdict, put forward in Schilpp (1963) without any change or any further explanation. Thus, it may be considered his last word on Carnap's inductive logic, which some, Carnap himself among them, considered to be the flagship of his philosophy of science. Nagel was not just anybody in the logical-empiricist community. After all, he was the author of *The Principles of Probability Theory* (Nagel, 1939), i.e., logical empiricism's official monograph on probability and induction, in *The International Encyclopedia of Unified Science.*

At the end of his philosophical career, Nagel's critique of Carnapian logical empiricism was no longer confined to Carnap's inductive logic. Its target had been widened and become Carnap's philosophy of science in general. In one of Nagel's introductions (1979), he singled out Carnap's version of the logical empiricist philosophy of science, calling it obsolete in its entirety. Compared with the classical pragmatisms of Peirce and Dewey on the one hand and other versions of logical empiricism such as those of Frank and von Mises, Carnap's orthodox logical empiricism came off the worst. Pointing out that the philosophy of science of the philosophy of the logical empiricists Frank and von Mises was not formalist or ahistoric, Nagel blamed the Carnapian philosophy of science as being responsible for the recent rise of a new orientation in the philosophy of science that was skeptical of the efficacy of the scientific method for attaining genuine knowledge:

> Much of the animus of the "new orientation" in the philosophy of the science is directed against the alleged ahistorical character of the "orthodox approach"; against the latter's supposed claim that the observational evidence for a scientific theory can be assessed by using the rules of a formal calculus; […] These characterizations of the "old philosophy of science" are conceivably true of *some* philosophers in this category (for example, Rudolf Carnap). These characterizations are a caricature of *most* of the older generation of writers on the subject (for example, C.S. Peirce, Josiah Royce, John Dewey, M.R. Cohen, or P.W. Bridgman), and they are not even true of some of Carnap's fellow logical positivists (such as Philipp Frank or Richard von Mises). Unlike Carnap, none of these thinkers subscribed to an ahistorical evaluation of the evidence for a scientific theory; and none of them identified the rationality of science with the use of exclusively formal canons for assessing claims to

[11] Nagel was not the only contributor who was rebuked by Carnap in this way: According to Carnap, Popper had—once again—completely misunderstood him, Putnam's claim that Carnapian inductive logic was impossible was simply dismissed. Carnap did not disagree, however, with Kemeny, who put forward the thesis that "the problem of induction […] [was] certainly the central issue in any philosophy of science" and ended with the hymnical conclusion that "we must class Carnap's contribution to the problem of induction among the greatest achievements of modern Philosophy (sic)" (Kemeny, 1963, pp. 711, 737). Recent assessments of the feasibility of an inductive logic in Carnap's do not confirm Kemeny's praise of Carnap's achievement (cf. Sterkenburg, 2018a, b).

> knowledge. It is misleading to ascribe to all representatives of the "orthodox approach" to the philosophy of science the beliefs that are idiosyncratic of what at best is a relatively small subset of that group of thinkers. (Nagel, 1979, p. 3)

This global criticism may be considered an extrapolation of Nagel rejection of Carnap's project of inductive logic. Nagel did not subscribe to a kind of "bipartite metatheory" such as that of Uebel, according to which logical empiricists such as Frank and von Mises on the one side and Carnap on the other side worked on the same project of a comprehensive logical empiricist philosophy of science.[12] Nagel did not recognize the allegedly possible division of labor between those who dealt with the logical aspects of science and those who concentrated on the empirical aspects. At the end of the day, Nagel considered Carnap's logical way to be misguided and fruitless.

While in his replies to Nagel, Putnam, and Popper in Schilpp (1963) Carnap sharply rejected the criticisms that these authors had put forward against his logic of induction, his replies to Morris, Cohen, and Frank are formulated in more reconciliatory tone. With respect to pragmatism, in rather vague terms he even expressed his gratitude to American pragmatism in general:

> The influence of the pragmatist ideas has been very fruitful for the development of my conceptions. It did not derive so much from the works of the founders of pragmatism (whose formulations I could often not easily accept, e.g., Peirce's metaphysics and Dewey's discussions of logical and epistemological problems), but from later representatives such as C.I. Lewis, Charles Morris, Ernest Nagel, and Sidney Hook, whose formulations seemed clearer and closer to those customary in science. (Carnap, 1963, p. 861)

Thereby Carnap elegantly placed himself on the side of rationality and science: Although classical pragmatists such as Peirce and Dewey scientifically left something to be desired, Carnap generously admitted that things had improved with the younger generation of pragmatists. This sounded more like a substantial rapprochement between pragmatism and logical empiricism than it truly was: Actually, it is hard to find any reference to Lewis in Carnap's work—with the exception of "Testability and Meaning" (Carnap, 1936–37). The same is true for references to Morris, Nagel and Hook.[13]

In any case, the alleged "fruitful influence of pragmatist ideas on [Carnap's] conceptions" was not later acknowledged by Nagel. In contrast, he did not recognize Carnap as a fellow pragmatist. At the end of his philosophical career, Nagel considered Carnap to be representative of an obsolete formalist philosophy of science. On

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[12] Perhaps surprisingly, in Nagel (1979), Neurath and the *International Encyclopedia of Unified Science* are not mentioned even once, whereas Hempel's account of functional explanation in biology is discussed in detail in the last section of the book.

[13] For instance, the above quotation above is the only one where Carnap mentions the work of Sidney Hook in Schilpp (1963). One of the editors (Tuboly) has pointed out to me that the lack of direct reference to Morris, Nagel, and Hook in Carnap's work leaves open the possibility that these authors "influenced Carnap informally by every-day discussions, joint seminars" and the like. I must admit that I have no idea what this could mean exactly. Rather, I would interpret Carnap's apparently liberal attitude to these and others scholars as a clever strategy designed to appear as a paragon of tolerance and open-mindedness and at the same time pursue his own very specific philosophical projects without paying too much attention to others.

the other hand, Nagel later explicitly sympathized with Frank's Austro-American version of logical empiricism. That is, he did not subscribe to an approach that, much later, Uebel baptized as a "bipartite metatheory" of the philosophy of science to which the members of the left wing of the Vienna Circle subscribed. According to Uebel, Carnap's formal philosophy and science and the more empirically oriented psychological, sociological, and historical works of his fellow Vienna Circle philosophers of science were to be considered two components of a comprehensive "bipartite metatheory" of the philosophy of science. Whatever the virtues of the bipartite metatheory might be, Carnap, as one of its early practitioners, did not convince his "good old friend" Nagel of them.

5.5 A Role for the History of Science in the Philosophy of Science

For some time, it has been commonplace knowledge that a good philosophy of science cannot be developed without taking into consideration a good amount of the history of science.[14] For Nagel, the thesis that the philosophy of science has to take into account the history of science would hardly have been exciting news. For his *John Dewey Essays*, he chose the title *Teleology Revisited and Other Essays in the Philosophy and History of Science.* Throughout his career as a philosopher of science, issues of history of science played an important role for him. This holds true particularly for the history of mathematical sciences, which he had pursued since the beginning of his career as a professional philosopher; for details on this, see his "'Impossible Numbers': A Chapter in the History of Modern Logic" (Nagel, 1935/1979) and "The Formation of Modern Conceptions of Formal Logic in the Development of Geometry" (Nagel (1939/1979).

Nagel's contributions to the philosophy and history of mathematics are still discussed today (cf. for example, Kitcher (2012), Stump (2015), and Blanchette (2017)). The fact that Nagel republished these early pieces in his last book after more than forty years is evidence that he considered them permanently relevant pieces of his philosophical work.[15]

Let us briefly discuss "Impossible Numbers" and compare it with a piece by Carnap written slightly earlier that deals with the issue of "impossible numbers", although of a different kind. Today, the "impossible numbers" Nagel dealt with are

[14] For a discussion of the various ways in which the philosophy and history of science may collaborate in order to provide a better understanding of their common subject, see the introduction of the Festschrift for Michael Friedman *Discourse on a New Method—Reinvigorating the Marriage of History and Philosophy of Science* edited by Domski and Dickson (2010).

[15] This is confirmed by the following late comment of Nagel, found in Nagel (1979): "[These papers] are the sole fruits of a long since abandoned plan to write a comprehensive history of changes during the nineteenth century in methodological ideas employed in various branches of inquiry—in the natural, psychological, and social sciences, but also in a number of humanistic disciplines (such as history, legal scholarship, and hermeneutics" (Nagel 1979, p. 318).

just the familiar "complex numbers". They have the form $(a + ib)$ and appear as roots of polynomials $p(x): = a_n x^n + a_{n-1} x^{n-1} + \ldots + a_1 x + a_0$. From the perspective of modern mathematics, the numbers $(a + ib)$ (in particular their "imaginary parts" ib) are no more mysterious than ordinary "real" numbers Thus, some conceptual effort is required to understand why, for such a long time in the evolution of mathematics, these numbers (and others as well) were considered epistemologically and ontologically dubious.

Traditionally, a "number" is conceived of as being an answer to the question "How many?" and, in cases of extensive measure, "How much?" Evidently, complex numbers cannot be considered reasonable answers to these traditional questions. Consequently, they are "impossible numbers". Negative numbers are difficult to conceive of as answers to questions concerning quantity and extension. On the other hand, "impossible numbers" are undeniably useful. Thus, if mathematics is to be the science of quantity, then complex numbers, not being quantities in any intelligible sense, must be considered "impossible numbers". Thus, it cost mathematicians and philosophers alike a great deal of conceptual effort to understand that mathematics should not be understood as the science of quantity in the traditional sense.

In the following, only "Impossible Numbers" will be treated in some detail. It should be noted, however, that in "The Formation of Modern Conceptions of Formal Logic in the Development of Geometry", Nagel argued for the very same general thesis, namely, that for a more profound understanding of modern science, in particular for the understanding of modern logic and mathematics and that their role in the ongoing development of modern science and the history of the sciences is essential. This attitude stands in strong contrast to that of Carnap, who around the same time discussed another kind of "impossible number" but with a quite different attitude than Nagel. Thus, Nagel's philosophy of mathematics was interested in a history of mathematical ideas or concepts that went beyond a mere presentation of their logical relations. More precisely, Nagel argued that some.

> Central doctrines of contemporary logic will become illuminated and made more persuasive by examining the developments in which they terminate. In particular, a consideration of the procedures of mathematics within the historical settings in which they operate may provide materials for a just appraisal of the limitations of traditional conceptions of mathematics and logic, as well as of the more recent views that have replaced them. (Nagel, 1979, p. 196)

In Carnap's logicist philosophy of mathematics, one does not find anything even remotely similar. From a Carnapian perspective, there was always a strict distinction between a logically correct formulation of a concept, and some logically flawed precursors. The on-going evolution of science was assumed always to end in "unobjectionable logically precise definitions". An instance of this attitude is provided by Carnap's remarks on infinitesimals as a kind of "impossible number" vividly discussed by many mathematicians and philosophers of mathematics since the end of the nineteenth century. Carnap gave them short shrift:

> The inventors of the infinitesimal calculus (Leibniz and Newton) [...] could not say [...] what actually is to be understood by the "derivative" of a function. They could ... not give a precise definition of the concept "derivative". However, their formulations for this definition used

> such expressions as "infinitesimally small magnitude" [...] turn out to be pseudoconcepts (empty words). It took more than a century before an unobjectionable definition of the general concept of a limit and thus of a derivative was given. Only then all those mathematical results which had long since been used in mathematics were given their actual meaning. (Carnap, 1928/1967, pp. 307–308)

For Carnap, the history of science did not teach anything to the philosophy of science. This holds even after Carnap came to emphasize the importance of "logical tolerance". This is evidenced by the fact that he completely ignored the logical and philosophical relevance of Robinson's "Non-standard analysis" that offered a logically flawless definition of "infinitesimally small numbers".

In contrast, according to Nagel, we may learn a lot about science from the history of a science. This holds true, in particular, for mathematics. For instance, from the history of mathematics we can learn that "[t]he proper and exclusive subject matter of mathematics is not quantity", as many philosophers have claimed (Nagel, 1935, p. 167). This thesis is elaborated in considerable detail in his papers "'Impossible Numbers': A chapter in the History of Modern Logic" and "The Formation of Modern Conceptions of Formal Logic in the Development of Geometry". The logical empiricists' official "identification" of mathematics and logic, or, more precisely, the "derivation" of the former from the latter, is quite useless in this endeavor. Thus, Nagel's work in the philosophy and history of mathematics goes in a quite different direction than Carnap's logicist approach.

5.6 Philosophy of Science—Who is the Audience?[16]

The difference between a logical empiricist philosophy of science and a pragmatist philosophy of science, as exemplified by Carnap and Nagel, should not be conceived of solely as a matter of different personal styles. Rather, it points to a profound difference in how the role of the philosophy of science is understood.

Nagel's account was based on the pragmatist assumption that the task of the philosophy of science is to clarify the broad significance of science for human life. This significance goes beyond the practical control over nature that science yields. Science is more than a set of practically useful technologies. For Nagel, science made the world intelligible. It satisfied the human craving to know and understand, as Aristotle had asserted.[17] The task of a philosophy of science is to contribute to a "scientific culture", as the Neo-Kantian Cassirer would have said. A Carnapian philosopher of science might not have militated explicitly against this aim, but he or she would have preferred to describe the philosopher's task more theoretically as

[16] The second half of this section's title is borrowed from Kitcher (2019).

[17] Indeed, as Dewulf (2018, pp. 156–157) recently argued convincingly, Nagel's philosophy of science may be characterized as a kind of Aristotelian philosophy of science. See also Dewulf's contribution to this volume.

kind of conceptual engineering directed towards the improvement of the conceptual apparatus of science.

Nagel was an Aristotelian naturalist who considered the desire for knowledge to be an ingredient of human nature. Patrick Suppes, who was Nagel's student and later became one of the most influential philosophers of science of the second half of the twentieth century, wrote in a biographical memoir of his teacher:

> [What] is most important to emphasize about [Nagel's] more than forty years' association with Columbia University is the central role he played in the intellectual life of Columbia, and more generally, of New York City. To many students he was the outstanding spokesman of what philosophy could offer in terms of the analysis of the scientific method, as it is practiced in many different sciences, and in the relation between science and perennial problems of philosophy such as those of causality and determinism. […]
>
> [...] Throughout his career Nagel tried to combine the best elements of Cohen's philosophical realism and Dewey's radical instrumentalism. […] It is fair to say that the range of his scientific interests and knowledge exceeded that of any other philosopher of science of his generation in the United States. (Suppes, 1994, pp. 258–259)[18]

Nagel's role as a philosopher of science, as described by Suppes, was quite different from the Carnapian model of the philosopher as a linguistic engineer engaged in the construction of conceptual apparatuses that work in an optimal way for some purpose or other. Without denying the existence of far-reaching differences between their philosophical convictions, philosophers such as Nagel, Lewis, Dewey, and even Rorty should be grouped in a different class of philosophers from the conceptual engineer Carnap, who more resembled a player of Hesse's *glass bead game* than a pragmatist committed to the actual world.[19] For Carnap, societal praxis remained a matter of private commitment, so to speak. Nagel's praxis of a professional philosopher of science relied on a quite different idea.[20]

Natural, although somewhat embarrassing, questions for many philosophers of science are "What is the philosophy of science good for?", "Who is the audience of the philosophy of science?", or "To whom is the philosophy of science addressed?" (cf. Kitcher, 2019). Kitcher proposed three possible answers: "Philosophers, scientists, and interested citizens within and beyond the academy. I argue that our discipline is potentially relevant to all three, but I particularly press the claims of the interested citizens" (Kitcher, 2019, p. 1).

Obviously, for a Carnapian conceptual engineer, the educated citizen is not the first addressee. Scientific engineering aims to improve the conceptual apparatus of

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[18] Further information about Nagel as a leading figure in the philosophical and cultural life of New York can be found in Jewett (2012) and Dewulf (2018).

[19] This is not to deny that Carnap personally showed much social commitment, but this commitment was not founded in his theoretical convictions as a professional philosopher of science. Expressed in a somewhat unfriendly way, the "boundless ocean of possibilities" that he evoked emphatically in *The Logical Syntax of Language* (Carnap, 1937) may have some features in common with Hesse's *Glass Bead Game* (1943/2002). There are some authors who would strongly reject such a suspicion, see for instance Carus's book *Carnap and Twentieth-century Thought: Explication as Enlightenment* (Carus, 2007) where Carnap's philosophy is praised enthusiastically as the outstanding paradigm of a radically modern enlightenment philosophical position "strikingly relevant in 21 century".

[20] For this aspect of Nagel's philosophy see Eric Schliesser's contribution to this volume.

the sciences. Whether this kind of logical philosophy of science actually has been relevant to scientists can be doubted as well. Be this as it may, Nagel's way of doing the philosophy of science came rather close to Kitcher's comprehensive idea of the discipline.

5.7 Concluding Remarks

In the 1930s, Nagel's role in the ongoing encounter between logical empiricism and American pragmatism can be accurately described as a committed mediator and bridge builder between the two movements. In later years, this description is no longer true. Nagel's negative assessment of Carnap's later philosophy (first, his account of probability and inductive logic and later, his approach in its entirely) shows that for Nagel, the much-evoked convergence between logical empiricism and American pragmatism had not taken place.[21]

Nagel's rejection of Carnap's logical approach cannot be misunderstood as the reaction of a philosopher who was simply unable to understand formal arguments. Rather, Nagel rejected Carnap's philosophy as an ultimately non-pragmatist way of doing philosophy that did not take into account (or even explicitly rejected) the fundamental connection of knowledge, action and valuation that the pragmatist Lewis expressed in a concise way as follows:

> Knowledge, action, and evaluation are essentially connected. The primary and pervasive significance of knowledge lies in its guidance of action; knowing is for the sake of doing. And action, obviously, is rooted in evaluation. For a being which did not assign comparative values, deliberate action would be pointless; and for one which did not know, it would be impossible. Conversely, only an active being could have knowledge, and only such a being could assign values to anything beyond his own feelings. (Lewis, 1946, p. 5)

Despite paying lip-service to the contrary, hard-boiled logical empiricists such as Carnap never acknowledged this connection without caveats. This attitude rendered impossible a close relationship between Carnapian logicist empiricism and American genuine pragmatism.

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[21] This fact does not exclude that there have been logical empiricists, who may be said to have "converged" or "drifted" to pragmatism, e.g., Frank and Hempel (cf. Mormann 2017; Wolters, 2003); see also Frank (2021).

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Part II
The Method(s) of the Sciences

Chapter 6
Nagel on Idealization in Science



Raphael van Riel

Abstract The goal of this paper is to reconstruct Nagel's approach to idealization in the sciences and present his views as a viable option. In a nutshell, the theory that emerges can be described as follows: There are various types of idealization, which can be found in theoretical and experimental laws, and which, according to Nagel, play various important epistemic roles. In particular, they help organize complex knowledge and allow for approximations to truth. A cognate of idealization, which Nagel does not refer to under this label but describes in terms of assimilation to the *familiar* and in terms of *analogy*, is key to scientific models, whose value is primarily heuristic in nature. The fact that theories involve idealization supports an instrumentalist perspective on science, a perspective which also determines a particular scope of the account of idealization. By these lights, particular types of questions simply do not arise, for instance: questions pertaining to the alleged peculiarity of scientific representation, the truth of law statements, etc.

Keywords Idealization · Explanation · Models · Laws · Instrumentalism

6.1 Introduction

Contemporary theories of idealization and modeling touch upon the epistemology, ontology, semantics, and pragmatics of modeling and idealization in the sciences, and conceptions of scientific representation play a key role in the debate. Various theses have been put forward: particular types of idealization are sometimes said to increase the value of explanations, modeling discourse or use is taken to be similar

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to fictional discourse or the practice we engage in when engaging in games of make-believe, or model-based explanations are regarded as providing "how-possibly"—explanations.[1]

Nagel was fully aware of the fact that science involves idealization, at various points. For instance, he states, in *The Structure of Science*:

> It is common if not normal for a theory to be formulated in terms of ideal concepts such as the geometrical ones of straight line and circle, or more specifically physical ones of instantaneous velocity, perfect vacuum, infinitely slow expansion, perfect elasticity, and the like. (Nagel, 1961, p. 131)

Among the examples he cites we also find the concept of a gene in biology and the concept of elasticity of demand at a point in neo-classical economic theory (Nagel, 1963, p. 212). However, according to Nagel, idealization does not only occur in connection with the descriptive terms of theoretical laws (whose signature feature is that they are not directly associated with conditions under which they can be predicated of observables), it also occurs within experimental laws. Moreover, Nagel discusses at length the various beneficial and potentially problematic roles of the use of scientific models associated with theoretical laws, a topic which is today typically regarded as closely tied to the issue of idealization.

Despite the rich and detailed comments on scientific modeling and idealization we find in his work, Nagel is not referred to in the contemporary literature on modeling and idealization (Frigg & Hartmann, 2020 is an exception). In fact, there is a tendency to locate the origins of the contemporary debate in the early years of the semantic view in the philosophy of science. In this spirit, Frigg and Hunter claim that a non-"deflationary" take on scientific modeling did not emerge until "the tides changed and the syntactic view of theories came under attack" (Frigg & Hunter, 2010, p. xvii), a view to which Nagel clearly subscribed.

In the only systematic discussion of Nagel's views on matters of idealization, Leszek Nowak (1972) argues that what Nagel has to say about idealization constitutes the main flaw of *The Structure of Science*. According to Nowak, Nagel failed to recognize both the importance and complexity of scientific idealization. In addition, Nagel's remarks on idealization clash, says Nowak, with his characterizations of theoretical and empirical laws and related concepts (Nowak, 1972, in particular Sect. VI). In addition, it will turn out that Nagel's instrumentalism blocks his view, to some extent, from questions pertaining to scientific representation, which have recently gained prominence in the modeling debate. So, perhaps Nagel's views do not have a lot going for them.

The goal of this chapter is to reconstruct Nagel's approach to idealization in the sciences and present his views as a viable option. The paper thereby serves a reconstructive as well as a constructive purpose. I will propose Nagel's view for further discussion. The position's core claims are interesting, and by the lights of the

[1] The literature on these topics is vast. Key texts include Suppes (1960), Cartwright (1983), McMullin (1985), Swoyer (1991), Morrison (1999), Weisberg (2007), Godfrey-Smith (2007), Strevens (2008); for an overview, see Frigg and Hartmann (2020).

reasons Nagel provides, they are not obviously foolish either. I believe them to be interesting enough to be worth examining and perhaps developing in more detail.

In a nutshell, the theory that emerges can be described as follows: There are various types of idealization which can be found in theoretical and experimental laws, and which, according to Nagel, play various important epistemic roles. In particular, they help organize complex knowledge and allow for approximations to truth. A cognate of idealization, which Nagel does not refer to under this label but describes in terms of assimilation to the familiar and in terms of analogy, is key to scientific models, whose value is primarily heuristic in nature. The fact that theories involve idealization supports, though does of course not necessitate (cf. McMullin, 1985; Niiniluoto, 2018), an instrumentalist perspective on science, a perspective which also determines a particular scope of the account of idealization. By these lights, particular types of questions simply do not arise, for instance: questions pertaining to the alleged peculiarity of scientific representation, the truth of law statements, etc.

The paper is structured as follows: In Sect. 6.2, I will introduce a preliminary taxonomy of potential types of idealizations. In Sect. 6.3, I introduce the elements of Nagel's reconstruction of the concept of a theory and briefly place potential occurrences of idealizations identified in the first section within this reconstruction. In the following sections, I turn to these types of idealization within Nagel's reconstruction in more detail, commenting on idealization in theoretical laws (6.4), in experimental laws (6.5), and the use of scientific models (6.6). Section 6.7 contains a discussion of the place of idealization within Nagel's broader instrumentalist framework. In the final section, I present Nagel's critical remarks on one type of fictionalist approach to idealization.

6.2 Nagel on Types of Idealization and Their Place in His Reconstruction of Scientific Theories

The goal of this section is to introduce four types of idealization which Nagel commented upon. Types of idealization within this taxonomy will be distinguished in terms of what *grounds* the fact that these types involve idealization, in some sense (as opposed to, say, the different functions idealizations may play). I will here mainly draw on Nagel's critical discussion (1963) of Milton Friedman's essay "The Methodology of Positive Economics" (1953), where Nagel discusses types of statements involving idealizations, as well as on Chap. 6 of his *Structure of Science* (1961). These sources provide a good starting point for a discussion of Nagel's views on idealization, for the theses Nagel here presents are largely independent of his more general and more fundamental views on theories and science.

In his (1963, p. 213), Nagel drops, for the sake of the argument, his instrumentalist perspective, assuming "that economic theory is a set of genuine statements, so that considerations of their truth or falsity are not irrelevant to the objectives of economic analysis." He also assumes that economic theory is structurally similar to theories

in the natural sciences. Accordingly, Nagel's presentation of types of idealization in this paper does not (or at least: not on every occasion) presuppose his own views on theories, which raise, in connection with his views on idealization, all sorts of subtle and intriguing questions. Once we have an idea of how Nagel thinks about idealization in less committal terms, we will be in a better position to turn to these questions.

6.2.1 Abstraction

The first type of being unrealistic or idealized is related to what is today typically referred to as 'abstraction.' Nagel describes it as follows:

> A statement can be said to be unrealistic because it does not give an "exhaustive" description of some object, so that it mentions only some traits actually characterizing the object but ignores an endless number of other traits also present. (Nagel, 1963, p. 214)

Nagel adds that "no finitely long statement can possibly formulate the totality of traits" of any concrete object, and that even if such a statement were available, it would not be of any value (1963, p. 214).

6.2.2 Falsehood

A second type of statements' (in particular: universally quantified conditionals which have the status of a theoretical statement) failure to be realistic is cashed out in terms of falsity. Statements of this type are said to be unrealistic in the sense that they are either *false* or *have a false antecedent* (and are, thus, vacuously true). Nagel here appears to have a particular type of falsehood in mind—what he refers to as "factual" falsehood. A factual falsehood, according to Nagel, requires that a statement of this sort formulates something false *of existing things* or something false *of properties of existing things* (1961, p. 132). Apart from trivial examples (when we predicate, say, of a flower that it is a stone or a marshmallow), we may think of the statement that real economic subjects are perfectly rational and (relevantly) omniscient, or that a real gas consists of objects lacking spatial extension but having mass.

6.2.3 Inapplicability

Nagel regards theoretical terms to be both, irreducible to observational terms, yet significant. He identifies an additional type of unrealistic statements, referring to laws which involve theoretical terms, but are not false of anything:

> Such statements may be said to be unrealistic but in a sense different from the two previously noted. For they are not distinguished by their failure to provide exhaustive descriptions, nor are they literally false of anything; their distinguishing mark is the fact that when they are strictly construed, they are applicable to nothing actual. (Nagel, 1963, p. 215)

In particular, Nagel has here in mind laws which capture relations involving limiting cases of various, actually non-terminating series (we will, in Sect. 6.5, discuss the question of whether cases of this sort can also be captured in experimental laws).

Nagel's rejection of the view that theoretical statements are not "literally false of anything" requires some elaboration. In *The Structure of Science*, he writes:

> [...] the use of [...] limiting concepts in the formulation of a theory presents difficulties to the view that factual truth or falsity can be significantly predicated of the theory. [...] [I]f a theory formulates relations between properties that ostensibly do not (or cannot) characterize existing things, it is not clear in what sense the theory can be said to be factually true or false. (Nagel, 1961, p. 132)

Factual truth, in Nagel's sense, requires just that: formulation of some indicated relation between existing objects or events or between properties of such entities. Relatedly, he seems to suggest that predicating a relation of either existing things, or of properties of existing things, is a necessary condition on having *some* truth-value. And Nagel appears to think that this condition is not met by paradigmatic theoretical laws which involve heavy idealization (and, as a consequence, his comments on this type of idealization are not *completely* independent of his broader views on science and scientific laws).

For the moment, suffice it to see that one may maintain that this type of idealization constitutes a genuine, distinct type of idealizations, independent of whether one follows Nagel's argument. Even if these statements are either vacuously true or even false, they are neither true nor false *of actually existing objects*, nor are they false *of properties exemplified by actual, existing objects*. Thus, one can draw this distinction independent of Nagel's particular views on the lack of truth-value of highly idealized theoretical laws.

6.2.4 Analogy

Apart from statements, Nagel also comments on a close cousin of idealizations in the sense just discussed within the realm of scientific models (1961, Ch. 6). Examples include pictorial representations of atoms or molecules, or pre-theoretic conceptions of force. In order to shed light on the type of idealization involved here, he makes heavy use of the concept of an assimilation to what is familiar, which he further explicates in terms of analogy (and it seems to resemble conceptions of similarity). This sort of assimilation to what is familiar does not require the use of statements; rather, imagery may play a key role here. Now, Nagel does not further explicate the concept of analogy—it is used as a primitive in his description of this sort assimilation

to what is familiar. As we shall see, Nagel was exclusively interested in the potential cognitive or heuristic value of this type of idealization, rather than the details of its functioning. He does, though, suggest that the use of models is closely related to the use of metaphors.

We thus have here four different sorts of idealization, which Nagel officially recognizes: three types of idealizing statements, where idealization is cashed out in terms of *abstraction*, *factual falsehood*, and *inapplicability to anything actual*, and a broader type of objects which are used as analogues.

Nagel believes that abstraction is something we find in *every* statement, so, according to him, it is not really worth mentioning in a theory of idealization—it involves idealization in a trivial sense. Factually false statements are things we should get rid of, according to Nagel; according to him, they lack any epistemic value (Nagel, 1963, p. 215).[2]

The value of the remaining two types of idealization, however, Nagel discusses in detail: whereas idealization in statements which are not true of anything actual play an important epistemic function, idealization by analogy may fulfill various heuristic and cognitive purposes. I will turn to these in the following sections, once we have a more detailed understanding of the relevant background against which Nagel discusses these issues.

6.3 Nagel's Explication of a Theory

Nagel explicates the concept of a *theory* in terms of three distinct components. A theory, on this reconstruction, consists of.

> (1) an abstract calculus that is the logical skeleton of the explanatory system, and that "implicitly defines" the basic notions of the system; (2) a set of rules that in effect assign an empirical content to the abstract calculus by relating it to the concrete materials of observation and experiment; and (3) an interpretation or model for the abstract calculus, which supplies some flesh for the skeletal structure in terms of more or less familiar conceptual or visualizable materials. (Nagel, 1961, p. 90)

Nagel takes theoretical laws to be mere *schemata*, rather than full-blown statements. On this view,

> [...] theoretical formulations [do not have] the status of "genuine" statements and are [...] simply rules which are instrumental for drawing inferences from genuine statements but which cannot be properly characterized as true or false. (Nagel, 1963, p. 213)

Nagel thus rejects both theories according to which the meanings of theoretical terms can be translated into, or otherwise correspond to, the meaning of observational vocabulary, and any form of realism, according to which theoretical terms designate

[2] Here, Nagel also discusses the value of conditionals with vacuously true antecedent conditions. This is mainly due to a potential ambiguity in Friedman's use of the term 'assumption'; I will here bypass this question.

objects that exist beyond experience (in fact, Nagel's views are a bit more subtle; we will return to the question of realism in Sect. 6.6). He regards theoretical laws as "inference tickets" (Nagel, 1961, p. 130).[3] Put differently: inferences are drawn *in accordance with* these inference tickets, while these schemata do not occur *as premises* in the derivation.[4]

Thus, theoretical laws need to be connected to observation statements. This is the main task of the set of rules mentioned in the characterization above. Rules assigning empirical content connect the theoretical laws of the logical skeleton of a theory to empirical laws, a fourth component which Nagel does not take to be part of the theory in the narrow sense, and which he distinguishes from theoretical laws primarily in terms of their relation to observation statements. Here is an informal characterization: empirical laws.

> [...] formulate relations between things or features of things that are commonly said to be themselves observable, whether with unaided senses or with the help of special instruments of observation. (Nagel, 1961, p. 79)

Examples include the "law that pressure of an ideal gas whose temperature is constant varies inversely with its volume" (Nagel, 1961, p. 80).

Whereas theoretical laws are primarily, if not exclusively of instrumental value, implicitly define the descriptive terminology which they contain, and are such that their descriptive vocabulary is *not* directly associated with application conditions in, say, lab conditions, the descriptive terms of experimental laws are associated with "at least one overt procedure for predicating the term of some observationally identifiable trait" (Nagel, 1961, p. 83).

Of course, Nagel did *not* believe that actual formulations of theories are transparent with respect to this structure. After all, he suggests it as an explication or reconstruction. In this spirit, he explicitly states that these distinctions "are rarely given explicit formulation in actual scientific practice, nor do they correspond to actual stages in the construction of theoretical explanations" (Nagel, 1961, p. 90).

Now, idealization, says Nagel, occurs on various points within this structure; and as we shall see, all four components—the three components which constitute a theory plus experimental laws—will play a role for an adequate interpretation of Nagel's views on idealization in science. Nagel himself uses the term 'idealization' in connection with theoretical laws, and examples of experimental laws he provides reveal that the same type of idealization can be found here, too. His remarks on models and the imagery accompanying theories (and sometimes used to formulate theories in actual scientific practice) suggest that by contemporary standards, these may be subsumed under idealizations as well, although Nagel himself, as we have seen, highlights their *analogical* character. Finally, rules of correspondence play a key role when it comes to understanding the value of idealization in science. We may

[3] Nagel traces his instrumentalist views to the works of Dewey (1929), Peirce (1932, 1933, 1934), Schlick (1938), Ryle (1949), and Toulmin (1953), a view which is often regarded as based on remarks in Wittgenstein's Tractatus (Wittgenstein 1922; see also Musgrave 1980, for an elaboration).

[4] I assume that Nagel's remark that the "raison d'être" (Nagel 1961, 129) of a theory is to function in this way suggests that he takes theories to be ultimately nothing more than inference tickets.

now proceed to a discussion of the details of the different functions Nagel believes these types of idealization to fulfill.

6.4 Idealization in Theoretical Laws

Theoretical laws involving idealization "serve as a powerful means for analyzing, representing, and codifying relations of dependence between actual phenomena", says Nagel (1936, p. 216). The leading idea here is that these laws describe *pure* cases, i.e. capture

> how phenomena are related when they are unaffected by numerous factors whose influence may never be completely eliminable but whose effects generally vary in magnitude with differences in the attendant circumstances under which the phenomena actually recur. Accordingly, discrepancies between what is asserted for the pure case and what actually happens can be attributed to the influence of factors not mentioned in the law. Moreover, since these factors and their effects can often be ascertained, the influence of the factors can be systematically classified into general types; and in consequence, the law can be viewed as the limiting case of a set of other laws corresponding to these various types, where each further law states a modified relation of dependence between the phenomena because of the influence of factors that are absent in the pure case. (Nagel, 1963, pp. 215f)

So, theoretical laws involving idealization perform an organizational function, and can play this function *because* they involve idealizations. They enable us to organize complex knowledge in a simple fashion, and help "directing efficiently the course of experimental procedure" (Nagel, 1950, p. 177). Nagel insists that replacement of theoretical laws by sentences which drop theoretical terms is bound to fail, for logical reasons on the one hand, but also because the resulting substitute would *fail* to fulfill its organizing function. As an example, he cites a formulation of Galileo's law stated for falling bodies in a vacuum, and a potential substitute of the following sort: "Under a wide range of circumstances, the distance a body falls in a specified time is given by the formula $s = ½gt^2$" (Nagel, 1963, p. 216). Nagel suggests that it is the function of *statements of correspondence* to allow us to establish a connection between theoretical laws and experimental conditions.

But this may raise a further problem: obviously, derivations based on theoretical laws involving idealizations will often fail to perfectly match observation statements, unless additional assumptions about the application of the law block potentially problematic results. It is precisely these additional assumption which are supposed to do the trick:

> [W]hen a concretely given situation is analyzed with the help of a law so formulated [that is, formulated for an ideal case], additional assumptions or postulates must be introduced to bridge the gap between the ideal case for which the law is stated and the concrete circumstances to which the law is applied. (Nagel, 1961, p. 463)

In his earlier work, discussing confirmation and theory-based prediction, Nagel gives a more precise picture of how application of an idealized law based on additional assumptions bridges the gap between law and actual situation (we find a similar idea

in his 1963, see below)—so I think we may safely assume that he accepted some such view throughout his career).

> [F]or various reasons, a theory will state only an idealized schema of the relations between the selected phases. These observations lead to the consequence that the confirmation of a theory by experiment is only approximate. (Nagel, 1936, pp. 13f.)

Nagel suggests that there is a measure which determines an acceptable degree of approximation involved in prediction and confirmation, which need not be explicitly formulated, and which is not determined by the theory itself but is, rather, contextual:

> Now the degree of approximation which must hold between the consequences of an acceptable theory and the observation propositions is not determined by the theory. The degree may vary for the different situations to which a theory is applied and may even be left unexpressed as a tacit rule of inquiry. [...] [T[he degree of allowable approximation is determined by various material considerations [...]. (Nagel, 1936, pp. 13f.)

So, we may summarize the epistemic functions of idealization in theoretical laws as follows: Adequate idealization in theoretical laws, by describing pure cases, enables systematic generality in the analysis and codification of dependencies among actual phenomena, and, together with rules of correspondence, may guide experiments. Potential problems resulting from idealizations of this sort are countered by adequate rules of correspondence *and* implicit measures of approximation, so that based on laws formulated in terms of ideal cases, we may still arrive at true statements about observable phenomena. From this perspective, idealization in theoretical laws does not seem to be a flaw which needs to be reconciled with more general considerations about the function of theories; rather, idealization itself is the key to understanding why theories can perform their function. Let us thus turn to idealization in experimental laws.

6.5 Idealization in Empirical Laws

Nagel does not comment in detail on idealization in experimental laws. The list of examples he provides for these laws suggest that they may be formulated for "pure cases" as well. This is precisely what Nowak complains about:

> Boyle's law, to which Nagel here refers, is an idealizational statement, since it is to hold for ideal gases. The full formulation of this law thus is as follows:
>
> (11) if x is a portion of a perfect gas, then, if $T(x) = \text{const}$, then $p(x)v(x) = \text{const}$,
>
> where 'T', 'p', 'v' stand, respectively, for the temperature, the pressure, and the volume of a portion of a gas. This formulation shows that Boyle's law cannot be an experimental law, since the rubric 'experimental law' signifies simply that a statement so characterized formulates a relation between things (or traits of things) that are observable [...], and that the law can be validated [...] by controlled observation of the things mentioned in the law. (Nowak, 1972, p. 539)

Thus, Nowak suggests that Nagel cannot reconcile his views on idealization with his description of an experimental law. Other examples for experimental laws Nagel

mentions are of the same sort; consider, for instance, his discussion of the force function in the study of vibratory motions (Nagel, 1961, p. 105). Since pure cases are not observable, and since the descriptive vocabulary of experimental laws is said to "formulate relations between things or features of things that are commonly said to be themselves observable" (1961, p. 79) and the non-logical terms occurring in experimental laws must be associated with conditions under which they can be predicated of observables (1961, p. 83), we may here face a tension.

What would we have to accept if Boyle's law were an experimental law in consonance with Nagel's description? Pressure, volume, and temperature *are* observable, by Nagel's standards. It is the term 'perfect gas' which seems to cause trouble. We would then have to assume that it may nevertheless be associated with observable features of objects which render its application conditions precise. If this is enough for a descriptive term to be associated with "at least one overt procedure for predicating the term of some observationally identifiable trait" (Nagel, 1961, p. 83), then the term 'perfect gas' expresses an experimental idea, for there are observationally identifiable traits which make the experimental law applicable to portions of real and non-ideal gas. And then, the ideal/non-ideal distinction would be orthogonal to the theoretical/experimental distinction. An indication of this view is provided by Nagel's observation that some experimental law statements include "as part of their meaning assumptions that are admittedly theoretical" (1961, p. 83), which would account for the use of 'perfect gas' in this experimental law.

Of course, we then face the undesirable consequence that if "application" really amounts to "predication," we end up with a conditional that has a false antecedent (for no real gas is a perfect gas). This, I think, reveals that Nowak's criticism boils down to the observation that Nagel is committed to a weak notion of predication *or* that he has to accept that *applications* of experimental laws contain a false antecedent.

But perhaps, Nagel does not have to bite the bullet. The discussion of approximation above suggests that contextual parameters will determine a degree of approximation that is acceptable in a context of an application of a law. We could make this explicit by introducing two obvious, minor modifications, which would do the trick. We may either drop "perfect" in the statement of the experimental law, or substitute "sufficiently similar to a perfect gas" for "perfect gas," for which application conditions could easily be provided, and add, in Nagel's spirit (see above), an "approximately"-condition. The counterpart would then read:

> if x is (sufficiently similar to) a (perfect) gas, then it is approximately true that: if $T(x) = \text{const}$, then $p(x)v(x) = \text{const}$.[5]

So, I think there is either a straightforward response to Nowak's worry, based on a particular interpretation of 'application' and 'predication,' or there is a solution easily available to Nagel which still is perfectly in Nagel's spirit. And we should keep in mind that Nagel intends to provide reconstructions which need not perfectly match all sorts of allegedly experimental and theoretical laws—the distinction is vague (Nagel, 1961, p. 81ff).

[5] Interesting discussions of the connection between approximation and idealization can be found, for instance, in Norton (2012) and Lawler (2019).

There is another related complication, though. On at least one occasion, Nagel provides a characterization of the concept of a *theoretical term* which obviously clashes with the idea that experimental laws may contain idealizations. He claims that statements containing theoretical terms.

> formulate relations specified to hold under highly "purified" conditions between highly "idealized" objects or processes, none of which is actually encountered in experience. Statements of this kind contain what have previously been called "theoretical terms," which connote what are in effect the limits of various nonterminating series and which are not intended to designate anything actual. (Nagel, 1963, p. 215)

If Nagel here not merely wishes to report how the term 'theoretical term' has been employed, but also endorses this use, there will be no room for idealization of this sort within Nagelian experimental laws, whose signature feature is their *lack* of theoretical terms.[6]

On the other hand, this characterization of a theoretical term does not mirror the official (and officially vague) distinction between theoretical and empirical terms or concepts. Nagel's characterization of theoretical terms as occurring in statements which implicitly define their use, while not being associated with a fixed set of rules of correspondence, is logically independent of assumptions as to what "is actually encountered in experience" (ibid.) and, hence, of the actualness of what is signified by these terms. We may thus conclude that some characterizations of theoretical terms are compatible with Nagel's view that experimental laws may contain idealizations, whereas other less prominent characterizations clash with this assumption.

Thus far, we have not commented upon the epistemic benefit of idealizations in experimental laws, and indeed, Nagel does not give any indication of why we should expect idealization in experimental laws to be of *special* value. However, idealization in theoretical laws plays at least one role which is obviously also played by idealization in experimental laws. Let me quote again the passage cited above: By pointing to a limiting case, experimental laws, just like theoretical laws, describe.

> how phenomena are related when they are unaffected by numerous factors whose influence may never be completely eliminable but whose effects generally vary in magnitude with differences in the attendant circumstances under which the phenomena actually recur. Accordingly, discrepancies between what is asserted for the pure case and what actually happens can be attributed to the influence of factors not mentioned in the law. Moreover, since these factors and their effects can often be ascertained, the influence of the factors can be systematically classified into general types; and in consequence, the law can be viewed as the limiting case of a set of other laws corresponding to these various types, where each further law states a modified relation of dependence between the phenomena because of the influence of factors that are absent in the pure case. (Nagel, 1963, pp. 215f)

Clearly, Nagel here refers to consequences of idealization in experimental conditions (ascertaining influence of other factors, discrepancies between pure case and

[6] This is at least suggested by Nagel's claim that "each "descriptive" (i.e. on-logical) constant" in an experimental law is "associated with at least one overt procedure for predicating the term of some observationally identifiable trait […] thus [fixing] a definite, even if only partial, meaning for the term." (Nagel 1961, p. 83) This condition seems to rule out that an experimental law might contain theoretical terms, whose meaning is, by definition, only implicitly defined.

what actually happens). Thus, idealization does not only occur on both, experimental and theoretical laws; it also serves a similar epistemic purpose.

6.6 Familiarity, Analogy, and the Use of Scientific Models

Nagel writes (1950, p. 181): "It would be clear folly to deny the heuristic value of the imagery associated with theories, or to challenge the cognitive worth of theoretical constructions." Reference to "imagery" associated with theories, or "theoretical constructions," concerns the various objects, pictorial representations, etc. employed in science to formulate theories (which Nagel (1861, p. 113) then describes as being "embedded" in the model), to provide illustration or to connect pure theoretical laws to real world situations, using ordinary concepts, and thereby inspiring rules of correspondence. These constructions are, according to Nagel, of purely heuristic and cognitive value. In this section, I will summarize the main functions of models of this sort identified by Nagel. As I have indicated above, Nagel does not provide a detailed account of *how* or *when exactly* these models fulfill this function—he contends himself with the observation that use of these devices in an analogical fashion is key for these things to fulfill their function, while maintaining that quite the opposite result may ensue, when, for instance, the model is mistaken for the theory, or when inessential features of the model are taken to be relevant (Nagel, 1961, p. 115). In short: use of analogies does not guarantee success.

6.6.1 Theory Construction

Nagel (1961, pp. 108f) thinks that use of "models served both as a guide for setting up the fundamental assumptions of a theory, as well as a source of suggestions for extending the range of their applications". As one among many examples he cites the kinetic theory of gases and its source of inspiration, the "behavior of an immense number of elastic particles, whose motions conform to the established laws of mechanics" (Nagel, 1961, p. 108). This is also an example of what he calls a "substantive" analogy—an analogy where the model consists of a "more or less visualizable set of elements which stand to each other in familiar relations" (1961, p. 110). He distinguishes from these so called "formal" models, where a structure of abstract relations plays a key role in the construction of an analogy. Consider, for instance, "Maxwell's example of the identity in structure of the mathematics of gravitation theory and the equations of heat conduction" (1961, p. 111). Ideally, this will result in the "construction of another theory with a more inclusive scope of application than the original one" (1961, p. 111).

6.6.2 Theory Development and Experimental Laws

Moreover, models may play a role in exploring a theory already stated: "[A] model may be heuristically valuable because it suggests ways of expanding the theory embedded in it" (Nagel, 1961, p. 113). For instance, the model for the kinetic theory of gases "suggested questions about the ratios of molecular diameters to the distances between the molecules" (1961, p. 113). That is, these questions occurred when considering the model, not the theoretical core.

In addition, models play a crucial role in connecting theoretical terms with observational vocabulary. The model for the kinetic theory of gases suggests that the theoretical term "'product of the mass of each molecule and the number of molecules' can be made to correspond to the experimental notion of the mass of a gas" (Nagel, 1961, p. 113).

6.6.3 Acceptance

Nagel starts his discussion of models and analogy by reference to familiarity conceptions of explanation, according to which scientific explanation is tightly connected to the reduction of the unfamiliar to the familiar, a view which he rejects. He does, however, suggest that.

> explanations can be regarded as attempts at understanding the unfamiliar in terms of the familiar, insofar as the construction and development of explanatory systems are controlled, […] by the desire to find and exploit structural analogies between the subject matter under inquiry and already familiar materials. (Nagel, 1961, p. 107)

Thus, models play a key role in the development and exploration of theories and, thus, in the construction of explanations. Moreover, Nagel observes (while refraining from any explicit evaluation) that familiarity plays a key role in theory acceptance. Briefly: If we have a familiar model, we are more likely to accept a theory. Thus, familiarity (and, since familiarity in the realm of science is typically mediated by scientific models: models) plays a key role in theory development and theory acceptance.

Now, while Nagel clearly sees numerous advantages in the use of scientific models, he also claims that "it would be no less naive to fail to recognize that much if not all of the imagery is excess baggage from the point of view of the operative significance of theories in inquiry" (1950, p. 181). This brings us to the next section, namely, how Nagel's instrumentalism shaped and is simultaneously based on his views on idealization in science.

6.7 Idealization Within the Bigger Picture

As we have just seen, Nagel thinks the role of models is of minor importance, if we adopt an instrumentalist perspective, focusing on the "operative significance of theories in inquiry." In this section, I wish to place Nagel's comments on idealization within the bigger picture, and summarize what turns out to be a complex instrumentalist view on idealization and models in science, with a special focus on the limitations of such a view.

According to Nagel's instrumentalism, the role of "unrealistic theoretical statements" is to "serve as a powerful means for analyzing, representing, and codifying relations of dependence between actual phenomena" (1963, p. 216). Moreover, theories and experimental laws are supposed to guide empirical inquiry, provide explanations and enable predictions.

Nagel's instrumentalism and his views on idealization are not a matter of coincidence. In fact, Nagel's endorsement of instrumentalism is partly based on his view that instrumentalism can best account for idealization in theoretical laws.[7] He puts the argument as follows, thereby suggesting that instrumentalism can better account for idealization in theoretical laws than its rivals:

> The fact that theories contain expressions which describe or designate nothing in actual existence, or which are not associated with experimental notions is indeed taken as confirmation for the claim that theories must be construed in terms of their intermediary, instrumental function in inquiry, rather than in terms of their adequacy as objective accounts of some subject matter. (Nagel, 1961, p. 133)

In a related argument, Nagel observes that scientists frequently use incompatible theories to account for different phenomena in different contexts (in particular: use of theories which treat a gas as consisting of discrete particles, and use of theories which treat a gas as a continuous medium). Clearly, these theories are "mutually incompatible" (Nagel, 1961, p. 133). The instrumentalist solution is simple and elegant: reject the view that there is more to the epistemic function of such theories than to organize observations and to play a key role in derivations, which then enable prediction, confirmation and, perhaps, explanation.[8]

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[7] This remark is to be taken with a pinch of salt. In fact, Nagel proposes a more nuanced view on the distinction between instrumentalism and realism, or descriptivism. On his preferred interpretation, a careful formulation of both, realism and instrumentalism leads to the conclusion that "the opposition between these views is a conflict over preferred modes of speech" (6.1961, p. 152). But this hinges, of course, on a number of assumptions many a realist would presumably not be willing to accept. For instance, passages like the following suggest that Nagel takes a very liberal stance on "merely verbal difference" (which even those who remain neutral on the realism/instrumentalism issue may find wanting): "In short, to assert that […] atoms exist [in the sense that there are a number of well-established experimental laws related in a certain manner to one another and to other laws by way of a given atomic theory] is to claim that available empirical evidence is sufficient to establish the adequacy of the theory as a leading principle for an extensive domain of inquiry. But as has already been noted, this is in effect only verbally different from saying that the theory is so well confirmed by the evidence that the theory can be tentatively accepted as true" (Nagel 6.1961, p. 151).

[8] How idealizations figure in explanations is far from clear; see van Riel (6.2015) and (2017).

In the previous sections, we have seen which epistemic benefits come with idealization statements according to Nagel, which heuristic role modeling plays for the construction, development, and acceptance of theories, and how we can move from idealized theoretical and perhaps empirical laws to truth or approximate truth. We have thus sketched the "positive" part of Nagel's take on idealization in science. This view covers a lot of ground. But more importantly, perhaps, his instrumentalism also limits the scope of questions one may want to ask in connection with idealization in science, and, thus, his instrumentalism explains why he did not address a number of topics which may seem of particular importance from a contemporary perspective. These include, amongst others, the peculiarities of scientific representation, the ontology of model objects, and the semantics of fictional statements and their role in scientific modeling.

It is worth starting with the observation that some questions concerning idealization become particularly pressing (or perhaps arise only) if we adopt a robust type of semantic or metaphysical *realism*; or, more broadly, if we believe that *representation* is key to understanding the function and value of scientific theories. Nagel does not engage in a systematic discussion of scientific representation in general. In an early paper of his, where he offers a criticism of Feigl's 'Existential Hypotheses' (Feigl, 1950), he merely claims that.

> the emphasis of operationism is upon the instrumental role of hypothetical constructs in the conduct of inquiry; it is not (as is the case with phenomenalism and realism) upon their supposed status as *symbolic representatives of some realm of events or objects.* (Nagel, 1950, p. 176; emphasis added)

Note that here, he uses the term 'operationism' rather than 'instrumentalism.'[9]

It is important to notice that Nagel does not believe his reconstruction to help elucidate what actual scientists are doing when building or asserting statements which, within this reconstruction, might figure as theoretical idealizations. He describes the status of his operationism, again, as reconstructive. He explicitly states that he does not.

> wish to suggest that a substantial fraction of practicing scientists consistently adopt such essentially puritanical standards in evaluating scientific theory. I am maintaining that [...] operationism is a possible standpoint from which to interpret science. (Nagel, 1950, p. 177)

Nagel's operationism is an interpretation of science, which, as such, highlights merely those aspects of the complex actions, artifacts and procedures that constitute the actual course of science that are deemed relevant for an understanding of science in terms of those features which distinguish it from ordinary knowledge in their respective explanatory and predictive functions (Nagel, 1961, Chap. 1). Thus, Nagel appears to be committed to the view that idealizing techniques which occur in actual scientific practice beyond those reflected in his reconstruction, which they may very well do, do *not* contribute, in a significant way, to the explanatory and predictive function of science. In this sense, Nagel's instrumentalism bears on the scope of

[9] For an account of the background of Nagel's view on operationism, see Verhaegh (2020), in particular Sect. 2.

his remarks on idealization. His views need not be applicable to some potentially paradigmatic idealizing statements made by working scientists, in contexts where particular attitudes or conventions determine whether these scientists commit to the existence of alleged designata of theoretical terms, engage in games of make-believe or the like; and since no demanding notion of scientific representation enters his instrumentalist reconstruction, questions pertaining to idealization in scientific representation would turn out to be idle, if (i) one is interested in the explanatory and predictive function of theories, and (ii) if Nagel's reconstruction identifies all those aspects of scientific practice which play a substantive role with respect to this function. Of course, one may question these two claims; but the mere fact that Nagel did not comment on various topics which may seem relevant today (say, the ontology of model objects or the semantics of model talk) does not, all by itself, speak against the notion of idealization which we can extract from his writings.

It is worth noticing, though, that that there may be a tension in Nagel's remarks on idealization on the one hand, and his instrumentalism on the other. Assume that theoretical laws are inference tickets, falling beyond the scope of an evaluation in terms of truth and falsity. It is then not clear in what sense these statements, or statement schemata, are supposed to involve *idealization*. After all, it seems that the concept of idealization, even in Nagel's sense, is itself a concept involving conceptions of representation, or at least semantic concepts such as truth and falsity, so that perhaps it can be cashed out, if not in terms or designation of its constituent parts or in terms of resemblance, at least in terms of *approximate* truth. And in fact, Nagel (1950) verbally rejects semantic realism. This appears to be in need of explanation.

Nagel's instrumentalism is a bit subtler than one might expect. He argues that instrumentalism and a particular form of semantic realism (which he does not describe under this label) are perfectly compatible (Nagel, 1961, pp. 137ff). Theoretical laws, says Nagel, *correspond* to statements that are either true or false, and these statements' truth or falsity matters. His rejection of Feigl's type of realism (Nagel, 1950) mainly rests on the fact that Feigl makes heavy use of a concept of *designation*, which Nagel believes to be obscure; and his instrumentalism appears, again, to be a claim about a fruitful reconstruction of science. Instrumentalism, which assigns theoretical laws (construed as schemata) a purely instrumental value, provides a fruitful explication of the concept of a theory. There is simply no *need*, from this perspective, to consider corresponding sentences which possess a truth-value. But of course, these corresponding sentences do exist, and they are true or false. The alleged tension then boils down to the question of how Nagel thinks he can provide a crisp explication of how *sentence schemata* involve idealization. My best guess is that idealization comes in when theoretical laws are combined with "a set of rules that in effect assign an empirical content to the abstract calculus by relating it to the concrete materials of observation and experiment" (see above). Idealization then bottoms out in *approximate* truth of the derivations in accordance with theoretical laws. To my knowledge, Nagel did not bother to elaborate on this point.

6.8 'As if' Constructions and Fictionalism

We now have a picture of Nagel's theory of idealization and modeling in the sciences. However, Nagel did not merely present his "positive" account of idealization, he also critically examined a particular thesis concerning the alleged relevance of 'as if' constructions in the application of theories of actual events. He discusses the following example:

> Professor Friedman [...] states [the "rational maximization of returns" hypothesis in the theory of the firm] as follows: 'Under a wide range of circumstances, individual firms behave as if they were seeking rationally to maximize their expected returns and had full knowledge of the data needed to succeed in this attempt.' (Nagel, 1963, p. 217)

Nagel notes that he and Friedman agree that "as a rule businessmen lack such knowledge and do not perform the intricate calculations required for ascertaining the indicated maximum". As a consequence, this hypothesis does not specify any "determinants in explanation of that behavior" (1963, p. 217). The view Nagel here rejects is nicely summarized by Mäki:

> Explanatory practices typically involve theoretical modeling, and theoretical modeling is typically a matter of isolating causal mechanisms. [...] Characteristically, mechanisms reside inside input-output systems, where they serve as the mediating causal chains between the input and output phenomena. [...] By representing a mechanism inside an input-output system, economists not only convey knowledge *that* the input and the output are connected, they also conjecture *how* the input, together with the mechanism, produces the output. (Mäki, 2009, pp. 85f., original emphases)

This is precisely the sort of interpretation Nagel would not be willing to accept (at least for the hypothesis he discusses). However, Nagel fears that Friedman's rendering of the hypothesis would commit one to some such view: according to Nagel, the 'as if'-construction requires "that firms do seek to maximize their returns in a rational manner, since otherwise it appears to be asserting nothing whatsoever" (1963, p. 217). This seems a bit hasty. After all, when in *The Great Gatsby* (1925/2009, p. 55), we read that Jordan Baker is "extended full length at her end of the divan, completely motionless, and with her chin raised a little, as if she were balancing something on it which was quite likely to fall", it is not even tentatively suggested that she is actually balancing something on her chin which is quite likely to fall, nor would this sentence, if uttered with assertoric force, fail to assert anything; and it appears that this would be the case independent of whether or not the concept of *balancing something on one's chin which was likely to fall* is empty.

A quick look at Nagel's reconstruction of how the hypothesis is used in actual explanations may help shed light on Nagel's reluctance to accept the 'as if' construction. He puts what he calls the 'operative premise' as follows:

> Under a wide range of circumstances, the behavior of individual firms brings them returns approximately equal to a certain magnitude (called the maximum of expected returns by economists). (Nagel, 1963, p. 217)

Obviously, this rendering does not make reference to any of those features of the theory which appear to be fully irrelevant with respect to the actual causal patterns whose outcome they are supposed to help calculate. And this is precisely what Nagel stresses: on this interpretation, the theory may be conceived of as a set of rules for calculation.

On one charitable reading, then, we can reconstruct Nagel as claiming that on a weak interpretation of the 'as if' construction, it is a somewhat convoluted way of making, pragmatically, essentially the same point his own operative premise is supposed to make. If, however, the 'as if' construction has substantial, irreducible import, it seems to commit the speaker to the view that the hypothesis provides some insight into some mechanism which connects input and output states. This squares well with Nagel's remark that the models employed in connection with pure theory are fictions (1961, p. 134). That is, we may take Nagel to claim that Friedman's 'as if'-construction, when taken literally, is either costly, when it is supposed to play some explanatory role, or should be regarded as pure fiction, in Nagel's sense, which reduces to the assumption that there is either no evidence in favor of the physical reality of the model, or that the evidence is in fact negative (1961, p. 134). Perhaps, Nagel overshot the target when claiming that 'as if' constructions have the consequences he thinks they have. Nevertheless, he provided a promising substitute for 'as if' constructions, which does not mention allegedly explanatory mechanisms. Intuitively, the description of an alleged "mechanism" is turned into a description which determines a property of individual objects which these objects at least approximate. Whether this idea can be generalized remains to be seen, but at first sight, it seems to constitute an interesting hypothesis.

6.9 Conclusion

Nagel has provided a detailed theory of idealization from an instrumentalist perspective. He drew distinctions between four types of "idealization" in terms of abstraction, falsehood, inapplicability, and analogy. The latter two play a key role within his instrumentalist perspective on science.

While at least some of Nagel's comments on idealization may not seem particularly controversial, a critic may want to claim that this is partly due to the fact that Nagel at best provided a mere skeleton of a theory of scientific idealization, only scratching the surface and failing to appreciate the relevance and complexity of idealizing techniques in the sciences (this also captures part of the gist of Nowak's critique; see Nowak, 1972, Sect. V). This is fine, as far as it goes: In the recent debate on modeling, a number of questions have emerged with which Nagel simply did not engage. On the other hand, Nagel could respond that some of these questions are irrelevant if discussed in connection with the topic he was primarily interested in: a reconstruction of science which makes transparent the peculiarities of scientific explanation and prediction. This is, of course, not to say that scientific practices involving idealization are irrelevant *irrespective* of the research question one wants

to pursue. Things may look rather different if one is interested in, say, the semantics of 'as if' constructions or of fictional talk, or in the details and nature of similarity, analogy, or familiarity. Alternatively, one may of course challenge Nagel's instrumentalist view on science, and his take on explanation and prediction.

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Chapter 7
Putting the Cart Before the Horse: Ernest Nagel and the Uncertainty Principle



David Atkinson and Jeanne Peijnenburg

Abstract In *The Structure of Science*, Ernest Nagel finds fault with Werner Heisenberg's explication of the uncertainty principle. Nagel's complaint is that this principle does not follow from the impossibility of measuring with precision both the position and the momentum of a particle, as Heisenberg intimates, rather it is the other way around. Recent developments in theoretical physics have shown that Nagel's argument is more substantial than he could have envisaged. In particular it has become clear that there are in fact two uncertainty principles; as a result, there are four pairs of quantities to examine, whereas Heisenberg considers only one. These findings throw new light on Nagel's criticism. They enable us to see that his intuition was surprisingly apposite, but also make clear where his argument misses the mark.

Keywords Ernest Nagel · Quantum mechanics · Uncertainty principle

7.1 Introduction

Generally the act of observing macroscopic quantities does not alter those quantities appreciably. One can observe for example the position of a ballistic missile at two successive times, t_1 and t_2, and infer what the momentum of the missile was in the interval between t_1 and t_2. No appreciable alteration of the trajectory of the weapon is effected simply by looking at it.

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As is well known, the matter is different in the submicroscopic domain of the fundamental particles. The position of an electron, for example, can be observed by letting a particle of light, a photon, collide with the electron. The photon will be absorbed and a new one will be emitted, the detection of which enables us to determine the electron's position with fair precision. However, our knowledge of its momentum will be poor, for the interaction of the photons with the electron causes an alteration in the electron's momentum which cannot be made negligible, because of the discrete nature of light. Thus it seems impossible to determine both position and momentum at a particular moment of time with great precision.

The above comprises the celebrated uncertainty relation; since Werner Heisenberg published it in 1927, it has become the centerpiece of the orthodox (or Copenhagen) interpretation of quantum mechanics. It is therefore interesting that at a relatively early stage philosophers of science have expressed criticism of the uncertainty principle. In this paper we concentrate on the criticism voiced by Ernest Nagel, who in Chap. 10 of *The Structure of Science* launches a lengthy critique of Heisenberg's discovery. Taking recent developments in physics as our point of departure, we argue that Nagel's criticism is more substantial than he could have envisaged. However, as we shall explain, the new results also allow us to see where exactly Nagel's argument falters.

We start in Sect. 7.2 by summarizing Nagel's criticism of the way in which Heisenberg presents his uncertainty principle. In Sects. 7.3, 7.4 we explain recent findings in theoretical physics. We first make clear that there are in fact two different uncertainty principles, both dating from 1927, which have incorrectly been put on a par (Sect. 7.3). Then we show that the original uncertainty principle of Heisenberg has questionable validity, and we describe the introduction in 2003 of a corrected version, which has a stronger claim to be valid (Sect. 7.4). In Sect. 7.5 we explain the relevance of these results for Nagel's criticism: they reveal the extent to which his criticism is correct or misses the mark.

7.2 Nagel's Criticism of Heisenberg

In Chap. 10 of *The Structure of Science*, Ernest Nagel discusses the question of the precise sense in which classical physics is deterministic while quantum mechanics is not. A theory is deterministic, Nagel writes, if "analysis of its internal structure shows that the theoretical state of a system at one instant logically determines a unique state of that system for any other instant" (1961, p. 285). The word "theoretical" is important here, for Nagel makes it clear that he is not addressing errors in empirical measurements or the consequences of these errors in actual practice.

Newtonian mechanics is clearly deterministic in this sense.[1] Moreover, Nagel notes that under this explication even quantum mechanics can be said to have a deterministic structure. In quantum mechanics the theoretical state of a system is described by a wave function; if the wave function is given at one time, then the Schrödinger equation of motion specifies the wave function at another time, much as Newton's equations of motion in classical physics tell us how the positions and momenta of bodies change as time passes.

Why then is quantum mechanics so often put forward as a standard example of a theory that is indeterministic? According to Nagel, this is because of Heisenberg's uncertainty principle:

> [...] the usual ground for regarding quantum mechanics as an "indeterministic" theory is the set of formulas logically derivable from the assumptions of the theory and known as the "Heisenberg uncertainty relations" (Nagel, 1961, p. 294).

Nagel himself disagrees with this view:

> Quantum mechanics cannot be validly characterized as indeterministic merely on the grounds that the uncertainty relations exclude the possibility of precise values for the simultaneous "positions" and "momenta" [...] (Nagel, 1961, p. 305).

Nagel is of course right to reject the idea that the uncertainty principle is the source of indeterminism in quantum mechanics, for it is mathematically impossible to infer the latter from the former.

He recalls that the core of the uncertainty principle is the impossibility to obtain simultaneously precise values for position and momentum; and the reason for this impossibility is that the product of these uncertainties can never be smaller than a bound given by Planck's constant:

> One of the [Heisenberg uncertainty relations] is expressed by the formula $\Delta_p \Delta_q \geq h/4\pi$. In this formula the variables 'p' and 'q' are commonly read as the instantaneous coordinates of the "momentum" and "position", respectively, of an electron or other subatomic element, and 'h' as the universal Planck's constant. On the other hand, 'Δ_p' is interpreted as the coefficient of dispersion (or deviation, sometimes also called the 'uncertainty') from the mean value of the momentum at a given instant; and similarly for 'Δ_q'. The formula therefore asserts that at any given instant the product of the dispersions of momentum and position, respectively, of a subatomic "particle" is never less than $h/4\pi$ (Nagel, 1961, p. 294).

Nagel then notes that the uncertainty principle has been understood in the following way:

> A widely held [...] interpretation of the uncertainty relations is that they formulate the relatively large but inherently unpredictable variations in certain features of subatomic particles and processes, produced by the interaction of the latter with the instruments used in measuring those features (Nagel, 1961, p. 295).

[1] Of course the domain has to be specified, e.g. certain singular situations have to be excluded, but this is to be understood.

He stresses that even Heisenberg interpreted his principle in that manner:

> Heisenberg declared that when measuring large-scale objects, the effects generated in those objects by the processes of measuring them can be neglected, since the magnitudes of the disturbances thus produced are relatively small. In subatomic physics, on the other hand,
>
> > "the interaction between observer and object causes uncontrollable and large changes in the system being observed [...] every experiment performed to determine some numerical quantity renders the knowledge of others illusory, since the uncontrollable perturbations of the observed system alters the values of previously determined quantities" (Nagel, 1961, p. 295; Heisenberg, 1930, p. 3).

So according to this interpretation, supported by Heisenberg himself, the interaction between the observer and the observed leads to unpredictable and uncontrollable variations in subatomic particles and processes, which in turn imply the uncertainty principle:

$$\text{uncontrollability} \quad \rightarrow \quad \text{uncertainty relations.}$$

Despite its defense by a "high authority", Nagel (1961, p. 297) finds this interpretation fundamentally mistaken. For it places the cart before the horse:

> [It is] as if the uncertainty relations were the conclusions of a purely factual examination [...] for testing quantum theory. In point of fact [...] such a contention puts the cart before the horse. For the 'uncontrollable' [...] alterations that the electrons are said to suffer [...] do not constitute the *evidence* for the uncertainty relations, but are part of the *consequences* drawn from the relations (Nagel, 1961, p. 297, original emphases).[2]

Nagel's point is that one does not simply observe uncontrollable alterations to the coordinates of the electrons and then infer from this that we are apparently dealing with uncertainty relations. Rather it is the other way around, the uncertainty relations are the reasons for the uncontrollability:

> In consequence, it is hardly cogent to argue that the simultaneous positions and momenta of electrons cannot be ascertained with unlimited precision, on the ground that perturbations are produced in electrons when they are measured. In short, the impossibility of such unlimited precision follows from the uncertainty relations and not, as is sometimes maintained, simply from the familiar experimental facts (Nagel, 1961, p. 298).

That is,

$$\text{uncertainty relations} \quad \rightarrow \quad \text{uncontrollability.}$$

[2] It is important not to confuse Nagel's criticism with that of Niels Bohr. In 1927, both at the Volta conference in Como, Italy, and at the Solvay conference in Brussels, Bohr objected to Heisenberg's claim that the uncertainty relations follow from the gamma ray thought experiment; according to Bohr, that thought experiment is defective (see Sect. 7.3). Bohr was right, but the objection is not the same as Nagel's, which is that the uncertainty relations do not follow from uncontrollability.

The fact that Nagel criticizes Heisenberg's explication is somewhat remarkable. True, Nagel had a Master degree in mathematics and he wrote his Ph.D. thesis on the concept of measurement; but it is nevertheless impressive that he, being first and foremost a philosopher, mastered quantum mechanics sufficiently to be able to make a trenchant criticism of Heisenberg's achievements in physics. One might argue that Nagel was not reprimanding Heisenberg on a strictly physical matter, but was merely making a philosophical point about the theory ladenness of observation. A recurring theme in Nagel's work is that reports of what are commonly regarded as simple experimental observations often are couched in the language of some theory. This goes for quantum mechanics, but also for classical physics. For example, Nagel would have said that the statement with which we started this paper, namely that observing macroscopic quantities does not alter them, is actually a theoretical assumption, not the report of a straightforward empirical fact. Indeed, as Nagel writes:

> [...] we remind ourselves that perturbations produced by measuring instruments in the objects measured were fully recognized in classical physics. In classical physics, however, the extent of such perturbations can in principle be precisely evaluated with the help of established physical laws, so the mere fact of such perturbations does not lead to the uncertainty relations. (Nagel, 1961, p. 297)

Although that may well be the case, there is more to be said about Nagel's criticism of Heisenberg. For recent developments in theoretical physics have shown that this criticism is actually more substantial than Nagel himself could have envisaged. As we shall indicate in Sect. 7.3, it has been found that there are in fact two uncertainty principles; and, as we explain in Sect. 7.4, there are four pairs of quantities to consider, whereas Heisenberg considers only one. In Sect. 7.5 we discuss the relevance of these new findings for Nagel's argument. We shall see that his basic intuition was apposite, but also that he unknowingly went back and forth between what were later identified as two different versions of the uncertainty principle.

7.3 Two Uncertainty Principles

As has been explained carefully by Jan Hilgevoord and Jos Uffink (2001/2016; 1990 and Uffink & Hilgevoord, 1985), there are actually two versions of what is called the 'uncertainty principle' or the 'uncertainty relation'. The first was introduced by W. Heisenberg in his paper of March 1927 in the *Zeitschrift für Physik*, the second was described by E.H. Kennard four months later in the same journal (Heisenberg, 1927; Kennard, 1927). The Heisenberg version basically concerns a single pair of measurements, one of the momentum and the other of the position of a particular particle. The Kennard version, on the other hand, refers to an ensemble of particles, in which the momenta and positions display a spread about their respective average values—it does not concern measurements at all. Heisenberg's formula can be expressed as

$$\delta_p \delta_q \sim h,$$

where δ indicates some measure of accuracy ('Genauigkeit') or error ('Fehler') which however remains completely unspecified; the formula as a whole says that the product of δ_p and δ_q is roughly equal to Planck's constant.[3] Kennard's uncertainty principle is

$$\Delta_p \Delta_q \geq h/4\pi$$

where Δ stands for the usual standard deviation in statistics, and where the entire formula says that Δ_p times Δ_q cannot be less than Planck's constant, h, divided by 4π.

Before Hilgevoord and Uffink published their finding, no clear distinction between the two versions was made, and when people talked about the 'Heisenberg uncertainty principle' they were usually referring to the Kennard version. Nagel is no exception in this respect; however, as we will make clear in Sect. 7.5, his case is a bit more complicated, since he appears to oscillate between the two versions.

Heisenberg's principle is often called 'the observer effect', but we shall follow Hilgevoord and Uffink (2001/2016, p. 42) in adopting a more perspicacious name, 'the measurement uncertainty principle'. Heisenberg explains it on the basis of a thought experiment. After having recalled that one is able to obtain information about the positions and momenta of a single electron by detecting a photon scattered from the electron in question, he notes that a photon of visible light would be too crude to do the job very well, because the wavelength of visible light is much larger than an electron. To localize an electron accurately in this manner one needs much smaller wavelengths, of the order of the size of the electron itself. That is why Heisenberg described a thought experiment in which a 'gamma ray microscope' was used. A gamma ray is a very hard X-ray, and it would in principle allow one to pin down the position of the electron with only a very small uncertainty.

However, the difficulty is that the shorter the wavelength the higher the energy of the photon: that is part and parcel of the quantum nature of light. So one can determine the position of an electron with high accuracy at the expense of giving it a massive recoil from the energetic photon that is being used to observe it. This changes the momentum of the electron, making it uncertain what that momentum was before the collision. Heisenberg argued that there is a trade-off between the accuracy of a position measurement, and the ineluctable disturbance that this measurement brings about. This leads to a lower bound on the product of the uncertainties in the position and the momentum of the electron. This lower bound is of the order of Planck's constant of action and gives us $\delta_p \delta_q \sim h$.

[3] The actual formula in Heisenberg's seminal paper is $p_1 q_1 \sim h$, where p_1 is "die Genauigkeit, mit der der Wert p bestimmbar ist" and q_1 is "etwa der mittlere Fehler von q" (Heisenberg, 1927, p. 175).

At two conferences in 1927, Niels Bohr famously explained that the trade-off which Heisenberg assumes does not exist (see footnote 2). He questioned the soundness of the gamma ray thought experiment on the grounds that one needs a short wavelength gamma ray to determine the position precisely, whereas to determine the momentum precisely one must use a large wavelength gamma ray. The change in the momentum, as a result of the position measurement, can therefore never be precisely determined *in the same experiment*.[4]

Earle Hesse Kennard, a theoretical physicist from Cornell University, discovered what we have called the second version of the uncertainty principle. Kennard spent a sabbatical in 1926–1927 at the University of Göttingen, from which Heisenberg had just departed.[5] Hilgevoord and Uffink (2001/2016, p. 41) refer to his discovery as the 'preparation uncertainty principle'. Instead of a single electron, Kennard considered an ensemble of electrons, prepared in some way that assured that they all had very approximately the same position and momentum. He showed that the statistical spreading of the position, multiplied by that of the momentum, must be greater than or equal to Planck's constant, h, divided by 4π:

$$\Delta q \, \Delta p \geq \frac{h}{4\pi}.$$

As noted before, the measure of statistical spreading, Δ, is the familiar standard deviation of statistics, to be distinguished from the unspecified and rather vague δ of Heisenberg. Planck's constant arises because of the following fundamental relation of quantum mechanics that requires the commutator of two conjugate quantities like q and p to be determined by this constant:

$$[q, p] = qp - pq = \frac{ih}{2\pi}.$$

The details of Kennard's calculation are given in Appendix A. All this is quite different from the measurement uncertainty principle which Heisenberg considered in his 1927 paper.

Yet Kennard takes his own version of the relation to be only a generalization of Heisenberg's uncertainty principle. Referring to the inequality concerning the product of the standard deviations, i.e. his own preparation uncertainty principle, he writes:

[4] Camilleri (2009, p. 93) has argued that for Heisenberg the matter is actually not epistemic but semantic: an accurate measurement of the position of an electron renders the concept of its momentum meaningless, rather than meaningful but unknowable. This distinction is interesting, but for our argument not relevant.

[5] See Kennard (1927). Heisenberg was formally *Privatdozent* in Göttingen from 1924 to 1927; but from May 1926 to the end of 1927 he was actually employed in Copenhagen as lecturer and assistant to Niels Bohr.

> This is the somewhat generalized uncertainty relation of Heisenberg. It places a lower bound on the product of the uncertainty measures for every pair of canonical variables (Kennard, 1927, p. 339).[6]

Interestingly, Kennard's assumption that the measurement uncertainty principle is a special case of the preparation uncertainty principle is taken over lock, stock and barrel by Heisenberg three years later. Heisenberg, too, thought of the two principles as essentially being the same. This is for example clear from the following passage, taken from a book which Heisenberg based on lectures he gave in 1930 at the University of Chicago:

> It must be emphasized again that this proof does not differ at all in mathematical content from that given at the beginning of the section [...] (Heisenberg, 1930, p. 19 of the English translation).

Here 'this proof' is Kennard's demonstration, which Heisenberg reproduces just before the above quote; and 'that given at the beginning of the section' refers to the gamma ray thought experiment that Heisenberg had used to explain what we have called the measurement uncertainty principle.

Given the positions of both Kennard and Heisenberg, it is hardly surprising that textbooks in quantum mechanics do not make a distinction between the two versions of the uncertainty principle. For example, in Leonard Schiff's justly famous book of instruction, the Kennard proof is given (with reference only to Heisenberg's 1930 book, not to Kennard's 1927 paper), with the concluding remark: "This is the precise expression of the Heisenberg uncertainty relation (3.1), when the uncertainties Δ_x and Δ_p are defined as in Eq. (12.1)" (Schiff, 1955, p. 55).

Schiff's Eq. (12.1) is simply the definition of the uncertainties as standard deviations, and he uses Δ_x instead of Δ_q to mean the dispersion of the position. However, his inequality (3.1) is the original Heisenberg version, and Schiff bolsters its interpretation by a discussion of Heisenberg's gamma ray microscope. He claims that

> [t]he relation (3.1) means that a component of the momentum of a particle cannot be precisely specified without our loss of all knowledge of the corresponding component of its position at that time (Schiff, 1955, p. 7).[7]

Kennard, Heisenberg, Schiff and others are however mistaken when they assume that the two versions are the same. Heisenberg's claim that Kennard's inequality

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[6] Our translation from the German: "Dieses ist das etwas verallgemeinerte Unbestimmtheitsgesetz von Heisenberg. Es setzt eine untere Grenze für das Produkt der Unbestimmtheitsmaße für jedes Paar kanonischer Variabeln fest." The inequality that Kennard is talking about is inequality (27) in his text.

[7] Another example is the textbook by L.E. Ballentine (1990, p. 166), who first derives in a very elegant way what is actually the preparation uncertainty principle and then ascribes it to Heisenberg. On a personal note, one of us remembers from his time as student a feeling of unease when confronted first with one version and then with another as if they were equivalent. At the time he did not worry too much about the matter but now finds it rather satisfying to learn that his unease was justified after all.

'does not differ at all in mathematical content' from his own uncertainty principle, is misleading. At a formal level the two mathematical formulae are the same, but their interpretations are very different: Heisenberg's formula is about one single particle, whereas Kennard's applies to an ensemble. As Hilgevoord and Uffink (2001/2016, p. 21) note: "both in status and intended role there is a difference between Kennard's inequality and Heisenberg's previous formulation".

One might even go further. For as we will explain in the next section, Kennard's principle is always true, whereas there exist situations in which Heisenberg's is false.

7.4 Universally Valid Uncertainty Relation

For a long time, the widespread practice of using the term 'the Heisenberg uncertainty principle' to refer to the preparation uncertainty principle appeared perfectly legitimate. At worst, it seemed merely a matter of employing a somewhat confusing label. But appearances can be deceptive. About the validity of the preparation principle there is no disagreement: it is a consequence of the mathematics of quantum mechanics. In a paper of 2003, Masanao Ozawa has argued that the same can however not be said of Heisenberg's measurement uncertainty principle. It is doubtful and may well be false.

In Heisenberg's original gamma ray thought experiment, the position is measured before the momentum. This order is however arbitrary and could be reversed: a position measurement typically disturbs the momentum and a momentum measurement typically disturbs the position. We shall nevertheless follow the convention and take the position as being what is measured first and the momentum as what is measured second.

When one tries to estimate the position of an electron, the best one can do is to measure the sum of two quantities: the position plus some perturbation or deviation.[8] And when one tries to measure the momentum just afterwards, one is confronted with the sum of the momentum plus the perturbation that is caused by that position measurement. Ozawa calls the perturbation in what one measures first (here: the position) *noise* and the perturbation in what one measures afterwards (here: the momentum) *disturbance*. He then rephrases the uncertainty relation in Heisenberg (1927) as the claim that the noise times the disturbance cannot drop below $h/4\pi$:

> "By the γ-ray thought experiment, Heisenberg […] argued that the product of the noise in a position measurement and the momentum disturbance caused by that measurement should be no less than $h/4\pi$." (Ozawa, 2003, p. 1; Ozawa has $\hbar/2$, which is equivalent.)

[8] This goes not only for an electron, but for all objects and processes in both the micro- and the macroworld.

Ozawa argues that this claim of Heisenberg is *not universally valid*. It would be universally valid if the noise and the disturbance were independent of the position and the momentum. However generally there is such a dependence, and then Heisenberg's claim is false. This can be seen once one realizes that the existence of noise and disturbance gives rise to four pairs of quantities which are conceptually different:

(1) The position and the momentum of the electron.
(2) The noise and the disturbance.
(3) The noise and the momentum of the electron.
(4) The disturbance and the position of the electron.

The pair (1) involves the variables that quantum mechanics deals with. These variables are operators, the combination of which leads to the appearance of Planck's constant, which reflects the finite granularity of energy and matter. The noise and disturbance in (2) constitutes the pair that Heisenberg considered in his gamma ray thought experiment (although the terms 'noise' and 'disturbance' are Ozawa's).

Ozawa's major contribution lies in his analysis of (3) and (4). Normally the noise and the momentum in (3) are correlated, and the same goes for the disturbance and the position in (4). As Ozawa realized, these correlations must be included in the calculation in the form of two new terms which have to be added to the Heisenberg term (2). That is, whereas Heisenberg considered only (2), Ozawa insists that he should have added (3) and (4) to (2) before confronting the result with (1), the diktat of quantum mechanics.

Taking the sum of (2), (3) and (4) leads to a new, expanded version of the uncertainty principle, which Ozawa calls 'the universally valid uncertainty relation'. Ozawa's relation applies to one particle, just like Heisenberg's principle, but if differs from the latter in implying that (2) + (3) + (4) is bounded by the Planck constant. Mathematically Ozawa's relation reads

$$\Delta_n \Delta_d + \Delta_q \Delta_d + \Delta_n \Delta_p \geq \frac{h}{4\pi}.$$

A derivation of this inequality is given in Appendix B. This new uncertainty principle gives rise to a remarkable possibility. While $\Delta_n \Delta_d + \Delta_q \Delta_d + \Delta_n \Delta_p$ may not be smaller than $\frac{h}{4\pi}$, the Heisenberg term, $\Delta_n \Delta_d$, could be zero. What is needed for this possibility is that $\Delta_q \Delta_d + \Delta_n \Delta_p$ be no less than $\frac{h}{4\pi}$. This means that Heisenberg's (1927) uncertainty relation, aka the measurement uncertainty principle, would be false. There is no positive lower bound for the product of the noise and the disturbance! As Ozawa has it:

> The universally valid uncertainty relation shows that for measurements of dependent intervention, the lower bound of the noise-disturbance product depends on the premeasurement [...] standard deviations [of the position and momentum]. In order to obtain the trade-off among the noise, the disturbance, and the premeasurement standard deviations uncertainties, we apply the [Kennard] uncertainty relation (Ozawa, 2003, p. 3).[9]

Note that Ozawa's work does not undermine the preparation uncertainty principle in Kennard's form. The latter only involves the statistical spreading of position and momentum as a result of the preparation of the state. Since it does not refer to any measurement, noise and disturbance do not play a role in that calculation. Thus Ozawa's principle is in a sense an amalgam of Kennard's preparation and Heisenberg's measurement principles, for it uses the former and corrects the latter.

While Ozawa's (2003) paper contained only theoretical results, his findings have subsequently been tested empirically. Two experiments deserve mention. Firstly, an investigation headed by Yuji Hasegawa has verified Ozawa's relation, although not for measurements of the position and momentum of an electron, as in Heisenberg's thought experiment, but rather for those of the spin of a neutron in two perpendicular directions (Erhart et al., 2012). These spin components are, like position and momentum, also subject to the measurement uncertainty principle; and Ozawa's two new terms have been measured in the experiment: the Heisenberg inequality is seen to be violated, while that of Ozawa is respected. Secondly, an entangled state of two photons was examined by a Chinese team led by Yang Liu and Zhihao Ma (see Liu et al., 2019). Again, the two measured quantities are not position and momentum, but rather amplitude and phase, which are also subject to uncertainty relations. Here, too, Heisenberg's relation was shown to be violated, indeed in some cases the product of noise and disturbance was zero.

Although it is generally agreed that Ozawa's work is mathematically correct, it has been objected by Busch et al. (2013) that Ozawa's standard deviation estimation of noise and disturbance is not the only measure possible. They propose an alternative measure that involves a different estimate of the noise and the disturbance, and they claim that with this measure the original uncertainty principle of Heisenberg would be correct. If Busch et al. are right, then for a different choice of measures Ozawa's claim no longer holds.

The final word in this dispute has clearly not been voiced, and it is not our intention to defend a definitive claim. Having said this, we are inclined to think that the balance at the moment would appear to be on the side of Ozawa. We have two reasons for this. The first is related to the experimental results mentioned above. Since they explicitly falsify the Heisenberg formulation, everything that vindicates this formulation seems *prima facie* suspect. Second, the alternative measure of Busch et al. has itself been criticized. Three kinds of criticism are worthwhile mentioning.

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[9] Because of the structure of quantum mechanics, it is possible in special cases for the noise to be zero (a so-called noiseless measurement). According to Heisenberg's 1927 version of the measurement uncertainty principle, this would mean that the disturbance in the momentum is infinite; but Ozawa's correction gives merely a finite, and not necessarily large lower bound for this disturbance.

The first came from Hilgevoord and Uffink, who argue that the alternative measure provides an estimate that is unnecessarily large; as a result, we might find ourselves not close to the inequality but rather a long way above it.[10] In itself, this criticism is not a fatal objection. It merely states that the measure by Busch et al. is a long way from being optimal, but the same can be said of Ozawa's method, so this objection does not favor one approach above the other.

The second criticism, also from Hilgevoord and Uffink (2001/2016), is graver. It is that the measure used by Busch et al. does not do the job it claims to do. It fails to estimate Heisenberg's measurement uncertainty of the momentum *in the same state* as that in which the uncertainty of the position is evaluated.

The third kind of criticism was put forward by Ozawa (2019). He argued rather convincingly that the measure of Busch et al. suffers from more serious deficiencies, while his own measure does not. According to Ozawa, it does not satisfy *correspondence*, i.e. agreement with classical measures where these should be applicable. Nor does it meet *completeness*, i.e. the requirement that the error measure never vanishes for inaccurate measurements. If Ozawa is right, then this lends great plausibility to his contention that Heisenberg's version of the measurement uncertainty principle is erroneous.

7.5 Nagel's Criticism Revisited

When *The Structure of Science* appeared in 1961, nobody realized that Kennard's uncertainty principle differed from Heisenberg's original version, let alone that the latter stands in need of correction. Small wonder, then, that Nagel's work is also innocent of the distinction. Yet it is interesting to ask ourselves what exactly Nagel had in mind when he was criticizing Heisenberg, and to what extent his criticism is sustainable in the light of later discoveries.

As we have seen in Sect. 7.2, Nagel accuses Heisenberg of "putting the cart before the horse". It is not the case that the uncertainty relation follows from the uncontrollable alterations:

$$not\text{: uncontrollability} \quad \rightarrow \quad \text{uncertainty relation.}$$

Rather it is the other way around:

[10] Hilgevoord and Uffink (2001/2016), and (1990). In a somewhat similar vein, Uffink and Hilgevoord (1985) earlier criticized the Kennard measure, explaining that for some exceptional probability distributions it gives too ample an estimate of the real spreading of position or momentum. One is more interested in where, say, 95% of the distribution is located than in what the least-squares measure would give. For normal, or near normal distributions with Gaussian tails, the criticism would be of little consequence; but one can think of other cases in which the Kennard measure would give a gross overestimate of the real uncertainty. This is not to say that the measure is wrong; rather for some abnormal distributions it is too crude.

$$\text{uncontrollability} \quad \leftarrow \quad \text{uncertainty relation.}$$

Our previous considerations have shown that these claims are in need of disambiguation. We have distinguished no less than three uncertainty relations:

H: Heisenberg's measurement uncertainty principle

K: Kennard's preparation uncertainty principle

O: Ozawa's universally valid uncertainty relation.

In addition, the notion of 'uncontrollability' is unclear. It could mean

K-uncontrollability: The inverse relation between the dispersions of position and momentum in an ensemble of particles, such as electrons.

H-uncontrollability: The impossibility of determining the position and momentum of a particle, e.g. an electron, with arbitrary accuracy.

What exactly does Nagel mean by 'uncertainty relation'? Obviously not *O*, for Ozawa's discovery had not yet seen the light of the day. That leaves *H* and *K*. Earlier we noted that for decades scholars have been talking about Heisenberg's measurement principle while actually referring to Kennard's preparation principle. The same goes for Nagel. When he comments on Heisenberg's *H*, he gives the formula that corresponds to Kennard's *K*:

> One of the [Heisenberg uncertainty relations] is expressed by the formula $\Delta_p \Delta_q \geq h/4\pi$. In this formula [...] 'Δ_p' is interpreted as the coefficient of dispersion (or deviation, sometimes also called the 'uncertainty') from the mean value of the momentum at a given instant; and similarly for 'Δ_q' (Nagel, l961, p. 294).

However, when talking about 'uncontrollability', Nagel is referring to Heisenberg's idea of the notion, and thus to *H*-uncontrollability rather than *K*-uncontrollability. This is for example clear from what he writes in Chap. 10:

> For the 'uncontrollable' [...] alterations that the electrons are said to suffer when they interact with instruments of measurement do not constitute the *evidence* for the uncertainty relations, but are part of the *consequences* drawn from the relations. This will be clear if we ask ourselves what ground we have for claiming that the alterations are uncontrollable and unpredictable, and if we remind ourselves that perturbations produced by measuring instruments in the objects measured were fully recognized in classical physics. [...] According to the Heisenberg uncertainty relations, however, the alterations produced in electrons by measurements on them cannot be calculated even in principle, because in this case electrons undergo "uncontrollable changes" (Nagel, 1961, p. 297, original emphases).

From this it follows that what Nagel actually means when he criticizes Heisenberg is expressed by the following two claims:

$$\textit{not}: H\text{-uncontrollability} \quad \rightarrow \quad K,$$

but rather the other way around:

$$H\text{-uncontrollability} \quad \leftarrow \quad K.$$

Are these claims correct? The first one evidently is. For irrespective of whether one has in mind K or H, and irrespective of whether one is thinking of H-uncontrollability or K-uncontrollability, it is never the case that the uncertainty relations follow from the uncontrollable alterations. So in this sense Nagel's criticism is perfectly appropriate.

This can however not be said of the second claim. Since K refers to the properties of an ensemble rather than those of one particle, the second claim is a *non sequitur*. It differs in important ways from the correct:

$$K\text{-uncontrollability} \quad \leftarrow \quad K.$$

In this connection, it is interesting to note that

$$H\text{-uncontrollability} \quad \leftarrow \quad O.$$

is also valid. That is, Ozawa's universally valid uncertainty relation entails the impossibility of determining the position and momentum of a particle with arbitrary accuracy. For such a determination would be tantamount to making both the noise and the consequent disturbance inherent in two linked measurements arbitrarily small. That Ozawa's relation does not permit this is clear from Ozawa's inequality $\Delta_n \Delta_d + \Delta_q \Delta_d + \Delta_n \Delta_p \geq \frac{h}{4\pi}$, since $\Delta_n = 0$ and $\Delta_d = 0$ would make all three terms on the left of this inequality zero, which would lead to inconsistency. Although Nagel knew nothing of Ozawa's discovery, we might perhaps grant that he was seeing the truth through a glass darkly. For although he erroneously thought that Kennard had validated H, nevertheless the corrected version of H, namely O, does indeed provide an explication of the claim that uncontrollability follows from the uncertainty relation.

In addition, there is another unclarity in Nagel's treatment of the uncertainty relations. In Sect. 7.2 we noted that, according to Nagel, many people see the existence of the uncertainty relations as the main reason for calling quantum mechanics indeterministic. We have also seen that Nagel rejects this view, and rightly so. This, however, raises the question what according to Nagel is the real reason for the indeterminism of quantum mechanics. Here is his answer:

> [D]espite the fact that quantum mechanics is deterministic with respect to the state description defined by the Psi-function, [...] outstanding physicists maintain that quantum theory is "indeterministic" in the important sense that its state description is associated with a statistical interpretation and that its predictions are based on statistical assumptions. This characterization is unquestionably appropriate (Nagel, 1961, p. 308).

Nagel thus accepts the view that quantum mechanics does not apply to individual particles, only to averages of properties in an ensemble. This is the 'statistical interpretation' of quantum mechanics, which Einstein and others espoused. It still has its advocates (see, e.g., Ballentine, 1990), despite the fact that there are experiments which indicate that quantum mechanics really does work at the level of individual particles. Oddly enough Nagel does not mention the 'standard' view that quantum mechanics is indeterministic because the wave function undergoes a collapse after an observation, the so-called projection postulate of John von Neumann (1955). There are many versions of how precisely the collapse takes place, but Nagel, who in 1961 must have been aware of at least some of them, apparently chose to ignore them all. On his reasons for doing so we can only speculate.

7.6 Conclusion

In this paper we have endorsed Nagel's criticism of Heisenberg. Nagel arraigns Heisenberg on the grounds that the latter has ineptly put the cart before the horse. The uncertainty principle is not a consequence of the uncontrollability of certain measurements in the submicroscopic domain. On the contrary, it is part and parcel of a theoretical edifice, viz. quantum mechanics; and it is this theory that entails the uncontrollability in question.

However, when Nagel goes further and forays into the sequence of implications

$$\text{quantum mechanics} \rightarrow \text{uncertainty principle} \rightarrow \text{uncontrollability}$$

he plays on a sticky wicket. For he confounds Heisenberg's and Kennard's uncertainty principles, and he fails to discriminate between uncontrollability at the level of an individual electron and the ineluctable dispersion in an ensemble. Nevertheless Nagel succeeds in his negative claim, i.e. his criticism of Heisenberg; and if he falters in his positive assertion, it is a shortcoming shared by many.

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Appendices

A Kennard's Uncertainty Relation

The position and momentum of the electron are represented by the self-adjoint operators q and p respectively. These operators on Hilbert space are conjugate, and they satisfy the commutation relation $[p, q] = -i\hbar = -\frac{ih}{2\pi}$. Let $\langle q \rangle$ and $\langle p \rangle$ be the expectation values of q and p with respect to some quantum state; and define the shifted operators $\tilde{q} = q - \langle q \rangle$ and $\tilde{p} = p - \langle p \rangle$. Clearly $[\tilde{p}, \tilde{q}] = -i\hbar$. The variances are

$$\begin{aligned} \Delta_q^2 &= \left\langle \Big(q - \langle q \rangle\Big)^2 \right\rangle = \langle\, \tilde{q}^2 \,\rangle \\ \Delta_p^2 &= \left\langle \Big(p - \langle p \rangle\Big)^2 \right\rangle = \langle\, \tilde{p}^2 \,\rangle\,. \end{aligned}$$

Define the operator $t = \tilde{p} + i\omega\tilde{q}$, where ω is a real number, so

$$\begin{aligned} 0 \;\leq\; \langle t^\dagger t \rangle &= \langle (\tilde{p} - i\omega\tilde{q})(\tilde{p} + i\omega\tilde{q}) \rangle \\ &= \langle \tilde{p}^2 \rangle + i\omega \langle [\tilde{p}, \tilde{q}] \rangle + \omega^2 \langle \tilde{q}^2 \rangle \\ &= \Delta_p^2 + \omega\hbar + \omega^2 \Delta_q^2 \\ &= \left(\Delta_p + \frac{\omega\hbar}{2\Delta_p} \right)^2 + \frac{\omega^2}{\Delta_p^2} \left(\Delta_q^2 \Delta_p^2 - \frac{\hbar^2}{4} \right) \end{aligned}$$

Minimize this by setting

$$\omega = -\frac{2\Delta_p^2}{\hbar}$$

so

$$\Delta_q^2 \Delta_p^2 - \frac{\hbar^2}{4} \geq 0$$

from which Kennard's result follows:

$$\Delta_q \Delta_p \;\geq\; \frac{\hbar}{2} \;=\; \frac{h}{4\pi}\,.$$

B Ozawa's Uncertainty Relation

Consider the operators

$$q^{out} = q + n \quad \text{and} \quad p^{out} = p + d\,.$$

q^{out} and p^{out} are measured in different experimental setups, so they commute:

$$[q + n, p + d] = 0$$

i.e.

$$[q, p] + [n, d] + [q, d] + [n, p] = 0\,.$$

So successively

$$[n, d] + [q, d] + [n, p] = -[q, p]$$

$$\langle [n, d] + [q, d] + [n, p] \rangle = -\langle [q, p] \rangle$$

$$|\langle [n, d] \rangle| + |\langle [q, d] \rangle| + |\langle [n, p] \rangle| \geq |\langle [n, d] + [q, d] + [n, p] \rangle| = |\langle [q, p] \rangle| = \tfrac{h}{2\pi}\,.$$

By Kennard's method,

$$\Delta_n \Delta_d \geq \tfrac{1}{2} |\langle [n, d] \rangle|, \qquad \Delta_q \Delta_d \geq \tfrac{1}{2} |\langle [q, d] \rangle|, \qquad \Delta_n \Delta_p \geq \tfrac{1}{2} |\langle [n, p] \rangle|,$$

and therefore

$$\Delta_n \Delta_d + \Delta_q \Delta_d + \Delta_n \Delta_p \geq \tfrac{h}{4\pi}\,.$$

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Chapter 8
Ernest Nagel on Determinism as a Guiding Principle and Its Compatibility with Quantum Mechanics



Marij van Strien

Abstract According to Ernest Nagel, determinism is central to the scientific enterprise. Faced with the claim that determinism fails in quantum mechanics, Nagel proposed a notion of determinism which does not rely on a fundamental level of description, and can play a role in different scientific disciplines irrespective of their reducibility to physics. Nagel argues that determinism ultimately plays the role of a guiding principle in scientific research. In this way, Nagel argues that determinism has an enduring relevance in all domains of science, from quantum physics to the social sciences.

Keywords Determinism · Quantum mechanics · Reductionism · Autonomy of scientific disciplines

8.1 Introduction

Determinism is generally defined as the principle that everything that happens is uniquely determined by an initial state of the world (or of the system under consideration) plus the laws of nature. Defined in this way, determinism is closely related to physical reductionism: the laws of nature according to which everything is determined are usually taken to be the fundamental laws of physics, so that in the end, everything that happens is determined by the *physical* state of the world (or the system under consideration) plus the laws of *physics*. In the formulation of scientific determinism that remains the most well-known, Laplace states:

> An intelligence which, for one given instant, would know all the forces by which nature is animated and the respective situation of the entities which compose it, if besides it were sufficiently vast to submit all these data to mathematical analysis, would encompass in the same formula the movements of the largest bodies in the universe and those of the lightest atom; for it, nothing would be uncertain and the future, as the past, would be present to its eyes. (Laplace, 1814, pp. 3–4)

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A more exact formulation of this Laplacian determinism is the following: given the values of positions and momenta of all particles in the universe at an instant, plus the force laws for all the forces which act between the particles, the entire future (and past) evolution of the universe is uniquely determined and can in principle be calculated. The state of the universe, here, is given by the positions and momenta of all particles. This determinism can be understood as ontological: it is a feature of nature, of the universe as a whole. In Laplace's formulation of determinism, it is thus assumed that all occurrences in the universe can be analyzed in terms of the dynamics of point particles. The fact that determinism has often been seen as posing a threat to free will, as well as to the autonomy of scientific disciplines other than fundamental physics, is closely related to this reductionism which is inherent in Laplacian determinism: if everything that happens is in the end determined by the laws of physics, then it seems that it is only physics which really makes a difference.

It is generally thought that up until the early twentieth century, physics was indeed deterministic, since the basic laws of classical (Newtonian) mechanics are thought to have a deterministic character: they always yield a unique solution for given initial conditions.[1] Especially in the second half of the nineteenth and early twentieth century, determinism in physics was often seen as oppressive, and connected to crude materialism, reductionism and scientific arrogance. However, things changed with the introduction of the modern theory of quantum mechanics in 1925/26, a fundamental theory of physics which was generally interpreted as being indeterministic and involving fundamental chance, and the development of which led to widespread claims that determinism had been overthrown. Quantum physicists themselves frequently argued that the refutation of determinism in quantum physics opened up possibilities for reconciling physics with the possibility of free will, vitalism, and certain religious views (Heilbron, 1988).

Whether determinism was definitively overthrown through the introduction of quantum mechanics can be debated, as there continue to be determinist approaches to quantum physics (notably, the approach known as Bohmian mechanics). Whether this abandonment of determinism really helped to secure the possibility of free will and the autonomy of other scientific disciplines can be debated as well, as an introduction of fundamental chance does not really seem what is needed for this end. It is significant that discussions on free will continue to be framed in terms of determinism, despite determinism having supposedly been overthrown almost a century ago.

In the work of Ernest Nagel, however, we find a conception of determinism which is not specifically connected to fundamental physics: one can ask whether a specific scientific theory, for example, a theory in biology or sociology, is deterministic, and this is independent of any consideration of fundamental physics. Determinism is essentially the statement that for any occurrence, it is possible to find the necessary and sufficient conditions under which it takes place. Nagel argues that determinism in its broad generality, as a claim about all occurrences in the universe, can neither

[1] Much more can be said about whether classical physics is in fact deterministic, see e.g. Earman (1986), Norton (2008), Van Strien (2014b), and Romizi (2019).

be proven nor disproven, and is thus not an empirical issue. Rather, he argues that it functions as a heuristic principle, a guiding principle in scientific research. As science is largely about finding the conditions under which phenomena take place, determinism, conceived of in this way, is essentially the assumption that science is possible. This means that determinism should not be given up lightly: as he writes on the very last page of *The Structure of Science* (1961, p. 606), "to abandon the deterministic principle itself is to withdraw from the enterprise of science".

In this paper, I show that although the conception of determinism as a regulative principle in science no longer seems to be the standard way of thinking about determinism, this conception was actually common in the late nineteenth and the first half of the twentieth century. Conceived of in this way, determinism is not a grand doctrine about the role of fundamental chance in the universe, but is a more modest statement, which is essentially about science as an activity: the scientists should never stop looking for the determining conditions of occurrences. However, one can still ask whether this conception of determinism as a heuristic principle is compatible with quantum mechanics. Supposedly, quantum mechanics is fundamentally indeterministic, which goes against Nagel's claim that determinism can never be disproven. Does the indeterminism of quantum mechanics mean that there is a limit to the very possibility of science in this domain, a limit to the extent to which nature can be analyzed and understood? Do we have to conclude that quantum mechanics is not a science in the classical sense, or that we have to "withdraw from the enterprise of science" within the limits set by the Heisenberg uncertainty relations? A second question that is raised is whether determinism, conceived of in this sense, can really be regarded as a guiding principle in science: can such a broad general principle as the principle that one should look for determining conditions for any occurrence offer active guidance to the scientist?

I will argue that Nagel's answers to these two problems are connected. In the context of quantum mechanics, Nagel developed a broader notion of determinism as a heuristic principle, which according to Nagel is not violated in quantum mechanics: on the contrary, he argues that according to this conception, the theory of quantum mechanics is deterministic. This broader notion of determinism is not tied to mechanical reductionism and can therefore play a role in quantum mechanics as well as in other scientific disciplines, including in social and historical sciences, without affecting the autonomy of these sciences. I first show how Nagel's reflections on quantum mechanics led him to a more general conception of determinism, and then turn to the questions whether quantum mechanics is indeed deterministic according to this conception, and how it can function in other disciplines than physics.

8.2 The Supposed Violation of Determinism in Quantum Mechanics

In a review of Reichenbach's *Philosophic Foundations of Quantum Mechanics,* which appeared in *The Journal of Philosophy* in 1945, Nagel makes a number of strong claims of his own on the philosophical interpretation of quantum mechanics. Specifically, Nagel objects to the commonly accepted idea (also central to Reichenbach's account of quantum mechanics) that quantum mechanics is indeterministic. He argues in particular that Heisenberg's uncertainty relations, or indeterminacy relations, as Nagel refers to them in his review, do not imply a failure of determinism.

Laplacian determinism is formulated in terms of positions and momenta of particles: given the positions and momenta of all particles in the universe or in an isolated system, plus the forces which act between the particles, it is in principle possible to calculate the entire future evolution of the system.[2] Nagel reads Reichenbach as arguing that this does not hold in quantum mechanics, because Heisenberg's indeterminacy relations entail that there is a fundamental limit to the precision with which the parameters q and p can be predicted, where q and p stand for the instantaneous position and momentum of a subatomic particle, respectively.

However, Nagel objects to the identification of q and p with the position and momentum of a particle. As he writes in his review of Reichenbach:

> [A]lthough in quantum mechanics these parameters are *called* the coördinates of "position" and "momentum" of "particles," just how are we to understand these words in this context? According to the usage associated with them in classical physics, a particle has a determinate position and simultaneously a determinate momentum. On that usage, therefore, it is simply nonsense to say that a particle has a definite position but no determinate momentum, or vice versa; for the meanings of the words are so interrelated that nothing can be said to be a particle which does not necessarily have a determinate position and momentum. (Nagel, 1945, pp. 441–442)

In Nagel's interpretation, Heisenberg's indeterminacy relations do not entail that there is a limit to the precision with which q and p can be measured, but rather entail that a subatomic particle cannot be taken to simultaneously *have* a determinate value

[2] In a later paper, Nagel (1953) notes that in experimental practice, it is impossible to measure instantaneous values of position and momentum: there will always be some inaccuracy in the measurement, and in this way, statistic comes in. Nagel notes that some people have argued on this basis that classical mechanics is not deterministic, but argues that this confuses the issues of (1) the logical structure of the theory and (2) the relations of the theory with observations and experiments. Classical mechanics is deterministic because of its theoretical structure; that this determinism cannot be directly verified experimentally does not affect this property of the theory. "It is hardly more than a truism to maintain that classical mechanics is 'indeterministic' or statistical in nature, if the claim rests on no other grounds than that the experimental confirmation of classical mechanics involves the use of statistical procedures and that experiment confirms it only approximately. For any quantitatively formulated theory viewed from this standpoint is 'indeterministic' and statistical: the experimental measurement of physical magnitudes such as velocity always yields a 'spread' of values, and no law asserting a relation of dependence between continuous variables is in absolutely precise agreement with data of observation" (Nagel, 1953, p. 425).

for q and p (Nagel, 1945).[3] And this means that q and p cannot stand for the position and momentum of a particle, because it is part of the very definition of the concept of a particle that it has a well-defined position and momentum. Nagel argues that the expression 'the precise simultaneous values of the position and momentum of a sub-atomic particle' is meaningless, unless at least one of the words 'position', 'momentum' or 'particle' is not used with its customary meaning. In (1953), he elaborates on this line of argument by arguing that it makes no sense to ask for the exact simultaneous values for the position and momentum of (for example) an electron, because quantum entities are in part implicitly defined through Heisenberg's uncertainty relations (1953, p. 433).

In *The Structure of Science*, Nagel writes that the tendency to conceive of electrons and other quantum entities as particles stems from the desire to supplement an abstract physical theory with a visualizable model. According to Nagel, it is often fruitful to work with such models, since they enable a more direct understanding, but the use of visualizable models can also lead to misunderstandings: in particular, it can lead to taking an accidental feature of the model as essential, or to confusing the model with reality. As a concrete example, he writes that the use of the conception of particles is "a frequent source of misconceptions concerning the import of quantum theory" (Nagel, 1961, p. 115). The use of the term 'particle' tends to bring in an image of particles, which suggests that particles *do* have precise simultaneous values for position and momentum, and that the import of Heisenberg's uncertainty relations is merely that we cannot *know* these precise values. However, that would mean that we accept the existence of something which is fundamentally unknowable; "[s]uch a conclusion, were it really necessitated by the facts, would require at least the partial abandonment of the ideal of verifiability which dominates modern science" (Nagel, 1953, p. 430).

What this means for the issue of determinism is that Laplacian determinism does not so much fail in quantum mechanics, but is rather inapplicable, since the basic terms in which Laplacian determinism is formulated do not occur in quantum mechanics, at least not with their usual meanings. As Nagel writes in his review of Reichenbach:

> If, therefore, the "*q's*" and "*p's*" of quantum mechanics do not refer to positions and momenta of particles in the classical senses of these words, and if causal laws are those which connect physical states specified in terms of positions and momenta, then the fact that conjugate values of q and p satisfy the Heisenberg indeterminacy relation does not warrant any conclusion as to the failure of causal laws in quantum mechanics. (Nagel, 1945, p. 442)

In the classical conception of Laplace, the state of a system is given by the position and momenta of all particles. It is not the case that in quantum mechanics, the state of the system, defined in this way, fails to determine later states; rather, "the classical conception of the physical state of a system is *not appropriate* for quantum mechanics" (Nagel, 1945, p. 442, original emphasis).

[3] For more on Nagel's interpretation of Heisenberg's uncertainty relations, see Atkinson and Peijnenburg, in this volume.

According to Nagel, this argument refutes the claim that quantum mechanics is indeterministic. The argument rests on the assumption that the indeterminism of quantum mechanics is due to the Heisenberg uncertainty relations. This was a common view at the time, and Nagel reads Reichenbach as supporting this view. However, in a reply to Nagel's review, Reichenbach argued that Nagel had misunderstood him, and that it are in fact *not* Heisenberg's uncertainty relations which provides the decisive argument against causality, but rather what is known as wave function collapse (Reichenbach, 1946) (more on this in Sect. 8.5). But despite this criticism, Nagel did not change his views, and still maintained (1953, p. 427) that "[i]t is the occurrence of [the] Uncertainty Relations within modern quantum mechanics that is held to mark the indeterministic character of that theory".

8.3 Determinism as a Guiding Principle

According to Nagel, the classical, Laplacian conception of determinism is inextricably connected with mechanics, and can therefore only play a role in contexts in which one can work with the basic notions of mechanics. Nagel (1961, p. 286) argues that determinism is often identified with mechanism, but emphasizes that this identification "is a mistaken one". He argues that a broader notion of determinism is possible, in which the concepts of position, momentum and particle do not occur, and which can play a role in different areas of science, irrespective of their reducibility to mechanics. Nagel (1979, p. 262) defines determinism as follows: "In the loosest relevant sense of this word, it is a label for the claim that all things, events, processes, and traits come into existence, endure, or pass out of it, only under fixed and definite conditions". And in (1960, p. 296): "determinism in its most general form appears to be the claim that for every set of characteristics which may occur at any time, there is some system that is deterministic in respect to those occurrences". Thus, by attempting to give a deterministic account of events, the scientist looks for necessary and sufficient conditions under which these events come about.

One can ask whether a specific scientific *theory* is deterministic. According to Nagel, this can be decided on the basis of its theoretical structure: a theory is deterministic if it shows how the state of a system at a certain time can be derived from the state at an earlier time. For example, in *The Structure of Science*, Nagel (1961, p. 292) writes that the state is of a system is given by the values of a set of state variables, and that "[a] theory is deterministic if, and only if, given the values of its state variables for some initial period, the theory logically determines a unique set of values for those variables for any other period".

Furthermore, one can ask whether natural processes are *in themselves* deterministic, or whether *our world* is deterministic. However, these questions are fundamentally undecidable: Nagel (1960, p. 296) argues that determinism, defined as a universal statement about all occurrences, "has not been conclusively established, nor can it be conclusively refuted by the outcome of any empirical investigation". It has not been conclusively established, because there are many events for which we

have not found the determining conditions under which they take place, and "it is at least logically possible that for some of these events no determining conditions in fact exist" (Nagel, 1961, p. 605). And it cannot be conclusively refuted because, if we have not found the determining conditions for an event, this does not prove that no such conditions exist. In particular, if it seems that a system is not deterministic with respect to a certain set of properties, it is always possible that this is due to the system not being properly isolated, or that our account of the system is incomplete or incorrect (Nagel, 1960, p. 296).[4]

Nagel argues that therefore, determinism should not be understood as an empirical principle. He argues rather that it plays the role of a regulative principle in scientific inquiry:

> [O]n my construction of determinism, it is in effect a regulative principle which formulates the general objective of science as a search for explanations—as a quest for ascertaining the conditions upon which the occurrence of events is contingent. (Nagel, 1960, p. 317)

We can thus distinguish two conceptions of determinism in Nagel's work: (1) a specific theory can be deterministic, thus determinism can be a feature of a scientific theory, (2) determinism also functions as a regulative principle in science. Constructed as a regulative principle, determinism "formulates in a comprehensive way one of the major objectives of positive science, namely, the discovery of the determinants for the occurrence of events" (Nagel, 1961, p. 605). Nagel makes no clear distinction between the principle of determinism and the principle of causality, which have often been treated as synonymous.[5] In *Logic without Metaphysics*, he presents the principle of causality in a way that seems identical with the principle of determinism:

> [I]n its usual formulations—and there are a variety of them—[the principle of causality] states no "law of nature" and has no identifiable descriptive content. On the contrary, the principle functions as a maxim, as a somewhat vague rule for directing the course of inquiry, as an injunction to interpret and organize our experience in a certain manner. For what the principle in effect says is this: When some occurrence takes place, look for circumstances which are the necessary and sufficient conditions for that occurrence! Do not cease your quest until such circumstances have been discovered, and count no analysis as adequate or complete which does not terminate in the construction of a theory that conforms to the pattern of a deterministic schema of explanation! (Nagel, 1956, pp. 124–125)

[4] Within logical empiricism, there were extensive discussions about the question whether determinism, as a general principle, has empirical consequences or whether it is tautological (see Placek, 2014). Nagel's claim that determinism is not empirically testable stands within this tradition. As Placek points out, in more recent literature, the question whether determinism in general is empirically testable is no longer asked: rather, determinism is taken to be an attribute of scientific theories, so one can merely ask whether specific theories are deterministic. This leads to the different question, posed by Werndl (2009), of whether deterministic and indeterministic theories are empiricially distinguishable.

[5] Causality and determinism can also come apart, especially since causality has more possible meanings than determinism: causality can e.g. also refer to conditions of locality, or to the presence of conservation laws (for an overview see Ben-Menahem, 2018). But as far as I have seen, Nagel uses 'determinism' and 'causality' synonymously.

The conception of determinism as a regulative principle in science was in fact quite common in the late nineteenth century and in the first decades of the twentieth century. Already around 1900, there was a growing skepticism about the possibility to reduce all of physics to mechanics, and several physicists and philosophers argued that determinism should not be understood as an empirical claim or as an a priori truth, but rather as a regulative principle in science (Van Strien, 2021). For example, Mach argued in 1905 that determinism cannot be proven or disproven empirically, but plays the role of a presupposition which is needed in scientific research: "during research, every thinker is necessarily a theoretical determinist" (Mach, 1905, p. 277).[6] Planck wrote in 1932 that determinism is a "heuristic principle, a signpost and in my opinion the most valuable signpost we possess, to guide us through the motley disorder of events and to indicate the direction in which scientific inquiry should proceed in order to attain fruitful results" (Planck, 1932, translation: Earman, 2007, p. 1372). Schlick, in his *Allgemeine Erkenntnislehre* (1918) defines the principle of causality as the principle that everything that happens is fully determined through laws of nature. Essentially, science is about looking for laws through which events are determined, and therefore the working scientist has to assume that there are such laws to be found. Schlick argues that this principle cannot be accepted as an a priori truth and cannot be empirically confirmed either: it can only be justified practically. Cassirer, in *Determinism and Indeterminism in Modern Physics,* originally published in 1936, similarly defines the principle of causality, or determinism, as the assumption that there are lawful connections between the phenomena, and argues that this principle is to be understood methodologically, as an assumption made in scientific research (Cassirer, 1956).

One can ask whether this principle of determinism or causality, as a regulative principle, is a necessary principle in science. In *Logic without Metaphysics*, Nagel is not entirely conclusive about this issue:

> In the nature of the case no principle construed as such a maxim can be "refuted" by the facts, though to be sure it may be abandoned as a directive if repeated failures attend the quest which it enjoins. It is perhaps a debatable question whether the total rejection of the principle does not entail the complete abandonment of the scientific enterprise. But there is no doubt whatever that to the extent the principle is accepted as a regulative principle for inquiry, it functions as a maxim and specifies a criterion for what is to be counted as systematic knowledge. (Nagel, 1956, p. 125)

Thus, although it is not entirely unthinkable that the causal principle may be given up at some point in the future, it is central to our current conception of scientific knowledge: according to our current conception, finding the conditions under which occurrences take place is essentially what science is about. In *The Structure of Science*, Nagel similarly argues that, although the principle of determinism is not

[6] Mach argued, however, that also indeterminism plays a heuristic role in science: it is important to be aware of the limitations of scientific theories and pay attention to the unpredictable, since we can never be sure that our theories are fully accurate and complete; therefore, "also he who advocates an extreme determinism in theory must in practice remain an indeterminist, especially if he does not want to speculate away the most important discoveries" (Mach 1905, p. 278, my translation).

logically necessary in science, abandoning it would mean a significant change in the aims of science:

> [I]f the principle is a maxim, is it a rule that may be followed or ignored at will? Is it merely an arbitrary matter what general goals are pursued by theoretical science in its development? It is undoubtedly only a contingent historical fact that the enterprise known as "science" does aim at achieving the type of explanations prescribed by the principle; for it is logically possible that in their efforts at mastering their environments men might have aimed at something quite different. Accordingly, the goals men adopt in the pursuit of knowledge are logically arbitrary.
>
> Nevertheless, the actual pursuit of theoretical science in modern times is directed toward certain goals, one of which is formulated by the principle of causality. (Nagel, 1961, p. 324)

Nagel remarks that if an enterprise is not controlled by the principle of causality, one would probably not even call it "theoretical science": "it is difficult to understand how it would be possible for theoretical science to surrender the general ideal expressed by the principle without becoming thereby transformed into something incomparably different from what that enterprise actually is" (Nagel, 1961, p. 324). On the very last page of *The Structure of Science*, he makes an even stronger claim: "to abandon the deterministic principle itself is to withdraw from the enterprise of science" (1961, p. 606). This shows how high the stakes are: to abandon the principle of determinism would mean to change the fundamental character of science, or even to give up on science altogether.

8.4 Determinism as Relative to State Descriptions

Whereas Laplace offered a specific aim for science, namely, to predict future occurrences on the basis of calculations of the trajectories of point particles, the conception of determinism offered by Nagel is much more abstract. One may doubt whether the principle that one should look for determining conditions of events can really act as a guiding principle in science, as it seems too general to offer concrete guidance. In fact, Nagel mentions the concern that with his definition of determinism, determinism may be a truism. He refers to Philipp Frank (1907), who considered the status of the law of causality, which he defines as the statement that equal states are always followed by equal states. Thus, the law of causality states that if a system was once in a state A which was followed by a state B, then if the system is ever in state A again, it will again be followed by state B. Frank asks what is exactly meant here by the 'state' of a system. If the state of a system is given by its observable characteristics, then the law of causality is false: identically looking pieces of metal may or may not be magnetic, and this changes their behavior. But if the state of a system can also include properties that are not directly observable, then the statement "state A is always followed by state B" is not empirically testable, because whenever the system seems to be in state A but this is not followed by state B, one can always argue that the system was not in state A after all, and that there was a relevant difference that we missed. Frank argues that the concept of 'state' is actually defined through the

statement that equal states are always followed by equal states: we can only confirm that we have correctly specified the state of a system when, with this specification, it holds that equal states are always followed by equal states. Thus, the law of causality is not an empirical statement but is essentially conventional and even tautological (Frank, 1907).

The same remark can be made about Nagel's definition of determinism in terms of states of a system: we can only be sure that we have correctly defined the state of a system when we have a theory which is deterministic with respect to this definition of states. In all its generality, the claim that "the state of a system at a certain instant determines the state of the system at later instants" is thus merely a definition of the term "state of a system". Nagel essentially agrees with this judgment. However, the relevant point for Nagel is that the state of the system does not have to be its mechanical state, and that other state descriptions are possible and "have been used successfully in several branches of natural science" (Nagel, 1953). In fact, it is exactly the disconnection of determinism from mechanics and its abstract formulation which enables an account of determinism which can also play a role in other scientific disciplines, irrespective of their reducibility to physics. Nagel writes:

> It is worth noting, however, that determinism functions most effectively as a regulative principle, when the highly generalized formulation suggested above is replaced by a more specific one—one which stipulates more or less definitely what sort of characteristics are to be looked into in our search for the determining conditions of various types of events. (Nagel, 1960, p. 296)

Thus, specific *forms* of determinism can be useful as guiding principles. It may be trivially true that any theory is deterministic with respect to its state description, but the challenge is to find an appropriate state description.

As we have seen, Nagel argues that classical mechanics is deterministic with respect to mechanical states, that is, instantaneous values of position and momenta of each particle, plus a specification of the forces which act between the particles. Nagel emphasizes that classical mechanics "is deterministic *specifically and exclusively* with respect to the mechanical states of the system" (Nagel, 1953, p. 423, emphasis added): the mechanical state at one instant determines the positions and momenta of particles at later instants, plus other mechanical properties which are defined in terms of positions and momenta of point particles, but one cannot automatically assume that it also determines for example electromagnetic phenomena, or even the motion of liquids, unless it is established how these follow from the dynamics of particles. For this reason, Nagel argues against Laplace's statement that for an intelligence with perfect knowledge of the mechanical state of the universe at an instant and the forces at play, "nothing would be uncertain".[7] Such an intelligence could calculate all future mechanical states of the universe, but that does not yet mean that this intelligence would have perfect knowledge of e.g. electrodynamic, thermodynamic, biological

[7] "Quite clearly, then, a non-sequitur is involved in Laplace's dictum that 'nothing would be uncertain' for an intelligence possessing a requisite knowledge of mechanical states and forces" (Nagel, 1953, p. 423).

and social processes. To derive all of this directly from the positions and momenta of particles is logically impossible:

> Laplace was demonstrably in error when he believed that a Divine Intelligence could foretell the future in every detail, given the instantaneous positions and momenta of all material particles as well as the magnitudes and directions of the forces acting between them. At any rate, Laplace was in error if his Divine Intelligence is assumed to draw inferences in accordance with the canons of logic, and is therefore assumed to be incapable of the blunder of asserting a statement as a conclusion of an inference when the statement contains terms not occurring in the premises. (Nagel, 1961, p. 366)

In order to account for e.g. electrodynamic, thermodynamic, biological and social processes in terms of mechanical ones, one always has to introduce bridge principles to connect the terms of these different sciences.[8]

In the nineteenth century, there were extensive attempts to reduce all of physics to mechanics, which were partially successful; however, this mechanical reductionism was more and more criticized towards the end of the nineteenth century, and physicists such as Mach and Duhem argued that mechanics need not be regarded as the fundamental science, and that e.g. electrodynamics and thermodynamics could stand on their own, without requiring a reduction to mechanics. Nagel argues that towards the end of the nineteenth century, other forms of determinism were established:

> It is a familiar fact that the Laplacian notion of determinism was for a time a fruitful guiding principle for an extensive class of investigations, although its fertility eventually became exhausted, and by the end of the 19th century physical scientists adopted other special forms of the deterministic assumption. (Nagel, 1960, p. 296)

Nagel argues specifically that other conceptions of physical state emerged during the nineteenth century. The state of a system can, for example, be given by an infinite rather than a finite number of values, as is the case in field theories, such as the electromagnetic theory of Maxwell. Or, instead of instantaneous values for dynamical variables, the specification of a state may require their values at several distinct instants, or during an interval. This is for example the case in the branch of mechanics called "hereditary mechanics" which deals with the effects of stress of bodies over time and describes for example the phenomenon of metal fatigue: the deformation of a piece of metal depends not only on the stress applied to it at a particular moment, but also on the stress that has been applied to it in the past (see Ianniello & Israel, 1993). This means that in these cases, in order to make predictions about future states, it does not suffice to know the current state of the system, since one also needs information about past states. In 1910, Painlevé, noting that this went against Laplacian determinism, drew a dramatic conclusion: "The notion according to which one must know the whole past of a physical system in order to predict its future, is

[8] It does not really matter here at what moment this is done: say that from the mechanical state m_1 at time t_1, one can derive the mechanical state m_2 at time t_2. To arrive at the state at a higher level of description at time t_2, one can use bridge principles to derive this state from m_2. Alternatively, one could use bridge principles to connect the initial state m_1, to a higher level state h_1, and then derive the state h_2 from h_1—the latter would require having laws which determine the behavior of the system at the higher level of description.

the very denial of science" (Painlevé, quoted in Nagel, 1961, p. 289). For Nagel, however, it is merely a different form of determinism (1953).[9] He notes that it is also often the case in biology and medicine that in order to predict the future states of a system, one needs knowledge not only of the present state but also of past states (Nagel, 1961).

Yet another way to define the physical state of a system is through variables which describe statistical properties of a class of elements, rather than properties of individual objects—this way of describing states of a system is used e.g. in statistical mechanics. Nagel argues against the common conception that statistical mechanics is not deterministic: he argues that given the values for the variables describing the state of a system in statistical mechanics at an instant, the theory uniquely specifies future values for these variables. Therefore, the theoretical structure of statistical mechanics is deterministic, and the fact that the state variables happen to be statistical does not change this property of the theory (Nagel, 1961).

Thus, Nagel argues that there are different ways in which the state of a physical system can be defined, and theories can be deterministic with respect to a specific definition of physical states.

8.5 Determinism in Quantum Mechanics

It is exactly this idea, that determinism is relative to a specific definition of physical states, which enabled Nagel to argue that the principle of determinism is not violated in quantum mechanics.

With Nagel's notion of determinism, if a specific theory is not deterministic, this does not disprove determinism in general: we can continue our search for the determining conditions of occurrences. However, if it holds that within the quantum domain, it is fundamentally impossible to find determining conditions for certain occurrences, this would represent a problem for Nagel's account: it would mean that determinism is conclusively refuted in the quantum domain, and would thus go directly against Nagel's claim that determinism can be neither conclusively established nor conclusively refuted. And, according to the most common interpretations of quantum mechanics, it is indeed the case that it is fundamentally impossible to find determining conditions for certain occurrences.

For this reason, Schlick had argued in 1931 that the principle of causality, as a regulative principle in science, had turned out to be useless in the quantum domain, because the Heisenberg uncertainty relations show that there is a limit to the precision with which position and momentum of a quantum particle can be predicted (Schlick, 1931). In contrast to Schlick, Cassirer argued that the principle of causality, when correctly understood, is not violated in quantum mechanics: the principle of causality merely states that it is possible to find lawful connections between occurrences, and

[9] The equations of field theories and of hereditary mechanics typically yield well-posed problems, in the sense developed by Hadamard around 1900.

also quantum mechanics is based on strict laws of nature. The real import of quantum mechanics, according to Cassirer, is that it forces us to change our conception of matter: matter can no longer be thought of as consisting of particles which can be individuated and localized in space and time. Like Nagel, Cassirer argues that we can no longer think of electrons as having a well-defined position and momentum, and that it is not possible to visualize processes in the quantum domain. There are strong similarities between Nagel's and Cassirer's accounts of quantum mechanics, and Nagel was well aware of Cassirer's work on this topic: in 1938, Nagel published a review of Cassirer's *Determinism and Indeterminism in Modern Physics*, in which he wrote that "on many important points it is only his Kantian terminology and Kantian piety which distinguishes Cassirer from many naturalistic pragmatists and logical empiricists who have concerned themselves with similar themes" (Nagel, 1938, p. 230).

For Nagel, the idea that determinism is always relative to a certain description of physical states enables a straightforward answer to the question of determinism in quantum mechanics. He argues that the correct specification of physical states in quantum mechanics is given by the wave function. In quantum mechanics, a physical system can be described through its wave function, and the wave function evolves deterministically according to the Schrödinger equation: thus, given the wave function at an instant, the wave function at later times is uniquely specified. Nagel concludes: "[q]uantum mechanics is therefore deterministic with respect to the quantum–mechanical definition of state" (Nagel, 1953, p. 434). This enables Nagel to argue against the idea that determinism is conclusively refuted in quantum mechanics: he argues that on the contrary, quantum mechanics is a fully deterministic theory (Nagel, 1945).[10]

However, this argument encountered criticism by Reichenbach. As mentioned before, in his reply to Nagel's review of his book, Reichenbach argued that Nagel had misunderstood the grounds for indeterminism in quantum mechanics. According to Reichenbach, the indeterminism of quantum mechanics is not due to Heisenberg's uncertainty relations, and also not to the mere fact that the laws of quantum mechanics are statistical. Rather, it is due to what is known as wave function collapse. From the wave function, one can derive probabilities for measuring the position of a particle at a certain location; however, the theory does not describe how measurement can yield a single unique value. In fact, von Neumann had argued that the quantum state undergoes two different kinds of changes: a deterministic evolution of the wave function according to the Schrödinger equation, and a collapse of the wave function during measurement. The theory of quantum mechanics gives no account for how this collapse takes place, and it seems to violate the causal principle of local action. Reichenbach argues that if the state of a quantum system is given by the wave

[10] The claim that quantum mechanics is deterministic because the wave function evolves deterministically is not unique to Nagel—it was already made by Planck in (1932). Also Earman (1986; 2007) makes this argument, and Earman (1986) in fact refers to Nagel in this connection. However, as also Earman admits, to argue that quantum mechanics is really deterministic on this basis is tricky, mainly because the issue of how to relate the theory of quantum mechanics to measurement outcomes remains unsolved.

function, wave function collapse represents a "causal anomaly" (Reichenbach, 1946, p. 241). He argues that if the wave function is merely regarded as an instrument for formulating probabilities for measurement outcomes, then the reduction of the wave function does not have to represent a causal anomaly, but if the wave function is taken to represent the state of the system, as Nagel proposes, then "this way out is closed" (Reichenbach, 1946, p. 242).

The exchange between Reichenbach and Nagel continued with a reply by Nagel, also published in *The Journal of Philosophy* (Nagel, 1946). However, here, Nagel admits that he does not fully understand Reichenbach's criticisms about the indeterminism in quantum mechanics being due to wave function collapse, and does not explicitly address the issue. In "The Causal Character of Modern Physical Theory" (1953), he addresses the meaning of the wave function in more detail, and notes that the wave function cannot be interpreted through a visualizable model and does not have a direct empirical significance. The physically significant consequences that can be derived from it are statistical in nature: from the wave function, one can merely derive probabilities for measurement outcomes for e.g. the position or the energy of an electron. Nagel argues for a statistical interpretation of the wave function, according to which the wave function does not describe individual systems but only ensembles of systems, and (falsely) seems to assume that this is the standard interpretation. He writes that it is "maintained by outstanding quantum theorists" that quantum theory is indeterministic because it is a statistical theory, but argues that this is not correct: the fact that the wave function, as a state description, merely yields probabilities for measurement outcomes does not change the deterministic structure of the theory itself.

For Nagel, the case seems not essentially different from that of statistical mechanics, in which the state of a system is characterized by statistical variables, such as the average velocity of atoms within a volume, but which can nevertheless count as a deterministic theory if the equations of the theory enable to derive the state of the system from an earlier (similarly statistically defined) state. However, in *The Structure of Science*, Nagel seems to argue that whereas the theoretical structure of quantum mechanics is deterministic, the fact that it is a statistical theory means that there is also a sense in which it can be regarded as indeterministic. He argues that it may indeed be that there is fundamental chance in the quantum domain; however, this cannot be conclusively established, and the possibility that a non-statistical theory of quantum mechanics will be developed in the future cannot be excluded (Nagel, 1961).[11] This would still mean that the challenge of quantum mechanics for determinism is not much stronger than that of statistical mechanics.

However, these arguments do not address the issue brought up by Reichenbach, about the collapse of the wave function during measurement. In fact, this 'measurement problem' counts today as the main open problem in the foundations of quantum mechanics. There are various interpretations of quantum mechanics on offer, which

[11] Nagel refers to von Neumann's theorem which excludes the possibility of hidden variables, but argues that this theorem does not exclude the possibility that a hidden variable theory can be developed on a fundamentally different basis.

present different ways to deal with the measurement problem, and which also offer different answers to the question whether quantum mechanics is in fact deterministic. Earman (2007) argues that the evolution of the wave function in quantum mechanics is indeed deterministic, but that the question whether quantum mechanics as a whole is deterministic cannot be answered before the measurement problem is solved and we can decide upon a single interpretation of quantum mechanics. One way to deal with the measurement problem in a way that is fully deterministic is offered by the approach which was developed in the 1920s by De Broglie, and the similar approach published by Bohm in 1952, now variously known as De Broglie-Bohm theory or Bohmian mechanics. This account offers a fully deterministic alternative to standard interpretations of quantum mechanics, of which Nagel had at least some awareness, since he refers to De Broglie's account of quantum mechanics in (1960, 300) and to Bohm's in (1961, p. 312). However, for Nagel, the significance of De Broglie's and Bohm's proposals was merely that they promised a non-statistical alternative to quantum mechanics, in which there is no limit to the accuracy of predictions.[12] Also in this context, Nagel does not mention the issue of measurement. But it should be noted here that the measurement problem seems not to have been the central issue for Bohm either, and was generally not as central to the debates on the foundations of quantum mechanics around the 1950s as it is today (Freire, 2015, pp. 142–55; Van Strien, 2020).

In light of this, it is not surprising that Nagel could not offer a satisfying reply to Reichenbach's objection that the true reason for concluding that quantum mechanics is indeterministic lies in the collapse of the wave function during measurement. If one is interested in what quantum mechanics tells us about whether quantum processes are in themselves deterministic or not, then this is definitely an issue which needs to be addressed, and Nagel's arguments remain unsatisfactory. However, it is important to note that Nagel was mainly interested in the structure of scientific *theories*. For Nagel, the ontological question of determinism, the question whether our world is deterministic or not, is fundamentally undecidable. It is only possible to ask whether a specific *theory* is deterministic, and this depends on its theoretical structure (Nagel, 1953). From this perspective, it makes sense to argue that quantum mechanics, as a theory, is deterministic, since the evolution of the state of a quantum system as described by the wave function is indeed deterministic.

However, one can still ask for the necessary conditions under which a measurement will yield a specific outcome, and how this measurement outcome comes about. The theory of quantum mechanics does not provide an answer to this question—and in this situation, the regulative principle of determinism should tell you to keep looking for these conditions. This would mean that quantum mechanics, as a theory, may be deterministic, but that more work remains to be done in relating this theory to measurements and measurement outcomes.

[12] As an anonymous referee points out, it does not hold in Bohmian mechanics either that there are no limits to the accuracy of predictions.

8.6 Determinism in the Social Sciences

If determinism is a heuristic principle which is closely connected to the general aims of science, and if "to abandon the deterministic principle itself is to withdraw from the enterprise of science", then how does this work for example in the social sciences, in which there are generally no strict laws determining what will happen in the future? One concern may be that determinism, as a general principle, is formulated in such an abstract and general way that it can offer no concrete guidance; another concern is that it would recommend a search for strict and universal laws, and that this ideal may not be realizable in the social sciences and is not necessarily the most fruitful methodology.

Nagel admits that in the social sciences, the ideal of fully deterministic theories is far out of reach.[13] As we have seen in Sect. 8.4, Nagel argues that at some point during the nineteenth century, it turned out that the special version of the deterministic principle known as Laplacian determinism was not the most fruitful one in all areas of physics, and other versions of determinism were developed, which used other variables than the positions and momenta of particles. In (1960), he writes:

> No comparably fruitful specializations of this assumption have been proposed in the psychological and social sciences—although in these areas of study particular forms of determinism have also led to important findings, for example, those which have directed attention to such determining factors as heredity, attitudes acquired by training, repetition of exposure to stimuli, modes of economic production or social stratification and social mobility. (Nagel, 1960, pp. 296–297)

In *The Structure of Science*, Nagel (1961, p. 605) similarly argues that "special versions of the general principle are employed to fruitful ends in the psychological and social sciences—for example, regulative principles which stipulate as the determinants of various phenomena such factors as heredity, conditioning by training, modes of economic production, or social stratification". Thus, determinism can be fruitful as a guiding principle in the social sciences when it is specified what kind of factors should be investigated as potential causes of social events or processes.

However, Nagel's claim that there can be specific forms of determinism which play a role, for example, in sociology does not necessarily mean that in sociology, one should strive after fully deterministic theories in which one can make exact quantitative predictions on the basis of universal laws. To develop a strictly deterministic theory of society as a whole, one would have to be able to define the state of society with absolute precision—this goal is very far out of reach. Nagel argues that laws or generalizations in sociology are always statistical, and this is primarily due to the fact that the concepts with which sociology operates are generally not precisely defined. Nagel thinks that the only way to establish laws which hold absolutely within social science would be through the development of "more discriminating classifications of social phenomena" (Nagel, 1961, p. 506). He gives the example of the notion of "authoritarian parental attitude"; as a "grossly oversimplified and fanciful example",

[13] For the details of Nagel's views on the social sciences, see Matthias Neuber's paper in this volume.

we might suppose that this notion could be given an exact definition through "the use of variables referring to details of bone structure of each parent, quantity of calcium deposit in their joints, differences in chemical composition of their blood, and variations in the spatial distribution of their nerve filaments", among others (Nagel, 1961, p. 507). In this way, Nagel argues, it might be possible to derive absolute laws in the domain of social science. The fact that Nagel argues that to arrive at absolute rather than statistical laws, one has to move to a lower level of description, is remarkable: it shows that, although higher level sciences have a certain degree of autonomy, they are not fully on a par with more fundamental sciences: only in the latter, one can hope to derive universal laws of nature.

However, Nagel argues that the absolute laws of nature that one might be able to derive by moving to a lower level of description would be as useless for the understanding of social phenomena as they would be cumbersome: within social science, focusing on developing exactly defined concepts and absolute laws would therefore probably not be a good strategy, and may actually hinder the understanding of social processes. He emphasizes that this does not mean that objective knowledge in sociology is not possible: we can still gain knowledge about important factors which play a role in specific social processes. That the ideal of a deterministic theory has not been reached, and is perhaps unreachable, in itself does not mean that sociology is not scientific. Nagel argues that even if theories in sociology are typically not deterministic, and even if striving after the establishment of absolute laws of nature is generally not a fruitful strategy in sociology, determinism still plays a role as a regulative ideal.

To understand how, according to Nagel, determinism functions as a regulative principle in social sciences, it is helpful to look at what Nagel *does* regard as unscientific. This becomes clear in his text "Determinism in History" (1960). Here, Nagel considers the doctrine of inevitability of historical developments. A characterization of the idea of historical inevitability can be found in *The Structure of Science*, where Nagel writes that this idea of historical inevitability takes different forms, but

> these various doctrines of historical inevitability share a common premise: the impotence of deliberate human action, whether individual or collective, to alter the course of human history, since historical changes are allegedly the products of deep-lying forces which conform to fixed, though perhaps not always known, patterns of development. (Nagel, 1961, p. 593)

In "Determinism in History", Nagel mentions that this idea of historical inevitability has been thought of as scientific because of its emphasize on laws and lawfulness, but has rightfully been criticized. In history there are no general laws, and exact prediction is generally not possible as there are always unforeseen events. Moreover, people who argue against the idea of historical inevitability generally argue that people are not passively subjected to their fate, but can act on the basis of free will. But although Nagel agrees that the idea of historical inevitability is untenable, he argues that the criticism can be taken too far:

> [A] growing number of thinkers has been claiming that what is really at the bottom of beliefs in historical inevitability is the very notion that human events generally occur only under determinate and determining conditions. Many writers have in consequence argued

> that a thoroughgoing determinism is incompatible with the established facts of history as well as with a genuinely significant imputation to human beings of responsibility for their choices and actions. In the eyes of many, furthermore, it is this deterministic assumption which is ultimately behind current attempts at extending behavioristic (or more generally, naturalistic) methods of inquiry into the study of human affairs; and the undeniable crudities which have sometimes accompanied the use of such methods, have been therefore cited as the unavoidable fruits of the deterministic assumption itself. (Nagel, 1960, p. 292)

According to Nagel, it goes too far to argue that historical events are purely contingent, and that it is futile to look for causal connections. He argues that people who argue against determinism in history on the basis of the fact that there are no broad and absolute laws of human development have an "extraordinary narrow conception of what a deterministic system must be like" (Nagel, 1960, p. 298)—they think of a deterministic system as comparable to the solar system, which is extremely predictable and stable even over long periods of time. Less regular systems, even within physics, may exhibit much more complicated causal connections. Nagel argues that it is wrong to argue, merely on the basis of the fact that a domain is complex, that there are no causal connections at all. And it is certainly not the case that it is impossible to make reliable predictions in the social sciences:

> As I write this line, we cannot predict with certainty who will be the next president of the United States. But if we take for granted current American attitudes toward domestic and foreign powers, and also take into account the present alignment of the world-powers, we do have good grounds for confidence that there will be a presidential election this year, that neither major political party will nominate a Communist sympathizer, and that the successful candidate will be neither a woman nor a negro. (Nagel, 1960, p. 300)

Also a historian or a sociologist should assume that events are somehow determined, and that it makes sense to look for the causes of events. If a historian states that something happened by chance, this generally means that s/he is unable to explain this event (Nagel, 1960). Nagel admits that "the dogmatic adoption of various special forms of the deterministic principle has often hindered the advance of knowledge" (Nagel, 1960, p. 317)—however, if a certain specific form of deterministic assumptions turns out to be unfruitful, this is no general argument against determinism.

To summarize: The mere fact that in the historical and social sciences, the ideal of quantitative prediction and deterministic theories has not been reached and is perhaps unreachable, does not mean that these sciences are unscientific. The fact that in sociology, there are no absolute but merely statistical laws, can be understood if one looks at the types of concepts and questions sociologists work with, and is probably unavoidable—this does not mean that sociology is unscientific. What is unscientific, however, is to argue that something takes place purely by chance, and to give up any attempt at looking into what circumstances might have brought about this event and thereby at explaining the event. Nagel argues that it is important to criticize arguments against determinism in history, for "were those criticisms mistakenly accepted as sound, there would be a strong likelihood that premature limits would be set on the possible scope of scientific inquiry" (Nagel, 1960, p. 316):

> However acute our awareness may be of the rich variety of human experience, and however great our concern over the dangers of using the fruits of science to obstruct the development of human individuality, it is not likely that our best interests would be served by stopping objective inquiry into the various conditions determining the existence of human traits and actions, and thus shutting the door to the progressive liberation from illusion that comes from the knowledge achieved by such inquiry (Nagel, 1961, p. 606)

It is in this sense that determinism plays a role as a guiding principle in history and in social science: also in these disciplines, the researcher should always assume that it is possible to find determining conditions for events, and to give up this assumption indeed means to "withdraw from the scientific enterprise".

8.7 Conclusion

We have seen that there are two main aspects of Nagel's conception of determinism: (1) a specific theory can be deterministic relative to a specific state description; (2) whether determinism holds in general can be neither verified nor falsified; rather, determinism plays the role of a guiding principle in science, which states that one should always look for determining conditions of occurrences. Determinism can thus either be a feature of a specific scientific theory, or a general heuristic principle in science. In different areas of science, there can be different state descriptions and different circumstances can qualify as determining conditions.

In this way, Nagel argues that determinism has an enduring relevance in all domains of science, including quantum physics. This conception of determinism is not connected to a mechanical reductionism like that of Laplace, and can play a role in different domains of science irrespective of whether they are reducible to a fundamental level of description. It differs from standard accounts of determinism because the acceptance of determinism as a guiding principle does not require that one has established a theory which is fully deterministic, that exact prediction is possible or even that there are absolute and general laws—indeed, determinism as a guiding principle is a very weak principle, and is merely a commitment to the idea that it is in principle possible to find the conditions under which an occurrence takes place, even if these conditions have not been found yet, and even if they may never be found. According to Nagel, we are never justified in assuming that something takes place purely by chance, because to establish that an event takes place by pure chance would require a demonstration that we can never develop a theory which can account

for this event.[14] This would mean to give up on any future attempt at explaining the event, and this, according to Nagel, is unscientific.

However weak Nagel's conception of determinism is, it does have certain advantages. It can explain, for example, the fact that debates about free will continue to be framed in terms of determinism, even though no deterministic theory of human behavior is available, and even though the idea that all natural processes are ultimately reducible to a fundamental level of physics which is deterministic has been overthrown a long time ago. It has not been proven that human behavior is fully determined by a certain set of genetic and environmental factors, and this will likely never be proven; nevertheless, the idea that there are determining conditions for decisions functions as a working assumption in studies of human behavior.

It is very likely that Nagel first developed his conception of determinism in the context of quantum mechanics and through his engagement with the writings of Cassirer and Reichenbach on determinism and causality in quantum mechanics. Already in his review of Reichenbach (1946), Nagel argued that determinism should be disconnected from mechanism and is always relative to a description of states of a system, which can be different in different disciplines. This conception of determinism entails that determinism can play a role in different domains of science, irrespective of whether they are reducible to a fundamental level of description.

A failure of determinism in quantum mechanics would not immediately undermine this conception of determinism: Nagel argues against the idea that if nature is indeterministic at the fundamental level, determinism cannot hold at any level (Nagel, 1953).[15] Moreover, generally, if a particular theory is not deterministic, this does not undermine determinism as a regulative principle—we can always continue our search for more detailed accounts. However, if quantum mechanics is *fundamentally* indeterministic, meaning that it can be demonstrated that no deterministic account of quantum phenomena is possible, this goes against Nagel's claim that determinism can be neither conclusively established nor conclusively refuted, and that we can never be justified in stating that anything takes place by pure chance. As we have seen, Nagel's claim that determinism is always relative to a description of states of a system enabled him to argue that quantum mechanics is in fact deterministic. However, we have seen that although this claim is essentially correct as a statement about the theoretical structure of quantum mechanics, it ignores the tricky question of how this theory relates to observations and measurements.

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[14] "[I]t is impossible in the nature of the case to establish beyond question that any event is an absolute chance occurrence. For to show beyond all possible doubt that a given happening (e.g., the decomposition of an atom) is spontaneous and without determining circumstances, it would be necessary to show that there is nothing whatever upon which its occurrence depends. But this would be tantamount to showing that no satisfactory theory could ever be devised to explain what present theories already explain, and in addition account for the allegedly spontaneous event" (Nagel 1961, pp. 332–333).

[15] Nagel (1953) argues that the idea that indeterminism in quantum mechanics would also undermine determinism for macroscopic phenomena rests on the unwarranted assumption that if a complex is analyzable in terms of elements, then the elements are metaphysically prior and the complex can only have properties that can be attributed to the elements.

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Part III
Issues in General Philosophy of Science

Chapter 9
On Nagel's Truth-Frequency Theory of Probability



Maria Carla Galavotti

Abstract Probability is the subject of considerable attention by Nagel, who devoted to it several writings. In the 1930s, Nagel put forward his "truth frequency" view of probability intended as a variant of the frequency theory minus the flaws to which this latter was susceptible. This paper illustrates (1) how, according to Nagel, the issue of the meaning of probability should be addressed; (2) Nagel's criticism of the major interpretations of probability advanced in the literature, and (3) the main traits of his "truth-frequency" theory.

Keywords Ernest Nagel · Theory of probability · Truth-frequency · Pragmatism

9.1 The Meaning of Probability

Nagel's view of probability is rooted in the conviction that probability does not have a univocal meaning independent of the context in which it occurs, and therefore cannot be given an umbrella definition. In "The Meaning of Probability" (1936), this viewpoint is spelled out in clear terms:

> [W]hether the term 'probability' has one meaning or many must be determined by a study of the *different contexts* in which it is employed. [...] People assert propositions like 'it is probable that it will snow tonight', 'the probability that he will survive the operation is 3/5', 'the photon has a probability of 3/4 of being reflected', 'the helio-centric theory is more probable than the geo-centric one', and so on. Now we would discover what is meant by these propositions and consequently by 'probability', if we could ascertain rules controlling the assertion of each, and therefore the usage of the term. [...] [I]f we can obtain an answer to the question 'what sort of *evidence* would be regarded as relevant for propositions asserting something about probabilities?' we would obtain at the same time an answer to the question 'what is meant by probability?' (Nagel, 1936a, p. 11, emphases added)

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This passage points to the main ingredients making up Nagel's view of probability, namely the context in which probability is used, and the evidence supporting probability attributions.

If probability "has different meanings in different contexts" (1936a, p. 26), fixing its meaning requires first paying attention to the context in which it occurs. The context-dependence of probability is the price Nagel is ready to pay to safeguard its applicability. Importantly, Nagel regards probability and statistics as strictly connected, and having a broad field of application ranging across the natural and social sciences, as well as everyday life. In this respect—as in others to be highlighted later—he is a follower of Peirce. In short, probability for Nagel represents a measure of the success of our predictions. It is therefore mandatory that those who evaluate probability do not lose sight of scientific practice, including the conditions and assumptions under which experiments are performed, and the kind and amount of evidence taken into account. It is these considerations that place the assessment of probability within a context, since "the amount and kind of evidence required for probability statements depend on their interconnections with the body of our knowledge and theories at a given time" (Nagel, 1939a, p. 9).

With these beliefs in mind, Nagel examines in detail the debate on the foundations of probability, coming to the conclusion that "the frequency interpretation, when properly qualified, seems the most satisfactory one for analyzing the meaning of the term in the context of everyday discourse, applied statistics and measurement, and within many branches of the theoretical sciences" (1936a, p. 26). In view of claims like this, Nagel is often associated with the frequency interpretation, while his was a truth-frequency theory, which he meant as a variant of the frequency view embracing a wider field of application and able to avoid some of the difficulties affecting frequentism. Before looking at Nagel's truth-frequency theory in some detail it is worthwhile to recall his criticism of the main interpretations of probability advanced in the literature.

9.2 Nagel's Criticism of Other Interpretations of Probability

Right in the beginning of *Principles of the Theory of Probability*, the monograph published in 1939 as part of the "International Encyclopedia of Unified Science", Nagel praises Peirce for having emphasized the importance of probability and the role it plays in both science and everyday life. In Nagel's words: "The American logician once remarked that in the exact sciences of measurement, such as astronomy, no self-respecting scientist will now state his conclusion without their coefficient of probable error. He added that, if this practice is not followed in other disciplines, it is because the probable errors in them are too great to be calculated" (1939a, pp. 1–2).[1] Like Peirce, Nagel regards complete certainty as an unattainable ideal, and deems

[1] Peirce's passage to which Nagel refers is CP 1.9, see Peirce (1931–1958).

the ability of scientists to estimate probabilities and errors of measurement, whatever their field, "a sign of maturity and not of defect" (1939a, p. 2).

Nagel groups the major interpretations of probability into three categories: classical, logical and frequentist. The classical theory is described as the approach holding that "a degree of probability measures our subjective expectation or strength of belief, and the calculus of probability is a branch of combinatorial analysis" (1939a, 18). The chief representative of the classical viewpoint is identified with Laplace, whose perspective according to Nagel has been followed by "almost all writers on probability in the nineteenth century (e.g., Poisson, Quetelet, De Morgan, Boole, Stumpf), and many contemporary mathematicians (e.g., Borel, De Finetti, Cantelli, Castelnuovo)" (1939a, p. 45). The fact that Nagel qualifies as followers of the classical outlook certain authors who were in fact critical of it, such as the logicists Boole and De Morgan, or the subjectivists Borel and de Finetti, is matter of perplexity.[2] That said, Nagel ought be praised for having drawn attention to the work of a number of important authors, like Bruno de Finetti, at the time almost unknown to the English speaking world and introduced to English speaking scholars only in the fifties after Savage's work.[3]

In connection with the classical theory, Nagel first recalls Laplace's rule for measuring probability as the ratio of the number of favorable alternatives to the number of all possible alternatives (provided these alternatives are judged equally possible). To this he objects that not all problems allow identification of all the alternatives. Moreover, he deems Laplace's definition circular because it assumes equally possible alternatives to be equally probable. Nagel also objects to the fact that Bernoulli's theorem is taken to provide a connection between probability, understood according to the classical definition, and frequency. In his words:

> There is in fact no *logical* relation between *the number of alternative ways* in which a coin can fall and *the frequency* with which these alternatives in fact occur. [...] [T]he supposition that Bernoulli's theorem establishes a relation between a priori (i.e., determined in accordance with the classical definition) and a posteriori probabilities (i.e., determined on the basis of relative frequencies of occurrence) is a serious error" (Nagel, 1939a, pp. 47–48, original emphases)

Nagel's view of subjectivists is that "the proposals of Ramsey and De Finetti, to measure strength of beliefs by the relative size of the bets a man is willing to place, are based on a dubious psychological theory" (1939a, p. 46). De Finetti's interpretation of probability is described by Nagel (1937, p. 54) as holding that "the degree of probability of a proposition is the same for all normal individuals because

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[2] See Galavotti (2005) for a historical-philosophical survey of the major interpretations of probability advanced in the literature.

[3] The 'selected bibliography' included in *Principles of the Theory of Probability* mentions de Finetti (1937). In addition, in volume 2 of *The Journal of Symbolic Logic* (1937) Nagel published short reviews of the papers by Reichenbach, de Finetti, Zawirski and Hosiasson delivered at the First Congress for the Unity of Science (Congrès international de philosophie scientifique) which took place in 1935 in Paris, and were subsequently published in volume IV of *Actes du Congrès International de Philosophie Scientifique (Sorbonne, Paris 1935)*, in *Actualités Scientifiques et Industrielles*, 1936, n. 391.

of a 'spontaneous' agreement between them". Moreover, he objects to the subjectivist approach that if probability is only subject to subjective evaluations and there is no objective connection between a probability statement and the evidence on which it is based, there can be no sound method for comparing probability evaluations. In Nagel's eyes, subjectivism has dire consequences for science, because those who embrace the idea that probability is the expression of personal convictions "must also maintain that the acceptance and rejection of various propositions in the sciences are controlled wholly by non-rational considerations and attitudes. From this point of view, the undeniable predictive power of modern science can be understood only as a happy accident, essentially unrelated to current fashions in weighting evidence" (Nagel, 1945, p. 498).

Nagel's image of subjectivism does not, however, do justice to the attitude of its supporters, who maintain that subjective probability is not just the expression of the hunches of the evaluators, but the result of a complex procedure deriving from a combination of a subjective component, consisting of the evaluator's experience and expertise, and an objective component, consisting of the evidence available. Even for the uncompromising subjectivist de Finetti (1974, p. 16), "subjectivism is one's degree of belief in an outcome, based on an evaluation making the best use of all the information available to him and his own skill. […] Subjectivists […] believe that every evaluation of probability is based on available information, including objective data" (1974, p. 16; see also Dawid & Galavotti, 2009). It should, however, be kept in mind that Nagel's misunderstanding regarding subjective probability dates back to a period prior to the publication in 1954 of Savage's *The Foundations of Statistics*, which put de Finetti's work at the forefront of statistical literature. In any event, Nagel's subsequent writings do not provide evidence that he may have changed his mind, even if the last chapter of *The Structure of Science* titled "Problems of the logic of historical inquiry", contains a timid concession, when in connection with subjectivism, he maintains that "[m]ore recently […] this interpretation has been revived in improved versions, and under the label of 'personalistic probability' it plays a prominent role in current developments of statistical decision theory" (Nagel, 1961, p. 561).

The logical interpretation is portrayed by Nagel (1939a, p. 48) as holding that "probability is an objective logical relation between propositions analogous to the relation of deducibility or entailment", while the probability relation, unlike logical implication, comes in degrees. Nagel identifies the forerunners of this view in Leibniz and Bolzano, and claims that it "is central to von Kries, Keynes, J. Nicod, F. Waismann, and several other writers" (ibid.). While in *Principles of the Theory of Probability* and other writings of the 1930s Nagel devoted special attention to Keynes' version of logicism, elsewhere he directed his attention also to Carnap and Jeffreys.

As reported by Nagel, for Keynes

> probability is a unique, unanalyzable relation between two propositions. No proposition as such is probable; it has a degree of probability only with respect to specified evidence. This relation of probability is not a degree of subjective expectation; on the contrary, it is only

> when we have perceived this relation between evidence and conclusion that we can attach some degree of 'rational belief' to the latter. (Nagel 1939a, p. 48)[4]

Nagel welcomes the fact that Keynes, unlike subjectivists, endows probability with an objective character and holds that, if taken as a degree of belief, probability must be seen as a degree of *rational* belief. A major point of disagreement between the two is Keynes' idea that probability is apprehended by intuition, to which Nagel (1939a, p. 49) objects that "few if any students will be found who claim for themselves such an intuitive power […] Moreover, the possession or lack of this power is wholly irrelevant in the actual estimation of probabilities by the various sciences." In addition, Nagel extends to Keynes' logicism the objection raised to the classical interpretation, charged of being unable to "establish any connection between numerical probabilities and relative frequencies of occurrence" (1939a, p. 50). In conclusion, Keynes' conception of probability is deemed too abstract, therefore incapable of tackling the problems discussed in physics and statistics, and is likened to "a vestal virgin" rather than to a theory useful for scientific purposes. For Nagel, in other words, the mistake of the logical interpretation derives from building probability theory a priori, hence losing contact with practice. To sum up: "a conception of probability according to which we cannot in principle control by experiment and observation the probability statements we make is not a conception which recommends itself as germane to scientific inquiry" (1939a, p. 50).

A similar attitude inspires Nagel's criticism of Carnap's view of probability, which is discussed at length in "Carnap's theory of induction", published in *The Philosophy of Rudolf Carnap* edited by P.A. Schilpp (1963). Prior to it, the 1946 volume of *Journal of Symbolic Logic* contains Nagel's review of two papers by Carnap, both published in 1945, namely "Two concepts of Probability" and "On Inductive Logic". Without going into the technicalities of Nagel's detailed criticism, a major reason for dissent lies with the fact that, in his eyes, Carnap's inductive logic is far removed from scientific practice because he did not pay enough attention "to what he calls the 'methodological' problems that are generated when the applicability of the logic to actual scientific procedure is considered" (Nagel, 1963, p. 786).[5] A further point of disagreement concerns Carnap's attempt at justifying induction along the lines suggested by Reichenbach, namely by appealing to the success of the inductive method, put forward in "On Inductive Logic". According to Nagel, Carnap misses the point, because he does not produce an argument showing that the inductive rules assumed by inductive logic lead to successful predictions. In Nagel's words, Carnap

> claims that his inductive rules for predictive inference yield values for c* which approximate to relative frequencies in the long run. [...] To be sure, there may be a synthetic, empirical connection between the value of c* for the outcome of the next drawing from one bag, and the relative frequency of successes in a series of drawings from a large class of bags. But no reasons are supplied in the present paper for the supposition that such a connection does in fact exist. [...] Accordingly, the major part of the problem of justifying inductive rules is left entirely unresolved (Nagel, 1946, p. 23).

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[4] For a critical survey of Keynes' version of logicism see Gillies (2000).

[5] On Nagel's criticism of Carnap, and their basic difference, see Thomas Mormann's paper in the present volume.

It is worth mentioning that in the 1960s, Carnap abandoned the pragmatic approach to the problem of justifying induction and turned to the notion of "inductive intuition" (Carnap, 1968). Contextually, he moved away from his 1950s approach of grounding the choice of one particular inductive method on success. In "Replies and Systematic Expositions", appearing in the Schilpp volume, Carnap (1963, p. 978) holds that the reasons for accepting the axioms of inductive logic "are based upon our intuitive judgments concerning inductive validity, i.e., concerning inductive rationality of practical decisions (e.g.) about bets." Carnap's refusal to take success both as a criterion for preferring one inductive method over another and as a canon for the justification of induction, is a source of disagreement with Nagel, who maintains that "[i]ndividual inductive policies can be justified […] in terms of the de facto success-ratios that are associated with the inductive rules underlying those policies" (Nagel, 1963, p. 824).

Nagel takes a slightly more sympathetic attitude towards the British geophysicist and statistician Harold Jeffreys. In a review of his *Theory of Probability*, Nagel praises Jeffreys'

> concrete sense for the realities of scientific practice [that] is revealed in many illuminating remarks. [...] [H]e finds little nourishment in any of principles frequently offered as the foundations for induction, such as the principle of the uniformity of nature or the principle of causality. Thus, he explains that observations are never in exact agreement with the predictions of causal laws. But this fact is not taken by him to denote the 'breakdown' of causality; it is taken to show that a law is satisfactory if the predicted variations account for the greater part of the changes which actually occur, provided that the law is accompanied by a statement of the margin of 'uncertainty' within which its predictions hold. No reader of recent literature on the import of modern physics can fail to appreciate the incalculable quantity of nonsense which these remarks exclude (Nagel, 1940a, p. 525).

Nagel's appreciation for Jeffreys' pragmatic and genuinely probabilistic view of scientific knowledge does not, however, prevent him from criticizing some of its aspects, first of all Jeffreys' commitment to the logical interpretation of probability in open contrast with frequentism. Jeffreys takes probability as a primitive notion expressing a degree of confidence in the occurrence of an event on the basis of a given body of evidence; then he assumes by way of an axiom that probabilities can be linearly ordered according to the relation of being more, equally, or less probable, deriving the basic properties of probability functions from that assumption. Nagel objects that by taking "as primitive the relation of one proposition being more probable than another on certain data" Jeffreys cannot explain disagreement between different evaluations of probability. "His strongest argument", Nagel argues,

> is that people differ because their estimates are wrong, just as in pure mathematics people may differ because an error is committed in the calculation. This is, however, a poor analogy and is therefore a lame defense. For if there is disagreement in arithmetic on the value of a sum, people can discover their error by a careful application of the explicit rules governing the calculation. There is no possibility of doing this in cases of disagreement over degrees of reasonable belief; for if there were, 'probability' in Dr. Jeffreys' sense would not be a primitive notion (Nagel, 1940, p. 526).

In addition, Nagel observes that Jeffreys "fails to specify just how a particular numerical magnitude is to be assigned to a given degree of probability" (ibid.). A

further disagreement regards Jeffreys' postulate of simplicity—according to which simpler laws are assigned a greater prior probability—and more in general his tenet that the principles of probability are a priori propositions that are accepted independently of experience, to which Nagel objects that "they must be 'truths' comparable with those of a platonizing logic. But whatever be the merits of such a position, does it provide a foundation for induction which is consistent with the sensationalistic empiricism Dr. Jeffreys elsewhere professes?" (1940, p. 527).[6]

The primary motivation underlying Nagel's criticism of logical, classical, and subjective interpretations lies with his conviction that none of them can accommodate scientific practice. By contrast, he regards the frequency interpretation as the most suitable for this purpose, albeit leaving some problems open. Nagel (1939a, p. 18) describes frequentism as the view according to which "a degree of probability is the measure of the relative frequency with which a property occurs in a specified class of elements." He then adds that "this view already appears in Aristotle, was proposed by Bolzano and Cournot during the last century and further developed by Ellis, Venn, and Peirce, and was finally made the basis for a suitable mathematical treatment of the subject by von Mises and other contemporary writers" (1939a, pp. 18–19).[7]

In a number of writings, Nagel discusses in detail and compares the versions of frequentism developed by von Mises and Reichenbach. According to Richard von Mises, probability is defined relative to *collectives*, namely infinite random series of trials, or observations. If one wanted to find the probability of a certain attribute in a collective, one would have to determine the frequency with which such an attribute occurs in a sample of n elements. Supposing that the frequency of such an attribute relative to the given sample is m/n, its probability in the sequence is put equal to the limit to which the relative frequency m/n tends when $n \to \infty$. Nagel holds that "the definition of 'probability' as 'the limit of relative frequency' is suggested by common practice in assigning probability coefficients. […] has the requisite precision, and a logically consistent calculus can be developed on such a basis" (1939a, p. 51). In the face of these advantages, a definition in terms of limiting frequencies raises obvious problems of applicability, on the objection that probability statements so defined are not subject to confirmation or disconfirmation, given that only limited evidence can be obtained by observation. Although aware of this, von Mises regarded the limit assumption both necessary in order to define probability unambiguously and justified by the observation of the very many cases occurring in science and everyday life. In games of chance, for instance, "the hypothesis of the existence of limiting values of the relative frequencies is well corroborated by a large mass of

[6] Jeffreys' probabilistic epistemology is discussed in Galavotti (2003).

[7] In the same vein, in (1936a, p. 15) Nagel holds that the frequency view of probability is "implicit in Aristotle", without further clarification. The interested reader can find the thesis argued in detail by Franklin (2001/2015). The Czech logician Bernard Bolzano is actually closer to logicism, and his notion of probability as a "degree of validity" (Grad der Gültigkeit) can be seen as an ancestor of Carnap's "partial implication" (see Galavotti 2005, pp. 235–236).

experience" (von Mises, 1939/1957, p. 16).[8] It must be added that according to von Mises, probability only applies to mass phenomena, or indefinitely repeatable events, and physics represents its privileged field of application.

The impossibility of applying probability to single occurrences of phenomena is at the origin of the so-called 'single case' problem affecting the frequency interpretation of probability. An attempt to overcome this was made by Hans Reichenbach, who proposed a more flexible version of frequentism, meant for a wider field of application including the social sciences.[9] For Nagel (1936b, p. 504) "Prof. Reichenbach's definition has […] an undoubted practical advantage" over von Mises' theory, deemed too abstract and far removed from practical applications. Among other things, Nagel values the fact that Reichenbach devoted great attention to the inductive method and its justification. In that connection, he claims that Reichenbach's "discussion of induction as a methodological postulate whose use does not 'presuppose' a priori knowledge of the world seems to me enormously clarifying and in the main correct" (1936b, p. 514).

Reichenbach was deeply influenced by Peirce's pragmatism, to the point of calling his own perspective, as expounded in *Experience and Prediction*, a form of pragmatism. Appreciation for Peirce's view of induction as a self-corrective procedure marks a point of agreement between Reichenbach and Nagel, who praises Reichenbach's assertion that "the sciences employ a self-corrective method, and […] finality is not to be found in any statement certified on the basis of that method" (1936b, p. 514).[10] Confidence in the self-corrective character of the inductive method inspires the pragmatic attitude to its justification in terms of success shared by both authors in the wake of Peirce. The justification of induction coincides with the justification of the scientific method itself, which can be 'vindicated' as "the method which, in short, leads to a proportionately greater number of successful terminations of inquiry than the number which other methods may have to their credit" (Nagel, 1939a, p. 73).

Convinced that the impossibility of referring probability to the single case, as embedded in von Mises' frequentism, is in contrast with practice, Nagel analyzes in detail Reichenbach's attempt at solving the problem. This revolves around the notion of *weight*, the idea being that a probability attribution concerning a single occurrence of some event receives a weight from the probabilities attached to the reference class to which that event belongs, provided that the reference class is homogeneous, namely it includes as many cases as possible similar to the one considered while excluding dissimilar ones. Nagel (1939b, p. 217) finds Reichenbach's account unsatisfactory because "the weight of a proposition cannot be definitely established by any finite number of observations." Furthermore, Nagel criticizes the method of 'concatenated inductions', regarded by Reichenbach as suitable for prediction and

[8] Von Mises' version of frequentism raises similar problems of applicability in connection with the requirement of randomness imposed on collectives, which was shown to be too strong to be satisfied in practice. See Galavotti (2021) for more on von Mises' conception of probability.

[9] For more on Reichenbach see Galavotti (2011) and Glymour and Eberhardt (2008/2016).

[10] In (1940b) Nagel makes some insightful comments on Peirce's view of the method of science as self-corrective, wholeheartedly shared by him.

the assessment of hypotheses and at the very core of scientific method. The induction procedure starts with the so-called 'blind posits'—or 'primitive' evaluations, made on the basis of relative frequencies alone—and goes on to formulate 'appraised' posits—namely evaluations based on information about known probabilities—and gives rise to a complex, hierarchical system of higher order probabilities. Nagel (1939a, p. 65) judges the concatenated induction method clumsy and overly complicated: "Reichenbach's proposal of a hierarchy of probabilities, according to which the probability of a probability statement may be estimated, postpones this problem by referring it to higher level probabilities; but postponing a problem does not solve it."

All in all, Nagel thinks that despite his efforts, Reichenbach does not accomplish the task of developing a version of frequentism able to match scientific practice, and observes: "I would not like to be understood as minimizing the great importance and ingenuity of Professor Reichenbach's contributions to the calculus of probability; but I fail to see why a problem should be considered solved if the proposed solution involves difficulties of increasing complexity and obscurity" (Nagel, 1939b, p. 224).

Nagel is particularly critical of Reichenbach's tenet that his method applies to the confirmation of theories, and holds that he unduly conflates probability as frequency with degree of confirmation, namely "the thoroughness with which a proposition has been tested" (Nagel, 1939b, p. 229). In that connection, he regards Reichenbach's view as completely missing the point of what goes on within scientific practice. In his words: "I do not persuade myself that Professor Reichenbach's interpretation of 'the probability of a theory' formulates what physicists do with it or even think about it for the most part" (1939b, p. 230). In Nagel's eyes, Reichenbach's mistake consists in overlooking the fact that when evaluating hypotheses scientists make use of "numerical measures *obtained under specified conditions of experiment*' (1939b, p. 230, original emphasis), a mistake that can be avoided by defining probability relative to the context in which it occurs, along the lines of the truth-frequency theory.

9.3 The Truth-Frequency Interpretation

Nagel's *truth-frequency* interpretation is spelled out in an article published in 1933 titled "A Frequency Theory of Probability" and taken up in the volume *An Introduction to Logic and Scientific Method*, written with Morris Cohen (1934), and "The Meaning of Probability" (1936). The cornerstone of his interpretation is the notion, borrowed from Peirce, of *leading principle* defined as "a proposition formulating a class of inferences. It denotes a relation whose domain is a class of propositions p and whose converse domain is a class of propositions q such that q is inferrable from

p in virtue of this principle" (Nagel, 1933a, p. 537).[11] In a review of the first two volumes of Peirce's *Collected Papers*, which he published in 1933, Nagel praises Peirce for introducing leading principles, allowing for a "classification of arguments with respect to the form of their constituent propositions and the frequency with which true premises of a determinate type yield true conclusions. Each argument requires a habit of inference, which when formulated is the leading principle or canon of that type of argument" (Nagel 1933b, p. 378). Nagel also emphasizes that "the consequences of taking leading principles or generalized habits as the basis for valid inference are many", in particular

> it enables Peirce to supply an objective, matter-of-fact foundation for all inference [...] It also enables him to treat in a unified manner both necessary and probable reasoning, since the former is a limiting case of the latter when the leading principles employed have relative truth-frequencies whose values are always unity. (Nagel 1933b, p. 378)[12]

The notion of leading principle is the bedrock of Nagel's truth-frequency theory, according to which probability is defined with reference to an inference from one set of propositions to another, and denotes the relative frequency of the effectiveness of such an inference. In *An Introduction to Logic and Scientific Method* the distinctive traits of the theory are described as follows:

> [I]nstead of talking about *events*, such as 'heads', the truth-frequency theory discusses the proposition *This coin will fall head uppermost on the next throw*. Instead of talking about a *class of events*, this theory discusses a *class of inferences*. For it is very clear that the relative frequency with which *This coin falls head uppermost on toss X* is true, or when *This coin is thrown under specific conditions on toss X* is true, must be the same as the relative frequency with which the event 'heads' occurs in a series of tosses with the coin (Cohen & Nagel, 1934/1936, p. 171, original emphases).

By grounding probability relations on leading principles Nagel seeks to endow them with an objective basis rooted in empirical evidence. In Nagel's words: "an objective ground is supplied for asserting one conclusion as more 'correct' than another, on the basis of evidence considered. A proposition is probable, not because we may have an impulse to believe it, but because as a matter of fact the leading principle involved does lead to true conclusions a certain proportion of times" (Nagel, 1933a, p. 541). This marks a gap between the truth-frequency theory, on the one hand, and both logicism and subjectivism, on the other. In fact, the truth-frequency interpretation also regards probability as a relation holding between propositions, but "not between a proposition and the state of mind of some inquirer" (ibid.) as held by subjectivists; nor does it depend on intuition, as maintained by those who embrace the logical viewpoint.

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[11] The notion of 'leading principle' is discussed in several parts of Peirce's *Collected Papers*. Writing in 1933, Nagel is likely to refer to the discussion contained in Volume 2, which was published in 1932—see (among other passages) CP 2.588–589.

[12] In *The Structure of Science* Nagel (1961) discusses at length the notion of 'leading principle' and its role in connection with theory formation, distinguishing between 'material' and 'formal' leading principles, a distinction also made by Peirce. The present account focuses exclusively on the role of leading principles within Nagel's truth-frequency theory of probability.

According to the truth-frequency approach, probable inference has three components: (1) an observed fact; (2) a general rule (leading principle); and (3) the inferred proposition. Here are a couple of examples. The first goes as follows:

> Various substances like oxygen, copper, chlorine, sulphur, hydrogen, when they combine chemically do so according to fixed ratios of their respective weights; and when the same amount of one substance, say chlorine, combines with different amounts of another substance, say oxygen, to form different compounds, it does so in ratios that are all small integral multiples of one. (This is the *observed event*). We know also that if each of these substances were composed of similar atoms, or mechanically indivisible particles, the substance would combine in such integral ratios of their weights. (This is the *general rule*). We conclude, then, that these substances are composed of atoms. (This is the *inferred fact*) (Cohen & Nagel, 1934/1936, pp. 15–16, original emphases).

This inference is taken as having a high degree of probability because it is based on the general proposition (leading principle) that "matter has an atomic structure", which is backed by strong evidence and supports consequences repeatedly found to be true by observation and experiment.

Another example is taken from everyday life and features the kind of situation everyone has experienced. One day an instructor is found by his pupils to be irritable. Knowing that headaches are accompanied by irritability, one can conclude that the instructor has a headache. The evidence for such a conclusion

> consists of a proposition asserting the existence of a particular state of affairs (the instructor is irritable) and of another proposition asserting a rule or principle which may be formulated either as *All headaches are accompanied by irritability*, or *Many cases of irritability are due to headaches*. In neither case does our conclusion *The instructor had a headache* follow necessarily. His irritability may in fact be due to some other cause. But our inference is of the type that will lead to a true conclusion in a large number of cases, according to the extent to which irritability is connected with headaches. And we test the truth of the latter generalization (or induction) by deducing its consequences and seeing whether they hold in new situations" (Cohen & Nagel, 1934/1936, p. 15, original emphasis).

As these examples suggest, the possibility to test the conclusions of a probabilistic inference, that is, the predictions made on its basis, is regarded by Nagel as crucial.

In "A Frequency Theory of Probability", Nagel (1933a, p. 541) emphasizes that at the core of probability lies a mixture of logical and empirical elements: "questions of probability, like questions of relevance, always involve material or factual, in addition to purely logical, considerations." Elsewhere in the same paper he declares he is a follower of Peirce's idea that there is a "connection between formal principles of inference, and the existential traits which are explored by their help […] Consequently, the 'truth-frequency' theory of probability can not be only a logical theory. If its interpretation of the nature of probability is sound, the effectiveness of probable arguments should be grounded in certain generic traits of existence. The theory has metaphysical bearings as well as a logical function. It points to the existence of a certain type of natural structure and contributes toward the understanding of the objective nature of universals" (1933a, p. 551). In other words, the self-correcting character of the theory is made to rest on the physical world: "If mathematics can be employed successfully, it is because the world *does* possess a mathematical character" (ibid.). The attitude conveyed by these claims might be seen as bringing Nagel

close to the propensity theory of probability, whose paternity is usually ascribed to Peirce, and was later embraced by Popper. However, Nagel's belief that probability has no univocal meaning marks a notable difference with Popper's position (see Gillies (2000) for more on the propensity theory of probability).

Compared with the 'material' versions of frequentism developed by von Mises and Reichenbach, the truth-frequency theory has, in Nagel's eyes, the advantage of being suitable for a wider range of applications, for it "can take over the entire mathematical apparatus and method […] without committing itself to the narrow field of application of either" (1933a, p. 544). In particular, Nagel maintains that "[t]he great merit of the truth-frequency lies (1) in the success with which it can interpret definite numerical probabilities as well as indeterminate ones, and (2) in its ability to give an objective reading to the probability of propositions dealing with singular events" (Cohen & Nagel, 1934/1936, p. 171).

Let us comment on these claims. In connection with point (1), Nagel is ready to admit that in some situations, probability does not take a precise numerical value: "for many purposes we do not require an actual numerical value for the probability, since in some contexts an indeterminate value which is greater (or less) than ½ is sufficient, and although we may lack adequate statistical material to support such statements, we may nevertheless possess considerable general experience to corroborate them" (Nagel, 1936a, pp. 23–24). The flexible attitude taken in this regard is clearly in tune with Nagel's conviction that probability does not have a univocal meaning, independent of the context in which it occurs. Turning to point (2), namely the capacity to deal with single case probability attributions, for Nagel what matters is the probative force of the evidence available, together with the surrounding conditions and underlying assumptions: "The probability of actual singular events is evaluated by the truth-frequency theory in terms of the *kind of evidence* which is supplied for each" (Cohen & Nagel, 1934/1936, p. 171). More on evidence will be added in what follows.

Similar considerations apply to the probability of theories, which should depend on the whole body of information possessed by the scientific community. In "A Frequency Theory of Probability" Nagel maintains that according to the truth-frequency view "the probability that a theory is true is capable theoretically of the same sort of determination in terms of the frequencies associated with a leading principle, as is the probability of getting heads with a coin. The arguments leading to the discovery of the theory, the nature of the verifications to which it is submitted, are special instances of comprehensive leading principles which have had a certain frequency of successes" (Nagel, 1933, p. 545). In subsequent writings, however, Nagel takes a more cautious attitude, aware of the difficulties undermining the quantitative confirmation of scientific theories. In that spirit, he holds that the probability of theories does not need to be expressed by a numerical value, and admits that other elements, such as analogy, enter into the evaluation, which usually involves also a certain amount of convention. In fact, it can be said that over the years Nagel developed a skeptical attitude, described thus by Patrick Suppes (1999, p. 217): "he doubted that an actual working scheme for the general quantitative confirmation of scientific theories could be developed."

All in all, the evaluation of probability depends ultimately on evidence. In Nagel's words: "Just as it is meaningless to speak of a body at rest or in motion except in reference to some other body, so it is meaningless to speak of the probability of an event occurring, or a set of propositions being true, except relative to certain evidence or material assumptions" (Cohen & Nagel, 1934/1936, pp. 171–172). Furthermore, "although our beliefs cannot be established with absolute finality, we do, as we must, differentiate between them on the ground of the character of the evidence which supports them" (Nagel, 1939a, p. 16). Now, evidence supporting probability statements should be both relevant and reliable. In *Principles of the Theory of Probability*, Nagel calls attention to the need for rules apt to assess the degree to which a given body of evidence can be taken as sufficient and reliable in a certain context, and complains the lack of satisfactory treatments of the issue. After maintaining that "there is clearly an obvious need for canons to evaluate the evidence supporting any proposition, and for the formulation of the principles we employ in deciding that one statement is better grounded than another", he adds that "judging by the success of past attempts to supply them, it may be suspected that every proposed list of such canons and formulations will be incomplete and will require emendation with the progress of inquiry" (1939a, p. 16).

By contrast, elsewhere Nagel suggests that for practical purposes quantitative values are not really needed, because "For most conceivable purposes, a knowledge that one statement is more probable with respect to one evidential class than to another is all that we require" (Nagel, 1945, p. 502). The topic is resumed in *The Structure of Science*, where in the chapter on "Methodological problems of the social sciences", Nagel discusses the statistical methodology for testing hypotheses, taking a critical attitude observing that "statistical theory appears to support the thesis that value commitments enter decisively into the rules for assessing evidence for statistical hypotheses" (Nagel, 1961, p. 497).[13]

Nor is Nagel's attitude to Bayesian methodology any more favorable. In *Principles of the Theory of Probability*, he asserts that "[i]n most problems it is not practically or theoretically possible to assign values to the antecedent probabilities in Bayes's theorem which could have any empirical significance. For this reason, Bayes's theorem has only a limited use, and few writers today take it seriously as a means for determining the probability of a given hypothesis on the basis of a given evidence" (Nagel 1939a, p. 31). A few pages later, again referring to Bayes' rule, Nagel remarks that "Critical statisticians no longer make use of it" (1939a, p. 36). These claims sound quite surprising, given that Nagel knew the work of several outstanding probabilists and statisticians who were admirers and utilizers of the Bayesian method, including von Mises, de Finetti and Jeffreys, not to mention philosophers of science like Reichenbach and Carnap.

All things considered, Nagel's truth-frequency theory does not seem to keep its promises, namely, to perform better than all other approaches in connection with

[13] Footnote 39 of (Nagel 1961, p. 497) makes reference to Chapter 5 of Neymann (1950). Noteworthy, the bibliography included in *Principles of the Theory of Probability* mentions Neymann (1938).

evaluating single case probabilities and general hypotheses. Nagel offers very few examples to show the advantages of the truth-frequency theory. The examples he gives—like those mentioned above—make use of leading principles which are either very broad scientific laws, or 'common sense' generalizations, apt to support some conclusion with practical certainty or with an indeterminate probability.

9.4 Concluding Remarks

Nagel's truth-frequency theory of probability has not had much success, either with philosophers of science, or with probabilists. After proposing it in the 1930s, Nagel himself does not seem to have patronized it with great conviction in his subsequent writings. In *The Structure of Science*, the truth-frequency theory is mentioned only in the chapter dealing with "Problems in the logic of historical inquiry", where he defends the view that historical facts can be critically analyzed and subject to evidence-based explanations akin to those belonging to scientific disciplines. Having observed that historical explanations are probabilistic in kind, Nagel tries to clarify how probability in this context should be understood. The subjective interpretation of probability is discarded because it "is compatible with the assignment of different degrees of probability by different individuals to the same hypothesis relative to the same evidence" which makes it "not a completely 'objective' relational property of statements" (Nagel, 1961, p. 561). A somewhat more positive attitude is taken towards the 'personalistic' interpretation, described as holding that the probability of a hypothesis *h* is given by the odds that an individual "would be willing to accept were he to bet on the truth of *h* and against its falsity, so that the probability will measure the risk the individual is prepared to take in adopting *h* and rejecting its denial" (ibid.).[14] Subsequently, Nagel mentions the truth-frequency theory as a 'slight reformulation' of the frequency approach, hinting that, together with the 'personalistic' approach, it might be a viable candidate for interpreting the probability attached to historical explanations. That said, Nagel adds that both approaches have been the subject of criticism; in particular the personalistic approach has been charged with making probability judgments to "rest ultimately upon the variable idiosyncrasies of human beings" (1961, p. 563), while the truth-frequency has been criticized on account of the fact that "since a frequency cannot be significantly ascribed to a single statement, it is strictly nonsensical to talk about the probability of a given hypothesis relative to given evidence" (1961, p. 563). Although such an objection is deemed not fatal, in *The Structure of Science* the issue is not further developed and is left open. This is in contrast with the attitude taken in *An Introduction to Logic and Scientific Method* where the authors mention historical inquiry as a field in which

[14] Nagel refers to the 'personalistic' interpretation as an 'improved' version of subjectivism, without associating it with any specific author, although he is likely to have in mind Savage's work. Savage is indeed mentioned in footnote 10 of Nagel (1961, pp. 563–564), but only together with a number of other authors who contributed to the literature on the foundations of probability.

the truth-frequency can find "intelligible and interesting applications" (Cohen & Nagel, 1934/1936, p. 327). This is also at odds with the already mentioned assertion, expressed in the same volume, that the truth-frequency theory can handle single-case probability attributions.

The most crucial feature of Nagel's truth-frequency view is its focus on evidence, to be taken not just in connection with its quantity, but with the way in which it is obtained. It goes to Nagel's merit that he pays attention to experimental methodology for collecting and organizing evidence, and forming hypotheses. He relentlessly recommends careful use of statistical methodology, aware of its assumptions and consequences. *An Introduction to Logic and Scientific Method* contains not only one chapter on "Probabilistic inference", but also chapters on "Probability and induction", "Measurement", and "Statistical methods". Among other things, he discusses the "dangers and fallacies in the use of statistics", and assigns a crucial role to fair sampling not only in the construction of evidentiary data, but also in connection with analogical reasoning, which can be "seen to be a case of probable inference which depends on fair sampling" (Cohen & Nagel, 1934/1936, p. 288).

As already observed, focusing on evidence brings context to the fore. But the importance of context is much wider, since it concerns the scope of the problems being faced and the intended use of the probabilistic values obtained. Although a unifying trait of the different uses of probability in different contexts could be the fact that Nagel defines it as a measure of success of a certain type of inference, the classes of propositions to which that inference applies are determined in ways that vary according to the context in which they occur. The awareness of the complexity characterizing the formation of evidence and the constant attention paid to context are the most remarkable aspects of Nagel's truth-theory of probability. In that respect Nagel can be seen as a pioneer of a 'bottom-up' approach to the philosophy of science that puts scientific practice in the foreground and assigns crucial importance to the context, an approach enjoying increasing popularity among philosophers of science, whose most prominent representative is Nagel's pupil Patrick Suppes (see Galavotti (forthcoming) for more on this subject).

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Chapter 10
On Ernest Nagel on Teleology in Biology



Bohang Chen

Abstract This chapter revisits Ernest Nagel's view of teleology in biology. In some of his writings from the 1950s to the 1970s, Nagel contended for the legitimacy of teleology in biology but argued against its uniqueness. In addition, Nagel also offered a goal-contribution account in the biological function debate which emerged in the 1960s and the 1970s. While questions of legitimacy and uniqueness of teleology draw little attention today, the biological function debate remains in focus in philosophy of biology. Although the ongoing debate does mention Nagel's name occasionally, his view, often merely understood as the goal-contribution account, is treated as outdated. However, this treatment is limited, because it fails to consider Nagel's another important thesis, that is, eliminativism. In this chapter, I attempt to do three things. First, I articulate Nagel's view, by showing that the eliminativist Nagel considered it acceptable to eliminate teleological terms in biological discourse, and that he also treated them as anthropomorphic vestiges. Second, I defend Nagel's view and use it to clarify the current biological function debate, by distinguishing a descriptive dimension of this debate from its prescriptive dimension. Third, I hope to deepen Nagel's view, by pushing it to a further and perhaps its logical conclusion, that is, Kant's view of biological teleology.

Keywords Ernest Nagel · Teleology · Philosophy of biology · Eliminativism · Biological function debate · Immanuel Kant

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10.1 Introduction

Discussions about biological teleology[1] date back to ancient Greek philosophers. Aristotle already argued against ancient atomists and mechanists from his general teleological standpoint. In and after the Scientific Revolution, at least according to traditional historiography, teleology was viewed as abhorrent and went into a relative decline, due to the dominance of the so-called mechanistic worldview.[2] When it came to the 1960s and the 1970s, the formative period of the discipline *philosophy of biology*, questions concerning biological teleology motivated lively philosophical discussions (for a rare summary of these discussions, see Garson, 2016, pp. 17–32). Today although the interest in teleology in biology has not diminished, the focus has moved to a more specific type of teleology, that is, biological function. The biological function debate emerged in the 1960s and the 1970s and has since then remained an object of philosophical concern (see some collections of important articles, Eds by Allen et al. (1998); Buller (1999); Ariew et al. (2002); Huneman (2013)). An overview of questions concerning teleology in biology will be presented in Sect. 10.2.

[1] In this chapter, phrases like "biological teleology", "teleology in biology" and "teleological terms, statements and explanations in biological discourse" are used interchangeably. Admittedly, at the start only the third seems the most appropriate, since it is an uncontroversial fact that teleological expressions are widespread in biological discourse. The main difficulty of the other two is that some authors (e.g., Ghiselin 2002), while acknowledging the previous fact, deny the existence of "genuine" teleological phenomena in biological sciences. However, as will be clear in this chapter (Sects. 10.4 and 10.5), this difficulty can be removed by distinguishing non-intentional accounts of teleology from its intentional accounts. If non-intentional accounts are prescribed, it is unproblematic to use phrases such as "biological teleology" and "teleology in biology". Meanwhile, the same clarification also works for function terms, given the fact that some authors (e.g., Ghiselin 2001) also contend that function terms are not teleological terms. Yet, these authors, while attributing non-intentional accounts to function terms, insist on attributing intentional accounts to teleological terms and dismiss the latter as illegitimate in biological discourse. In this chapter I try to avoid these sometimes-frustrating terminological issues. I consider the possibility of attributing non-intentional accounts to both and prefer to treat function terms as teleological terms. Indeed, on these terminological issues, I am following the clarification approach ("clarifying…meaning and implications") endorsed by Nagel, rather than the legislation approach ("legislating what sort of things it is permissible or possible for the empirical sciences to investigate") he opposed (Nagel 1936, p. 6; also see Nagel 1945). The clarification approach is also supported by logical empiricists in general, and runs close to, in my view, the spirit of a Kantian Critique.

[2] For instance, Jeffrey McDonough (2020, pp. 151–152) recently shows from historical evidence that "most natural philosophers in the early modern period remained deeply committed to teleology", and that the persistent impression conveyed in traditional historiography that "teleology was simply undermined by the so-called scientific revolution" (2020, p. 152) should be corrected. However, in my view, while traditional historiography can be disputed in its use of historical materials, it is also essential to understand that it mainly plays an ideological role, in legislating a Whiggish narrative for either the triumph of mechanism (for instance, the success of mechanism started from the scientific revolution) or the return of teleology (like, teleology was banned for a long time, now it is time for its re-emergence). In these cases, the authors care little about the historical accuracy of their narratives. For an example, see Walsh (2008).

Ernest Nagel started working on questions concerning biological teleology in the 1950s. Nagel offered his first reflection on teleology in 1953, with an article entitled "teleological explanations and teleological systems". In 1956 he contributed another article, "a formalization of functionalism". One chapter from Nagel's, 1961 classic *Structure of Science* dealt specifically with biological teleology. Nagel's final thought was presented in his John Dewey lecture in 1977, later published as two journal articles, "Goal-Directed Processes in Biology" and "Functional Explanations in Biology", reprinted in his 1979 collection of essays, entitled *Teleology Revisited*.

Textual evidence confirms Nagel's active participation in discussions about biological teleology in the 1960s and the 1970s, a period alleged to be the formative period of *philosophy of biology* as a discipline (Odenbaugh & Griffiths, 2020). His view was discussed by a variety of authors active in this period (e.g., Hempel, 1959/1965; Wright, 1968; Beckner, 1969; Ruse, 1971; Wimsatt, 1972; Mayr, 1974; Cummins, 1975). In the current biological function debate, however, Nagel's name is rarely mentioned. Although Nagel did offer a goal-contribution account of biological function, it is now viewed as outdated, perhaps merely of historical interest. Most recent discussions do not even mention Nagel's name (see below).

However, in my reading, Nagel's view of teleology in biology is far from being adequately understood. In particular, for current interest, while Nagel did offer an account of biological function, he also endorsed the thesis of eliminativism, which claims that teleological terms in biological discourse are eliminable in principle. Indeed, this also reflects Nagel's general view of biological teleology: teleological terms are legitimate in biological discourse, but only when they are given adequate, non-intentional accounts; yet these accounts also make teleological terms dispensable. Now, in this chapter my intent is to articulate, defend and deepen Nagel's view. The main thesis is that Nagel's view remains defensible, helps clarify the current biological function debate, and, if pushed further, echoes Kant's view and alludes to the peculiarity of the concept of life. Concerning the structure of this chapter, Sect. 10.2 presents an overview of questions concerning teleology in biology, Sect. 10.3 articulates and defends Nagel's view, with an emphasis on the thesis of eliminativism, Sect. 10.4 deepens Nagel's view, by using it to clarify the current biological function debate, Sect. 10.5 further connects Nagel's view to that of Kant, and argues that teleology in biology alludes to the peculiarity of life, Sect. 10.6 concludes by briefly summarizing the whole chapter.

10.2 An Overview of Questions Concerning Teleology in Biology

Before looking at Nagel's view, it is helpful to go over some key questions concerning teleology in biology. In general, these questions are motivated by the following observation: explanations in biology often use statements referring to terms like "goal", "end" and "purpose", or "function"; in other words, teleological terms as

such, along with teleological statements and explanations, are widely present in biological discourse. For instance, microbiologists working on E. coli sometimes claim that the goal of an E. coli is to evade chemical stimulants, extreme temperature or strong competitors. The term "function" is also commonly used in biological discourse. Biologists often state that the function of certain chemical components, cellular organelles, cells, tissues, organs or traits is to benefit certain organisms. For example, plant biologists might assert that the function of chlorophyll is to enable plants to perform photosynthesis (this is Nagel's own example, 1961/1979, p. 403). Relevant biological knowledge in both cases ("goal" and "function") is easy to understand. The motion of an E.coli is goal-directed, to ensure its own survival and perhaps replication; the chlorophyll helps transform carbon dioxide and water, with the assistance of the sunshine, into carbohydrates and oxygen, which are essential to the living plant.

Given the presence of teleological terms in biological discourse, three questions have arisen in the history of science and philosophy. First, are teleological terms legitimate in biological discourse? While in the past teleological terms were sometimes viewed with suspicion, as anthropomorphic vestiges intrinsically connected to the possession of intentions and wills, today most philosophers accept their legitimacy in biological discourse. After the legitimacy of teleological terms is granted, there emerges the second question which concerns the uniqueness of teleological terms: are teleological terms unique to biological discourse, or are teleological phenomena unique to biological sciences (in contrast to physical sciences)? This question is more controversial, but some biologists and philosophers have also endorsed a positive answer. For instance, the philosopher of biology Michael Ruse (2000, p. 213) once claimed that "teleological explanations are thought appropriate in evolutionary biology, but not in the physical sciences". Further, as one can anticipate, if the question of uniqueness receives a positive answer, it helps strengthen the thesis of the autonomy of biology.

However, admittedly, questions of legitimacy and uniqueness are relatively old questions; although heatedly discussed in previous decades (from the 1950s to the 1970s, when Nagel wrote on teleology), they are no longer popular in the philosophy of biology literature today. Nowadays philosophers of biology are more interested in a third question: what do teleological terms mean in biological discourse? Indeed, this question is raised in a more specific form and the focus is on how to interpret function terms in biological discourse. This question motivates the biological function debate. In the last ten years, Justin Garson, a leading scholar in the current biological function debate, has contributed several articles and monographs (Garson, 2011, 2012, 2016, 2017a, b, 2018, 2019a, b). At the same time, a few others have also made contributions (Mossio et al., 2009; Nanay, 2010, 2012; Brunnander, 2011; Artiga, 2011, 2014; Saborido et al., 2011; Neander & Rosenberg, 2012; Kraemer, 2013; Saborido, 2014; Artiga & Martínez, 2016; Mossio & Saborido, 2016; De Prado Salas, 2018; Neander, 2017; Cusimano & Sterner, 2019; Matthewson, 2020).

Two approaches to interpreting biological function dominate the philosophy of biology literature: the evolutionary (etiological, more broadly speaking) approach

and the systematic (some might call it "organizational") approach (I adopt this classification from Saborido 2014).[3] The basic idea of the evolutionary approach is that the function of a biological trait is to be understood through its evolutionary history, possibly in relation to natural selection (functions as selected effects). The systematic approach, in contrast, suggests that the function of a trait is to be analyzed through its current contribution to the maintenance of a larger system (functions as causal contributions). Further, since the emergence of the biological function debate, each approach has enlisted more and more detailed accounts (for brief overviews, see Garson, 2008, 2016 and Wouters, 2005). The evolutionary approach has been articulated, with increasing levels of complexity, by a few evolutionary biologists and philosophers (e.g., Ayala, 1970; Godfrey-Smith, 1994; Griffiths, 1993; Millikan, 1989; Neander, 1991; Ruse, 1971; Williams, 1966; Wimsatt, 1972; Wright, 1973). At the same time, the systematic approach has also been well developed as the major alternative (e.g., Amundson & Lauder, 1994; Beckner, 1969; Bigelow & Pargetter, 1987; Cummins, 1975; Davies, 2001; Enc & Adams, 1992; Prior, 1985; Walsh, 1996; Wouters, 2003).

The generation of so many different accounts, as one can expect, is driven by objections raised against each other in the biological function debate. While specific objections are concerned with details of a given account, there is one objection that has been raised against almost every account. This objection goes as follows: a given account of biological function fails to capture actual biological usage, that is, how function terms are actually used in biological discourse. More specifically, given a particular account of biological function, it is judged as inadequate, if it yields cases of using function terms that do not conform to actual biological usage: either no biologist uses it in accordance with the given account, or biologists use it in a way that violates the given account. Indeed, the entire evolutionary approach has continuously been questioned for this objection. For instance, Arno Wouters (2005, p. 144) complains that "no etiologist has ever provided a detailed discussion of a real example of the use of etiological functions in biology", and elsewhere he even claims that, for this reason, "the [evolutionary] approach taken by the authors to tackle the question how function talk in biology…is legitimate is fundamentally flawed" (2003, p. 233). Some systematic accounts also appear open to this objection. According to

[3] Again, there is a terminological issue here. Some might object that it is impermissible to place currently popular organizational accounts under the systematic approach, because, first, current organizational accounts (Mossio et al., 2009; Saborido et al., 2011; Saborido, 2014; Mossio & Saborido, 2016), in contrast to earlier ones (McLaughlin 2003; Schlosser 1998), claim to have integrated the virtues of both evolutionary and systematic approaches; second, they are also interpreted as belonging to the etiological approach (Artiga and Martinez 2016). In this chapter, I hope to evade this terminological issue, by following earlier organizational accounts (especially Schlosser, 1998) to explicitly place the under the systematic approach as against the evolutionary approach; current organizational accounts, I believe, will not object to this classification, since they oppose Artiga and Martinez (2016)'s interpretation and stress their own ahistorical character. Overall, these terminological issues constitute a non-negligible part of current "philosophical" discussions, in which hasty dismissals by presupposing different meanings of the same term flow around. In my hope, the clarification approach endorsed by Nagel in particular and logical empiricists in general might play a limited role in relieving "the climate of opinions" and promoting rational discussions.

Garson (2017b, p. 1095), to refute these accounts, one just needs "a series of clever counterexamples".

This objection, along with other specific ones, motivates philosophers of biology to offer a variety of accounts of biological function, with the intent of capturing actual biological usage to a fuller extent. Given so many different accounts, then, which account from which approach is more appropriate? There is no easy answer to this question, because the biological function debate has remained unsettled. While earlier philosophers, especially those from the 1960s and 1970s, tended to defend a monist position (Cummins, 1975; Ruse, 1971; Wright, 1973), the majority now have preferred pluralism (Allen & Bekoff, 1995; El-Hani & Nunes-Neto, 2009; Garson, 2018; Godfrey-Smith, 1993; Griffiths, 2006; Hardcastle, 1999; Mitchell, 1993; Preston, 1998; Walsh & Ariew, 1996). This is confirmed by Garson (2018, p. 1101), "many philosophers of biology have embraced function pluralism"; he characterizes pluralism as "the idea that biologists use the term 'function' in different ways on different occasions" (2018, p. 1101).

It is plausible to conjecture that Nagel would not be able to anticipate the diversity of current accounts and the complexity of the whole biological function debate. As a result, Nagel's early view might appear too simple-minded, compared to subtler accounts offered in subsequent years. In the following, however, I show that this is not the case. As a matter of fact, Nagel's view is not only ill-understood, it is still defensible and is even able to help clarify the current biological function debate.

10.3 Nagel's View of Teleology in Biology: Articulation and Defense

This section articulates and defends Nagel's view. The key point is that although Nagel accepted the legitimacy of teleological terms in biological discourse, he denied their uniqueness and endorsed eliminativism.

10.3.1 Questions of Legitimacy and Uniqueness

Overall, there is no doubt that Nagel accepted the legitimacy of teleology in biology. On this he wrote: "Teleological concepts and teleological explanations do not constitute a species of intellectual constructions that are inherently obscure and should therefore be regarded with suspicion" (1977/1979, p. 316).

While granting the legitimacy of teleology in biology, Nagel also offered his own account(s): the system-property account of goal-directed processes and his goal-contribution account of biological function:

> [For processed to be goal-directed], one feature is the plasticity of such processes-that is, the goal of such processes can generally be reached by the system following alternate paths

> or starting from different initial positions. The second feature is the persistence of such processes-that is, the continued maintenance of the system in its goal-directed behavior, by changes occurring in the system that compensate for any disturbances taking place (provided these are not great) either within or external to the system, disturbances which, were there no compensating changes elsewhere, would prevent the realization of the goal. (Nagel, 1977/1979, p. 286)
>
> [A] function of item i in system S and environment E is F, presupposes (though it may not imply) that S is goal-directed to some goal G, to the realization or maintenance of which F contributes" (Nagel, 1977/1979, p. 297)

However, Nagel denied the uniqueness of teleology to biology as well as the thesis of the autonomy of biology:

> The prevalence of teleological explanations in biology does not constitute a pattern of explanation in comparably different from those current in the physical sciences, and…the use of such explanations in biology is not a sufficient reason for maintaining that this discipline requires a radically distinctive logic of inquiry. (1961/1979, p. 428)

10.3.2 Eliminativism

Nagel's denial of the uniqueness of teleology to biology results from his endorsement of eliminativism. Here is what Nagel said:

> Since the word "teleology" is ambiguous, confusion and misunderstandings would doubtless be prevented if the word were eliminated from the vocabulary of biology. [...]
>
> They [teleological explanations] can be reformulated, without loss of asserted content, to take the form of nonteleological ones, so that in an important sense teleological and nonteleological explanations are equivalent. (1961/1979, pp. 402–403)
>
> goal ascriptions can be explicated without employing any teleological notions in the interpretation…functional statements, as well as the presuppositions of functional ascriptions, can also be rendered without using functional concepts. (1977/1979, pp. 313–314)

The thesis of eliminativism is easy to understand, and perhaps more importantly, a defense of eliminativism is still possible today, regarding the current biological function debate. Indeed, the thesis of eliminativism applies to most accounts that have been offered in the biological function debate.

Let me briefly argue for this point, by referring to two latest accounts of biological function, one from the evolutionary approach, the other from the systematic approach. First, Garson (2017a, p. 534) elaborates his "generalized selected effects theory of function": "the function of a trait consists in that activity that historically contributed to its being differentially reproduced or differentially retained within a biological system". Given this account, it is easy to see that function terms can be eliminated, and we can simply use descriptions of "that activity" plus descriptions of how it is reproduced and retained. Second, Mossio and his colleagues' latest systematic (they call it "organizational") account defines the function of a trait as follows, "a trait T has a function if and only if: C1: T contributes to the maintenance of the organization O of S; C2: T is produced and maintained under some constraints exerted by O; C3:

S is organizationally differentiated" (2009, p. 828). While this account appears more complex, eliminativism holds trivially: the expression of "the function of T" can be replaced by stating the effect of T under the three conditions C1, C2 and C3.

Schwartz summarizes as follows the common strategy of most accounts offered in the biological function debate:

> Philosophers offer short lists of necessary and sufficient conditions for the application of the [teleological] concept-where the conditions involve only acceptable physical or biological notions-and claim that the set of conditions captures the import of function statements. (Schwartz, 2004, p. 136)

This passage entails logically, as Nagel anticipated, that those "function statements" can be translated by using "acceptable physical or biological notions". In consequence, biological knowledge can be expressed without function terms. Eliminativism holds rather trivially.[4]

Eliminativism appears a truism. If so, some might wonder why there are no declared eliminativists today, why teleological terms are so widespread in biological discourse, why there exist so many accounts of biological function, and why the biological function debate has so far failed to address (Nagel's argument for) eliminativism. The first rather obvious reason is that eliminativism is in general conceived of as an arrogant position which dictates that function terms and statements *must* be eliminated; yet, given the abundance of such terms and statements in biological discourse, eliminativism (understood as the arrogant position) goes radically against actual biological usage and appears rather absurd. However, As Nagel indicated, eliminativism can be received as a more moderate thesis suggesting that function terms and statements *could* be eliminated. Here follows the second reason for which eliminativism is often ignored. Indeed, eliminativism is more often concerned with a prescriptive question, that is, how could biological usage be? In contrast, the current biological function debate deals with a descriptive question concerning actual biological usage. Yet, the presence of function terms and statements in biological discourse does not strictly entail that they cannot be eliminated (see Sect. 10.4 for details). Finally, there is perhaps a third reason, mentioned by Peter McLaughlin:

> Biologists could in general probably get along fairly well, if they had to, without function terms by substituting either causal role or selective advantage or adaptive value. There is a very real danger that the vested interests of philosophy of mind in intentionality lead it to foist more teleology upon biology than the biologists need or want by providing a self-indulgent analysis of functional explanation. (McLaughlin, 2003, p. 10)

McLaughlin's formulation of "the so-called vested interests of philosophy of mind" alludes to the anthropomorphic root of biological teleology. Indeed, for Nagel, the possibility of eliminating teleological terms in biological discourse also indicates that they are anthropomorphic vestiges.

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[4] Indeed, while there are almost no declared eliminativists today in the biological function debate, the position of some philosophers (e.g., Craver 2001; Cummins 1975; Hardcastle 1999) runs rather close to Nagel's version of eliminativism. I learn this from Garson (2014) who prefers to call them conventionalists.

10.3.3 Teleological Terms as Anthropomorphic Vestiges

Following from Nagel's endorsement of eliminativism is that he also treated teleological terms in biological discourse as anthropomorphic vestiges. To make sense of Nagel's treatment, first of all, it is essential to understand that Nagel maintained a strict distinction between intentional teleology and non-intentional teleology. He mentioned this several times:

> Some teleological explanations patently do assume the existence of deliberate plans and mental purposes; but such an assumption is not illegitimate when, as in the case of teleological explanations of certain aspects of intentional agents, the facts warrant it. (1961/1979, p. 24)
>
> [...] teleological explanations are sometimes suspect in modern natural science because they are assumed to invoke purposes or ends-in-view as causal factors in natural processes. Purposes and deliberate goals admittedly play important roles in human activities, but there is no basis whatever for assuming them in the study of physicochemical and most biological phenomena. [...] [T]eleological (or functional) statements in biology normally neither assert nor presuppose in the materials under discussion either manifest or latent purposes, aims, objectives, or goals. Indeed, it seems safe to suppose that biologists would generally deny they are postulating any conscious or implicit ends-in-view even when they employ such words as "purpose" in their functional analyses [...]. (p. 402)
>
> If the intentional view of goals is taken literally, an organism can be described as goal-directed with respect to a determinate goal as if it is legitimate to ascribe intentions, desires, and beliefs to the organism. In consequence, goals and goal-directed behavior can be correctly predicated only of human beings, and possibly of some higher animals. It is therefore entirely inappropriate to use such language in connection with organisms such as protozoa and plants which are incapable of having intentions and beliefs; in connection with subsystems of organisms, such as the complex of glands and other parts of the human body, that are involved in the homeostasis of the blood temperature; or in connection with inanimate systems, such as steam engine provided with a governor and other servomechanisms that are sometimes described as goal-directed. (Nagel, 1977/1979, p. 279)

So, according to Nagel, human beings (and perhaps some higher animals) are genuine intentional agents who have recognizable wills and intentions; in contrast, in most cases of biology, intentional accounts of teleology are illegitimate. Then teleological terms, given the fact they still exist in biological discourse, are requested to receive non-intentional accounts. Hence, in the biological function debate, while the evolutionary approach and the systematic approach differ from each other at other aspects, neither accepts any intentional account of teleology.

Intentional accounts of teleology only apply to human sciences (or social sciences). Teleological terms in biology, in turn, must receive non-intentional accounts and are therefore eliminable. Then, for Nagel, it is plausible to view teleological terms in biological discourse as anthropomorphic vestiges. Nagel illustrated this point by referring to the history of physics. He first pointed out that teleology was also available in physics centuries ago: "the discovery that the principles of mechanics can be given such extremal formulations was once considered as evidence for the operation of a divine plan throughout nature […] widely accepted in the eighteenth and nineteenth century" (1961/1979, p. 407). Indeed, principles of mechanics would constitute genuine scientific knowledge, even when they were expressed by using

teleological terms. But according to Nagel, unlike the situation in the eighteenth and nineteenth century, physics at its present stage has largely discarded teleological terms, and this is because, for physicists, "teleology is apt to be misunderstood as connoting the operation of [intentional] purposes" (1961/1979, p. 408). However, the physicists' strategy of dealing with teleology differs sharply from that of biologists: while physics bans teleology directly, biology retains it by, perhaps with the help of philosophers, offering non-intentional accounts. In other words, teleological terms are made legitimate in biological discourse by discarding their anthropomorphic implications.

Then, given the fact that such teleological terms are also eliminable in biological discourse, it is then not incorrect to view them as anthropomorphic vestiges. In Nagel's summary:

> It is a plausible claim that the primary meaning of the phrase "being goal-directed" is stated by the intentional view; and it is not unreasonable to suppose that the prevailing more inclusive sense of the phrase is a "metaphorical extension" of its initial meaning. (1977/1979, p. 280)[5]

10.4 A Clarification of the Biological Function Debate

Section 10.3 has articulated and defended Nagel's view. Of particular interest is the thesis of eliminativism. Now this section applies Nagel's view to clarify the current biological function debate. More specifically, I distinguish two dimensions of the biological function debate, one descriptive, the other prescriptive.[6] A descriptive dimension attempts to capture actual biological usage and is certainly meaningful. The prescriptive approach tends to prescribe accounts of biological function as rules for biological usage. As a matter of fact, Nagel's view touched on both dimensions. In Nagel's words, given the fact that "biologists do use it [teleology], and say they are giving a teleological explanation" (Nagel, 1961/1979, p. 402), he would offer accounts of biological teleology to capture actual biological usage; at the same time, he would not hesitate to eliminate teleology in biology, because, for him, "the

[5] One objection often voiced against the systematic approach in the biological function debate refers to the normativity problem. According to this objection, most systematic accounts are problematic, because they are unable to capture the normative implications of function terms: the function of a biological entity is said to be different from its mere effect, because it is supposed to give rise to its effect; or put it positively, any account of biological function should distinguish functions from mere accidents, and captures the possibility of dysfunction or malfunction. For instance, Millikan (1989, p. 299) claims that "[systematic] accounts of purpose or function in terms of present disposition or structure run afoul exactly when they confront the most central issue of all, namely, the problem of what failure of purpose and defectiveness are". To avoid this normativity problem, the evolutionary approach recommends that the function of a biological entity should be interpreted as its selected effect. While it is impossible to argue in detail here, I agree with Davies (2000), Craver (2001), Boorse (2002) and Brunnander (2011) that the normativity problem does not exist, and it actually retains an anthropomorphic mode of thinking.

[6] Indeed, this echoes the distinction between descriptive semantics and normative/pure semantics made by other logical empiricists like Rudolf Carnap (1942/1948, pp. 11–15).

word ‘teleology’ is ambiguous, confusion and misunderstandings would doubtless be prevented if the word were eliminated from the vocabulary of biology” (Nagel, 1961/1979, p. 402).

10.4.1 The Descriptive Dimension

In the biological function debate, the most seen objection concerns actual biological usage. The general logic of this objection is simple: given a particular account, it is judged inadequate, if it either fails to capture actual biological usage, or yields cases of using function terms against actual biological usage. Here follows the descriptive dimension of the biological function debate. Accordingly, all existent accounts of biological function, viewed from this dimension, are intended to describe how biologists actually use function terms. Clearly, this concerns an empirical question about linguistic habits of biologists, and all given accounts serve as tentative answers.[7] Admittedly, an examination of actual biological usage at its present stage very much likely gives rise to a descriptive pluralism. As Garson (2018, p. 1101) indicates, “biologists use the term ‘function’ in different ways on different occasions”.

It should be emphasized that the descriptive dimension treats actual biological usage as the ultimate criterion of judging relevant accounts of biological function. For this, Wouters (2005, p. 144) is deeply skeptical about the evolutionary approach, since he is convinced that in biological literature one cannot even find “a real example of the use of etiological functions”. The evolutionary approach is alleged to be an invention of philosophers (e.g., Buller, 1999; De Prado Salas, 2018), and is indeed more popular among philosophers. Indeed, these claims about actual biological usage at its present stage might all be empirically correct. Yet, the use of them to disqualify evolutionary accounts makes sense, only when actual biological usage is assumed to be the ultimate criterion.

Further, to talk about actual biological usage, it is necessary to specify a particular time and perhaps even a particular community of biologists. So, while descriptive pluralism is empirically correct regarding actual biological usage at its present stage, it will not necessarily be so in the future, nor can it be guaranteed that it was so in the past (for this point, see also Schwartz 2004).[8] Indeed, if in a hypothetical situation biologists changed their linguistic habits upon recommendations from philosophers sympathetic with the evolutionary approach, then actual biological usage would

[7] I learn from Garson (2014) that Schyfter (2015) adopts a similar view from the standpoint of social epistemology.

[8] Very interestingly, one of the main participants of the early biological function debate and the chief supporter of the evolutionary approach Wright (1976) already provided such a hypothetical scenario, in which “a metaphor dies when the metaphorically extended use of a term becomes established more or less independently of the original paradigm” (p. 19). As a matter of fact, only after having detected the meaning change of function terms, could Wright assign himself the new task of meeting “the requirement of literal translatability” (p. 18).

change accordingly, and the evolutionary approach would also become more plausible. While this hypothetical situation might be unlikely to come, it reminds us that the descriptive dimension can hardly be the end of the biological function debate, and there should emerge the prescriptive dimension. After all, Nagel prescribed eliminativism, even though he knew that biologists use teleological terms rather frequently.

10.4.2 The Prescriptive Dimension

The thesis of eliminativism concerns the prescriptive dimension of the biological function debate. As argued above, eliminativism is defensible, because function terms are not necessary to expressing biological knowledge, and all functional statements and explanations can be translated into those without them. Further, eliminativism can be read as prescribing rules on how to use function terms in biological discourse, and in this case, importantly, actual biological usage is no longer the ultimate criterion. A further consideration of eliminativism and the prescriptive dimension of the biological function debate has the following implications.

First, since actual biological usage is no longer the ultimate criterion, the objection that treats actual biological usage as such loses its force. So, regarding the prescriptive dimension of the debate, even if Wouters and others' observation that the evolutionary approach captures almost no actual biological usage is empirically correct, the issue now is concerned with rules for biological usage, rather than biological usage itself. To put it more explicitly, if a given account is viewed as prescribing rules, whenever a violation of actual biological usage takes place, it is the particular biological use rather than the account itself that is mistaken. In McLaughlin's (2003, p. 7) words, "If we stipulatively define the term function in biology […] then intuitive counterexamples to the usage prescribed by the definition have no force."

Second, if new accounts were treated as prescriptive rules and biologists adopted them (although unlikely), actual biological usage would change resultantly. In such a hypothetical situation, some biologists would switch to the evolutionary side and use function terms in their evolutionary meanings, upon the recommendations from the evolutionary approach. In that case, seen from the descriptive dimension, some evolutionary accounts would also become empirically adequate, in accordance with new biological usage.

Third, since biological knowledge can be expressed without function terms, in turn any account of biological function becomes acceptable in principle. This is a subtler implication. Because function terms are eliminable anyway, it matters little which account is finally chosen, as far as it concerns the expression of biological knowledge itself. Indeed, *while biological knowledge contains a set of uncontroversial laws and facts, different accounts of biological function at best relate function terms to*

different portions of biological knowledge.[9] Further, it appears even possible to cast doubt on the whole biological function debate, especially in its relation to biological knowledge itself, which consists of uncontroversial laws and facts in biology. In giving a variety of accounts of biological function, philosophers seem to have merely taken in more and more laws and facts from biology.[10] While this is perhaps still useful to capture actual biological usage, its relevance to genuine biological knowledge remains rather suspicious.[11]

10.4.3 Some Concrete Examples

The foregoing clarification appears abstract, and now I offer some concrete examples. The main example is a controversy triggered by the systematic or organizational account from Mossio et al., (2009, p. 828). Here is their account: "a trait type T has a function if, and only if, it is subject to systematic closure C in a differentiated self-maintaining system S". Yet, to this account the earlier Delancey (2006, pp. 87–88) appears to pose a challenge, based on "the difference between cross-generational and intra-generational teleofunctions". This challenge can be stated as follows. To account for biological function, Mossio et al. (2009) rely on a crucial concept, namely, the self-maintaining system, to which a functional trait must contribute. However, according to actual biological usage, the sperm, as a cross-generational trait, does have functions (like generating fertilized eggs), yet it does not seem to have contributed to the self-maintenance of its paternal organism. Consequently, as Artiga and Martínez (2016, p. 109) summarize, 'OA [the organizational account] has the very counterintuitive consequence that sperm does not have any function'.

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[9] For instance, within the evolutionary approach, one can place emphasis on the recent past (e.g., Godfrey-Smith 1994), or other types of selection (e.g., Garson 2011, 2012), or the non-selective history (e.g. Buller 1998). Within the systematic approach, one can focus on plasticity (e.g. Nagel 1977/1979; Enç and Adams 1992), or negative feedback (e.g. Faber 1984), or reproduction (e.g. Schlosser 1998), or self-maintaining networks (e.g. Mossio et al., 2009). In principle, one can even devise a rather peculiar account and prescribes it as a rule on how to use function terms. While this peculiar account might violate actual biological usage in every word, the point is that it is not refutable if treated as offering prescriptive rules.

[10] Further, consider the objection resulting from the normativity problem, mentioned in Footnote 5. If this objection is driven by an anthropomorphic mode of thinking, then the normativity problem cannot be solved in principle, since, no matter how many different laws and facts from biology are defined into function terms, accounts of biological function remain non-intentional and are therefore unable to achieve a genuine anthropomorphic status. So, in addition to the motivation of capturing actual biological usage, another motivation of offering so many accounts of biological function seems a rather implicit anthropomorphic tendency, channeled through the normativity problem.

[11] As can be anticipated, some will take issues with this point and claim that, since the biological function debate attempts to describe actual biological usage and even recommend biologists new rules of using function terms, it helps vocabularies used by biologists to achieve a higher level of systematicity and comprehensiveness, and is resultantly still relevant to genuine biological knowledge. In fact, I do not dispute this claim, and my point is rather that giving so many accounts are unnecessary to adequately expressing biological knowledge.

To solve this problem, the earlier Delancey (2006, p. 91) split the systematic account: one sub-account for intra-generational functional traits with respect to 'self-sustaining' systems, the other for cross-generational functional traits (e.g., the sperm) with respect to 'self-reproducing' (2006, p. 91) systems. In response, Saborido et al., (2011, p. 599) stress a point only implicit in their earlier article: "various temporary instances of a system […] can be considered as instances of the same encompassing self-maintaining organization". As a result, self-maintaining organization includes not only individual organisms, but also, by definition, a reproducing lineage of organisms. In their words, "a self-maintaining organization […] maintains itself also through reproduction" (2011, p. 601).

Now the foregoing clarificatory results can be applied to this concrete controversy. This controversy has a clear descriptive dimension, since both sides agree on that an adequate account of biological function should capture actual biological usage (biologists do say that the sperm has a function). Now if actual biological usage is treated as the ultimate criterion, Delancey (2006) is reasonable to propose two kinds of functions, one for intra-generational traits with respect to self-maintaining systems, the other for cross-generational traits with respect to self-reproducing systems. The result indicates a descriptive pluralism. However, this does not necessarily imply that Saborido et al. (2011) are mistaken. Seen from the original account in Mossio et al. (2009), two strategies are available. The first is to expand their original account to recruit self-producing systems and to create an extension of the concept of self-maintaining systems. This strategy is actually adopted in Saborido et al. (2011). It yields at least a nominal unification, for which it seems fine to say that the sperm has a function. Importantly, the first strategy still treats actual biological usage as the ultimate criterion.

The second strategy is to insist on the narrower account from Mossio et al. (2009). Then, actual biological usage is no longer the ultimate criterion. Further, the narrower account should be prescribed as a rule, for which it becomes mistaken to say that the sperm has a function. Such a prescription does violate actual biological usage. Yet for emphasis, this violation is scarcely important, since no one will deny the significance of biological knowledge (laws and facts) about sperm and self-reproducing systems. Considering the prescriptive dimension of the controversy, both accounts are adequate in themselves as prescriptions. The result indicates a prescriptive pluralism. Finally, needless to say, one can also prescribe eliminativism, for which it becomes mistaken to talk about the function of a sperm.

In sum, concerning the biological function debate, it is essential to distinguish the descriptive dimension of the debate from its prescriptive dimension. Given any particular account, the criterion of judging its adequacy needs to be made clear in advance. If the descriptive dimension is stressed and actual biological usage is treated as the ultimate criterion, the given account should be judged on whether it conforms to actual biological usage. Yet if the prescriptive dimension is preferred, as far as it concerns biological knowledge itself, any account is acceptable in principle.

Moreover, the same rationale also applies to controversies over teleology outside biology, such as, firstly, in physics. Nagel once suggested that in physics one can still use teleological terms. In that case, teleology receives a minimal mechanistic

interpretation and functions or goals of a physical entity are defined to be its lawful effects. Let us refer to Nagel's own example. As to the gas law stating that "the volume of a gas at constant temperature varies inversely with its pressure", according to Nagel, it can be translated into a teleological statement claiming that "the function of a varying pressure in a gas at constant temperature is to produce an inversely varying volume of the gas" (1961/1979, p. 406). So, if the minimal mechanistic sense of teleology is prescribed, functions and purposes will turn out to be available almost everywhere in the physical world.

Further, the same goes for human sciences that concern intentional agents. For instance, Richard Taylor, in an earlier debate with cyberneticists, complained that the latter's use of "the term 'purpose' […] bears no similarity whatever to the meaning which is ordinarily attached to it" (1950, p. 310). According to Taylor, its ordinary meaning refers to the possession of "desires and beliefs" (1950, p. 331). Taylor's assertion might appear arbitrary, yet they can be read as prescribing intentional accounts of teleology as rules for actual usage (also see Nissen, 1997). In that case, no non-intentional accounts will be permitted, functions and purposes will be limited to a small part of the universe, that is, human beings and some higher animals.

10.5 On the Persistence of Teleology in Biology: From Nagel to Kant

Given that teleological terms are abundant in biological discourse, Nagel did two things. On the one hand, he attempted to make sense of them and capture actual biological usage by offering explicative accounts. On the other hand, he endorsed the thesis of eliminativism. However, eliminativism reveals that teleological terms are not necessary to the expression of biological knowledge. Then, if eliminativism holds, the persistence of teleology in biology becomes an intriguing phenomenon: if teleological terms are not necessary, why does not the history of biology eliminate them? Why do not biologists simply discard teleological terms? Nagel did not go further to reflecting on this intriguing phenomenon, he simply accepted it as given. On this point, in my view, Nagel's analysis falls short than that of Kant, who was fully aware of the peculiarity of teleology in biology.

Yet, as a matter of fact, Nagel (1977/1979) discussed Kant's view on teleology in biology in detail. He first pointed out that Kant in *Critique of Judgment* stuck to intentional accounts of teleology. Nagel wrote:

> The [Kant's] conclusion to be drawn from all this is that since we cannot really understand how final causes operate except in the case of our own actions, ascriptions of goals and functions to nonhuman organisms and their parts cannot be taken literally, as objective assertions about nature. (Nagel, 1977/1979, p. 304)

As Nagel rightly observed, if Kant merely accepted intentional accounts of teleology, he would inevitably be puzzled by the presence of teleological terms in biological discourse. Again, Nagel understood Kant well on this point:

> Kant [was] [...] therefore agnostic about the literal truth of any functional ascription [...] the agnosticism has its source in the assumption that a process cannot properly be characterized as purposive if it can be explained on the basis of physico-chemical laws and that an effect of an organic process can count as one of its biological functions only if that process was intended or designed to produce the stated effect. Agnosticism concerning the truth of function ascriptions seems to be the price that must be paid for explicating the notion of biological function in terms of conscious intent. But if the notion of being a function can be explicated, as I believe it can be, with no reference to intentions or choices of conscious organisms, the validity of ascribing a function to an entity can be decided by empirical inquiry, without any need for the elaborate made-believe that seems to be an inseparable accompaniment of the heuristic view. (Nagel, 1977/1979, p. 305)

Nagel's solution to this puzzle was to refer to non-intentional accounts of teleology. According to Nagel, Kant would not be puzzled, if he ever understood that teleological terms are also open to non-intentional accounts. However, as can be anticipated, Kant would not be impressed by Nagel's solution, exactly by referring to Nagel's eliminativism. Kant would question Nagel: if teleological terms are eliminable in principle, why not just eliminate them rather than embrace non-intentional accounts?

For hard-core mechanists (admittedly one side of Nagel who treated teleological terms as anthropomorphic vestiges), the persistence of teleology in biology might only attest to subjective projections of mind onto life. More tough-minded thinkers might even sneer that some biologists and philosophers have confused minds and cannot think with precise concepts. This might be correct in some cases. Yet, Kant would want to suggest a different and perhaps a more objective reason, in relation to the simple fact that biology claims to be the science of life. For Kant, the persistence of teleology in biology possesses a deeper implication that alludes to an important part of empirical reality, that is, the reality of life itself (I mean, the pure phenomenon of life).[12] This is indeed Kant's view, as he ascribed an identificatory function to teleology (Quarfood, 2006; Van den Berg, 2015, p. 111).

My term to illustrate the implication is "quasi-intentional". Again, hard-core mechanists might dismiss the anthropomorphic implication of biological teleology as sheer nonsense. For them, teleological terms in biological discourse might be acceptable, but these terms must receive non-intentional accounts and become therefore eliminable. There is nothing wrong with this judgment; however, it is doubtful whether this judgment does full justice to the almost inevitable anthropomorphic tendency developed in biological discourse. While it is a truism that only mechanism contributes to scientific biology, intentional accounts remains attractive, as sheer mechanism appears unable to capture certain aspect of living organisms. Nevertheless, the intentional sense of teleology fails to play a genuine role in biology, and almost all biologists abhor intentional teleology. Then, what is the missing aspect of organisms, which can be neither mechanistic, non-intentional teleology, nor human, intentional teleology? My view here, in agreement with Kant, is that the missing

[12] Here it is important to distinguish *objects predicatable of life* (extension) and *life itself* (intension). What we general call biological knowledge is only knowledge about the former, rather than the latter (Ben-Naim, ms). Indeed, it is safe to say that we possess no genuine knowledge about life itself, even though we recognize so many things as alive in the world.

aspect of organisms, attested by the anthropomorphic implication of teleological terms in biological discourse, alludes to life itself.

Let me elaborate by referring to the biological function debate. Recall those non-intentional accounts. They are all unproblematic if rendered as prescribing rules; but then, they all have a price to pay. Re-defining teleology through non-intentional accounts only leaves the anthropomorphic tendency and the quasi-intentional aspect of organisms untouched. As a result, life also remains untouched. Moreover, recall that teleological terms are eliminable in biological discourse, strictly speaking.[13] In consequence, life, indicated and attested by teleology in biology, becomes hardly relevant to genuine biological knowledge.[14] Then, in summary, if matter is understood to be all mechanistic (physico-chemical) laws governing organisms, and life indicates the quasi-intentional aspect of organisms, the logical conclusion is that it is simply impossible to understand life through knowledge about matter. Matter and life remain conceptually distinct. Kant already emphasized this point as early as 1769 in his notebook, "although one perceives much life in matter, one distinguishes this from material properties" (Kant, 1769/2005, 17: p. 348).

At the same time, life is also different from mind. Life does not have genuine intentional meanings, mirrored by the fact that teleology in biology cannot receive intentional accounts. No evidence warrants the claim that the property of being alive has detectable impacts on organismic behaviors. Unlike mind, life does not have any causal influence on objects predictable of itself. This is also the reason why I have characterized teleology in biology as having a merely quasi-intentional aspect. Life and mind also remain conceptually distinct.

It has long been common to treat or mistreat (organizations of) matter as proto-life (for a description of this position, see Gayon, 2010), and then life as proto-mind (for an actual case, see Jonas, 1966). Scholars are often fascinated by constructing an evolutionary scheme, yet largely by conflating non-intentional, mechanistic and intentional, human senses of teleology. Kant was aware of (and sometimes, admittedly, even allured by) such a scheme, already available in his time:

> The gradual approach of one animal genus to the other, from that in which the principle of ends seems best confirmed, namely human beings, down to polyps, and from this even further to mosses and lichens, and finally to the lowest level of nature that we can observe, that of raw matter: from which, and from its forces governed by mechanical laws (like those

[13] As Kant maintained, "information […] about […] the origination and the inner possibility of these forms [of living beings] […] is that with which theoretical natural science is properly concerned", and "positing ends of nature in its products […] in accordance with teleological concepts […] provides no [such] information at all" (1790/2000, 5: p. 417).

[14] Resonating with eliminativism in the biological function debate, in the life definition debate there is also an eliminativist stance which tends to remove the concept of life from biology (Machery 2012; Mariscal and Doolittle 2018); correspondingly, there are also biologists and philosophers who attempt to relate life, like those who relate biological function, to different portions of biological knowledge (Bich and Green 2016; Koonin 2012; Mix, 2015; Smith 2016; Trifonov 2011). Yet, eliminativism is less defensible regarding life: if the reality of life is not denied (very implausible), while biological teleology can be reduced to life as the anthropomorphic tendency, life (to emphasize again, I mean the pure phenomenon of life, not ordinary biological phenomena) is irreducible to anything else. For this point, see also Voegelin (1933/1998, p. 144).

> which are at work in its production of crystals), the entire technique of nature, which is so incomprehensible to us in organized beings that we believe ourselves compelled to conceive of another principle [of ends] for them, seems to derive (Kant, 1790/2000, 5: pp. 418–419)

Such an evolutionary scheme, characterized by the increasing governing force of the principle of teleology, can be understood through either direction: idealists claimed to have found mind in matter, but they only gave mechanistic (physico-chemical) laws teleological expressions; materialists treat minds as so-called complex organizations of matter, but they only conflate mental purposes and plans with their underlying material systems. Both like to entertain themselves with vague and suspicious expressions. In sharp contrast, Kant stressed the distinctiveness of life (from both matter and mind) with rigor:

> Perhaps one comes closer to this inscrutable property if one calls it an analogue of life: but then one must either endow matter as mere matter with a property (hylozoism) that contradicts its essence, or else associate with it an alien principle standing in communion with it (a soul), in which case, however, if such a product is to be a product of nature, organized matter as an instrument of that soul is already presupposed, and thus makes that product not the least more comprehensible, or else the soul is made into an artificer of this structure, and the product must be withdrawn from (corporeal) nature. Strictly speaking, the organization of nature [organism] *is therefore not analogous with any causality that we know*. (Kant, 1790/2000, 5: pp. 374–375, emphasis added)

Kant reached perhaps the deepest point implied by the persistence of teleology in biology: life, different from matter in kind, cannot be misidentified as any organization of matter; life, evidently having no causal influence on living objects, can neither be misconstrued as mind.[15]

10.6 Conclusion

This chapter articulates, defends and deepens Nagel's view of teleology in biology. Regarding teleological terms in biological discourse, although Nagel accepted their legitimacy, he declined to view them as unique to biology. Nagel's position stems from his endorsement of eliminativism. Eliminativism claims that teleological terms can be eliminated from biological discourse, without any detrimental consequence

[15] As is often known to Kant scholars, Kant conceived of the principle of teleology (and life) as merely regulative rather than constitutive. Then some judged Kant's pessimism over a scientific biology (due to the lack of constitutive principles) inadequate (e.g., Zammito, 2006). However, my view is that Kant's sophisticated discussion on teleology and life remains little understood, due to the failure of clarifying different meanings of the regulative-constitutive distinction. For details, see Friedman (1992) and Chen (2019, pp. 275–91).

to biological knowledge itself. In particular, teleological statements and explanations in biology can all be translated into non-teleological ones, with no loss of meanings; as a result, teleological terms are not necessary to expressing biological knowledge.

Nagel's view, especially his thesis of eliminativism, can be defended by considering the current biological function debate. Since accounts of biological function offered in this debate are necessarily non-intentional, function terms can be replaced by using ordinary, non-problematic physical and biological terms. Therefore, eliminativism remains valid for the biological function debate.

Nagel's view can also be deepened. Even though Nagel was committed to the thesis of eliminativism, he also offered his own accounts of biological function and teleology in general. Then, regarding the biological function debate, to deepen Nagel's view, it seems useful to distinguish its descriptive dimension from its prescriptive dimension. The thesis of eliminativism concerns the prescriptive dimension and offers rules on how to use function terms in biological discourse. Accounts of biological function address the descriptive dimension, which tends to capture actual biological usage. The distinction between the prescriptive dimension and the descriptive dimension helps clarify the biological function debate. In its prescriptive dimension, the biological function debate remains unfortunately suspicious. Similar to eliminativism, all accounts of biological function can be accepted if treated as mere prescriptive rules, as none of them is able to affect the expression of biological knowledge in any essential manner. This gives rise to a trivial prescriptive pluralism. In its descriptive dimension, the biological function debate concerns indeed a social-linguistic question about linguistic habits of biologists. While this is certainly a meaningful empirical question in itself, to which a descriptive pluralism is the most plausible answer today,[16] it is difficult to be convinced about its biological relevance. Strictly speaking, it is not a biological question at all.

Finally, perhaps a more intriguing question should concern the persistence of teleology in biology. Here Nagel's view can be deepened even more. Given that teleological terms in biological discourse are eliminable in principle, why do biologists develop an almost inevitable tendency of using them? To this question, in my view, Kant offered a deeper answer. Indeed, the persistence of teleology in biology pertains to the peculiarity of the concept of life itself. Life stands between matter and mind. When teleological terms are retained in biological discourse by receiving non-intentional, mechanistic accounts, life tends to "incarnate" itself in matter; when teleological terms in biological discourse allude to-but finally fail to receive intentional accounts, life attempts to "imitate" mind.

[16] As one reviewer helpfully points out, at least in the descriptive dimension of the debate there remains the possibility of selecting a best account in terms of its ability to describe and predict relevant biological usage. Indeed, even though pluralism might be the best solution given current biological usage, it is still plausible that in a different spatial–temporal world a dominant account exists as it best describes and predicts biological usage in that world.

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Part IV
The Social Sciences and the Social Role of Philosophy

Chapter 11
Nagel on the Methodology of the Social Sciences



Matthias Neuber

Abstract Ernest Nagel was one of the first philosophers of science who reflected systematically on the methodology of the social sciences. His cooperation with Paul F. Lazarsfeld at Columbia University proved to be instructive in this regard. Moreover, Nagel stood in close contact with representatives of sociological functionalism and published, in 1956, a contribution on the prospects of a formalization of functionalism. In his seminal *The Structure of Science* from 1961, Nagel devoted two long chapters to methodological and explanatory problems of the social sciences. The aim of the present chapter is to rationally reconstruct this 1961 account. I will proceed in three steps: first, I shall shed some light on Nagel's discussion of the problem of a clear-cut demarcation between the natural and the social sciences; then Nagel's interpretation of the presumptive 'immaturity' of the social sciences will be taken into account; finally, two case studies will be provided in order to illustrate the implications of Nagel's particular analysis.

Keywords Social sciences · Methodology · Explanation · Reduction · Value judgments

11.1 Introduction

The philosophy of the social sciences is a rather young discipline. In point of fact, it became fully established not before the mid-1960s among analytic philosophers. However, there were some stage-setting publications already in the 1950s. For example, Herbert Feigl and May Brodbeck's *Readings in the Philosophy of Science* from 1953 contains an entire chapter on the philosophy of the social sciences, with (among others) contributions by Theodore Abel, John Passmore, Sidney Hook, John Watkins, and Ernest Nagel. Already one year earlier, Nagel had published a paper on "Problems of Concept and Theory Formation in the Social Sciences," and in 1956 his "A Formalization of Functionalism" appeared. In 1958 Peter Winch's influential *The*

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Idea of a Social Science and Its Relation to Philosophy came out. And in 1961 Nagel published his *The Structure of Science*, with two long chapters on methodological and explanatory problems of the social sciences.

How did it come about that Nagel was so present in the formative phase of the philosophy of social science? There can be little doubt that the intellectual atmosphere at Columbia University played a crucial role in this regard. For one thing, Robert K. Merton (one of the 'founding fathers' of modern American sociology) taught at the Columbia University sociology department. In 1949, he had published a paper on "Manifest and Latent Functions," in which he was able to establish some sort of functional paradigm for the social sciences. Nagel's (1956) "A Formalization of Functionalism" surely resulted from intense personal discussions with Merton on the issue. Nagel's collaboration with Merton's colleague (and close friend) from the sociology department, Paul F. Lazarsfeld, turned out to be particularly instructive. Thus in the preface to *The Structure of Science*, Nagel points out: "I have obtained […] invaluable instruction on the methodological problems of empirical social research from the enlightening talks I have had with Paul F. Lazarsfeld for many years" (Nagel, 1961, p. x). As is well known, Nagel and Lazarsfeld regularly taught a common seminar on the logic of social inquiry (see Sills, 1987, p. 268; Turner, 2009, p. 198). Lazarsfeld himself also engaged publicly in the philosophy of social science. Thus in a paper given at the 1960 International Congress of Logic, Methodology and Philosophy of Science, Lazarsfeld complained: "Philosophers of science do not pay attention to the empirical work in social research which is actually going on today" (Lazarsfeld, 1966, p. 463). However, somewhat later he qualified this by stating:

> I do not want to claim that we have received no help at all from the philosophers of science. Ernest Nagel has analyzed a paper by Robert Merton on functional analysis which testifies to a close reading of the original text and as a result makes a fine contribution to Merton's effort. And I myself am certainly grateful to the way he has integrated my own work on survey analysis into a more general treatise on the philosophy of the social sciences [see Nagel (1961), 509–520]. (Lazarsfeld, 1966, p. 471).

Lazarsfeld could also have mentioned Carl Gustav Hempel who, in 1959, had published a paper on "The Logic of Functional Analysis" (and who extensively discussed explanatory problems of the social sciences in his 1965 *Aspects of Scientific Explanation*).

In what follows I am going to investigate Nagel's (1961) approach toward the social sciences. I will proceed in three main steps: first, I shall shed some light on Nagel's discussion of the problem of a clear-cut demarcation between the natural and the social sciences; then Nagel's interpretation of the presumptive 'immaturity' of the social sciences will be taken into account; finally, two case studies will be provided in order to illustrate the implications of Nagel's particular analysis.

11.2 Problems of a Clear-Cut Demarcation

Chapter 13 of *The Structure of Science* is titled "Methodological Problems of the Social Sciences". More precisely, the chapter contains a discussion of the following five problems: (1) the problem of controlled experiments; (2) the problem of the cultural relativity of social laws; (3) the problem of the variability of social knowledge; (4) the problem of the subjectivity of social subject matter; (5) the problem of the value-oriented bias of social inquiry. All of these problems are associated with worries concerning the "real science" status of social inquiry (see Nagel, 1961, p. 449). In point of fact, they pertain to the question if (and how) a clear-cut demarcation between the social and the natural sciences can be drawn so that the idea of a unity of science still makes sense or whether there is really an unbridgeable methodological gulf between both fields of inquiry, as so often is proclaimed by philosophers and social scientists themselves. A brief survey of Nagel's discussion of these problems, then, is worth the effort.

Begin with the problem of controlled experiments. Here, Nagel's initial question is whether controlled experimentation is a *sine qua non* for establishing general scientific laws. His answer is short and in the negative: as he points out, neither astronomy nor astrophysics is supposed to be an experimental science, although both disciplines avail of general scientific laws (see Nagel, 1961, p. 452). However, next to theory building, experimentation no doubt forms an essential part of the majority of scientific endeavors. Accordingly, it could be further asked: "Is there in fact only a negligible possibility in the social sciences for controlled empirical procedure?" (ibid.) Nagel thinks this not the case. On his view, there are at least two types of controlled empirical procedure in the social sciences: laboratory experiments and so-called field experiments. Whereas laboratory experiments are performed in artificially created miniature social systems, field experiments are carried out in 'natural' social settings, i.e., outside the laboratory. Concerning the first, Nagel (1961, p. 456) mentions the example of experiments made on the behavior of children when the conditions under which they engaged in play activities were varied. As for the second kind of experimentation, he brings the example of different groups of factory workers being more or less productive depending on their being organized more or less 'democratically' (Nagel, 1961, p. 457). To be sure, experiments such as these might be less reliable and more prone to deception than experiments performed in the natural sciences. But this is only a matter of degree. In general, Nagel assumes there is no reason to postulate a principled methodological separation between the social and the natural sciences: in both areas, controlled experimentation forms an integral part of the research process.

The second problem, the problem of the cultural relativity of social laws, grows out of the circumstance that there are obviously no "transcultural"—invariant—laws of social phenomena and hence no means of deducing precise social long-range predictions. This, in turn, seems indeed to suggest a sharp contrast to the natural sciences. For example, Snell's law of the refraction of light formulates relations between optical phenomena that are apparently invariant throughout the universe.

On the other hand, human birth rates for instance vary with status quite differently from cultural community to cultural community. However, as Nagel (1961, p. 461) makes it clear, in most domains of physical inquiry both invariant laws and long-range predictions are the product of certain *ceteris paribus* maneuvers.[1] Thus, Snell's law holds only for media whose optical properties are isotropic (although most of optical media are anisotropic). Accordingly, the law is restricted to special circumstances, and predictions can only be made within the limits of this provision. Moreover, there are many cases where physicists cannot alleviate ignorance of the relevant initial conditions necessary for making precise forecasts. It is for this reason that, for example, the movement of a leaf just fallen from a tree cannot be predicted with much accuracy. Nagel states:

> [W]e rarely if ever have at our command knowledge of such initial conditions. Inability to forecast the indefinite future is thus not unique to the study of human affairs, and it is not a certain sign that comprehensive laws have not been established or cannot be established about the phenomena (Nagel, 1961, p. 461).

Interestingly enough, Nagel briefly alludes to the option of establishing invariant "relational structures" (p. 462) in the social sciences. Given that option, transcultural social *laws* are within reach. Or so it is argued.[2]

The third problem, the problem of the variability of social knowledge, has to do with the relationship between hypotheses and their respective evidential base. In the case of the social sciences, this relationship becomes problematic because processes of gathering evidence for some hypothesis are frequently created by the gathering process itself. Nagel's example is interviews in the context of social research. Here, the respondent usually *knows* that s/he is interviewed, and this very fact might influence or even change her/his actual habitude or opinion. Nagel concedes that this difficulty is "undeniably a serious one" (1961, p. 467). However, "interaction with the measuring instrument" (ibid.) is nothing peculiar to the social sciences. One must also attribute it to the natural sciences, the most salient example being the dependence of the observation of phenomena on the observer and the instruments used in quantum mechanics. Nagel (1961, p. 467) therefore declares: "In both groups of disciplines, the difficulty arises because changes are produced in a subject matter by the means used to investigate that subject matter."

Closely connected with this issue is problem number four, the problem of the subjective nature of social subject matter. In social inquiry, it is often supposed, the subject matter *itself* is radically subjective and thus prevents the acquisition of purely objective knowledge. Nagel refers the reader here to respective works by Max Weber, Charles H. Cooley, and Peter Winch (see Nagel, 1961, p. 475, fn. 16). Yet he is not convinced that what is dealt with in the social sciences is subjective or psychological by its very nature. Nagel writes: "[E]ven when the behaviors studied by the social sciences are indisputably directed toward some consciously entertained ends, the social sciences do not confine themselves to using only distinctions that refer to psychological states exclusively" (1961, p. 475).

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[1] This, of course, reminds us of Nancy Cartwright's later *How the Laws of Physics Lie* (1983).

[2] For a more current account of social laws as invariant relational structures, see Emirbayer (1997).

What, then, is the alternative? On Nagel's view a certain variant of *behaviorism* should be taken seriously in this context. Relying, among other sources, on a paper by Lazarsfeld (1959), he pleads for the acceptance of behaviorism not as a "substantive theory", but rather as a "methodological approach" (1961, p. 478). According to this brand of methodological behaviorism, it would be erroneous to maintain that a social scientist can only account for men's actions if he has experienced in his own person the psychic states he imputes on them. Thus Nagel rhetorically asks: "Are mild-tempered and emotionally stable social scientists unable to understand the causes and consequences of mass hysteria, institutionalized sexual orgy, or manifestations of pathological lust for power?" (1961, p. 483) This is surely not the case. Social scientists have at their command a wide range of symbolic and propositional forms enabling them to 'objectify' the very matters they deal with. In consequence, we can, according to Nagel, objectively *know* that, for example, a man fleeing from a crowd that is animated by hatred toward him is in a state of fear—*just as* we can know that the temperature of a piece of wire is rising because the velocity of its constituent molecules is increasing. Just as we do not have to know what it is like to be a rapidly moving molecule, we do not have to imagine (or 'understand') the particular mental state of the fleeing man in order to know he is in fear.[3] Nagel therefore concludes: "[T]he logical canons employed by responsible social scientists in assessing the objective evidence for the imputation of psychological states do not appear to differ essentially […] from the canons employed for analogous purposes by responsible students in other areas of inquiry" (p. 484).

The fifth and last problem, the problem of the value-oriented bias of social inquiry, can be sketched very briefly for the moment: we will encounter it more extensively in Sect. 11.4. In a nutshell, the problem is that in social inquiry the aim of *value neutrality* seems impossible to be achieved. Personal values of individual researchers apparently prevent the adoption of a neutral and balanced sight on social issues. At any rate, it can hardly be denied that social scientists select their topics according to their particular interests. However, for Nagel, quite the same is true with inquiry into inanimate nature. Just as with social research, disciplines such as physics are, as it were, interest-driven. But this does not hinder the *quest for objectivity*. In Nagel's own words:

> [T]here is no difference between any of the sciences with respect to the fact that the interests of the scientist determine what he selects for investigation. But this fact, by itself, represents no obstacle to the successful pursuit of objectively controlled inquiry in any branch of study (Nagel, 1961, pp. 486–487).

As I already noted, we will come back to this below.

On the whole, then, Nagel's analysis points toward a definite rejection of what might be called methodological dualism. For him, the natural and the social sciences form a methodological *continuum*. Therefore, contemporary dualistic accounts, such as C. P. Snow's diagnosis of the 'two cultures' (Snow, 1959), are out of place in Nagel's view. Probably inspired by logical empiricist ideals, Nagel sought a unified

[3] For a very similar account, see the paper by Theodore Abel (1948), which is quoted by Nagel in the context given.

conception of *all* the sciences.[4] However, as we will see in a moment, Nagel had some serious reservations about the actual state of the art in accomplishing such a unification.

11.3 The 'Immaturity' of the Social Sciences

In his *The Structure of Scientific Revolutions* (1962), Thomas Kuhn devaluates the social sciences as immature and 'pre-paradigmatic.' Nagel, in contrast, is more charitable—but not uncritical. Thus, Chap. 14 of *The Structure of Science* is devoted to the purported 'immaturity' of the social sciences. Nagel's first observation in this context is that experimental laws in the social sciences are "perhaps exclusively statistical" (1961, p. 504). This in turn, Nagel explains, is due to four reasons: (1) the inherent complexity of social phenomena; (2) the element of volition that enters into the determination of human conduct; (3) the fact that there is no stable microstructure at the fundamental level of social research; (4) the fact that there are no adequate theoretical concepts in social research. Yet, as Nagel extensively elucidates by referring to a paper by Lazarsfeld (1955), the latter's conception of *relative frequencies* should be considered a promising approach of representing "empirically established relations of statistical dependence" (Nagel, 1961, p. 510) after all.[5] As a matter of fact, Nagel offers an interpretation of Lazarsfeld's account of statistical laws and (causal) explanations in the social sciences. As such, it is a topic of its own and outside the discussion at hand. Suffice it to mention that Nagel thought of Lazarsfeld's account as an important step toward strengthening the social sciences' explanatory and law-like (though mainly statistical) structure.[6]

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[4] That the logical empiricist Philipp Frank (whom Nagel befriended) might have played some role in this context is hinted at in Tuboly (2017, p. 266, fn. 15). At any rate, in the preface to *The Structure of Science*, Nagel declares: "Neither Rudolf Carnap nor Philipp Frank have been formally my teachers, but I have profited greatly from the numerous conversations I have had with them since 1934 on the logic of science." (1961, p. x) It is highly probable, I claim, that Nagel and Frank also engaged in conversations about the logical status of the *social* sciences.

[5] That Nagel himself was a frequentist in the theory of probability is documented by his early *Principles of the Theory of Probability* (1939). On Nagel's ideas about probability, see Maria Carla Galavotti's chapter in this volume.

[6] As Marij van Strien convincingly shows in her contribution to this volume, Nagel considered *determinism* a "guiding principle" also in the social sciences. Thus, in the long run, the social sciences in his view should also be in the possession of 'absolute' (and not merely statistical) laws. At the very end of *The Structure of Science* he therefore proclaims: "A dogmatic insistence upon the use of some special form of the deterministic principle has undoubtedly often hindered the advance of knowledge […]. Nevertheless, to abandon the deterministic principle itself is to withdraw from the enterprise of science. However acute our awareness may be of the rich variety of human experience, and however great our concern over the dangers of using the fruits of science to obstruct the development of human individuality, it is not likely that our best interests would be served by stopping objective inquiry into the various conditions determining the existence of human traits and actions, and thus shutting the door to the progressive liberation from illusion that

Nagel's next observation pertains to the issue of functionalism. Here, it must be seen that the discussion of this issue dominated most theoretical discourse in the social sciences during the 1950 and 1960s (see Giddens, 1976; further, with special focus on Merton, Turner, 2009). Historically, the functionalist approach grew out of investigations in social anthropology (see esp. Malinowski, 1944). Merton (see esp. Merton, 1949), Talcott Parsons (see esp. Parsons, 1951), and the social anthropologist Alfred Radcliffe-Brown (see esp. Radcliffe-Brown, 1952) helped to establish the functional 'paradigm' within sociology proper. In programmatic terms, functionalism amounted to a non-reductive position in theoretical social science. Thus its advocates refused reductionist positions such as class analysis and historical materialism. In his 1956 "A Formalization of Functionalism," Nagel critically discussed the functionalist idea, especially in the form defended by Merton. He vehemently rejected functionalism as a global theory of social phenomena and at the same time welcomed *functional explanation* as a legitimate objective, being grounded in biological (viz. teleological) analyses of self-regulatory processes (see already Nagel, 1953 in this context). In point of fact, Nagel offered a translation of Merton's analysis into a framework of formalized explanatory teleological propositions. Thus, his 1956 approach to Merton's analysis had an essentially constructive aspect. In *The Structure of Science*, on the contrary, the focus lies exclusively on the critical aspects concerning functionalism. Nagel discusses extensively the following three objections: (1) the notion of function is notoriously ambiguous; (2) functionalist models draw on a dubious conception of the 'survival of a society'; (3) the notion of a 'social structure,' as employed by many functionalists, is not empirically operationable. On the whole, Nagel (1961, p. 535) comes to the conclusion that "functional analyses […] cannot be rightly regarded as illustrating a distinctive theoretical approach in the study of human affairs".[7]

Like the issue before, Nagel's critique of functionalism is a topic of its own and cannot be further commented on here. Let us instead take a brief look at Nagel's final observation as to the contemporary state of the social sciences, the issue of methodological individualism. The principal motivation behind this particular stance is the conviction that the hypostatization of so-called collective terms such as 'the American people,' 'corporation,' or 'degree of social cohesion' must be avoided. According to Nagel's paraphrasing of methodological individualism, such hypostatizations lead to "irresponsible intellectual constructions" (1961, p. 535). Just as with vitalistic assumptions in biology, hypostatic interpretations in the social sciences are (again in Nagel's paraphrasing words), "useless as guides in inquiry and sterile as premises

comes from the knowledge achieved by such inquiry" (1961, p. 606). See also Nagel (1960) in this connection.

[7] For a very similar (if not identical) assessment, see already Kingsley Davis (1959, p. 757), according to whom functionalism "rests on the false assumption that there is a special method or body of theory called functional analysis which can be distinguished from other methods or theories within sociology and social anthropology. It is not that the work done under the functional label is poor or unscientific (quite the contrary), but rather that the label itself signalizes and fosters the myth of a homogeneous mode of analysis distinct from other sociological modes of analysis".

in explanations" (ibid.). They have, in other words, the status of (eminently unscientific) metaphysical speculations. Therefore, the methodological individualist goes with the strategy of *reduction*: collective terms, for example 'corporation,' should be reduced to exclusively individual terms such as 'factory worker', 'member of the parliament,' or 'student in medical school.' All of this sounds pretty close to Nagel's own overall empiricist outlook as well as to his theory of reduction as it is outlined in *The Structure of Science*. Somewhat surprisingly, though, he thinks of methodological individualism as misguided or at least premature. We will see in a moment why.

11.4 Two Case Studies

Thus far, our reconstruction of Nagel's approach toward the methodology of the social sciences has confined itself to a survey of the principal topics treated in the two relevant chapters of *The Structure of Science*. In this section, a more detailed discussion will be provided. More precisely, two case studies are presented to help illustrate the implications of Nagel's particular analysis. First, the portrayal of Nagel's discussion of methodological individualism will be continued more extensively. Then we will—as announced in Sect. 11.2—come back to the issue of the value-oriented bias of social inquiry and, in doing so, flesh out what has been said so far in that very context.

11.4.1 The Case of Methodological Individualism

As already outlined, methodological individualism is—at least in certain of its variants—closely connected with the strategy of reduction. For example, according to John Watkins—student of Karl Popper and one of the main proponents of methodological individualism—, the social scientist has to "continue to search for explanations of a social phenomenon until he has reduced it to psychological terms" (Watkins, 1952, p. 29). As Watkins puts it at another place, "the ultimate constituents of the social world are individual people who act more or less appropriately in the light of their dispositions and understanding of their situation" (Watkins, 1957, p. 106).[8] And he continues:

> If methodological individualism means that human beings are supposed to be the only moving agents in history, and if sociological holism means that some superhuman agents or factors are supposed to be at work in history, then these two alternatives are exhaustive (Watkins, 1957, p. 106).[9]

[8] As Nagel (1961, p. 540) correctly observes, the conception of *Verstehende Soziologie* is lurking in the background here.

[9] This of course reminds us of Popper's *The Poverty of Historicism* (also published in 1957).

The demand of reduction of statements about social phenomena to psychological statements about individual human conduct seems to follow immediately from this.[10]

However, according to Nagel, one must be careful in this regard. To see why, it is helpful to insert a short excursion to this chapter of *The Structure of Science*, the chapter devoted to the issue of reduction. There (1961, p. 364), Nagel distinguishes between two forms or, better, understandings of 'reduction:' reduction of *properties* and reduction of *statements*. Reduction of properties pertains to ontology and has thus a metaphysical ring to it. Reduction of statements, on the other hand, takes place at the level of language and of *language systems* such as scientific theories. Whereas reduction of statements is entirely appropriate (and actually happens in scientific practice) in Nagel's view, reduction of properties is problematic and misleading because it "converts what is eminently a logical and empirical question into a hopelessly irresolvable speculative one" (1961, p. 365). Note that, for Nagel, scientific theories must be conceived of as "systems of statements" (1961, p. 364) so that a reduction can only be performed at an essentially linguistic (or syntactic) level.[11]

Now, coming back to the issue of methodological individualism, it is interesting to see that Nagel (1961, p. 542) draws a similar distinction here, namely the distinction between what he calls "the ontological thesis" and "the reductive thesis". The ontological thesis implies that the *ultimate constituents* of the social world are individual human agents, whereas the reductive thesis implies that *statements* about social phenomena are deducible from *statements* about human individuals. According to Nagel some of the proponents of methodological individualism, for example Watkins, fail to distinguish between the two theses. This in turn obscures their view because a commitment to the ontological thesis "does not logically require" (ibid.) a commitment to the reductive thesis. Since Nagel himself would only go with the reductive thesis, the question remains why even a non-ontological interpretation of methodological individualism is not satisfactory according to his standards.

In order to adequately answer this question, a further short excursion to this chapter of *The Structure of Science* is in order. There (1961, pp. 353–354), Nagel introduces a distinction between two formal requirements in the context of reduction: the "condition of connectability" and the "condition of derivability." The condition of connectability pertains to the level of scientific *terms* and requires that there must exist some (logically or empirically) reconstructable linkage between the reducing term and the term reduced. Nagel employs the example of the relationship between the notion of temperature (term reduced) and the notion of the mean kinetic energy of gas molecules (reducing term) in physics in this context (1961, pp. 356–357). As he makes it clear, the linkage between both terms is guaranteed by certain "rules of

[10] However, Popper himself rejected such a demand for reduction. See Popper (1966, ch. 14), where he argues *against* the view that "society being the product of interacting minds, social laws must ultimately be reducible to psychological laws, since the events of social life, including its conventions, must be the outcome of motives springing from the minds of individual men" (p. 90).

[11] As is well known, this became later dubbed the 'received view' of scientific theories. See, e.g., Suppe (1972). For a recent defense, see Lutz (2012).

correspondence" (1961, p. 356).[12] However, as he further points out, the condition of connectability is not sufficient for reduction and must be supplemented by the second condition, the condition of derivability. This condition pertains to the level of scientific *statements* and requires that the statement to be reduced must be logically derivable from the reducing statement(s). Nagel's example in this context is the derivation of Boyle-Charles' law in thermodynamics from the kinetic theory of gases (1961, p. 355).[13]

Coming back again to the issue of methodological individualism, it should be noted first that Nagel applies the condition of connectability to the relationship between collective terms and individual terms in the social sciences. The latter are, according to methodological individualism, to be conceived of as essentially psychological terms. As Nagel points out, the linkage between both kinds of terms, as required by the condition of connectability, can only be achieved "by way of suitable rules of correspondence or of various empirical hypotheses" (1961, p. 542). However, neither of these alternatives helps achieving the overall objective of methodological individualism. For, according to Nagel,

> although either alternative would make it possible to *verify* statements containing collective terms by observing the conduct of individuals, neither alternative would permit the *elimination* of collective terms from such statements in favor of individual terms exclusively (Nagel, 1961, p. 542).

Unfortunately, Nagel provides no argument for this. But be that as it may, the *decisive* formal requirement in the context of reduction is the condition of derivability.[14] According to this condition, it should—given the framework of methodological individualism—be possible to logically derive statements about social phenomena from purely psychological statements about individual human conduct. Apparently, though, Nagel is rather skeptical about the realizability of this condition in the given framework. His principal reason is that

> no set of premises about the conduct of *individual* human beings might suffice for deducing some given statement about the actions of a *group* of men, and that at least one assumption of the latter kind might be required in any set of premises from which the given statement is deducible (Nagel, 1961, pp. 542–543; original emphases).

Accordingly, the demand set by methodological individualism is too strong. Even in physics, Nagel comments, it is often the case that a given set of premises needs to be supplemented by certain special assumptions in order to effect the desired reduction. For example, by reducing thermodynamics to statistical mechanics it does not suffice to simply lay down premises pertaining to mechanical attributes of individual

[12] It should be noted that, for Nagel, such rules of correspondence "receive no explicit formulation; and in actual practice the coordinations are comparatively loose and imprecise" (1961, p. 99).

[13] Reasons of space prevent a more detailed examination of Nagel's general theory of reduction. But see Schaffner (2012).

[14] Thus, in this chapter of *The Structure of Science*, Nagel states that while the condition of connectability is not sufficient for reduction, the condition of derivability is both necessary and sufficient for reduction, since "derivability obviously entails connectivity" (1961, p. 355, fn. 5).

molecules. Rather, it is always (Gibbsian) *ensembles* of molecules that form the basis of the reductive move in this connection.[15] Replace 'ensemble' by 'group' and the analogy with the social sciences becomes complete.

Nagel goes one step further by considering an example from economics. More concretely speaking, he confronts the microeconomic approach established by Alfred Marshall in his influential *Principles of Economics* (1890) with Marshall's student's John Maynard Keynes' contributions to macroeconomics during the 1930s (see esp. Keynes, 1936). In one of its varieties, the microeconomic view goes along with so-called marginal utility theory. According to this theory (whose classic formulation goes back to Marshall and was later, among others, further developed by Ludwig von Mises and Friedrich August von Hayek who, in turn, built on the parallel 'discovery' of marginal utility by Carl Menger), economic phenomena are to be analyzed in terms of assumptions concerning the economic preferences of individual consumers. The main objective of the theory is to reduce the totality of economic operations in a society to propositions characterizing those operations from premises pertaining to beliefs, desires, and resources of individual economic agents. Nagel (1961, p. 543) comments: "The objectives of microeconomics are […] in complete agreement with the program of methodological individualism." And indeed: some of the main proponents of microeconomics, especially in its variant of marginal utility theory, were *explicit advocates* of methodological individualism (see esp. Hayek, 1948; Mises, 1949). However, Nagel is all but impressed by the microeconomic 'style of reasoning.' In his view, "microeconomics fails to account for various important features that frequently mark the total economies of nations (such as recurrent unemployment crises) and does not provide effective tools for controlling the course of large-scale economic events" (Nagel, 1961, p. 543).

So what about the alternative, macroeconomics (or Keynesianism)? In Nagel's view, the macroeconomic approach is entirely legitimate—and also empirically adequate—, *although* there can obviously not be effected a deduction from microeconomic premises. What is more, macroeconomics has a wider range than its microeconomic alternative. Nagel writes:

> [T]he basic postulates of the theory are not exclusively "psychological" ones about *individual* economic agents, but include assumptions concerning relations between large-scale *statistical aggregates* (such as national income, total national consumption, and total national savings) (Nagel, 1961, p. 544; original emphases).

All in all, Nagel comes to the specific conclusion that

> if […] macroeconomic assumptions enable economists to account for aggregative phenomena no less adequately than do microeconomic postulates, the reduction of macroeconomic to microeconomic explanations appears to offer no substantial scientific advantages (Nagel, 1961, p. 544).

The more general conclusion at which Nagel arrives is that methodological individualism in its reductionist form does not possess "the unqualifiedly pre-eminent merits that are claimed for it" (1961, p. 546).

[15] For a more recent discussion of this issue, see Callender (1999).

Regarding the most outstanding implication or consequence of Nagel's critique of methodological individualism, it can be summarized that the very issue of reduction is significantly limited by Nagel: what might be possible within the physical sciences is apparently hard to achieve within the social sciences. Accordingly, any form of *radical reductionism* is precluded (at least for the state of social science of the day). On the other hand, as we have seen in Sect. 11.2, Nagel nevertheless argued for the methodological unity between the social and the natural sciences. His particular position has therefore aptly been characterized as "nonreductive naturalism" (see Uebel, 2003, p. 70).

11.4.2 The Case of the Value-Oriented Bias of Social Inquiry

It was Max Weber who famously argued that values do affect the social scientist's choice of problems and her/his identification of social facts. Although Weber counts as one of the main advocates of *value-free* social science, he nevertheless went as far as to argue that

> [o]nly a small portion of existing concrete reality is colored by our value-conditioned interest and it alone is significant to us. It is significant because it reveals relationships which are important to us due to their connection with our values. Only because and to the extent that this is the case is it worthwhile for us to know it in its individual features. We cannot discover, however, what is meaningful to us by means of a "presuppositionless" investigation of empirical data. Rather perception of its meaningfulness to us is the presupposition of its becoming an *object* of investigation (Weber, 1947, p. 76; original emphasis).

Of course, Weber at the same time maintained that the *evidenced results* of social research are not similarly influenced by values, but rather valid for all and thus objective.[16]

Now, as we have already seen in Sect. 11.2, Nagel has no problem with allowing values playing a role in the context of both the social and the natural sciences. But just as with Weber, he does not see an obstacle for objectivity in this. Nagel writes:

> [M]odern science encourages the invention, the mutual exchange, and the free but responsible criticism of ideas; it welcomes competition in the quest for knowledge between independent investigators, even when their intellectual orientations are different; and it progressively diminishes the effects of bias by retaining only those proposed conclusions of its inquiries that survive critical examination by an indefinitely large community of students, whatever be their value preferences or doctrinal commitments. It would be absurd to claim that this institutionalized mechanism for sifting warranted beliefs has operated or is likely to operate in social inquiry as effectively as it has in the natural sciences. But it would be no less absurd to conclude that reliable knowledge of human affairs is unattainable merely because social inquiry is frequently value-oriented (Nagel, 1961, pp. 489–490).

However, there is a "sophisticated" (1961, p. 490) or radicalized version of the value-ladenness thesis. According to this radicalized version, it is *in principle impossible* to attain an "ethically neutral social science" (ibid.) The reason for this is that

[16] For further details, see the brilliant reconstruction in Ringer (1997).

it is assumed that fact and value are so intimately fused that they cannot be distinguished from each other. For example, the political philosopher Leo Strauss declares at one place:

> The prohibition against value-judgments in social science would lead to the consequence that we are permitted to give a strictly factual description of the overt acts that can be observed in concentration camps, and perhaps an equally factual analysis of the motivations of the actors concerned: we would not be permitted to speak of cruelty. Every reader of such a description who is not completely stupid would, of course, see that the actions described are cruel. The factual description would, in truth, be a bitter satire (Strauss, 1951, p. 211).

Nagel concedes that it is indeed often difficult "to distinguish between the purely factual and the 'evaluative' contents of many terms employed in the social sciences" (1961, p. 491). However, at the same time Nagel maintains that assessments like Strauss' are overdrawn and not differentiated enough. It is for this reason that he introduces a distinction between *two kinds* of value judgments, epistemic "characterizing value judgments" on the one hand and non-epistemic "appraising value judgments" on the other (1961, pp. 492–493).

Nagel (1961, p. 492) explains the difference between the two kinds of value judgments as follows: While *characterizing* value judgments express "an *estimate* of the degree to which some commonly recognized type of action, object, or institution is embodied in a given instance" (original emphasis), an *appraising* value judgment expresses "*approval or disapproval* either of some moral (or social) ideal, or of some action (or institution) because of a commitment to such an ideal" (original emphasis). As a preliminary example to illustrate the distinction Nagel considers the following case from biology. The diagnosis of anemia implies a characterizing value judgment if an evaluative statement is uttered for the sake of expressing that a reduced number of red blood corpuscles is in some degree present (or absent) in a given instance. The very same diagnosis implies an appraising value judgment if an evaluative statement is uttered for the sake of expressing an undesirable condition concerning powers of self-preservation. The crux of the matter, Nagel maintains, is that "although characterizing judgments are necessarily entailed by many appraising judgments, making appraising judgments is not a necessary condition for making characterizing ones" (1961, p. 493). Consequently, an ethically neutral social science would admit characterizing value judgments but be free from appraising value judgments.

For example, in the context of the sociology of religion, one can distinguish between mercenary and non-mercenary attitudes of some community towards its acknowledged gods. Yet Nagel assumes it possible—and required—to restrict *scientific analysis* to characterizing value judgments, although in *everyday usage* terms like 'mercenary' have a widely recognized pejorative tone. Nagel points out:

> [T]he sociologist who claims that a certain attitude manifested by a given religious group is mercenary, just as the physiologist who claims that a certain individual is anemic, is making what is primarily a characterizing value judgment. In making these judgments, neither the sociologist nor the physiologist is necessarily committing himself to any values other than the values of scientific probity; and in this respect, therefore, there appears to be no difference between social and biological (of for that matter, physical) inquiry (Nagel, 1961, pp. 493–494).

Remember that it was Nagel's principal claim in this context that the natural and the social sciences form a methodological continuum. Thus both areas are 'infiltrated' with interest-guiding value judgments; but these value-judgments are only of the characterizing and not of the appraising sort.[17]

However, it could be objected that Nagel's account is hopelessly naïve because (just like Weber) he underestimates the actual range of value-ladenness in social science. In other words, it could be claimed that even the very *assessment of evidence* is 'appraising-value-laden' in the social scientific context. Nagel discusses this objection under the heading of "historical relativism" (1961, pp. 498–502). On this view, social institutions and cultural practices and conventions are constantly changing so that the intellectual instruments for understanding and validating them must also change. Nagel is quoting Karl Mannheim, one of the founders of the sociology of knowledge, in this connection. The latter writes at one place:

> Every epoch has its fundamentally new approach and its characteristic point of view, and consequently sees the "same" object from a new perspective. […] The very principles, in the light of which knowledge is to be criticized, are themselves found to be socially and historically conditioned. Hence their application appears to be limited to given historical periods and the particular types of knowledge then prevalent (Mannheim, 1959, pp. 288 and 292).

Nagel's reaction to this radically relativist account is mainly rhetorical, but not toothless after all. He reminds the reader that

> even extreme exponents of the sociology of knowledge admit that most conclusions asserted in mathematics and natural science are neutral to differences in social perspective of those asserting them, so that the genesis of these propositions is irrelevant to their validity. Why cannot propositions about human affairs exhibit a similar neutrality, at least in some cases? (Nagel, 1961, p. 500).[18]

And Nagel continues:

> [L]et us ask what is the cognitive status of the thesis that a social perspective enters into the content as well as the validation of every assertion about human affairs. Is this thesis meaningful and valid only for those who maintain it and who thus subscribe to certain values because of their distinctive social commitments? (Nagel, 1961, p. 500)

Beyond the shadow of a doubt, historical relativism runs danger of being self-defeating. Nagel therefore closes the discussion at this point.

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[17] One of the reviewers of this chapter suggests that interest-guiding value judgments and characterizing value judgments do *not* overlap since interest-guiding value judgments stand outside of the system of scientific propositions and guide the selection of problems to work on, whereas characterizing value judgments are part of the system of scientific propositions itself. This might be so, but I do not see any possibility to disentangle value judgments from *any* kind of interest. In point of fact, values in general and interests in general go hand in hand with each other (at least in my own down-to-earth—logical-empiricist—understanding of values).

[18] Ádám Tuboly (personal communication) has informed me that it was Otto Neurath who criticized Mannheim for the sharp separation between the social and the exact (mathematical) sciences (for further details, see Tuboly 2019).

Regarding the implications of Nagel's particular analysis, two insights should be kept in mind: (1) it is in principle possible to distinguish between characterizing (epistemic) value judgments that pertain to the 'context of justification' and appraising (non-epistemic) value judgments that pertain to the 'context of discovery;' (2) there are no compelling reasons that an ethically neutral social science is inherently impossible because it is only characterizing value judgments that play a role in scientific practice.

Now, Nagel's contribution to the discussion over value neutrality of social science did not remain uncontested. Thus, for example, Robert Bishop (2007, p. 128) objects that "the very drive to make social science value-free is, itself, a substantial value-laden position". Quite similarly, Heather Douglas writes:

> It was in the context of justification that science remained free from subjective and/or cultural idiosyncrasies; it was in the context of justification that science could base its pursuit of truth. Even within the context of justification, however, values could not be completely excluded (Douglas, 2007, p. 120).

In short, Nagel's strategy to shield science against the taints of subjectivity is, one might suspect, doomed to fail: neither the social nor the natural sciences are, according to the view defended by Douglas, free from what Nagel calls *appraising* value judgments. Admittedly, there can be little doubt that the more recent literature on value neutrality is dominated by views such as that of Douglas (see, for example Kitcher, 2001, pp. 65, 71, 118–9; Brown, 2001, pp. 187, 198–200; Longino, 2002, pp. 128–135; Koertge, 2003; and again Douglas, 2009). However, this—of course—does not mean that Nagel is refuted altogether. In point of fact, value neutrality is an objective still worth striving for (see, for example, Rudner, 2016). To be sure, the majority of authors in this arena would claim that admitting value-ladenness does not mean being subjective; furthermore, the condition of objectivity could be considered as implying that we admit explicitly and transparently our values (many people would cite here Weber and—more recently—Kevin Elliot's, (2017) *A Tapestry of Values*[19]). However, the critique provided by Douglas, in particular, does not really engage Nagel's reasoning but tries to get at value-ladenness from a different angle (inductive risk) and does furthermore not engage the value issue that worried Weber (politics or religion entering science). In fact, though, advocates of the 'thick-ethical-concepts' doctrine regarding social science (see, for example, Abend, 2019) would directly dispute that the evaluative and descriptive components of predicates can be separated in cases of 'thick' ethical concepts (like 'mercenary' etc.). Nagel, I claim, would have refuted the 'thick-ethical-concepts' doctrine as being *scientifically irrelevant*.[20]

[19] Thanks to Ádám Tuboly for pointing me to that volume.

[20] It is interesting to note in this context that Bernard Williams, the 'discoverer' of thick ethical concepts, does definitely not apply these concepts to the realm of science. See esp. Williams (1985), 135–6.

11.5 Concluding Remarks

It would require another paper to go into the details of Nagel's standing in the debate over value neutrality. The concern of the present chapter, though, was less ambitious: its central aim was to provide a concise summary of the most important theses and arguments in Nagel's approach toward the methodology of the social sciences. It remains to be hoped that further research on this issue will be carried out in the near future.[21]

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Chapter 12
Philosophy of Science as First Philosophy: The Liberal Polemics of Ernest Nagel



Eric Schliesser

Abstract This chapter explores Nagel's polemics. It shows these have a two-fold character: (i) to defend liberal civilization against all kinds of enemies. And (ii) to defend what he calls 'contextual naturalism.' And the chapter shows that (i-ii) reinforce each other and undermine alternative political and philosophical programs. The chapter's argument responds to an influential argument by George Reisch that Nagel's professional stance represents a kind of disciplinary retreat from politics. In order to respond to Reisch the relationship between Nagel's philosophy of science and his politics is explored and this chapter shows how both are anchored in what Nagel once called his 'contextual naturalism'—a metaphysics that resists imposing the unity of the world and treats all entities as embedded in a wider network of entities. Part of the argument traces out how Nagel's views on responsible speech and professionalism reflect a distinct understanding of the political role of philosophers of science.

Keywords Ernest Nagel: cold war liberalism · Responsible speech · Philosophy of science · Analytic philosophy

12.1 Introduction

In addition to being one of the pre-eminent philosophers of science of the twentieth century, and an influential PhD supervisor, Ernest Nagel was also a public intellectual, who for a generation was a kind of celebrity in the academy. Something of his status can be gleaned from his role in the very high profile methodological discussion among economists stimulated by Milton Friedman's methodological writings (Nagel, 1963). As Fritz Machlup (1963, p. 204) puts it in his introduction to the symposium, Nagel is

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treated as an "'exogenous' methodologist, an eminent philosopher of science invited to *referee* one of our most fundamental methodological controversies" (emphasis added).

Undoubtedly, Nagel's early *An Introduction to Logic and Scientific Method* (1934) co-written with Morris R. Cohen, which was well known to economists,[1] and Nagel's *The Structure of Science* (1961) also contributed to his stature among the economists. For example, in contributions to the discussion, Paul Samuelson (1965) called attention to the significance of the latter, especially, pp. 79–90. But I suspect he was already well known due to his often polemical book reviews and essays in which he took the measure of many intellectual currents of the age. These writings were collected in two volumes: *Sovereign Reason* (1954; hereafter: SR) and *Logic Without Metaphysics* (1956; hereafter: LWM). I use 'the Corpus' when referring to both collections. The Corpus appeared with a trade publisher, The Free Press,[2] and both carried the same sub-title: "other studies in the philosophy of science."

The significance of the identical sub-title(s) may elude more recent readers. For, while we tend to think of philosophy of science as a fairly focused enterprise—characterized by e.g., "the clarification of scientific procedures" (SR 307) and the stress-testing of concepts (SR 306)—, for Nagel, philosophy of science is a kind of first philosophy: "the boundaries of the philosophy of science are in fact the boundaries of philosophy itself" (SR 298). So, rather than indicating a limited focus, these sub-titles suggest the wide scope of the forty-six collected essays in SR and LWM, which range from metaphysics, philosophy of the special sciences, logic, aesthetics, political philosophy, early analytic philosophy, and the history of pragmatism amongst other topics. Nearly all the papers also intersect with material we would still recognize as philosophy of science.

This short chapter cannot hope to do full justice to forty-six sometimes complex chapters ranging from major self-standing essays on seminal thinkers to short book-reviews and a number of connected chapters in between. Even so, despite the diversity of topics discussed, I argue that a major strain of Nagel's polemics[3] in the Corpus has a two-fold character: (i) to defend liberal civilization against all kinds of enemies. And (ii) to defend what he calls 'contextual naturalism.' And (iii) I claim that (i-ii)

[1] In his 1942 textbook *The Theory of Competitive Price* (New York: MacMillan) George J. Stigler refers to Cohen & Nagel's *An Introduction to Logic and Scientific Method* as "recommended readings" at the end of his introductory methodological chapter. I thank David Levy for scanning relevant pages on my behalf. For more on the context, see Schliesser (2011).

[2] Founded in 1947, the Free Press was relatively new then. While it became increasingly known for a more conservative list, initially it was quite eclectic in its selections. Max Weber, Durkheim, Bernard Russell, and Morris R. Cohen were included among the authors it published.

[3] Nagel does not use 'polemic' very often. Indeed, when he criticizes Dewey's "logical writings" he calls them "sharp polemics" (SR 137). But Nagel is not criticizing Dewey's logical writings because they are polemical but because they misfire (either because the are aimed at the wrong target or because Dewey seems to miss the problems in his own conception). And, in fact, Nagel commends Dewey's "use of polemic" because it illustrates "that socialized, cooperative method of science for which he is a spokesman and a pleader." (SR 137) I treat this claim as an instance of Nagel's self-description, too.

reinforce each other.[4] In particular, I clarify how they reinforce each other and how they are supposed to undermine alternative political and philosophical programs. In what follows, I am less interested in tracing Nagel's sources than in the significance of his polemics. In Sect. 12.2, I put my cards on the table and explain my interest in Nagel. In particular, I explain that I frame my argument as a response to an influential argument by George Reisch that Nagel's professional stance represents a kind of disciplinary retreat from politics. In Sect. 12.3, I discuss the relationship between Nagel's philosophy of science and his politics. In Sect. 12.4, I deepen this analysis by showing how it is anchored in what Nagel once called his 'contextual naturalism'—a metaphysics that resists imposing the unity of the world and treats all entities as embedded in a wider network of entities. In Sect. 12.5 I conclude by responding to some objections. Part of my argument traces out how Nagel's views on responsible speech and professionalism reflect a distinct and easily misrepresented understanding of the political role of philosophers of science.

12.2 Nagel and the Retreat to the Icy Slopes

The Corpus I discuss was written between 1935–1954. For my present purposes this era matters for two reasons: first, at the start of the period it was by no means obvious whether liberalism and liberal democracy would survive in competition with Marxism, fascism, and national-socialism. In 1943, Nagel's own view of the matter is downright alarmist: it's clear he thinks it's a "period of social crisis" and "in the midst of actual and impending disaster" (SR 18; see also "the mounting economic and political tensions of our age," further down the page).[5]

By the end of the period, a (perhaps initially fragile) transatlantic, *Pax Americana* had secured liberal hegemony in the Atlantic world as the cold war developed. Along the way, how to think about liberalism was re-thought. My interest is to mark some features of Nagel's philosophy and polemics that illuminate this rethinking of liberalism. In general this period is of interest for liberal theorists for two reasons: first it is the age of New Deal and Bretton Woods; second it is the period in which neo-liberalism is developed. And while it is natural to assimilate Nagel to the former, Nagel's position is sufficiently distinct to deserve some attention.

[4] I do not mean to suggest that (i-iii) exhaust the historical significance of these essays. Elsewhere I have argued that "Impressions and Appraisals of Analytic Philosophy in Europe" (republished in LWM) is significant to the sociological and conceptual founding of analytic philosophy. See Schliesser (2013).

[5] See also the first sentence of the 1947 review of Morgenthau's *Scientific Man versus Power Politics*: "The defeat of human aspirations following profound social upheavals is frequently accompanied by loss of confidence in rational methods as ways of resolving the problems of society." (LWM 377).

Second, during that same period, and roughly the same geography, a heterogeneous number of schools—some originating in Cambridge, Vienna, Berlin, and Warsaw—, had merged with what I call the 'scientific wing of pragmatism,' and students of C.I. Lewis and Morris Cohen, to develop what is known as 'analytic philosophy.' As I have argued elsewhere (Schliesser, 2013), and which motivates my present interest, I take to be Nagel to be (one of) the prophet(s) of analytic philosophy who helped create, conceptually and sociologically, the new movement of philosophy.

To be sure, the relationship between these two reasons may be contingent, and I do not argue for causation. Even so, the Corpus helps reveal the significance of Nagel in both. So, for example, Misak (2013, p. 116) calls "Nagel, perhaps Dewey's best graduate student" in her influential, *The American Pragmatists*. She discusses him as "the paradigm of an analytic, logically inclined, philosopher of science in America" (2013, p. 150). But while she uses some of Nagel's writings throughout her work in discussing other thinkers, and hints, correctly in my view, that he may be intellectually responsible for the sociological synthesis between logical positivism and (the scientific wing of) pragmatism, (Misak, 2013, p. 163) she devotes little space to him.[6] In what follows I do not argue, anew, for the claim in the previous sentence. Rather I reinforce the claim by focusing on Nagel's polemics.

In many ways my analysis of the significance of Nagel's intellectual coalition-building and polemics echo much of what Reisch (2005) had already documented in *How the Cold War Transformed the Philosophy of Science*. But I argue that Reisch misunderstands Nagel's position by claiming that "he was resigned to quietism and disengagement. Only political and economic restructuring, and not resources offered by philosophy, could advance a "world community" or reduce international tensions" (2005, p. 311). While from Reisch's perspective this is not wholly false, on my more liberal view of Nagel, the political and economic restructuring has to be modeled on a certain conception of science, as presented by the philosophy of science, which itself is ground on a particular conception of metaphysics—Nagel's contextual naturalism—which grounds and mirrors a particular pluralist conception of society. In addition, Reisch misses Nagel's view on the constructive role of a future analytic philosophy in this because he overlooks his views on responsible speech. Or so I argue below.

To be sure, I am not the first to note to Nagel's liberalism. For example, Don Howard critically notes the strain of liberalism I identify in Nagel (and Nagel's role in the professionalization of philosophy). But he too criticizes Nagel for giving up on *science*'s role in the "selection of ends". He charges, while channeling the ghost of Dewey, that this leaves "the selection of ends as merely a matter of subjective preference something Dewey regarded as one of the lingering ill effects of the laissez faire liberalism of the nineteenth century" (Howard, 2003, pp. 60–61). Note that this

[6] One may, correctly, note that Philipp Frank and Charles Morris were as important as Nagel in promoting the congruence of pragmatism and logical empiricism. But on my account (Schliesser 2013) Nagel created the conceptual and sociological basis for this. I thank Ádám Tuboly for discussion.

position is not quietist; it is, for example, compatible with all kinds of (to extend the metaphor) individual or social entrepreneurial activity in the setting of ends. In fact, Nagel's position does leave an important role for the philosopher of science not in setting ends, but in helping the community to orient itself to the right sort of ends. Or so I argue now.

12.3 The Tasks of the Philosopher of Science

For Nagel, in "The Perspectives of Science and the Projects of Man," science shapes the broader culture in two distinct ways: first, as the fount of technologies and medicines. Second, by challenging established beliefs and intellectual habits (SR 297). And while the former is more emphasized, not the least by scientists in their appeals for resources (SR 296), the latter can also help re-shape (amongst others) our "basic aspirations," our "moral commitments," and "the principles" by which "actions" are evaluated (SR 297).

These two facts generate a "threefold task" for philosophy: (i) clarifying the bearing of trends in scientific inquiry upon pervasive conceptions of humanity's place in nature; (ii) making explicit the intellectual methods by which responsibly held beliefs are achieved; and (iii) of interpreting inherited beliefs and institutions in the light of current additions to knowledge. (SR 297–298) All three tasks involve philosophy of science.

While we tend to think of philosophy of science as a fairly focused enterprise (characterized by (ii)) for Nagel, philosophy of science becomes, as noted above, first philosophy: "the boundaries of the philosophy of science are in fact the boundaries of philosophy itself" (SR 298). And while (i) speaks to what we may call the existentialist call to philosophy, the third (iii) involves, more dangerously, philosophical activity within the life of her (imagined) community, that is, political philosophy or social theory of a certain sort.[7] These (i)-(iii) can be connected of course: philosophy becomes the spokesperson of mankind's existential place in the order of things in light of evolving scientific understanding of nature.

There is a crucial passage that reveals some of the internal connections between Nagel's philosophy of science and political theory. To understand it fully we must grasp that for Nagel in the wake of Einstein, the "classical conception of scientific knowledge" (SR 302) had to be replaced with something new, which I'll dub a 'post-classical conception of science.' This classical conception involves three commitments: "(1) Genuine scientific knowledge is demonstrative knowledge, and science seeks to "save the phenomena" […] (2) there must be transparently luminous universal truths which the intellect can grasp as self-evident. (3) The basic premises of a science must be necessary truths, which are better known and more certain than anything explained by them" (SR 302). By contrast, the post-classical conception

[7] This danger means that sometimes philosophers may require prudence. I explore this a bit more when I discuss Nagel's views on responsible speech below.

of science is fallibilist, embraces the holistic significance of what is now known as Duhem-Quine underdetermination, and is aware of possible incommensurabilities.[8] Nagel then comments:

> Scientific knowledge does not depend on the possession of an esoteric capacity for grasping the necessary structure of some superior reality, nor does it require modes of warranting beliefs which are discontinuous with operations of thought, identifiable and effective in the ordinary affairs of human life. The achievements of science are the products of a cooperative social enterprise, which has refined and extended skills encountered in the meanest employments of the human intellect. The principles of human reason, far from representing the immutable traits of all possible being, are socially cultivated standards of competent intellectual workmanship. The life of reason as embodied in the community of scientific effort is thus a pattern of life that generates an autonomous yet controlling ideal. That ideal requires disciplined dedication without servitude to any ultimate authority, imposes responsibility for performance upon individual judgment but demands responsiveness to the criticism of others, and calls for adherence to a tradition of workmanship without commitment to any system of dogma. To many commentators, the ideals realized in the enterprise of science are also the ideals which are indispensable to the successful operation of any society of free men. Many thinkers, indeed, like John Dewey in America, have based their hopes for the future of mankind upon the extension of the habits of scientific intelligence to every stratum of communal life and to every form of social organization. (SR 306)

The quoted passage is important for two main reasons: first, because Nagel rejects the cult of genius and thinks that science is a collaborative enterprise, which involves the systematic and socially sustained refinement of ordinary cognitive processes. So, rather than seeing a scientific society as a means toward a technocratic hierarchical elite of supermen (e.g., *Brave New World*), it is presented as fundamentally egalitarian in two ways: it is attainable by ordinary people and it has a relatively flat structure. And so this makes sense of the Dewey-ian ideal—which I also attribute to Nagel—that society needs to be modeled on, and shaped by, science as democratic in a non-trivial sense.[9]

This intellectual, egalitarian sensibility[10] informs Nagel's criticism of a whole range of opposing views (some of which he describes as 'malicious') that rely on either the intuition or insight of the special few (e.g., Hans Morgenthau's decisive statesman (LWM 377–382; or Aldous Huxley's pure mystics (LWM 389–393) or on the authoritative teaching of a privileged institution (e.g., the neo-Thomism thinkers

[8] This survives into his later *Structure*, and clearly is in the background of Kuhn's *Structure*. See, for example, Bird (2004).

[9] In light of LWM 247ff, I would expect that Nagel was familiar with Merton (1942), but Nagel avoids the kind of characterization we find in Merton (1942). I thank Treavor Pearce for discussion.

[10] In modern scholarship this is often treated as *methodological* analytic egalitarianism (MAE) that was given wider currency by Peart and Levy (2009). A key feature of MAE is, to simplify, that for modeling purposes, agents are at least initially treated as roughly equal; and that any differences one attribute to agents are a consequence of their interactions and environment. In recent philosophy of science, we find versions of MAE in the Zollman school of formal epistemology of science, e.g., Bright (2017); and, more indebted to Peart and Levy, Schliesser (2018). Nagel's commitment to MAE has metaphysical foundations which I will explore in the next section. Unfortunately, Nagel's egalitarianism is gendered male (see also note 42 below).

of the Catholic church (SR 27–31))[11] or require a method that Nagel claims is non-transparent (Marxist dialectical materialism (SR 45–46)). Many of Nagel's targets combine more than one of these elements (e.g., Marxists that are obedient to the Party's version of dialectical materialism (LWM 398)) or Thomists that rely on "superior…metaphysical wisdom" (SR 31).[12] As he puts it, "perhaps no intellectual tendency is more dangerous than that accompanying the claim that knowledge of human affairs is the exclusive property of men endowed with a "higher insight"—which is not subject to the control of well-established experimental methods" (Nagel, 1938, 55).[13]

Second, in the idea that science is a model for political life, Nagel here anticipates Michael Polanyi's reflections on the way the 'republic of science' can be a model for a 'body politic.' I mention Polanyi's approach to the philosophy of science because it represents an influential stream in cold war liberal thought and is, in certain respects, a legitimate target for the quietist critique Reisch directs at Nagel. Polanyi emphasizes the significance of individual, independent judgment in science and how the interaction of these judgments generates—like a market-place of ideas (Polanyi takes the analogy between markets and science very seriously)—a spontaneous order. While the republic of science is (despite the generation of hierarchy within) relatively flat, it has authority *over* the public in Polanyi's scheme.[14] But while instantiating a principle that one also finds in market relations, the republic of science also provides a model for society as a society of explorers (whose goal is, somewhat surprisingly, self-improvement).[15]

By contrast, on Nagel's model science is *not* a method of silencing others behind an authoritative consensus familiar from Kuhn's philosophy of science and the liberal political theory of the second half of the twentieth century.[16] Rather, for Nagel, science is a social mechanism by which one becomes *responsive* to reasons and participates in a social division of labor that both improves each of our cognitive functioning collectively and individually as well as being in some sense self-legislating. And so, this helps generate (through its spokesperson in philosophy of science, and in

[11] This also informs some of Nagel's impatience with and suspicion of the Wittgenstein circle (LWM 206).

[12] For the significance of Nagel's stance toward the development of what became 'analytic philosophy, see, especially, the use of the "keen, shining sword helping to dispel irrational beliefs" (LWM 197) and the "obscurantism" of "traditional philosophy" (LWM 196).

[13] Nagel (1938) falls outside the Corpus and in this paper I only use it as supporting evidence. Why Nagel decided to leave it aside is worth further investigation. I thank Ádám Tuboly for discussion.

[14] See Polanyi (1962, p. 60): "The authority of scientific opinion remains essentially mutual; it is established between scientists, not above them. Scientists exercise their authority over each other. Admittedly, the body of scientists, as a whole, does uphold the authority of science over the lay public.".

[15] As Polanyi (1962, p. 72) recognizes, his model "society does not offer particularly wide private freedoms. It is the cultivation of public liberties that distinguishes [his] free society.".

[16] See Schliesser (2012, 2016). For important nuance, see Hart (1998).

lived experience) an "attitude, at once critical and experimental, toward the perennial as well as the current issues of human life" (SR 307).[17]

In fact, Nagel is explicit that philosophy of science so understood, is "a champion of the central values of liberal civilization". And while in the early 1950s the survival of liberal civilization was more secure than it had been in a generation, it is clear, as I argue in subsequent sections, that securing such a civilization is one of Nagel's main aims in all his polemics. And, of course, by providing freedom of thought, liberal civilization is conducive to a humane philosophy of science (understood as first philosophy):

> The basis for a general outlook on the place of man in nature is supplied by detailed knowledge of the structure of things supplied by the special sciences—an outlook that contemporary philosophy of science has helped to articulate and defend. In the perspective of that outlook, the human creature is not an autonomous empire in the vast entanglement of events and forces constituting the human environment. Nevertheless, no antecedent limits can be set to the power of scientific reason to acquire theoretical mastery over natural and social processes. Every doctrine which pretends to set such limits contains within itself the seeds of intolerance and repression. Moreover, in the perspective of that scientifically grounded outlook, human aspirations are expressions of impulses and needs which, whether these be native or acquired, constitute the ultimate point of reference for every justifiable moral judgment. The adequacy of such aspirations must therefore be evaluated in terms of the structures of human capacities and the order of human preferences. Accordingly, though the forces of nature may one day extinguish the human scene, those forces do not define valid human ideals, and they do not provide the measure of human achievement. But an indispensable condition for the just definition and the realization of those ideals is the employment and extension of the method of intelligence embodied in the scientific enterprise. A judicious confidence in the power of reason to ennoble the human estate may seem shallow to an age in which, despite the dominant position in it of scientific technology, there is a growing and pervasive distrust of the operations of free intelligence. It may indeed be the case that the temper of mind essential to the exercise of such intelligence has no immediate social future. But the cultivation of that intellectual temper is a fundamental condition for every liberal civilization. By making manifest the nature of scientific reason and the grounds for a continued confidence in it, contemporary philosophy of science has been a servant of men's noblest and most relevant ideals. (SR 307–308)

There is a lot going on this passage. Recall that for Nagel philosophy's tasks are (i) clarifying the bearing of trends in scientific inquiry upon pervasive conceptions of humanity's place in nature; (ii) making explicit the intellectual methods by which responsibly held beliefs are achieved; and (iii) of interpreting inherited beliefs and institutions in the light of current additions to knowledge. (SR 297–298) Nagel assigns here two new tasks to philosophy.

First, philosophy of science has (iv) the task of making manifest "the grounds for […] continued confidence" in the nature of scientific reason. The future orientation of this task is quite striking.[18] What could merit such confidence? Lurking in Nagel's program is the presupposition that either skepticism about social induction from past

[17] Trevor Pearce called my attention to the fact that this echoes Dewey (1939). There are also echoes to the republican tradition of inspired by Rousseau.

[18] This goes well beyond (ii), which is backward-looking; (iv) is forward looking.

to future can be defeated or, well, that we can be persuaded to it ignore its challenge. No argument is offered for this.

Second, "philosophy of science" helps (v) "to articulate and defend" the basis "for a general outlook on the place of man in nature is supplied by detailed knowledge of the structure of things supplied by the special sciences". This follows naturally from task (i). I wish to make two connected observations about the present significance of (v). First, it can be taken to foreshadow the professional turn in philosophy of science lamented by Reisch and Howard; for, it requires considerable detailed knowledge of and engagement with rather esoteric areas of science. This inevitably gives rise to specialization of various kinds.

How much specialization is required is a complex matter. For example, Nagel's contribution to the methodological debate prompted by Milton Friedman among the economists mentioned at the start of the present chapter is at high enough generality that not much special knowledge of economics is required. But even so, at various points Nagel shows subtle command not just over what some prominent economists had to say about Friedman's views,[19] but, also, and more interestingly, distinguishing "many formulations of neoclassical theory" from nearby possible versions (pertinent to the interpretation of Friedman's article).[20]

Second, this philosophy of science focused on the special sciences does not merely describe ("articulate") the foundations of a general outlook on the place of man in nature, but it also advocates for it ("defend") it. So, this vision for philosophy of science is a *normative* enterprise in some clear sense.

Now admittedly, that "human aspirations are expressions of impulses and needs which […] constitute the ultimate point of reference for every justifiable moral judgment" leaves out a lot of detail, about the nature, sources, and content of this normative orientation for philosophy of science. And so I would grant the critics that Nagel's position is itself under-described.

So, while it is clear that Reisch and Howard are correct that for Nagel science does not set ends, science does play a role in what we may call the *fittingness* of these human aspirations. That is, rather than merely seeing science as a species of instrumental reason, providing the mechanism by which social ends are attained, Nagel clearly thinks that science, as a collective enterprise, and through its philosophical interpreters, also helps make clear if the collective ends are themselves fitting for creatures like us. As he puts it in "Notes Toward a Naturalistic Conception of Logic," a naturalist's task (vi) is to help humanity "wisely" base mankind's "ideals upon the capacities and limitations of his own body." (LWM 52).

This is not an especially quietist program. Nagel himself understands it as facilitating a form of "liberation" from what he calls an "unfree intelligence" (LWM 52). Science plays an "indispensable" role in intelligent and pluralist social decision-making in Nagel's hands ("the just definition and the realization of those ideals is the employment and extension of the method of intelligence embodied in the scientific

[19] He discusses Koopmans (1957, p. 140); see Nagel (1963, p. 219).

[20] See, especially Nagel (1963, p. 218), and the sophisticated references to Knight and Samuelson.

enterprise" (SR 307–8)). For the point of his democratic vision is to "acquire theoretical mastery over natural and social processes". From the perspective of a more laissez faire attitude (here represented by Polanyi and Hayek) the aim of "mastery" over social processes is horrifying.

At this point of my argument I hope to have demonstrated that Nagel is a rhetorical forceful advocate of liberal society. His vision of liberal society and his understanding of science in it is both distinct from the socialist programs offered by Neurath[21] and the more classical liberal positions advocated by Michael Polanyi and Hayek which, for present purposes only, I have treated as exemplars of the 'quietist' position.[22] While it would be tempting to describe Nagel as a New Deal progressive, he shares with Dewey a preference for a more bottom up conception of science and rejects the top-down collectivism and managerialism one may discern in the New Deal. And that's because of the fundamental importance of pluralism in Nagel's thought.

12.4 Contextual Naturalism and Pluralism[23]

While I tend to see Nagel as the leader of the scientific wing of twentieth century pragmatism, in his essay, "Philosophy and the American Temper," (hereafter: American Temper) Nagel describes pragmatism as a decaying research program (SR 53). This essay was written for a "European audience, and appeared in a French translation in the *Chronique des Etats-Unis*, a bi-monthly publication of the American office of Information in Paris, In April 1947," (SR 309). I have found almost nothing about this periodical, but it stands to reason it was an instrument of the US State Department or the CIA. I leave to others reflection on the very idea of speaking about America, or the idea of America, to a *European* audience on the ruins of two world wars.

I assume here that it was written before the cold war was full reality, although Nagel was already hostile to Marxism (see, e.g., the scathing review of Haldane LWM 331–333). So, while anachronistic, it would not be wholly misleading to call it 'a philosophy for cold war liberalism.' "American Temper" is a short essay. Part of the argument of the essay is a denial that there is a one, unified, distinctive American philosophy. In practice many philosophical programs flourish Stateside (SR 50–52; Nagel shows no interest, alas, in indigenous thought.)

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[21] Not all socialist programs are alike. Nagel is scathing about top-down unity of science proposed by Cornforth (LWM 399); he is much sympathetic to the Neurathian 'unity of science' (in Nagel 1938), presumably because it is more collaborative and democratic. I thank Ádám Tuboly for discussion.

[22] From my perspective, it is no surprise, then, that Nagel offered a fierce criticism of Hayek (LWM 361–368). This review is primarily directed against Hayek's view that the "extension of the methods of the natural sciences into social inquiry is an abuse of reason" (LWM 368). But it seems pretty obvious that Nagel's sub-text is that adopting the methods of the natural sciences, when properly understood and conceptualized, in social inquiry does not lead to the (now quoting Hayek) "hubris of collectivism" (LWM 363).

[23] For the deeper roots of Nagel's contextual naturalism, see also Chap. 6 of Pearce (2020).

However, in "American Temper", Nagel describes one intellectual current, even movement, "contextual naturalism," in favorable terms. That here Nagel is describing his own commitments as a 'contextual naturalist,' we can accept on no less authority than Isaac Levi (1998, p. 641; 2005, p. 717; Levi was a student and colleague of Nagel). While Nagel's association with contextual naturalism was noted by contemporary readers and reviewers of Nagel's essay, the term never caught on.[24]

In a beautiful essay, Lawrence Cahoone (2016) makes an excellent case that Nagel's contextual naturalism is very indebted to Morris Cohen.[25] In "American Temper", Nagel himself grants (53–54) that it grows out of later writings of Dewey, Sheldon's (1942) *America's Progressive Philosophy*, which is a species of process philosophy, and a collection edited by Krikorian (1944) *Naturalism and the Human Spirit*, which includes many philosophers associated with Columbia at mid century. Despite the vicinity of his thought to Dewey's, Nagel himself disowns the 'pragmatist' label because he lacks commitment to the "technical pragmatic doctrines concerning the nature of truth or the function of knowledge" (SR 53).

The cardinal thesis of contextual naturalism is "the essentially incomplete fundamentally plural character of existence, in which no overarching pattern of development can be discerned, and which qualitative discontinuities and loose conjunctions are as ultimate features as are firm connections and regular cycles of change" (SR 54). Nagel puts this plural character in another related fashion as follows, "the manifest plurality and variety of things, of their qualities and their functions, are an irreducible feature of the cosmos" (LWM 7).[26] This is now a relatively unfashionable, metaphysical doctrine. But we can hear in its pluralism echoes in the (dappled world, disunity) commitments of those associated with the Stanford School of the Philosophy of Science.[27] Since Nagel was Suppes' supervisor the connection is not wholly coincidental.[28]

The point of calling it 'contextual' is explained by Nagel in terms of the "emphasis upon the contextual conditions for the occurrence and for the manifested properties of everything whatsoever—upon the fact a quality an objective constituent of nature even though its existence depends on the relations in which it stands to other things"

[24] See the book review by Bidney (1955). Contextual Naturalism is listed as one of the forms of naturalism in the survey by Riepe (1958, 734). An interesting exception is Frank's last book, The Humanistic Background of Science, where Frank discusses Nagel under the label "contextual naturalism" in various sections (see, especially, pp. 271–274). I thank Ádám Tuboly for alerting me to this.

[25] For the development of Nagel's naturalism, see Sander Verhaegh's chapter in this volume.

[26] This helps explain Nagel's polemic with, say, Blanshard, against the reality of internal relations and monism in the essay, "Sovereign Reason" (SR 266–295). Nagel's view echoes James and Skrupskelis (1977). I thank Trevor Pearce for alerting me to this.

[27] See Cartwright (1999); Dupré (1995). On the Stanford School, see Cat (2017), especially https://plato.stanford.edu/entries/scientific-unity/#StanScho. The position also anticipates what has become known as "Minnesota Pluralism" (associated with Helen Longino, Ronald Giere, and Ken Waters). See Solomon and Richardson (2005, p. 218).

[28] Suppes (1994/2012) wrote a lovely, informative obituary of Nagel, but he does not speak about Nagel's metaphysics. I warmly recommend the interview conducted by Herfeld (2016), but she did not ask about Suppes' attitude toward Nagel's metaphysics.

(SR 55). This generates an anti-reductionism and a general mistrust of what we may call eliminativist strategies.

Now, in the previous section I noted that Nagel's philosophy of science is not attracted to the idea that science is a means of silencing others in the name of a unified authority. And while science contains differential expertise and skill in virtue of the evolving social division of labor and standards, Nagel views science more as an open-ended conversation responsive to criticism and reasons. This both echoes the nineteenth century, classical liberal emphasis on "government by discussion" (a term invented by Bagehot), of which Knight was the most prominent defender in the period under review (Emmett, 2020)[29]; and anticipates Habermas' focus on deliberative democracy. In fact, after Habermas later promoted deliberative democracy, it has been natural to see this as continuous in some respects with the earlier liberal tradition (see Roháč, 2012, Gutmann and Thompson 2009, p. 9). My claim is that Nagel has a natural place in this broader liberal current.

The point is echoed in "American Temper": science as understood by contextual naturalism "involves the continued criticism of its findings in the light of evidence capable of public inspection" (SR 56). And in this sense Nagel views science as a "responsible" (SR 56) model for democratic society worth having. To what degree the science of our day would still be thought of as such an apt model is worth asking.

In the previous section I noted that Nagel anticipates the methodological egalitarianism (MAE) we also find in contemporary strands of philosophy of science. Nagel's adherence to MAE is not merely political and methodological; it is ground in, and an expression of his (contextual naturalist) metaphysics. For Nagel, claims that "this gift of intelligence man owes to the organization of his body and the character of his environment" (SR 56). This suggests that Nagel treats MAE as grounded, at least in part, in facts of ontology (the human body).[30] One may object that Nagel's use of 'body' and 'environment' is not ontological in character, but merely treating empirical facts.[31] But, as noted before, it is Nagel, who focuses on the "irreducible feature of the cosmos." So, I claim Nagel invites the ontological interpretation.

From a distance one might expect that such an embrace of MAE leads to a homogenizing picture of human nature. But for Nagel the diversity of environments gives rise to diversity of social organizations and so (there is an echo of Max Weber here—see Beiser, 2011, p. 558) a diversity of human needs and ends. This pluralism also expresses the "democratic way of life," which resists prescribing "for its citizens a set of beliefs concerning the nature of the cosmos and man's place in it" (SR 50). At the end of American Temper, Nagel concludes with a passage crucial for my present purposes:

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[29] The claim about Bagehot is made by Emmett (2020, p. 303). Not all references to Knight are positive in Nagel (see SR 32n).

[30] The significance of the human body might make one suspect that Nagel could be a fellow traveler of, say, progressive embrace of eugenic practices. But as far as I can tell he resists this lure, and is scathing about racialized eugenics (e.g., SR 32; in context, this is partial concession to the neo-Thomist critique of modernity).

[31] I thank an anonymous referee for the objection.

> The possession of needs and preferences, and the exercise of reflection upon them in the interest of fulfilling and harmonizing them, are as natural to man as is, for example, the property of a magnet to repel or attract another magnet. In any event, it is in the radical plurality of men's needs and in the limitations which their physical and social environment impose upon their fulfillment, that contextualistic naturalism locates the source and urgency of moral problems. Accordingly, it does not conceive the primary moral problem to be that of discovering or actually instituting some fixed set of ethical norms valid everywhere and for all time. For basic moral problems are plural in number and specific in character, and are concerned with the adjustment, in the light of causes and consequences, of competing impulses occurring in specific environmental contexts. There can therefore be no general or final solution to the moral predicaments of mankind; the moral problem is the perennial one of finding ways and means for eliminating needless suffering and for organizing in a reasonable manner the energies of men. (SR 56)[32]

This is a rejection of ethics as a monistic, authoritative discipline imposing normative rules on the rest of us. Rather Nagel views ethics more in the spirit of what is now known as 'mechanism design.' That is, its task is to find local, temporary ethical solutions to practical problems in light of our plural commitments in "specific environmental contexts". If there is an overarching theme it is the elimination of needless suffering. Obviously this slogan does not settle important questions (whose suffering will count; by whose light needless; and at what cost elimination?, etc.), but it gives a sense of the spirit of the project which is really about the never-final organization of the energy of human-kind. The focus on avoiding suffering anticipates the strain of liberalism we find in Shklar's liberalism of fear (1989).

We can also see that Nagel's vision for the philosophy of science as a template for a democratic society is itself ground in a pluralist metaphysics in which no single perspective is ultimately privileged, but each has standing. In addition this metaphysics provides a kind of template for society: each individual has objective existence, but is simultaneously socially embedded, and improved by participation in the practice of receiving and offering mutual criticism in light of socially available facts.

As Nagel puts it in summarizing the revolution in thought influenced by Dewey, "we are suspicious both of large-scale generalizations in social theory and history which are supported primarily by appeals to alleged necessities of thought, and also of statistical and other factual studies which are not controlled by clearly formulated theoretical assumptions grounded in ascertained facts. We are less easily taken in by the claims of scientific workers that their most recent conclusions are revelations of a final reality" (SR 119). This is very much the spirit of trial and error (SR 307) and piece-meal engineering (now primarily associated with Popper, 2020).[33]

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[32] See Pearce (2020, p. 282) for the roots of this material in Dewey and Mead.

[33] While some (including Popper and Hayek) have a tendency to treat Popper as a fellow traveler with Austrian economics, there is no doubt that Popper views are compatible with elements of the New Deal or social democratic sensibility. On the role "working hypothesis" in pragmatism, see Pearce (2020, pp. 329–331).

Given that circumstances are constantly changing this is a philosophy of forever-unfinished-business, self-conscious of the fact that any proposed solution in the moment will seem archaic at a later date.[34] As Nagel puts it:

> [C]ontextualistic naturalists exhibit a profound distrust of philosophic systems which attempt to catch once for all the variegated contents of the world in a web of dialectical necessity. They are keenly conscious of the limitations of purely formal analysis even when they engage in it. For they recognize that a logic, no matter how subtle, provides no warrant concerning matters of fact unless it is supported by controlled observation. Indeed, they sometimes show an almost pathological fear that those concerned with formal analysis may be deceived into supposing that nature is as coherently organized and as simple as are their intellectual constructions. (SR 54)

Contextual naturalism is self-aware of the fact that the assumption of nature's coherence may be more an artifact of one's (formal) models, or a regulative assumption, than an established metaphysical fact. The self-understanding of contextual naturalism is one of self-disciplining[35] and forms of intellectual/metaphysical humility (captured by the slogan 'logic without ontology').[36]

But by this I do not mean to suggest contextual naturalism is modest. Nagel calls his own contextual naturalism "sane and reasonable" and a "sober" alternative to more fashionable and fanciful philosophies. (SR 57) There is an unapologetic willingness to develop, and suddenly we are in a Nietzschean register, the "Apollonian" edifices of "civilization" (SR 57). But this way of framing it, and the acknowledged pathology—the fear that controlled technique may facilitate a form of intellectual self-deception—, suggests recognition of the fact that some forces in our environment, including our own impulses, have Dionysian roots that may haunt us. Not unlike Carnap, Nagel's analytic head is conjoined to the heart of a disciplined or responsible romantic.[37]

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[34] While I do not deny that in his bottom up sensibility, Nagel is closer to Dewey than he is to Walter Lippmann (who has a fondness for the circulation of elites through society and government), they (Nagel and Lippmann) share in a liberalism that embraces what I have called a 'spirit of adaptation' in Schliesser (2019).

[35] Part of the contrast between pragmatism and contextual naturalism, is the fact that the latter contain not just the keenest, but many of the "best disciplined minds among the younger men" (53). For the contemporary significance of discipline within philosophy, see Williamson (2006, esp. p. 182).

[36] Crucially, for Nagel 'logic' or the 'inclusive sense of logic' studies the "methods employed by men aiming at stable knowledge, assays their efficacy in achieving this aim, examines the role of critical thought in every department of human activity, and institutes a rigorous inquiry into conditions upon which the significance and effective operation of discourse rests. It is a genuine organon for achieving a *rational life and society*." (LWM 52).

[37] A speculation: this is also obliquely indicated in the criticism of Morgenthau. For Nagel liberalism, of the sort represented by Mill, can take on board not just a more historicized understanding of social explanation/science, but also romanticism's insights into human need and individuality. That is to say, against Morgenthau's "irresponsible romanticism" (explicitly indebted to Nietzsche), Nagel places a more responsible romanticism indebted to Mill (LWM 380).

12.5 Objections and Conclusion

In this final section I respond to a criticism that probably has occurred to many discerning readers. The critic may grant that Nagel's liberalism does not naturally belong in the camp of quietist laissez-fair liberalism (which I have associated, perhaps unfairly, with Polanyi and Hayek above). Such a critic may also grant that Nagel's rather broad conceptions of the philosophy of science (and logic) are compatible with at least some tasks (i–vi) that have substantial political significance. But Nagel's vision, so the critic could argue, is also compatible with a kind of retreat into the ivory tower and a professionalization of philosophy of science that, in practice, is just specialists talking to each other (over minutiae, one can add for good measure.)

So, from the perspective of the critic it is no surprise that in 1935 Nagel wrote:

> Santayana remarks in one of his books that he would be ashamed to hold views in philosophy which he did not believe in daily life, and that he would deem it dishonest and cowardly to sail under colors in an argument which were not those under which he habitually lived. The precept implied by these comments can serve as a criterion for evaluating the quality of integration which a thinker achieves in his own life but more significantly, it helps to fix the subject matter and task of philosophy, and so to disclose the relevance of the philosophic pursuit to the society which nourishes it. It is a precept which I take for my own wholeheartedly, and one which I wish were taken as a guide in all philosophic inquiry. (LWM 39)

1935 was in the midst of the great depression. And so the question of nourishment was not merely a metaphor.[38] Nagel discerns a way in which existential concerns with integrity between thought and deed are connected to the social purposes of one's society. For Nagel, a philosopher's (what we may call) 'philosophical integrity' involves the way(s) in which one's professional arguments, professional credit, and public utterances and comportment cohere.

I do not mean to deny that there are tensions in Nagel's position. His views are, as we have seen, resolutely pluralist, and, if his "all philosophic inquiry" is not meant to refer to his own, he may be thought inconsistent with his other views here.[39] And he is a bit quick, too quick, to assume that the disclosure to society of the relevance of one's philosophical pursuit will be met by approval of that society. Even a democratic philosophy may irritate, even bore, the masses, after all. We should neither assume a kind of efficient market in ideas nor the automatic, effective uptake of useful ideas (see Lefevere & Schliesser, 2014 and Schliesser, 2019).

The hypothetical critic, who is friendly to Reisch's larger narrative about the retreat from political science by philosophy of science, can readily acknowledge that early Nagel would say the things I have just quoted. The crux of the matter is what happens when philosophers of science "have become" mere "specialists in our professional activities" (LWM 4).

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[38] As noted in Schliesser (2013), Nagel himself was about to embark on a funded tour of Europe where he would discover and partially help legislate a very different philosophical scene.

[39] There is clearly a limit to Nagel's pluralism that is partially set by his commitment to rigor and clarity. Unlike, say, Lippmann (Schliesser 2019), Nagel leaves very little space for the confused and those he suspects lack a liberal temper.

As the reference reveals, Nagel understands and articulates unsparingly the perspective of the hypothetical critic at the start of a presidential address to the APA in December 1954, where he discusses this feature of "recent analytical literature". I quote two paragraphs from the (near) beginning of the lecture:

> The past quarter century has been for philosophy in many parts of the world a period of acute self-questioning, engendered in no small measure by developments in scientific and logical thought, and in part no doubt by fundamental changes in the social order. In any event, there has come about a general loss of confidence in the competence of philosophy to provide by way of a distinctive intellectual method a basic ground-plan of the cosmos, or for that matter to contribute to knowledge of any primary subject-matter except by becoming a specialized positive science and subjecting itself to the discipline of empirical inquiry. Although the abysses of human ignorance are undeniably profound, it has also become apparent that ignorance, like actual knowledge, is of many special and heterogeneous things; and we have come to think, like the fox and unlike the hedgehog of whom Mr. Isaiah Berlin has recently reminded us, that there are a great many things which are already known or remain to be discovered, but that there is no one "big thing" which, if known, would make everything else coherent and unlock the mystery of creation. In consequence, many of us have ceased to emulate the great system-builders in the history of philosophy. In partial imitation of the strategy of modern science, and in the hope of achieving responsibly held conclusions about matters concerning which we could acquire genuine competence, we have tended to become specialists in our professional activities. We have come to direct our best energies to the resolution of limited problems and puzzles that emerge in the analysis of scientific and ordinary discourse, in the evaluation of claims to knowledge, in the interpretation and validation of ethical and esthetic judgments, and in the assessment of types of human experience. I hope I shall not be regarded as offensive in stating my impression that the majority of the best minds among us have turned away from the conception of the philosopher as the spectator of all time and existence, and have concentrated on restricted but manageable questions, with almost deliberate unconcern for the bearing of their often minute investigations upon an inclusive view of nature and man.
>
> Some of us, I know, are distressed by the widespread scepticism of the traditional claims for a philosophia perennis, and have dismissed as utterly trivial most if not all the products of various current forms of analytical philosophy. I do not share this distress, nor do I think the dismissal is uniformly perspicacious and warranted. For in my judgment, the scepticism which many deplore is well-founded. Even though a fair-sized portion of recent analytical literature seems inconsequential also to me, analytical philosophy in our own day is the continuation of a major philosophic tradition, and can count substantial feats of clarification among its assets. Concentration on limited and determinate problems has yielded valuable fruits, not least in the form of an increased and refreshing sensitivity to the demands of responsible discourse. (LWM 3–5)

Nagel reminds us that the very best criticisms of analytic philosophy have often been stated within the tradition. More subtly, his idea that ignorance is not univocal strikes me as worthy of ongoing rediscovery. More pertinent here, Nagel grants de facto that philosophical professionalization and specialization in the intellectual division of labor necessarily involves generating the conditions of mental mutilation.[40]

Now, Nagel's diagnoses of the source of professionalization is worth further reflection; for he thinks such bitter medicine worth our pain. He thinks that the professionalization has a number of causes (both internal to scientific development and due to

[40] Adam Smith noticed the same phenomenon about the effects on the machine laborer.

social changes). But I leave most of these aside. Because the crucial claim for present purposes is this one: "in partial imitation of the strategy of modern science, and in the hope of achieving responsibly held conclusions about matters concerning which we could acquire genuine competence, we have tended to become specialists in our professional activities" (LWM 4).

The desire for professionalization is the desire for living up to the demands of responsible speech. And responsible speech here means something like 'being in the position of stating claims that have some or sufficient warrant.' (On warrant recall SR 56; see also SR 20.) And this, in turn, requires obtaining genuine competence of the sort associated with (and modeled on) scientific expertise. It is clear that in Nagel's own practice he was capable of being reasonably competent interlocuter to many special sciences (this is why I have commented on his engagement with economics),[41] while still contributing to philosophy of science in a broad sense. To what degree the increasing esotericism of the special sciences allows this is a challenging question that goes beyond my present remit.[42]

Nagel's stance on responsible speech is characteristic of analytic philosophy also in its more 'heroic' phase of the 1930s. Carnap's voluntarism, which is most majestically expressed in the free choice of a stipulative language, is, as Abe Stone (2006) has shown, an expression of our freedom to take "responsibility" for the choice of language, for own "self-legislation as a rational (i.e., speaking) being". That is, Carnap's theoretical philosophy is, in part, rooted in a practical concern. This Carnapian stance is accompanied, as Stone (2006, p. 232) emphasizes, by a second species of responsible speech: "outside the borders of theory […] [speech] may be attempted in brief hints". (Stone quoting the Foreword to the *Aufbau*).[43]

That is to say, while Reisch is clear on the anti-metaphysical animus behind such statements (83), he misses a/the core strain in the self-understanding of (what became) analytic philosophy, namely responsible speech.[44] This understanding of responsible speech is itself ground in a moral and political conception of the possible fruits of philosophy.[45] Of course, on my view, that it is a core strain is itself the long-term effect of Nagel's polemics gestured at the present essay and his philosophical politics (see also Schliesser, 2013). It is important to see that this stance is not the effect of cold-war intimidation. That is, of course, compatible with the sociological argument that Reisch advances: that competitor views lost funding or were discouraged by effective forms of social sanction and intimidation. I do not mean to suggest that the

[41] For non-exhaustive overview, see Suppes (1994/2012).

[42] In Nagel (1938, p. 46), Nagel makes this very point about formal logic: "In our own day formal logic, traditionally a part of philosophy, is becoming so specialized that in the near future perhaps only men with a thorough mathematical training will be capable of following its development." In larger context Nagel is exploring what might be subject matter for philosophers. (Sadly, he does not seem to be alert to his own gendered presuppositions here.).

[43] A curious feature of Reisch's book is the absence of Heidegger. This is presumably why the question of responsible speech is not salient for him.

[44] Concern over responsible speech also has a long history in liberal political theory; see Schliesser (2017).

[45] On the consequentialist justification of Carnap's methodology, see especially Stein (1992).

professional status quo exists in virtue of its intellectual merits. Rather my point is that such merits exist in so far as one takes a liberal orientation seriously.

To be clear, in the previous paragraph I am claiming two things: first, that in the case of Nagel we do not have a "back to the icy slopes" account because the nature of his social engagement has been missed by Reisch. And, second, in the case of Nagel, this engagement has remained constant. Thus not all fellow travelers of logical empiricists were transformed, and so Reisch's narrative is not comprehensive. Part of the problem here is that Nagel's professionalism, which Reisch attributes in the case of logical empiricists to the intimidation caused by cold war, is, in fact, an expression of his view that responsible speech requires considerable specialized and expert knowledge. By making Nagel stand in for a *disciplinary* transformation—on which I have taken no stance—, Reisch misses that from Nagel's individual perspective there was no transformation at all.[46]

The embrace of responsible speech as a professional norm results from a moral and social vision of the utility and possible contributions of such a responsible philosophy of science. One of the fruits of such a responsible philosophy of science is, in fact, a kind of philosophical self-disciplining ("an increased and refreshing sensitivity to the demands of responsible discourse"). But as we see in the example with which I started—with Nagel contributing to the methodological debate in economics—this self-disciplining also provides the philosopher of science with *authority* to participate in, say, philosophical debates *within* the special sciences.

Now, the *apparent* effect of this self-disciplining is not just a focus on minutiae and a reliance on (let's stipulate) "dependable method of inquiry" (LWM 6), but also a retreat into "affirmations […] meager in content" (LWM 7).[47] But this appearance is an expression of a form of self-command that resist ungrounded sweeping claims. But it is not a retreat from the tasks of a responsible philosophy of science articulated above. As Nagel puts it, "[w]hen ideals are handled *responsibly,* they therefore function as hypotheses for achieving a balanced exercise of human powers […]. If moral problems can be resolved at all, they can resolved only in the light of specific human capacities, historical and acquired skills, and the opportunities (revealed by imagination disciplined by knowledge) for *altering the physical and social environment and for redirecting habitual behaviors*" (LWM 11; emphases added). I quote this passage because it shows that even when responding to hypothetical critic, Nagel's commitment to responsible speech and concomitant professional specialization remains wedded to the idea that one of the roles of philosophy science is to establish the fittingness of our ideals. In addition this focus on responsible (and warranted) speech is not a retreat into doing nothing, but it can play a role in non-trivial social change. As Nagel concludes, in virtue of the fact that our ideals and dispositions are the "products of society," logico-empirical philosophy of science—even the kind that seem to focus narrowly on the "methods of evaluating evidence"—helps, by promoting means for "achieving reliable knowledge,"

[46] I thank Don Howard for helping me get clear on this.

[47] Nagel is describing what he calls 'naturalism' here, but it is naturalism that is supposed to result from professional methods.

and recognizing its conditions, generate a naturalistic moral theory, which is "at the same time a critique of civilization, that is, a critique of the institutions that channel human energies, so as to exhibit the possibilities and limitations of various forms and arrangements of society for bringing satisfactions to individual human careers" (LWM 12; Nagel uses logico-empirical at LWM 13).

So, to be clear. Nobody would confuse Nagel's stance with the more revolutionary aspirations that animated left Vienna and unity of science movement originally. But even after the turn to professionalism it was animated by moral and political commitments that may entail non-trivial criticism of the status quo and advocacy for social change. Of course, it is pretty clear that Nagel thinks some changes may be gradual; and he recognizes that it is quite possible that human reason may not be able to remedy all social ills (LWM 17). In particular, he thinks that it is "impossible to decide responsibly, *antecedent* to inquiry, *which* of the many human ills can be mitigated if not eradicated by extending the operations of scientific reason into human affairs" (LWM 17; emphases in original).

Now, there is a larger question to be pursued in how to think about what happens to professional philosophy of science once it is disassociated from Nagel's vision. And one may well, then, be tempted by Reisch's judgments about the discipline. But that goes beyond the present task of this chapter.

I conclude: I have tried to alert the reader to some of the weaknesses in Nagel's meta-philosophy. I have not tried to evaluate its significance. For this must be judged, in part, by the epistemic and social fruits it has born. My sense is that Nagel's philosophy can survive such scrutiny. All I have argued is, first, that Nagel's position is neither (quietest) "despair" or (socialist) "foolish optimism" (LWM 17). Rather, second, Nagel articulated a philosophy of science that both *expresses* the egalitarian and pluralist commitments of a liberal society and is supposed to *advance* them. In so far as Nagel's philosophy seems out of date, we should not discount the possibility that this is so because science and society have drifted toward inegalitarian and anti-liberal positions. For those of us who share in Nagel's liberal aspirations, we must expect to construct a philosophy of science worth having anew in each generation.[48]

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Part V
Previously Unpublished Papers

Chapter 13
Interview with Ernest Nagel by Remmel Nunn



Matthias Neuber and Adam Tamas Tuboly

13.1 Editorial Introduction

This interview with Ernest Nagel, conducted by Remmel Nunn is to be found among the "Ernest Nagel Papers", Box 6, at Columbia University's Rare Book & Manuscript Library.[1]

Remmel Nunn entered Columbia's Ph.D. program in 1974, and was selected later by the department to make the Nagel interview. Nunn left the Ph.D. program in 1978 after receiving an M.Phil degree to pursue a career in publishing. He is now vice president for new product development at Readex, where he leads the strategy and acquisitions team for the "Archive of Americana" publishing program. Prior to joining Readex, Remmel was vice president for academic library strategy at Gale Publishing. His previous positions include Publisher at Facts on File Inc. and Publisher at the Grolier Educational Corporation. Remmel holds an M.Phil. from Columbia University (Philosophy), a B.A. and M.A. (specifically a degree called "PPP"—meaning Philosophy, Psychology, and Physiology) from Oxford University (Corpus Christi College), and a B.A. from the University of Arkansas.

[1] We edited this interview and reproduce it with the kind permission of Yvonne, Sidney, and Alexander Nagel, and Remmel Nunn. All rights reserved. Adam Tamas Tuboly was supported by the MTA BTK Lendulet Morals and Science Research Group and by the MTA Premium Post-doctoral Scholarship. We are grateful to Fons Dewulf as well, who called our attention to this interview and helped us at the beginnings.

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With his substantial background, Remmel conducted a semi-structured interview, where on the base of Nagel's published works Nunn drafted a preliminary list of interview questions with the guidance of the oral-history staff (see below) but the questions changed significantly in reaction to this responses during the course of the sessions.

At the time, as noted in the interview's typescript as well, Nagel lived in an apartment near the Columbia campus, and he suggested that they conduct the interview there in a series of two-hour tape-recorded sessions, which they held weekly.[2]

The interview was part of the Columbia Center for Oral History (CCOH) which was founded by historian and journalist Allan Nevins in 1948. CCOH has over 10,000 interviews, with the goal to "record unique life histories, document the central historical events and memories of our times, provide public programming, and to teach and do research across the disciplines."[3]

When editing the interview, we aimed at the minimum of changes in the text. We noted all the pencil additions and later remarks within parentheses and brackets in the footnotes. Nonetheless, some too obvious typos have been corrected, while others are noted with [brackets]. In other cases, we corrected a name, for example, in the main text, and noted the change in the footnotes. But the original typescript was in a well-advanced and adequate form, thus our editorial work focused mainly on providing biographical and bibliographical notes to the text. Given that, we tried to focus on those figures who are either unknown for contemporary readers, or could be suspected that are not known among all those readers who would be interested in this interview. Thus, while we did not provide any background on Rudolf Carnap and Moritz Schlick (presupposing that many people interested in the Vienna Circle would found this interview useful), we still noted some issues about Friedrich Waismann and Philipp Frank because they still did not get much attention from historians and philosophers. The same goes for many members of the Philosophy Department at Columbia (Randall, Woodbridge, etc.), while we assumed that no information is needed on Edmund Husserl, Ludwig Wittgenstein, or Martin Heidegger.

13.2 The Interview

Interview 1.

Interview with Ernest Nagel by Remmel Nunn

New York City, February 11, 1983

Q: This is the first of a series of interviews with Ernest Nagel, emeritus professor of philosophy at Columbia University. It is being conducted on February 10th, 1983

[2] All this is gathered from Remmel Nunn, via email communication, July 15, 2020. We are extremely grateful for his kindness and help.

[3] See https://library.columbia.edu/libraries/ccoh.html (accessed on 15 July, 2020).

at the Nagel's apartment on Claremont Avenue, New York City. The interviewer is Remmel Nunn.

Professor, before we discuss your career at City College, could we talk about what you did prior to that?

Nagel: Before going to City College?

Q: Right.

Nagel: Well, as I was remarking, I had some difficulties when I went to high school because of the financial condition of my family. And I left school once for about a half a year and eventually returned. Then a similar crisis arose a year after that, and my father asked me to go out and earn some money. The money couldn't have been very large, because I remember I served as an office boy, and the salary there was $5 a week. Five dollars meant a good deal in those days, but still it wasn't a magnificent salary either.

Now, the second time around when I think I went to see the vice-principal to say, "I have to leave school," I forget what the laws were—why I was able to do this—but apparently one could at that time. And this time both the vice-principal and a teacher in history whom I knew made a very serious effort to find me some work after school hours, which they did. I served as a page in one of the public libraries. That was I think something like $20 a week. You put books in their places and that kind of thing.

Q: What year was this? Do you recall exactly what year it would have been?

Nagel: It probably was in 1920, something like that. I finished elementary school in 1914, and I went to the Lutheran high school, and I was there I think for four years. So it must have been around 1918, 1919 that I started City College. Just what year it was that I left the school and came back … in between those.

I continued this job of being a page in the library even after I went to college. I did that regularly, and I forget now just how long that continued. When I finished college—this was in 1923—, by that time I had a license to teach in the elementary schools. One could do that in those days if one took the required education courses while one was still in college.

So I did that. But I wanted to start the graduate work, and I did that in mathematics. I got a master's degree at Columbia.

Q: So you didn't really plan to teach high school as a career.

Nagel: One couldn't very well do that in those days. You had to have a certain number of years of experience before you did. And I taught elementary school for a year and a half, and I took the examination to teach with a high school license, and I taught mathematics in high school.

Now, it was rather a busy, strenuous life in a sense. I taught high school, and then I went on with this job of being a page. And then when you could squeeze in some graduate courses, you did that. So I had at least three responsibilities.

Q: Did you have to do a great deal of traveling back and forth? Were you in very disparate parts of the city?

Nagel: Well, I worked in several public libraries. Some were way downtown on the east side. Eventually I worked in the library on East 79th Street, and we lived on 79th Street, and so that was quite easy. But it was this constant running around from one place to another. And so in a sense I didn't get the most out of my college education. I was involved, of course, in these other things.

Well, let's see: I taught mathematics in the high schools until…it must have been 1928. Meanwhile, my father died, so I was the breadwinner of the family—that sort of thing. And while I continued taking graduate courses—first in mathematics until I got the master's, and then I transferred to the Department of Philosophy—, there was no hope (at least I didn't have any hopes) that I would ever be able to do any college or university teaching. One took graduate courses because you wanted to, not because it offered any way of…

Q: Why was there no hope?

Nagel: Well, for a number of reasons. In the first place, the demand for people teaching in college was not very great. Secondly, the number of places where somebody with my background could teach was very small.

For example, I was brought up as an Orthodox Jew. In fact, that was the situation. And in those days, although the Department of Philosophy was free of this kind of outlook, many of the departments of Columbia were strongly ethnic-oriented; and to go out of town would have been even more difficult, partly because I couldn't very well leave. I had a sister who was going through college and my mother, so I had to be around to run the place, so to speak.

Q: You mentioned being a breadwinner. Was it a large family that you were brought up in?

Nagel: No, it was my mother and my sister and myself.

Q: So you actually taught entire time that you were at City College.

Nagel: Yes.

Q: When you went to Columbia, was there a reason why you chose it?

Nagel: I suppose it didn't really occur to me that I might go anywhere else. You see, even other institutions in New York required tuition, while City College at that time required no tuition whatsoever. If there hadn't been a free college, I'm sure I would never have gotten a higher education.

Q: Did you go to Columbia on a fellowship?

Nagel: No, you see, in those days the idea that a graduate student would be supported by the government was fantastic. Nobody would ever think of that. And so both I and friends of mine who were following a parallel path took graduate courses for which you paid yourself by doing some work outside. Things have changed—I know that. You see, I've always felt that Columbia should never institute a rule which made it impossible for people to take part time graduate work, which would have excluded

many people in my generation. I don't know what the rules are at present. Probably they require a larger number of hours of attendance than I was able to give. My recollection is that I took about six points a semester. In those days it wasn't very expensive, because I think they charged $12 a point. That, of course, was eventually changed.

So I continued this sort of many-dimensional existence for quite a number of years until I finally took the qualifying examination in the Department of Philosophy, the qualifying examination for being a candidate for a doctor's degree. These things have changed completely, but it may be of interest. At that time what you were required… the examination that you had to pass consisted of the following:

First of all, you had to take a written examination in the history of philosophy for two days. I never had any courses in the history of philosophy at Columbia. Of course, Woodbridge[4] was teaching the courses. He taught them at 9 o'clock in the morning, however, when I was busy with other things.

Q: But you had taken history of philosophy courses at City College.

Nagel: Yes, the qualifying examination consisted of that. If you passed that, then you could present yourself for a two-hour oral examination before the Department. At that time the requirements were that the oral examination consisted of answering questions on four philosophers in the history of philosophy. Two of them had to be ancients, and two of them had to be moderns. And my choice was Plato and Plotinus for the ancients and Spinoza and Charles Peirce for the moderns.

Now, I'd like to just mention something that was really critical in my own life. The chairman of the committee was John Coss.[5] I don't know whether the name is familiar to you. John Coss was the executive officer of the Department. He was also director of the summer session, and he was really the king pin in many ways. The real power behind him was Woodbrige. I don't think bothered him [sic], but Woodbridge did.

Well, toward the end of the examination, Coss asked me (this is very vivid in my memory) why did I pick these four. I said well, all of them represent what I thought was a strong, rationalistic outlook on the world and that I'd learned to respect this approach very highly from my old teacher at City College, Morris Cohen.[6]

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[4] Frederick J. E. Woodbridge (1867–1940) was a philosopher mostly at the Department of Philosophy at Columbia University, also one of the founders and first editors of *The Journal of Philosophy* (under the name of *The Journal of Philosophy, Psychology, and Scientific Method* between 1904 and 1920).

[5] John J. Coss (1908–1952) was a philosopher originally from Indiana, and the department's executive officer at Columbia. Although he was also a mentor figure to many, he was famous for the development of "Contemporary Civilization" course and for his organizational and institutional role.

[6] Morris R. Cohen (1880–1947) was an American philosopher and lawyer; he was professor of philosophy at the City College from 1912 to 1938.

Well, Irwin Edman[7] was present at that time, and he was a good friend of Cohen; and the next time he saw Cohen, he told Cohen about this. I suppose I must have done fairly well in the oral examination, because Cohen suddenly became interested in me; and he was able to get me a fellowship/scholarship, which enabled me to give up teaching while I tried to do the dissertation for the degree; and he did that for two years. I think the stipend on that fellowship was something like $2,000 a year, which again, of course, is absurd in these days, but $2,000 was an awful lot of money then.

Well, that's the point I think I would want to make about my career before I actually began teaching philosophy.

When I finally got the degree (I got the degree in 1930), Cohen then offered me [a post].[8] He wasn't the chairman of the Department, but he was effective in getting me an offer to teach at City College, which I did for one year full time.

Toward the end of that year I had a letter from John Coss, the committee chairman—would I come and see him, and I did; and he said "Would you be interested in getting a teaching position in the Department of Philosophy?".

Well, this was quite something; and, of course, I talked it over with Cohen and my family. There was no assurance of any kind of tenure at that time. As a matter of fact, I agonized over that for a long time, but I accepted the position of instructor. Again the salary there was $2,000. Let's see: I started teaching in 1931 I think and continued teaching at Columbia until '37 in that capacity—as instructor in the Department.

Edman was very kind to me and he was very frank in talking to me. A real problem arose because Cohen was going to retire from City College, and then he asked me whether I would want to come back to the College full time. This was a real problem because I realized how important it was to teach at an institution where you had a chance to teach graduate students. But there was no immediate prospect of getting tenure at Columbia. As a matter of fact, there was one incident, which I suppose if I had been very cautious I would have acted upon right away: One day (this was even before I actually began teaching at Columbia) Woodbridge asked me to come and see him. He said, "There's a temporary vacancy at the University of Texas. A professor there is going on sabbatical"…. and would I be willing to accept a temporary appointment at the University of Texas? Well, I told him I couldn't do that because of my family responsibilities, but this produced really a kind of a tailspin. I mean even before I had a chance to teach, I was invited to find a job elsewhere. This was disconcerting, and I really didn't know what was going to happen until 1937 when I finally got a tenure appointment as associate professor.

Well, that's the background.

Q: You had mentioned once before a trip to Europe that you made. Was this while you were at Columbia?

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[7] Irwin Edman (1896–1954) was an American philosopher, who was associated with Columbia during his entire career. Besides his philosophical work, he was known for writing poetry and fictional works.

[8] Addition in brackets in the original.

Nagel: This was while I was at Columbia. I applied for a Guggenheim. This was in '33 I think or '34. I got the Guggenheim Fellowship in '34, maybe in the spring of '34, and the next fall I went abroad.

There was an International Congress of Philosophy in I think early September of '34.[9] This was an International Congress at Prague. I attended that, and I wrote a report of the Congress for the Journal of Philosophy.[10]

I had sort of a grand tour of Europe. I went to Prague first, and I had some relatives at that time in Vienna, and I stayed in Vienna for about a month, maybe two; and I got to know some of the people in Vienna. For example, I got to know Schlick; I got to know Waismann[11]; and also people in other departments—I mean people who were members of the Vienna Circle.

Then I went on to Poland. I went to both Warsaw and at that time it was known as Lwow. It's no longer part of Poland, because Russia took it. The center of logic, at least in East Europe in those years, was Warsaw. I got to know Lukasiewicz[12] and some others. And also in Lwow there was a most interesting man by the name of Jan Chwistek.[13]He was kind of a polimath. I mean he made an interesting contribution to logic.[14] He was a painter. He was a literary critic. And he was strongly left-oriented in his political views.

That was really a wonderful experience, to meet a person like that.

Well, from Poland I went then to Prague and spent at least a month there, and I got to know Rudolf Carnap and Philipp Frank[15] very well—some other people as well, but they were the main people.

And from Vienna I went to Italy, because I wanted very much to meet some of the Italian people interested in what I was interested in—philosophy of science—who were in Rome. So I spent about a month, maybe two months, in Rome. A man I got

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[9] Nagel has in mind the Eight International Congress of Philosophy, held in Prague, from September 2nd to September 7th 1934. Before the congress was held the "Preliminary Conference of the International Congresses for the Unity of Science", organized mainly by Otto Neurath to promote the scientific world-conception.

[10] See Nagel (1934).

[11] Friedrich Waismann (1896–1959) was an Austrian philosopher, student of Moritz Schlick, and member of the Vienna Circle. In 1938 he emigrated to the United Kingdom at taught philosophy first at Cambridge, later in Oxford. About Waismann's life and work, see Makovec and Shapiro (2019).

[12] Jan Łukasiewicz (1878–1856) was a Polish logician and philosopher, member of the so-called Lvov-Warsaw School He is known for of his many contributions to logic, including the formal investigations of Aristotle's logical and methodological works.

[13] Nagel definitely has in mind Leon Chwistek (1884–1944), the Polish philosopher, logician, and avant-garde painter, who was also a member of the Lvov-Warsaw School.

[14] The English translation of one of Chwistek's papers on logic has been recently updated and republished as Chwistek (2018).

[15] Philipp Frank (1884–1966) was an Austrian physicists-turned-philosopher, member of the Vienna Circle. He was the successor of Einstein at the German University of Prague (1912–1938) but he emigrated to the United States later and taught at Harvard between 1939 and 1954. About his life and works see the edition of his last unpublished book, Frank (2021).

to know (was) a man by the name of Enriques [Frederigo Enriques],[16] who was a mathematician and also published a book at that time on the philosophy of science. And I also met somebody that I was not sure I would ever see in the flesh, and that was Santayana.[17] He was living in Rome at that time in a hotel.

If I may just interrupt this sequence, I read Santayana when I was in college in a course that Cohen gave called The Philosophy of Civilization. It was a year course, and the first semester consisted of a survey of various theories of social progress. There was the biological theory, a psychological theory, an economic theory and so on. He went through a whole bunch of them, and of course he indicated what the limitations were. The second semester was devoted to reading Santayana's Life of Reason,[18] and that I must say was an unforgettable experience for me. Here you had a man who had been all over the universe, so to speak; and yet his points of view on different sorts of things as they were absorbed by one mind: I can't really say how much I appreciated that.

I only met him once in Rome. I didn't know where he lived. I wrote to his agent in London, and eventually I heard from him in Rome, and he invited me to have lunch with him, and he was really a wonderful host. At that time his assistant or secretary was Daniel Cory.[19] I don't know whether that name is familiar to you. He was there. We had lunch in a very elegant restaurant. Santayana ordered what you would have to eat and then told you how to eat it and so on. And after the lunch Cory left, and then Santayana took me for a walk on the Capitoline Hill in Rome, and he talked the way he wrote.

He had in his hand a book which surprised me. This was Heidegger's Sein und Zeit.[20] And I said, "I was very much surprised that you should read that book, which seems to me so incompatible with your general point of view." And he said, "Oh, no, no. You must understand this book. You must interpret it." And he began to interpret Heidegger, and it turned out to be Santayana's philosophy, you see. (chuckles) But it was really wonderful.

Well, from Italy I eventually went to Holland, where there also was a kind of a logic school, particularly involved in the philosophy of mathematics. I don't know whether you happen to be familiar with this trend in the philosophy of mathematics, the so-called intuitionist theory.

Q: Yes, is that Brouwer…?[21]

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[16] Bracketed note in the original typescript. Federigo Enriques (1871–1946) was an Italian mathematician who is also known for his contributions to the philosophy and history of mathematics.

[17] George Santayana (1863–1952) was an American philosopher and essayist (originally from Spain).

[18] See Santayana (1905–06).

[19] Daniel M. Cory (1904–1972) was an American author and Santayana's literary secretary and assistant who published later Santayana's correspondence.

[20] Heidegger's major book, *Sein und Zeit* (*Being and Time*) was first published in 1927.

[21] L.E.J. Brouwer (1881–1966) was a Dutch mathematician, often regarded as the founder of mathematical intuitionism.

Nagel: Yes, Brouwer is there and also another man who is not so well known, also a mathematician, by the name of Maoueri.[22] And so I saw some of the people there. And from Holland I went on to England.

I knew somebody in London. There was this philosopher by the name of Susan Stebbing[23] there. I don't know whether the name is familiar to you.

Q: Yes.

Nagel: She was teaching in one of the colleges of the University of London. I knew her because the year before that she had come to Columbia.[24] Woodbridge was going to be away on a year's leave as a Roosevelt (?) professor in Berlin.[25] And so in order to replace him they got Susan Stebbing, and that was my first experience in meeting an English philosopher. She was a very determined lady.

This will be typical. When she came was when I started teaching a graduate seminar at Columbia, and this was supposed to be a graduate seminar in logic or something like that. And I was asked whether I would share this seminar with her, and I said, "By all means." And she really dominated the seminar, and I barely opened my mouth for fear of the lady, you know. I remember sometimes I would make a comment, and then she would turn to me: "Now, don't be a fool, Nagel," and this sort of thing obviously didn't inspire me to say more.

I wasn't reconciled to that until in December of that year when there was a meeting of the Philosophy Association. It was at Harvard. I attended that, and there was a joint discussion on something or other—I forget now what—by Susan Stebbing and Whitehead.[26]

Q: Alfred North Whitehead?

Nagel: Yes. At one point Whitehead made some remark and then she said to him: "Don't be a fool, Whitehead." (laughter).

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[22] The original typescript contains "Maoueri", but given that the transcript had many typos, Nagel presumably had in mind Gerrit Mannoury (1867–1956), the Dutch philosopher and mathematician, who was a central figure in the so-called Significs movement, and a regular attendant of logical empiricists events.

[23] Susan Stebbing (1885–1943) was a British philosopher, one of the leading figures of the analytic movement in Britain. She played a major role in the so-called Cambridge School of Analysis and she was the first woman to hold a philosophy chair in the United Kingdom. Stebbing also played a vital role in bringing logical empiricists (like Carnap, Schlick, Hempel, and Neurath) to England.

[24] Stebbing was a visiting professor at Columbia University from 1931 to 1932.

[25] Question mark within parentheses in the original. Woodbridge was "Visiting Roosevelt Professor of American History and Institutions" at the University of Berlin from 1931 to 1932.

[26] Nagel presumably had in mind the Annual Meeting of the Eastern Division of the American Philosophical Association, at Harvard University, held between Christmas and New Year in 1931, where Whitehead delivered the presidential address and Stebbing her paper on "Substances, events, and facts.".

On this world tour I took at Cambridge (I met Susan Stebbing in London) a course by Moore, by Braithwaite[27]…. I also once sat in on a course that C.D. Broad[28] was giving. I couldn't really take more of that. He was giving a course in the history of philosophy. And when I was there he was talking about Schopenhauer, and he did this thing that I didn't understand why he did it that way: he would say a sentence and repeat it twice. That is, every sentence was said three times. And it wasn't until much later that I realized that he was brought up [to do that].[29] He was a Scot, and it was customary in Scotland at Scottish universities. Students were too poor to buy books, and so the professor would say these things three times so they could really have it. But one session was [enough].[30]

Q: It would be pretty slow going.

Nagel: Then I retired to listen in on Wittgenstein, and I asked Moore was it possible for me to do this. And he said, "Oh, of course. You'll have to ask his permission, Wittgenstein's permission." So I went to see him in his rooms (they were in Whewell Court, as I remember, in Cambridge) and asked whether he would permit me to go to his courses. And his first response was no. He said he doesn't like philosophical tourists. He was very impressive, had very piercing eyes, a big shock of black hair. Then he added: "You know, you wouldn't understand what I'm saying anyhow." So I made a terrible mistake. I said, "You know, I spent some time in Vienna and I went to some of the lectures of Schlick and Waismann[31] and so I think I have some preparation for this." And he became really angry. He said, "You don't think that this is to be an adequate preparation to understand what I'm saying!" Well, I still persisted, and finally he said to me, "You know, whenever I lecture I suffer; and if I see a stranger, I suffer even more. Now, would you want to see me suffer even more?" And what can you say to that?

Q: So you didn't ever get to hear Wittgenstein?

Nagel: So I was out. I couldn't do that. But I think I profited by the experience I had in Cambridge. It was a curious place in those days, because Wittgenstein was the only thing that students talked about. Moore was no longer top dog and neither were the other people.

I remember I was introduced to Margaret Macdonald.[32] She was a philosopher. I think she eventually taught in London. And when we were introduced, the first thing

[27] Richard Braithwaite (1900–1990) was an English philosopher mainly at Cambridge. He worked on the philosophy of science and the philosophy of religion.

[28] C.D. Broad (1887–1971) was an English philosopher, associated mainly with Cambridge University, who specialized in philosophy of science and moral philosophy.

[29] The original typescript contains the addition within the brackets.

[30] The original typescript contains the addition within the brackets.

[31] The original typescript had here "Leischman", but Nagel definitely had in mind "Waismann".

[32] Margaret MacDonald (1907–1956) was a British philosopher, associated with Cambridge. She studied with Moore and was a member of Wittgenstein's internal circle; some of her notes of Wittgenstein's lectures were later published. She also edited an influential volume on *Philosophy and Analysis* (1954, Oxford).

she said to me was: "Don't you think Wittgenstein is the greatest man alive?" I said, "Well, probably."

I got to know something of Wittgenstein's views because there was something called "The Blue Book," which was a sort of under-the-counter circulation. I don't know who really took the notes on this course,[33] but I was able to borrow a copy of that; and you got the sense of what Wittgenstein was about, at least thought you had a sense of it. Eventually there was also a "Brown Book." Both of them have been published since Wittgenstein's death.[34]

So it was to me a very rewarding experience, particularly since the contrast with Columbia was considerable. In those days the emphasis at Columbia was largely on the history of philosophy, while here you had problems that you discussed, you see. While one gets adjusted to all these things, it was a good complement to the kind of training I was getting at Columbia with its emphasis upon really turning to some of the problems of philosophy.

Q: Had Whitehead left Cambridge by this time and come on to Harvard?[35]

Nagel: Yes.

I forget now when he did leave. He was still teaching when I finished City College, and I wanted very much to take work with Whitehead, because I'd read so many of his books. He published two books while I was still in college. One of them was called The Concept of Nature, and the other one was called Principles of Natural Knowledge,[36] and both of them at least gave me the impression that this is the way to solve the fundamental problems of philosophy.

I met Whitehead eventually. In '36 there was a bicentennial of the founding of Harvard, and they invited a lot of foreign visitors, including Carnap, who came[37]; and Carnap spent the summer in Cambridge, and he invited me to stay with him. At that time Paul Weiss[38] was also in Harvard, and he asked me whether I would be interested in going to a soiree that Whitehead held. Of course, I'd be delighted. And I sat next to Whitehead for a short while, and Whitehead was telling me what the contents of Volume 4 of the Principia Mathematica would be. It's never been

[33] Wittgenstein dictated many notes in 1933 and 1934 and they were circulated internally in a blue cloth, hence the name. The notes were dictated to H.M.S. Coxeter, R.L. Goodstein, Francis Skinner, Margaret Masterman, R. Braithwaite, and O.K. Bouwsma.

[34] Wittgenstein dictated many notes also in 1934 and 1935 and they were circulated internally in a brown cloth, hence the name. The notes were dictated to Francis Skinner and Alice Ambrose. The two books were published in 1958 as Ludwig Wittgenstein, *Preliminary Studies for the "Philosophical Investigations", Generally known as The Blue and Brown Books*.

[35] Whitehead was a professor of philosophy at Harvard between 1924 and 1937.

[36] See Whitehead (1920); and (1919).

[37] In fact, in 1936 they organized the tercentenary of Harvard University. Carnap was able to go to America because of the Harvard-invitation. At the conference Carnap lectured about logic as a determining factor in human behavior (see Carnap 1937). After the celebration, Carnap was awarded a honorary doctoral degree at Harvard.

[38] Paul Weiss (1901–2002) was an American philosopher, founder of *The Review of Metaphysics*. He was mainly associated to Harvard and is known for his metaphysical works.

published, you know. And so he began this, and I think I knew what he was talking about because I'd read an early book of Whitehead's called The Universal Algebra,[39] which dealt with a kind of mathematics that I hadn't been familiar with. And just as he was in the midst of telling me about this, somebody in the family tapped me on the shoulder and said, "It's time to circulate."

Mrs. Whitehead also was quite a lady. She was an interesting woman. She began talking about Russell, and she hated Russell. And why? Well, because he didn't recognize her husband's genius. Personally I don't think it was something that Russell gave a hoot about, and Mrs. Whitehead resented this.

Q: And in a sense Whitehead was Russell's mentor to a degree. He was his teacher at Trinity.

Nagel: Yes. And Mrs. Whitehead felt the same way about Morris Cohen. Again, he didn't make proper obeisance to her husband. So this was very very fascinating.

Q: Was Whitehead himself an outgoing, genial man?

Nagel: Yes. He looked like a foxy grandfather. There used to be a comic strip of somebody with white hair and little tufts around here. He talked in a high-pitched voice. He was very pleasant, very pleasant, not difficult at all to talk with—unlike Moore, who was difficult. You had to initiate all the conversation.

Q: Well, you were speaking earlier of being in Cambridge with Professors Moore, Braithwaite, Broad; and at this point Whitehead had left. Where was Russell at this point?

Nagel: Let me see: this was in '36 I think.

Q: Had he come to the United States also?

Nagel: I guess he was in the United States. You see, he first went to the University of California in Los Angeles; and then he got an offer from City College and a better salary, and Russell accepted it, and when he came he ran into trouble, because the Bishop of New York, a man by the name of Manning,[40] made a public protest at Russell teaching at a city institution because he was an immoral man—his views on marriage were anathema to him. And so the City Council on LaGuardia's orders did not defend Russell, did not defend Russell [sic], and so he was forced out.[41]

Q: Did you have any dealings with him during that period?

Nagel: No. Eventually I met him just sporadically. There used to be a program on Sundays—it was a regular program—called "Invitation to Learning." I don't know whether you heard about that one. This was usually a threesome, somebody who

[39] See Whitehead (1898).

[40] William T. Manning (1866–1949) was a U.S. Episcopal bishop of New York between 1921 and 1946. Manning was successful in the fight of preventing Russell to obtain a professorship in New York because of his atheism and advocacy of free love.

[41] See Dewey and Kallen (1941).

was the chairman of this thing discussing one great book with two other people. For example, one of the books that was discussed was Hobbes. Another one was James' Psychology; another one was Boethius. I was on one or two programs with Russell.

Russell was an aristocrat, you know. He really was an aristocrat. I think he held you at his arm's length.

Q: Someone that you had mentioned in passing, John Dewey, one of the most famous of the Columbia philosophers of that era: what were your impressions of Dewey when you were studying at Columbia?

Nagel: Well, while I was still teaching (I think it was) in the high school in New York I had only one course with Dewey—I couldn't tell you of any other. This was this course in Types of Logical Theory, and I think that course used to meet from three to four twice a week, something of that kind. So I could just make it from teaching in the school system.

He was a very difficult lecturer. You didn't know what on earth he was talking about….almost as bad as his writing. I still recall the way he would come in with a piece of paper no larger than the palm of his hand on which he had something written. And then he would talk, and you had the sense of a man who didn't have a finished thesis to teach but he was thinking through a problem, and it was terribly impressive. And every once in a while he would stop and go to the window and look out; he was still thinking about this.

I remember some of us used to keep awake by taking notes, because he was so slow and he drawled.

I remember there was a sequence of his courses—maybe two or maybe three of them—in which he talked about the meaning of "this". Finally, at the end of this, he said, "Well, it's clearer to me now than it was before." (laughs)

But I never got to know him very well. One of the reasons was that he had the appearance so much of a sage that I felt I didn't have anything to say to him.

Another reason I suppose—I'm not sure: much as I respected him and learned so much from his views, he had an attitude which wasn't entirely congenial to the young. He was very much of an activist, you know, and his thinking was preparation for acting. And while up to a point this sounded fine, I was more of a contemplative kind, and I think I had the prejudices of Morris Cohen. So I didn't quite feel that I was able to talk to him at any length. He once did ask me to look at one chapter he had written for his book on logic, and I made some comments, and he was very kind and appreciative and so on.

But then there was an interesting man: Arthur Bentley.[42] I don't know whether that name is familiar to you—not Eric Bentley but Arthur Bentley, who came from Indiana, and had published a book around 1909 The Process of Government,[43] and this was apparently the first book devoted to pressure groups in government.

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[42] Arthur Bentley (1870–1957) was an American philosopher and political scientist, known for his work on behaviorism in political philosophy and for his pragmatism.

[43] See Bentley (1908).

Several of us joined that course. One of them was Randall[44] and Edman and myself. Bentley got to know Dewey very well, and they collaborated in a series of articles that appeared in the Journal of Philosophy and were eventually printed in a book.[45] It was jointly Dewey and Bentley, although Bentley I think did all the writing. This was a critique on a lot of the contemporaries. I think I've got the book here. It was sharp criticism of some of the logical positivists, and I thought that this was unfair, that some of the remarks made were unfair; and I wrote to Dewey and said, "I don't believe that his is an accurate statement." And Dewey wrote to me rather sharply and said he had absolute confidence in Bentley.

Q: Well, I guess then Professor Cohen remained one of your closest colleagues during this whole period.

Nagel: Yes. I was no doubt very much influenced by him. What Cohen did for many of us: he opened up fields for us. In the first place there was the philosophy of science; there was social philosophy; the philosophy of law; also metaphysics. And I think most students who continued in some kind of academic career were oriented to some of the things that Cohen was interested in. So even though I think some of his former students have been highly critical of him, I don't think they've been very fair. I don't think they really recognize what they owe to him. I'm thinking of two people who've written on Cohen. One of them is Lewis Feuer,[46] and the other one is Sidney Hook.[47] And both of them have said things about him that I think are just out of this world.

Q: About him or about his philosophical viewpoints?

Nagel: About him. About him as a teacher, about him as a thinker.

Q: Well, when one does consider Professor Cohen's philosophy as opposed to Professor Cohen—do you feel like his views have formed an enduring part of the…?

Nagel: No, I'm afraid I don't. Of course, I reread him very often, and I think myself that his best work has been in the philosophy of law rather than in the books [n/a].[48] He prided himself in particular on Reason and Nature.[49] Reason and Nature is not really a book—it's a collection of essays. They've never been congealed. And he had this very frustrating habit of stating the position of a man and refuting him almost in the same sentence. So though he talked about the virtues of careful analysis, in a lot of his writings that doesn't appear except in the legal works. And so in a way the

[44] John Herman Randall Jr. (1899–1980) was an American philosopher, historian, and educator, teaching at Columbia from 1925.

[45] See Dewey and Bentley (1949).

[46] The original typescript had "Louis Feuer", but Nagel definitely had in mind Lewis Feuer (1912–2002), an American sociologist and philosopher, who was a student of Morris and was known for a long time for his Marxism.

[47] Sidney Hook (1902–1989) was an American philosopher, known for his pragmatism. Hook was initially a socialist, but after the Second World War he was converted to conservatism and became an ardent anti-communist.

[48] Missing phrase from the interview's typescript.

[49] See Cohen (1931).

neglect into which his writings have fallen: I think that's justified. He was I think a very significant teacher, and he had this gift of talking about things that otherwise you'd never heard about, and you'd run to the library to find out what it was about.

Q: Well, I think perhaps that's enough for today, and then next week we can take it from here.

Interview 2

Interview with Ernest Nagel by Remmel Nunn

New York City

Feb. 17, 1983

Q: This is the second of a series of interviews with Emeritus Professor Ernest Nagel. The date is 17 February, 1983. The interview is being conducted in Professor Nagel's apartment on Claremont Avenue, New York City. The interviewer is Remmel Nunn.

One thing that you spoke about last week was your trip through Europe and your discussions with members of the Vienna circle. You said that with Santayana, for example, that you wrote and set up the meeting. How did you make your connections with the people in Vienna, for example, and Poland?

Nagel: Well, of course, I knew a number by name because I'd read some of their things, and I simply got in touch with them; and I think without exception they were very friendly, and I sat in on a seminar that Schlick was giving and also Waismann.[50] Carnap was no longer in Vienna. He had already gone on to Prague.[51]

Another man I got to know quite well was a man who eventually came to this country, Felix Kaufmann.[52] I don't know whether the name is familiar to you.

Q: No.

Nagel: He was a rather interesting person, too. He was both a great admirer and partly follower of Husserl and also was interested in the philosophy of law, and so he was a great admirer of Kelsen,[53] who was a jurisprudent. Kaufman[n] himself had only a slight connection with the University of Vienna. He was a private dozent. Private dozents lived on what they obtained from the people who attended the lecturers, which wasn't very large. So Kaufman[n] actually made his living by being a representative

[50] The original again had "Leischman" here, though it was crossed out with pencil and "Weismann" was introduced instead.

[51] Carnap was in Prague between 1931 and 1935. On his time there see Tuboly (2021).

[52] Felix Kaufmann (1895–1949) was an Austrian philosopher, specialized in the philosophy of law, though he made his living as a manager of the Anglo-Iranian Oil Company in Vienna. He was known for his work to find bridges between logical empiricism and phenomenology; after his emigration to the United States, he was working in the relationship between pragmatism and positivism.

[53] Hans Kelsen (1881–1973) was an Austrian jurist and legal philosopher. Kelsen was the author of the 1920 Austrian Constitution; he also had many ties to the Vienna Circle. In 1930 he went to Germany, but left it for Geneva and later in 1940 he went to the United States.

of a Dutch oil company. They got oil from the Middle East. And he visited in his office and he took care of his actual business very shortly and briefly and spent his time doing philosophy.

When the Nazis came into Vienna he came to the United States and spent a number of years at the New School of Social Research, but he died suddenly.

One other man that I don't think I mentioned before: a professor of mathematics by the name of Karl Menger.[54]He was regarded as the white-haired boy, the hope of the mathematicians of Vienna. His father was a very distinguished (he's no longer alive; he was there when I was in Vienna) economist. It was the so-called Vienna School of Economics, which developed an alternative for the labor theory of value. He, too, was a member of the Vienna circle, and I got to know him, too. He came to this country, too. He's now at the… I forget what it's called. I think it's called the Roosevelt University in Chicago.[55] Maybe it's not called that anymore. It's very strange. He had a tremendous reputation…in Vienna at any rate. None of the promise that he apparently showed at that time has been realized, so he really is kind of in obscurity.

Q: Last week you mentioned that at that point in mathematics you had an intuitionist school and a formalist school. Did he belong to either?

Nagel: Well, I think by and large he was a formalist. He didn't write, as I recall, very much on some of these issues in mathematics.[56] His specialty in mathematics was dimension theory. I don't know whether this is the notion: how do you define the notion of dimension?[57] For example, a vector would be in two dimensions, and the question is, is there any formal way of defining this, and he wrote what at that time was regarded as a very important and original book on that subject. But it all lacked something; it was interesting, but I think that most of his professional colleagues [?].[58]

In Vienna itself the one man who was interested in it and I think very much influenced by the intuitionist school was Felix Kaufmann. He, as a matter of fact, published a book I had read before I went to Europe, in a course in general philosophy. At that time it was not translated. The English title of the translation is "The

[54] The original typescript had "Carl Menger", but Nagel is obviously talking about the mathematician Karl Menger (1902–1985) who was a member of the Vienna Circle. Carl Menger (1840–1921), the father of Karl, was though also a renowned economist from Vienna.

[55] At first, Menger spent the academic year 1930–31 at Harvard and at the Rice Institute in Houston, Texas as a visiting scholar. While he went back to Austria, due to the raise of Nazism he emigrated to the United States in 1937. He went to the University of Notre Dame at Indiana, but in 1948 he went to the Illinois Institute of Technology in Chicago and remained there until the rest of his life.

[56] In fact, Menger wrote a lot about the criticism of intuitionism from very early on as he was an assistant to Brouwer in Amsterdam between 1925 and 1927. See his papers in Menger (1979).

[57] See Menger (1928).

[58] The end of the sentence is missing from the typescript and only a [?] mark is noted there.

Infinite and its Elimination from Mathematics".[59] And like some of the Intuitionists, Brouwer, for example, or Bridgman,[60] he thought that a good part of the theory of infinite numbers must be dropped because it didn't conform to the usual standards. And Kaufman wrote about this and defended the conclusions and took the position that every question that can be intelligibly asked—for example, in arithmetic—would have a definite answer, yes or no, purely from deductions, from the axioms of arithmetic.[61]

Well at that time the logician, Gödel[62]—I don't know if that name is familiar to you—, was still in Vienna; and he had already published his own most important paper.

Q: Gödel's Theorem?

Nagel: Yes. And in that paper Gödel showed that there are questions in arithmetic which can neither be proved nor disproved. What Kaufman did about it, I don't know. I don't know that much about his reaction, but when the book was translated into English…. I'm not sure whether that was before Kaufmann died. It probably came out about three or four years ago, and I wrote an introduction to the English edition.[63]

Q: Did you keep in touch with the other members of the Vienna Circle over the years?

Nagel: No. Most of them are just gone. The only ones that I know are still among the living: one of them is Carl Hempel.[64] He used to be in Princeton. Now he's retired from Princeton and works at the University of Pittsburgh. Him I've known certainly since he came to this country.

The same is true with another member, original member, of the Circle, Herbert Feigl.[65] He's still alive but he's been very ill. I don't hear from Hempel, but I talk to him quite often.

There are other people, too, but they've narrowed down quite a lot.

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[59] While the German original indeed corresponds to what Nagel indicates above (Kaufmann 1930), the English translation became a bigger volume with further papers, see Kaufmann (1978).

[60] Percy W. Bridgman (1882–1961) was a Nobel Prize winning American physicist and philosopher, known among the public as the founder and defender of operationalism.

[61] The last sentence was supplemented by Nunn; a note with pencil was introduced: "Nagel should check this.".

[62] Kurt Gödel (1906–1978) was an Austrian logician and mathematician, mainly known for his incompleteness theorems regarding mathematics. He emigrated to the United States in 1939 (after several visits) and worked in the Institute for Advanced Studies at Princeton.

[63] See Nagel's preface as Nagel (1978). Kaufmann's original book appeared in 1930, a year before Gödel's results were published, thus their significance is not reflected in Kaufmann's work on the philosophy of mathematics, which merits much of its strength and historical novelty for pursuing a phenomenological approach as well.

[64] Carl G. Hempel (1905–1997) was a German philosopher who studied with Reichenbach and visited the Vienna Circle as well several times. He emigrated to the United States teaching at various universities, but mainly at Princeton and Pittsburgh.

[65] Herbert Feigl (1902–1988) was an Austrian philosopher, who studied with Moritz Schlick in Vienna, and emigrated to the United States in 1930. He established the Minnesota Center for Philosophy of Science in 1953.

For example, there is a philosopher, Gustav Bergmann,[66] who gave up the hope of ever getting a position to teach philosophy in Vienna because he was Jewish. So he switched his way of earning a living and became a lawyer, and then when he came here he became a philosopher again. He's now retired from the University of Iowa. But I haven't seen or spoken to him for a long time. I've been meaning to.

But most of them are… Well, Carnap came to this country, and I knew him very well. He's dead. Reichenbach[67] is dead. A man by the name of Otto Neurath[68]: he traveled all over. He was kind of an impresario. He's no longer alive. So there's just a few, a straggling few, who are still left.

Q: Was it Neurath that…? I know a basic part of the Vienna Circle's positivistic viewpoints was the question of testing scientific theories by basically disproving them—submitting them to continual testing. Wasn't that one of Neurath's contributions?

Nagel: Well, at that time the logical positivists held onto the view that every proposition must be capable of complete verification. Well, that was too simple a rule. That was abandoned. But, you know, Karl Popper was in Vienna at that time. I forget whether I talked to you about Popper.

Q: You mentioned Popper.

Nagel: Well, Popper's book was eventually translated into English. The German title was Logik der Forschung—The Scientific Theory of Discovery; that's the translation.[69] There the point of view was developed—and it's been very influential—that a proposition can be meaningful even if it's not completely verifiable. But a proposition is not a scientific proposition if it is not possible to refute it.

Q: Right.

Nagel: So he made the possibility of refutation as the hallmark of a scientific theory. Well, this has been strangely, terrifically influential, terrifically—both at that time

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[66] Gustav Bergmann (1906–1987) was an Austrian philosopher and a member of the Vienna Circle. As he was unable to obtain a permanent position because of his Jewish background, he got a degree in law in 1935 to work as a lawyer and moved to the United States eventually in 1938 where be became a professor of philosophy and psychology in 1939.

[67] Hans Reichenbach (1891–1953) was a German philosopher, the leading member of the so-called Berlin Group of logical empiricism. Reichenbach was among the very few students who attended Einstein's first course in Berlin on relativity, and he became an ardent defender of Einstein for decades. In 1933 he moved from Berlin to Turkey because of the rise of Nazism, and eventually he fled to the United States in 1938 where he taught at the University of California at Los Angeles until his death.

[68] Otto Neurath (1882–1945) was an Austrian polymath who worked on philosophy, education, sociology, economics, physics and logic. He led various museums and developed a new pictorial statistical method known first as the "Vienna Method of Picture Statistics" later as ISOTYPE, International System of TYpographic Education. After leaving Austria in 1934, Neurath went to The Hague, which he left in 1940 and went to England where he lived and worked in Oxford until 1945.

[69] See Popper (1934) and (1959).

and on continental Europe. And then after he escaped from Vienna, he spent I don't know how many years in New Zealand where he wrote this book called The Open Society and Its Enemies.[70] And then he finally came over to England again. Fabulous career.

Some of these things are really quite puzzling—why this position is ever so prominent. It's usually called the Hypothetical Deductive Method of Scientific Hypothesis. Deductions from the theory you try to suit whether the deductions are conformable with observation. This, too, is central to Popper's view.

Now, that this should have been hailed as a great innovation in England is strange because during the first half of the nineteenth century there was a very well-known…well, he was a kind of polymath, a man by the name of William Whewell.[71] He wrote essentially on the history of science as well as on ethics. Oh, I forget now all the things he did, but he was very much influenced by Immanuel Kant, and so the hypothetical deductive method: that was essential to his position. But the English forgot about him, and Popper made a career out of it. It's interesting.

Popper was—still is, I think—a very difficult person. When I met him in Vienna he was in a kind of discouraged mood. His book had come out, but he was not on good terms with the Vienna Circle. He attended the meetings. He didn't get along with Schlick. And the reason why he didn't was because Schlick had no use for Immanuel Kant while Popper thought he was a great man. And so, while he was on good terms with Carnap, Carnap had already left Vienna, and there he was all by himself. He was crotchety, and I thought: "Well, when the book gets better known, he'll have a reputation and he'll change into a more agreeable person." Well, he did become very well known and also honored. He became a Sir in England. But his temperament was still the same.

Let's see: I don't know who else I can tell you about. I did not know the Berlin Group, which was represented by Reichenbach chiefly, and also the mathematician Von Mises.[72] You have to remember Von Mises had a brother. The mathematician Von Mises was Richard Von Mises, and his brother was a conservative economist, and they are sometimes confused.[73] Von Mises, too, has a strong philosophical interest in and is a great admirer of Ernst Mach. He published a fairly good sized book, never

[70] See Popper (1945). Popper was teaching philosophy at the University of Canterbury in New Zealand between 1937 and 1946.

[71] William Whewell (1794–1866) was an English polymath, known as a scientist, philosopher and priest. He was working mainly in Cambridge and earned a special status among philosophers of science for his work on the *History of the Inductive Sciences, from the Earliest to the Present Times*, published in three volumes in 1837.

[72] The Berlin Group, as it was noted above, was a counterpart to the Vienna Circle, though some of its members tried to disassociate the two groups on philosophical grounds. Richard von Mises (1883–1953) was an Austrian mathematician who worked on statistics, probability and the application of mathematics. He was a professor in Berlin until he went (similarly to Reichenbach) to Turkey in 1933, and finally emigrated to the United States, where he worked at Harvard. Von Mises is also known for his work on aerodynamics and he became a recognized expert on Rainer Maria Rilke.

[73] Ludwig von Mises (1881–1973), brother of Richard von Mises, was an Austrian economist. He emigrated from Vienna to the United States in 1940.

translated, with the German title, Ein Kleines Lehrbuch des Positivismus, a little text of positivism.[74] It's an interesting book and covers a great array of problems. He was, among other things, a great admirer of the German poet Rilke.

Eventually both Reichenbach and Von Mises managed to get out of Austria,[75] and they spent some time in Turkey, in Istanbul. Then both came to this country, and Reichenbach eventually went to UCLA, and Von Mises became a professor of mathematics at [Harvard] University. They were interesting people, had all sorts of crotchety sides to them. I used to go to listen to them.

Q: You also mentioned Braithwaite and Moore and some of the others at Cambridge.

Nagel: Yes, well, Braithwaite was at Cambridge, England, as was John Wisdom.[76] I got to know both of them.

Maybe I can tell you another Wittgenstein story. One day Mrs. Wisdom invited myself and my wife for a late Sunday breakfast, and so we went; and when we arrived, among the other people that were there was Wittgenstein. And I thought, "Gee, this is wonderful. I'm here with the real McCoy."

Well, Mrs. Wisdom asked us if we wanted tea or coffee, and Wittgenstein said coffee, and he began to glow about the stimulus he gets from coffee. And I thought this was just the foothills of a conversation, and so I really put my foot into it. There was a psychologist here at Barnard College who performed an experiment on some different people. To some of them he gave coffee, but they didn't know it was coffee. Others he didn't give coffee, but he told them it was coffee. And he found that people responded to their expectations. Since Wittgenstein was talking about it, I told him this story. And my God, that spoiled the whole morning! He became furiously angry, and the rest of the time there was a denunciation of experimental psychology.

Q: Another fellow who springs to mind was P. F. Strawson?[77] He was an Oxford man, I believe.

Nagel: Yes, I didn't meet him at that time. I met him…. He spent one year or one semester at Princeton. I met him then.

Q: Well, you certainly had quite a trip as far as meeting some of the great figures is concerned.

Nagel: Oh, yes, it was an education.

Q: How long did the trip last?

Nagel: A year, not quite. I think I left in September and came back in June.

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[74] See von Mises (1939); for the translation see von Mises (1951).

[75] In fact, both Von Mises and Reichenbach have fled to Turkey from Germany.

[76] John Wisdom (1904–1993) was an English philosopher who is often associated to Cambridge School of Analysis and to the ordinary language philosophy.

[77] P.F. Strawson (1919–2006) was an English philosopher, working mainly in Oxford. He is also often associated with the ordinary language philosophy movement.

Q: And at what point were you then in terms of your own career? Was this still while you were in graduate school or had you just gotten your doctorate?

Nagel: I got the doctorate in '30, and I got the Guggenheim in '34.

Q: I see. So you had actually been teaching at Columbia.

Nagel: Yes, I was an instructor here at Columbia.

All the interests of the people I got to know in Vienna were really my interests. It's not surprising, because I looked up the people who were interested in what I was interested in largely—largely in science, the probability theory and so on. It was really wonderful to get to know them.

Q: Were there any other members of the Columbia faculty at that time that were interested in these areas?

Nagel: Well, in a way, yes. Woodbridge, for example, was interested in the history of science. He was also interested in the foundation of mathematics. There was a book at that time, also in German. There was a German mathematician named Adolf Fraenkel[78] who published a book on the theory of sets. Fraenkel was in the First World War, and while he was in the trenches just to have something to do with his free time, he gave lectures on the theory of sets to the soldiers. And he was apparently very successful then, and so eventually he made a book out of it, and it was published. And Woodbridge was very much fascinated by it and thought it ought to be translated and he asked me whether I would do it. Luckily somebody else had jumped on this, and the translation was already in process. And I got in touch with him—I forget his name—and I looked at his translation, and it was really impossible. I mean the translation was so poor, I thought that if I joined up with him, I'd have to do it all myself.

Well, Woodbridge was one person, but, you see, his real interest was in the history of philosophy.

Dewey obviously should have been interested, but I don't think he really cottoned to that kind of thing, you know—never mastered any of this sort of recent developments in logic. What he knew about it he disliked, partly because Bertrand Russell's presentations of some of these developments in logic were based upon a very dubious epistemology.[79] So that was the attitude of the department when I first entered into it. The late Professor Randall used to say, "Only a madman could have done what Russell has done."

Well, that was it. I mean Woodbridge and Dewey were two people, and their knowledge of this material at best was rather slight.

So it was a fairly lonely life, for me at any rate, in those days because—I think I may have mentioned this the last time we met—I came from City College where it

[78] Adolf Abraham Fraenkel (1891–1965) was a German mathematician who contributed mainly to set theory. Fraenkel was first known as Adolf but he changed his name to Abraham when he arrived at the newly established Hebrew University in 1929. See Fraenkel (1919).

[79] Russell himself, in turn, had severe reservations about Dewey's own account of logic ("inquiry"). For further details, see Misak (2013, 121–122).

was normal to discuss and take sides in philosophy, and this was not looked on with much favor at Columbia.

Q: It seems hard to believe that the great philosopher of education, John Dewey, would….

Nagel: Well, that's…

Q: What was the atmosphere like then—simply a matter of just lecturing and taking notes?

Nagel: Pretty much. The chairman of the department, at that time called the executive officer, was a man that I mentioned called John Coss, who disliked very much to have students ask the professors questions or to pursue a point beyond the point where it became a bit impolite to press the matter.

If I've already said this, please stop me: there was a seminar that Woodbridge gave which was called Metaphysical Analysis, and I took that because we would take that in the late afternoon, and one member of the class, after several things had gone by, interrupted Woodbridge and said, "Professor Woodbridge, I don't understand what you mean by this." And Woodbridge was taken aback by it—this question and so on. He repeated himself, and the student said, "I still don't understand," and the rejoinder was: "Well, anybody who doesn't understand the English language doesn't belong here." That's really true! Well, it's a different world now.

Q: So I guess it took a little adjusting for you back then.

Nagel: Oh, yes, yes.

Q: Did you stay in contact with Professor Cohen?

Nagel: Yes, yes. I used to visit him every so often. Then he had a stroke in 1941. So it was extremely difficult to talk—he couldn't talk. But I went to see him, and he would ask me to read some things to him, which I tried. This was I think the year of some kind of a Galileo centenary, and I forget now what it was I read to him—maybe Cassirer[80] or somebody like that on Galileo. But finally, he said, "I don't want to hear any more of that." Then he pointed [to something else].[81]

Cohen had a really very sad life in a way. He had three children, two sons and a daughter. All of them died. And the oldest, Felix, had a Ph.D. in philosophy, and then eventually a law degree.[82] And he was his father's right hand. All the books that came out, including Reason and Nature: this was pieced together by Felix.

Q: Wasn't Professor Cohen, Morris Cohen, quite a fiery debater and lecturer?

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[80] Ernst Cassirer (1874–1945) was a German philosopher, trained as a neo-Kantian. He is known for his work on symbolic forms and the philosophy of culture, as well as for his book on the theory of relativity. From 1944 until his death in 1945 he taught philosophy at Columbia. In 1942, Cassirer published a paper about Galileo; see Cassirer (1942).

[81] Brackets in the original typescript.

[82] Felix S. Cohen (1907–1953) was an American lawyer who worked on legal philosophy as well.

Nagel: No, no, no, he wasn't fiery at all. He was sarcastic; he didn't suffer fools gladly. And he had a great wit and a great presence of mind. I mean: I don't know whether you've heard any of these stories about him. This course I think I mentioned in the philosophy of civilization, in which one part was a critique of various areas of social progress. And at the end of the semester one student spoke up and said, "Professor Cohen, you've destroyed all our illusions about these various theories, but you haven't replaced them with anything positive." And Cohen came back as quick as a flash and said, "Look, it's not recorded that Hercules had any other job except to clean the Aegean stables."

Q: Good rejoinder. The two of you worked together on some publications.

Nagel: A book—An Introduction to Logic and the Scientific Method.[83] And then I helped edit a book that was a posthumous publication.[84]

Q: Was he easy to work with when you were actually working that closely on a project?

Nagel: Well, in a way it was and in a way it was not. It was easy because he was my teacher and I had a great respect for him, so obviously I didn't dispute and so forth. So in that respect it wasn't easy. That is, I changed my mind on a number of things and tried to alter the text, in the present form it was, and he wouldn't have it. He had the truth, but he was very sharp in the classroom. I still remember: he hadn't given a course in the philosophy of science for years, and during my last year at the college he offered a course again and I was to take it, and one day he called out and said, "Well, maybe we can talk more easily if you come to my house; we'll sit at leisure and talk." So he appointed a day. Well, we didn't dare go up alone, so we met in advance and went up in a body. And when we arrived (he used to live up on 180th Street) the professor wasn't at home, and so his wife said, "Well, sit down." And he eventually came back and said he'd been out for a walk, and when he saw us he said, "Why did you come so soon?" Well, that put a damper on all the…being at ease with him.

A number of us, after we graduated from college, met on Saturday mornings in the library of the physics department at City College. We read a number of books together. I think we read Boole[85] and something of Russell—I just don't remember what. And every once in a while we figured it would be appropriate for us to call upon Professor Cohen; he used to spend Saturdays in a room on the third floor. But we had to prepare ourselves for this by figuring some good questions that we wanted to ask him. And the man who was always asking the questions was Sidney Hook, who was a classmate of mine. And, well, when we got there we stood up in the back

[83] See Cohen and Nagel (1934).

[84] Nagel was involved presumably in the edition of Cohen's *Reason and Law*, see Cohen (1950). The title of the book reflects Cohen's major work from 1931, *Reason and Nature*. Nagel later published an article as well on Cohen, see Nagel (1981).

[85] George Boole (1815–1864) was an English mathematician, philosopher and logician, known for his work on the algebra of logic and for his role in the development of modern symbolic logic.

row like soldiers on parade, and Sidney would ask this question, and Cohen would say "yes" or "no"—you know, a brief answer. And there we were, you know, stuck! Oh, dear, oh dear. Well, these are the stories.

Q: Thinking about this same period, after your trip, your early teaching days still at Columbia—this would be your fifth year or so in the department at Columbia—you said earlier that when you first went to Columbia the idea of tenure seemed a very far-fetched notion to you. When did you begin to realize that this was possible and that things were working out quite well for your career there?

Nagel: Well, I didn't know; and the one that I felt I could let my hair down a bit with was Irwin Edman (he died quite early). And I would ask him. You see, my difficulty was I did have the opportunity to return to City College on a tenure basis. Edman said he couldn't give me an answer to that; he didn't know. I don't know just how it happened, but I managed to coast along not making a decision about City College. I kept a sort of a hand in because I used to go up to the college and teach one course. I was taking just a chance. I think it was not until 1938 or'39 that I got tenure.

Q: At that point had any other young faculty members who had been students with you also joined the department, or were you one of the younger members at that point?

Nagel: Of course, I knew some of the younger people. One of them was Corliss Lamont,[86] who was an instructor at that time; and he eventually resigned his job because he wanted to become active in politics. There were another one or two I don't remember very well. But it just happened that none of the younger instructors had an interest in formal logic, which I was expected to teach.

Now, what did happen—and I'm terribly grateful about that: I managed somehow to get to know people in other departments at Columbia and through that get to know people in other institutes. It was customary for just about everybody to have lunch at the Faculty Club at that time. It wasn't a cafeteria; you could sit down at a table. And it wasn't very expensive. For 50¢ you could get a good lunch. And there was one table which was sort of reserved for people who knew each other, and I got to know who they were: I.I. Rabi[87] and a couple of people who were in the department of mathematics. I'm not sure I remember their names now. I got to know very well a biophysicist, Selig Hecht.[88] I'm not sure. I'd have to really try to think of their names. But this was really wonderful. I didn't have any sort of soulmates in the philosophy department at that time, but I got to know people outside it. And this I think [?][89] created.

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[86] Corliss Lamont (1902–1995) was an American philosopher. He was a socialist, who hit quite friendly note about the Soviet Union and became one of the founders of the National Council of American-Soviet Friendship.

[87] I.I. Rabi (1989–1988) was a Nobel Prize winning American physicist. He chaired the Physics Department at Columbia between 1945 to 1949.

[88] Selig Hecht (1892–1947) was an American physiologist and wrote an influential popular book on atomic theory (*Explaining the Atom*, 1947).

[89] Brackets and question mark in the original typescript.

Many of the younger people didn't eat at the Faculty Club because it was too expensive. Some went home if they lived around here. And there was a kind of interchange.

For example one of the things [I can say][90] maybe in retrospect (I'm not sure I felt that way at the time it was happening): to serve on committees was a very valuable thing for me. I got to know [many people].[91] If you served on a dissertation defense, it was customary to have people from other departments for that. Then there were other committees, too. For example, I got to know Paul Lazarsfeld,[92] who was a professor of sociology, and eventually suggested that we give a joint graduate seminar—a methodological seminar—which was extremely valuable to me. We did this for a number of years. And I got a sense of what empirical sociology is like. I think I would really have to scratch my head hard to think of some of the other people I got to know this way. I'm not sure whether things like this go on now; for that time. Certainly the luncheon meetings are no longer what they used to be. We do have a standing {?} have lunch on Tuesdays: some philosophers, physicists and {?}. We keep in touch somehow, but it didn't have the excitement {?}.[93]

Q: Back in the 30's was the Journal of Philosophy put out by the philosophy department of Columbia?

Nagel: Yes, that was established around 1905, 1906, jointly by Woodbridge; Bush,[94] who was a professor of philosophy; and the psychologist, Catell.[95] They all began it, and eventually I think Catell sold out his share to Woodbridge and Bush. And eventually Bush gave it up, and Woodbridge was chief editor. Then eventually some of the younger men in the department like Herbert Schneider[96] also became [part of][97] the journal. And then much later, after I got tenure, I was asked to serve as a reviewer for the journal.

That was fun. It was hard work, because at that time Woodbridge had retired so Schneider had taken it over.

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[90] Brackets in the original typescript.

[91] Brackets in the original typescript.

[92] Paul Lazarsfeld (1901–1976) was an Austrian sociologist who went to the United States in 1933. He is mainly known for his early work with Hans Zeisel and Marie Jahoda on the social impact of unemployment (*Marienthal: The Sociography of an Unemployed Community* (originally published in 1933).

[93] Brackets and question marks in the original typescript.

[94] Wendell T. Bush (1866–1941) was an American philosopher who specialized also in the sociology of religion. He became associated with *The Journal of Philosophy* as co-editor in 1906.

[95] James McKeen Cattell (1860–1944) was an American psychologist who completed his Ph.D. with Wilhelm Wundt in Germany. Besides founding *The Journal of Philosophy*, Cattell was the editor of *Science* for nearly fifty years after he purchased the journal in 1894 because of its financial difficulties.

[96] Herbert Schneider (1892–1984) was a German-American philosopher, who worked mainly on the philosophy of religion. He was the editor of *The Journal of Philosophy* for decades after 1924.

[97] Brackets in the original typescript.

Q: I wanted to ask about the operations of the Journal of Philosophy, because as you were saying, this sort of cross-fertilization was good with other departments. I wondered about other universities and other philosophers and whether the journal functioned as a way or sort of channel through which you met other faculty members?

Nagel: I don't think so, not outside the university. You got to know some of the people in other universities at meetings of the Philosophy Association at Christmas. Have you every attended a meeting of that?

Q: Yes.

Nagel: Well, they're tremendously large crowds. You can't really get to know people. But in those days the Association was small enough so that we met at a college. There was enough room in dormitories to take care of everybody that came. And so we had smaller meetings. But after a while it just [got too big].[98] I used to go to meet a few friends, and that's about it.

Q: In your area, even though you mentioned how you were one of the only people interested in it a Columbia, it would seem—at least it would be my impression—that during those years there must have been a burgeoning interest in logic and the philosophy of science. Was that true among your students?

Nagel: I joined the department in'31, and by that time Gödel's paper[99] was out, and not only didn't anybody in the philosophy department know anything about that, but there was nobody in the math department. This was shown very clearly. Tarski[100] had come over from Poland, and for a number of years he was footloose—he couldn't get a job—and so he went the rounds lecturing. And it was suggested that he ought to give a lecture or two to the department of mathematics. They didn't know who Tarski was, but he eventually was invited to give a talk, and what he had to say was of no interest to any of them.

I remember there was a first-rate mathematician—a man by the name of Ritt.[101] He shared an office with another mathematician, a man by the name of Pfeiffer.[102] And I once went to see Pfeiffer, because he had some interest in sort of these methodological questions. I came into the office and Ritt saw me and said what do I want? I told him I wanted to talk to professor Pfeiffer. "Well, he's not here, and I don't want to have my head filled with Boolean algebras" [laughs]. Very short.

Q: What was your reaction to this ignorance of some of the newest trends?

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[98] Brackets in the original typescript.

[99] Nagel refers presumably to Gödel's famous paper on incompleteness, see Gödel (1931/1986).

[100] Alfred Tarski (1901–1983) was a Polish logician, who made revolutionary contributions to logic and semantics. He emigrated to the United States in 1939 where he taught at the University of California, Berkeley until his 1983 death.

[101] Nagel presumably has in mind Joseph Ritt (1893–1951), an American mathematician, who joined the faculty at Columbia in 1921.

[102] Presumably George Adam Pfeiffer (1889–1943), an American mathematician who taught at Columbia from 1919 to 1943.

Nagel: I don't know. You just swallowed your pride I guess.

Now, there was one man in the math department I got to know—Bernard Koopman[103]—, whose uncle was professor of mathematics at Harvard, and who did have some general interest in these broader questions. We didn't see eye to eye on a number of the issues, but that was all right. It was {mostly—?}.[104]

And he knew Tarski and admired Tarski. So there was some contact with men who knew area of work.

Q: Was this as true in physics where quantum mechanics must have raised all sorts of theoretical…

Nagel: No. {—?—}.[105]

When I was still a graduate student I took one course in the physics department, a summer course. It was given by a very distinguished physical chemist who eventually won a Nobel Prize. But the department in those days was just made up of elderly men who didn't keep up with what was going on. That department began to build up when I.I. Rabi became chairman.[106]

I got to know some of the physicists and got to know some of the mathematicians; I got to know some of the sociologists; I got to know some of the biologists partly through Selig Hecht, who was a biophysicist. And partly there were some other people in the biology department at that time that tolerated philosophy …[107]a man by name of L.C. Dunn,[108] a geneticist; and there was another man by the name of Dobzhansky,[109] who was also ?'s[110] sister. I got to know them.

But they are all gone. I don't know whether there's anybody there in the biology department who's interested in philosophy. Oh, yes. There's a professor named Walter Bock.[111] He's an evolutionist. He's an ornithologist, and he has a good deal of interest in the broader questions. He was a former student of Ernst Mayer.[112]

There is a university seminar on the history and philosophy of science which meets once a month here, and Walter Bock comes fairly regularly to that. It was started about three or four years ago and I sat in on it for a while. We had a good representation

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[103] Bernard Osgood Koopman (1900–1981), a French-born American mathematician, who worked on mathematical physics and pursued confidential military work as well.

[104] Pencil notes in the original typescript.

[105] The original typescript contains the question mark and dashes.

[106] The original typescript contains the following elliptic sentence: "…. had a number of Nobel Prize Winners …… physics department ….. greatest.".

[107] The three points were late pencil additions noting a missing phrase in the original typescript.

[108] Leslie Clarence Dunn (1893–1974) was an American developmental geneticist, a critic of the eugenics movements.

[109] Theodosius Grygorovych Dobzhansky (1900–1975) was a Russian-American geneticist, a prominent defender of modern synthesis.

[110] The original contains the question mark to note the missing name.

[111] Walter J. Bock is an American biologist and ornithologist who is a professor emeritus at Columbia.

[112] Nagel has in mind Ernst Mayr (1904–2005) who was a German-born American professor of biology at Columbia.

from many of the disciplines, including sociology, because there are several people in the sociology department with an interest in the sociology of science. Merton,[113] for example, is one; also a man by the name of Jonathan Cole.[114] So we had a very good representation from a number of departments like that, but that's all gone unless it's someone very prominent… I mean the seminars usually consist of having somebody read a paper. And on this the paper reader is a very prominent man in attendance there.

What else can I tell you about Columbia?

Q: One thing I wanted to ask is whether there would be interest among the students in logic and the philosophy of science was something that was increasing.

Nagel: For a while, yes. For a while that was so. Logic perhaps not so much. I mean I had some really excellent students when they were still in Columbia College, and I used to give this graduate course in mathematical logic, and some of the people from Columbia College attended that.

Now I had (maybe these names don't ring a bell with you) Leon Hankin,[115] who's now professor of mathematics at Berkeley; Patrick Suppes,[116] who is at Stanford, and several other people whom I got to know first learned a little about it but then did their graduate work usually in the problems of mathematics. That's very sensible. If you want to go into, you'll want to have a very good mathematical training.

Q: Well, after going back to your trip, we've pretty well taken you up to the early part of your career. Perhaps next week we could begin talking about your early publications, such things as you began in your own name.

Nagel: Yes. I think I'd like to say something about how I see philosophy.

Interview 3

Interview with Ernest Nagel by Remmel Nunn

New York City, March 3, 1983

Q: You had mentioned some of the changes at Columbia, Professor. Are there any others you'd like to talk about?

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[113] Robert K. Merton (1910–2003) was an American sociologist, spending most of his career at Columbia. Merton also started a joint project with Nagel and the logical empiricist Philipp Frank on "the sociology of science" in the 1950s.

[114] Jonathan Cole is an American sociologist at Columbia University. He defended his Ph.D. dissertation on the "Social Structure of Science" (echoing Nagel's major work, *The Structure of Science*) under Merton's supervision.

[115] The original typescript contained "Leon Hanken", but Nagel presumably had in mind Leon Henkin (1921–2006) the American logician and mathematician.

[116] Patrick Suppes (1922–2014) was an American philosopher, known for his work on the philosophy of science.

Nagel: Well, one big change that certainly made an impression on everybody who was connected with Columbia was the student uprising in '68. Not only did it affect the relations between students and faculty, but what was more significant I think was the effect of this upon the relations of members of the faculty among themselves. One discovered that people whom you thought were your friends and shared similar views on a great many things… that that was a mistake; that some of the faculty members really played up to the students and broke up years of friendship among the faculty. And for a long time I thought that this would never be healed. I was very glad to find out that I was mistaken in that; that after a number of years some of the old friendships were renewed. It took a long time and did change…perhaps not quite as intimately as before '68, but sufficiently to feel that you really belonged in the right university.

Another thing, of course, that hurt a great many people was the attitude of some of the people in the administration.

Q: Which was what really?

Nagel: Well, some members of the administration—for example, I think this was true of President Kirk[117]—really were very insensitive to what was going on. Kirk in effect permitted some of his administrators to take the brunt for what had happened. I'm thinking particularly of Dave Truman,[118] who had been a model college dean, who was very well-liked by the college students; and then he became vice-president I think the year before '68. I'm not sure about the exact date. And he was extremely loyal to Kirk. When the riots began he thought the thing to do is to bring a quick halt to it by calling in the police. I myself was in favor of that. It seemed to me that this kind of behavior that the students showed was really not defensible. The fact that they were students and young didn't seem to me to make all that much difference. But he was persuaded by some of the faculty members to delay calling the police, and Truman did do that. As a consequence, the riot went the full length of the thing. And once that began, it was really very difficult to stop it.

I don't think Kirk really appreciated what Truman did for him. Truman was extremely faithful and loyal and never mentioned any criticism of Kirk's behavior, and as a consequence Truman was in a way blamed for something that happened, though everybody I think knew that he was essentially the next president of the University, because Kirk was close to retirement. And this eventually made it impossible, and so he left Columbia and went to a New England college.

That I think is a very brief account.

Truman, contrary to what many of us supposed, never presented his side of the picture: again I think out of loyalty to Kirk. During the period of investigation of what happened at Columbia, there was a commission appointed by I think the chairman of

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[117] Grayson Louis Kirk (1903–1997) was president of Columbia between 1953 and 1968. Originally he was a political scientist with a Ph.D. from the University of Wisconsin-Madison.

[118] David Truman (1913–2003) was an American political scientist and Vice-President and Provost at Columbia between 1967–1969. After that he became the 15th President of Mount Holyoke College between 1969 and 1978.

the executive committee at that time—a man named Mike Sovern, who's the current president. He had Professor Cox of Harvard to serve as chairman of that committee, and there was a good deal of interviewing and interrogation. I went to some of these to hear what people were saying, and Truman presented his side of the story in a very modest fashion, but it made no difference. Cox himself I thought was a rather silly man. He proclaimed this sort of thing would never happen at Harvard, and I think it was a year or two after that it happened at Harvard also. And then we'd have a period of sort of temporary presidents of the University. Butler[119] had retired too late. He was already in his senility. Then for a while we had an acting president—Fackenthal[120]—who I think was acting president for about a year. Then Eisenhower became president, and he had no interest in Columbia at all. Then after Eisenhower was Kirk.

After Kirk there were a series of sort of makeshift presidents. I forget the names now of all the people that served that way. And as a result, Columbia was really without a head for a much longer period than I think is healthy for an institution.

Of course, there were serious financial problems, but also a lack of leadership, which I think played havoc with the morale of the institution. And that went down for a while.

Q: Have any of the more recent presidents…?

Nagel: Well, see, there was a search committee. They finally selected William McGill,[121] who had been at Columbia at one time in the department of psychology, and then he went to California; and he came back as president, and he stayed just ten years.

Now, he thought it was his function simply to put Columbia on a sound financial basis, to cut down on expenses and things of that kind.

Then Sovern[122] succeeded McGill. Sovern had been very active trying to raise enough money to enable Columbia to continue as a first-class institution. Just exactly what he was been doing, I don't think anybody really knows, apart from the fact that he had been interested in the finances.

I don't get the sense (I've been out of things for a long time) that he's made his presence very strongly felt at Columbia. And in retrospect some of the previous men like Butler look like giants. Those days I suppose are gone.

That was one thing that I thought might be worth at least a brief reference.

There were some other changes that I think took place, partly as a result of the government's support of education. That is, government in effect supported a great

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[119] Nicholas Murray Butler (1862–1947) was an American philosopher and educator, and president of Columbia between 1902–1945.

[120] Frank D. Fackenthal (1883–1968) was an American administrator at Columbia and he was the president of the university between 1945 and 1948.

[121] William McGill (1922–1997) was an American psychologist and the 16th president of Columbia between 1970 and 1980.

[122] Michael Ira Sovern (1931–2020) was the 17th president of Columbia and was known for his work as a legal scholar.

many students doing graduate work for the Ph.D., and for a while we had a very large graduate enrollment.

This was I think a worthwhile experiment. Everybody thought this was the way things were going to be run—that government would support higher education indefinitely. But that came to an end, and of course students were very unhappy, and they were not used to having to provide their own tuition.

Q: When did this peak would you say in terms of the availability of funds and the resulting increase…?

Nagel: It was certainly during Kirk's period, and this was I suppose roughly from the early 60's to around the 70's, something like that.

And so one could understand that students were very much affected by this change in government support, and I imagine it is true that many of the older people on the faculty didn't have very much sympathy for the students, largely because those who came to Columbia before were without government support. They had to do their own financing. So that seemed like the natural way for a while, to have government support; but it was a luxury that couldn't last indefinitely.

I don't know whether this is a consequence of '68 or whether it's a consequence of the changing economic status of the country. I think we may have touched upon the point that it was the custom I think for nearly all members of the faculty to have lunch over in the Faculty Club. Well, that gradually disappeared, partly because of the expense, partly because many of the faculty didn't feel they could afford going to the Faculty Club. Compared to today, of course, the cost of meals was miniscule. I mean you could have a rather full lunch for about 50-, 60¢. But some of the younger people thought that this was excessive and so lost this opportunity to get to know people who were their colleagues and they were going to be their colleagues for a long time.

There's only one compensation for this. I forget now the year this was established. There is a system of university seminars. This was introduced I think some time in the 50's, 60's, by the late Professor Frank Tannenbaum,[123] who was a historian, and he thought it would be important to have a series of university seminars of which faculty would be members and graduate students if faculty members decided.

Now, Tannenbaum thought of these university seminars as each dealing with what he called "permanent institutions in society," like peace and religion and science and so on. And they were extremely successful for a long time, and you really got to know other members of the University who were not in your own department or even close to the department.

For example, there was a University seminar in peace and religion. And, as I recall, there were people from philosophy, from sociology, from history. A lot of people came to that regularly.

[123] Frank Tannenbaum (1893–1969) was an Austrian born American historian. The seminars he initiated are still noted on the webpage of the university: https://universityseminars.columbia.edu/archive/history-of-the-seminars/ (accessed on July 1, 2020).

I never belonged to that one, but I did belong to one that was called Concepts in Social Sciences. This was a seminar that was essentially run by the late Professor Lazarsfeld. And for quite a number of years we had people from different departments—biology, sociology, of course; some in mathematics; some in mathematical statistics. And I think all of us learned a good deal, both about the material that was being discussed, but also a good deal about who your colleagues were and what they were doing.

Q: How were the seminars set up? Was there a panel?

Nagel: No, as I recall, the only requirements were that a certain number of people thought that it would be a good idea to have a seminar, and they informed somebody in the administration, and there was no formality. Subsequently, you had to be sort of elected to the seminar. But for a while it was a kind of free-for-all.

The seminars with which I was connected never had very many graduate students. There was essentially faculty meeting. I still belong to a seminar (this was established much later)—a seminar on the history and philosophy of science. And this started out with a bang but now is sort of crawling along. I'm not sure that it's going to last indefinitely.

Some of the other seminars, too, although they went very well for a while, later on they decided to rest for some years and suspend their activities. They went back and started all over again.

As I say, that was an extremely useful institution. Members of the seminar were not only members of the Columbia faculty but also came from other parts of the city or out of the city.

I suppose that's about all I can think of.

Q: Well, another thing occurred to me: did the formal requirements for the doctorate in the philosophy department change at all as a result—not as a result of the riots necessarily, but did they change during those years?

Nagel: Oh, yes. Well, when I was a graduate student, the department of course was strongly into the history of philosophy. And so the requirements were that you take a two-day written examination in the history of philosophy, the whole spread of philosophical systems. But in addition to that, there were two books at each year that you had to somehow master, and there were questions on that.

When I took that, the two books that were set were Aristotle's Ethics and John Stuart Mill's Logic.

Well, that eventually was abolished, and for a while the only requirement was that you take the course in the history of philosophy and get a grade of "B" or better. Woodbridge, who had given that course, retired in the 30's; and it was taken over by Professor Randall, and that continued now I forget how many years, but then there was a change in that. And it was decided that just familiarity with the history of philosophy was not sufficient, and that you ought to have some competence in the various branches or areas of philosophy.

Q: Were there no language requirements during this early period?

Nagel: Oh, yes. There were two. You had to have two foreign languages. I don't know that there was any strictures on the kind of language, but I know that two languages were required.

The next change was to have written examinations in so-called four fields. One was ethics and political theory. The second was logic and the philosophy of science. Another was the theory of knowledge. The fourth was the philosophy of religion or something like that.

Now, that would eventually change, too, because people had different ideas as to how students be advised to prepare for the examination.

For some years members of the department thought that you just ask any question about anything that falls into those fields. And this was quite a hardship. I mean, after all, taking in such an old discipline like ethical analysis—I mean the variety of ethical systems since the beginning of philosophical thought was just enormous. And so the next change consisted in students being told that the examination questions would be based upon the set of books being enumerated.

Now, I think that's where I left the matter. That is, I retired when that was still in operation.

Now I think there's still some difficulty, but I'm not sure just exactly what it is. I think it is still true that students are told that the questions would be based upon some standard books—I'm not really quite sure about that.

Then there was a hustle for some time as to what happens if a student didn't do satisfactorily on the examination. At one time the opinion of the department was: "Well, in that case you had to take the whole set of examinations over again." And eventually that was thought to be not quite fair, and so a student now has the opportunity to repeat an examination in one area if he hadn't done well the first time. That's the main change.

Q: Once the students passed these examinations, they simply began researching and writing their dissertations?

Nagel: Yes, yes. There was one change, too, that happened while I was still there teaching. There was always some difficulty in what kind of advice a student would receive while working on his dissertation. And at the beginning—at least when I first came to Columbia—you asked some member of the department to approve what you're doing, and then you saw him at irregular times; and he was in the main responsible.

Now, this had its difficulties, because sometimes members of the department would agree to sponsor a student's work on a certain area when members of the department were not entirely familiar with the range of things. And so a committee was appointed at the time that the student decided to work in some area who would jointly, so to speak, be responsible. And this, of course, meant also that you could have somebody from outside the department being on this committee. Those I think were the fundamental changes.

Q: And then a big change, I guess over the last two decades, must be the job situation waiting for these students once they leave.

Nagel: Oh, yes, of course. I mean that's really a terrible situation of students working very hard, getting a degree, and then finding out they could do nothing with it. They can't make a living with it. Things have not yet improved. I mean I haven't been at a meeting of the American Philosophical Association for several years, but some of my colleagues who go to meetings—they find that you have maybe two or three jobs available and 200 or more students apply for them. That's I suppose particularly serious in some subjects more than in others. As far as I understand it, the people, for example, in physics have less difficulty provided that they are experimental physicists. In theoretical physics they also have difficulty.

Q: Are there any areas within philosophy—perhaps the philosophy of mathematics, logical philosophy—that are easier now than say someone in aesthetics or someone in ethics?

Nagel: Well, I really don't know whether that's true. For example, a student that's primarily interested in logic—of course, he would be in mathematical logic—: he would specialize very narrowly. And as a consequence many departments thought that he was really not very useful to them because mathematical logic was not the center of their interest. So for a number of years the history of philosophy went out of fashion, and a person who specialized in the history of philosophy had a difficult time in getting a position. Now the tables are turned, and now there's a demand for people who are trained in the history of philosophy. But it takes a long time to do that; so that there is still a need for a historian of philosophy, although I don't think the supply is adequate.

I think the effect of this difficulty was to persuade students that they ought to have a very broad interest, so that they can teach almost anything really; and also to be reconciled to what I suppose must be a very difficult thing to reconcile to. And that is they would not be teaching exclusively philosophy. They might have to teach English or history or something of that kind, especially if they found jobs in a junior college or something of that kind.

Q: Do you feel like that's had an adverse effect on the level of scholarship in philosophy?

Nagel: I haven't seen that myself. I don't know.

Q: You mentioned the departments being interested, in one area or another. What would you say the trends have been in the department at Columbia—the focal point of the faculty's interest?

Nagel: In Columbia entirely or just in philosophy?

Q: No, in the philosophy department.

Nagel: Well, as I think we mentioned, initially when I was a student the interests were historical. Also, there were some associations between the department of philosophy and the Union Theological Seminary; so that there was a good deal of interest in the philosophy of religion. This was before an independent department of religion was established.

That eventually was changed, because the department thought that it wasn't desirable to mix philosophy and theology in that way, and especially when a department of religion was instituted the department no longer offered courses in the philosophy of religion.

Now, there have been several relatively new developments. I mean, for example, when I was graduate student (I think I may have mentioned), there were no courses in logic at all in the graduate school except for a person who gave a course in types of logical theory, but that was really his conception of logic and completely out of touch with developments in mathematical logic or anything of that kind.

One of the things for which I was really hired was to do something about that, and so courses in mathematical logic were part of the staple offering.

Another thing that was rather frowned upon: both Woodbridge and Dewey had very little use for traditional epistemology; and they looked sort of askance at anybody who showed an interest in that. But I thought if you are going to have students who will be prepared to teach all over the lot in philosophy, they ought to be exposed to some discussion; so I began giving courses in the theory of knowledge, both on the college level and graduate level.

And the third thing that I think was an innovation—and this was hardly a regular subject when I was a student—was the work in the philosophy of science, and so that, too, became a rather major part of the offering of the department. Not only did I do that, but then eventually we appointed people who could continue doing that kind of a thing. And, of course, currently we have a number of people who are interested in that. I mean first of all there's Levi.[124] I think that's about it.

One other interest which for some reason was never made part of the department's work was the history of science. Way back when the late Professor Harry Carmen[125] was dean at Columbia College, he appointed a committee to institute some in the history of science, and he had some very distinguished people on it—I.I. Rabi, the physicist; Robert Merton, the sociologist. I think there was a historian on it—I don't remember—but I also served on that committee. And we tried for a good many years to find somebody who would come to Columbia and offer a regular course in the subject. We were not very successful. And so there was a repetition of committees of this kind that continued down to the very end when I retired—there were still some committees. Nothing has ever happened.

It's hard to understand why that's so. Most other major universities have either an independent department of the history of science or the history of science taught in the history department, and we didn't succeed in getting that. We thought we were going to be successful.

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[124] Isaac Levi (1930–2018) was an American philosopher and John Dewey Professor of Philosophy at Columbia. Levi wrote a lot about decision theory and epistemology.

[125] Harry Carmen (1884–1964) was an American historian and dean at Columbia between 1943 and 1950.

But I remember it was during the period when Jacques Barzun[126] was the dean of faculty, and he had some ideas on this subject; but then we had this committee that was appointed by the then dean of Columbia College, who died recently:

We went for a meeting in the dean's office (I.I. Rabi was on that committee), and the question of finances came up, and I.I.Rabi said, "Well, we can get enough money easily." He called up Waterman,[127] who was head of the National Science Foundation, and explained what the Columbia problem was. This was over the phone. And he got an immediate answer from Waterman. He said, "Well, look, what is it you need?" And Rabi said, "Well, we need "$50,000 a year for three years to start this thing." And Waterman said, "Well, just said in your application and you'll get it."

Well, Barzun was dead against that. He thought: "That's not the way to build a department. The way to do is to get some freshly caught Ph.D. and let him develop."

So that was just a stupid error on his part. As a matter of fact, you needed more than one person and then leave the department alone. It's difficult for a man to come to a place, build the department and be all alone in that sort of thing.

Well, we had some temporary teachers but never to the extent that it goes on in other universities. Occasionally we would have students in the department of philosophy write a dissertation which dealt with some aspects of the history of science, but it's not a major enterprise…sometimes entirely the fault of members of the faculty who didn't want it. For example, we had some extremely able people that I think would have been willing to come if they could get a professorship not in the department of history, because they would feel strangers there, but in the department of physics.

There was a man who is a bona fide physicist in Cleveland: [missing part].[128] He decided he did enough physical research and became interested in the history of science, and he wrote some remarkably good articles on various aspects of modern physics, wrote a first-class book on a very distinguished Dutch physicist, Ehrenfest.[129] And I think he would have been very glad to come to Columbia. But the department also contained a member who thought well, he could do all that himself, and so the thing just fell to pieces.

Well, those are the hazards of….

Q: Has the faculty within the philosophy department stayed roughly the same size throughout your career there?

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[126] Jacques Barzun (1907–2012) was a French-American historian and served as dean of the Graduate School between 1955 and 1968.

[127] Alan Tower Waterman (1892–1967) was an American physicist and the first director of the National Science Foundation between 1951 and 1963.

[128] At this point the original transcript's pencil note says: "Get Nagel to fill this in." But the physicist Nagel has in mind was Martin J. Klein (1924–2009), an American science historian who worked as a physicist first at the physics department of Cleveland's Case Institute of Technology. Eventually he became the chair of Yale's Department of the History of Science and Medicine in 1971.

[129] Paul Ehrenfest (1880–1933) was an Austrian-Dutch physicist, known for his work—among others—on quantum mechanics. For the biography see Klein (1970).

Nagel: Well, I'd really have to count on my fingers to tell you. When I was a student the members of the department were Woodbridge and Bush and Dewey, Schneider and Randall and Horace Friess.[130] These were tenured people, and then there were a number of instructors or assistant professors.

Currently there's Robert Cumming[131] and Morgenbesser,[132] Danto,[133] Levi….

Q: Professor Walsh.[134]

Nagel: Professor Walsh and Professor Kuhn[135]—I would say about the same size roughly.

Q: I wondered whether it had gone up during the 60's and 70's when the number of students must have gone up considerably.

Nagel: I don't think so. Of course, our load at one time was heavier. That is, when I first came to Columbia the requirement was that everyone in the department teach three courses each semester. Eventually this was sort of whittled down to two courses each semester—never by any kind of an official act but by a subterfuge essentially. That is, somebody would be down to take care of graduate students and such and such: direct them in their dissertation. And this eventually came to be counted as a course though there were no regular meetings.

The Columbia Department of Philosophy: that was perhaps unusual and wasn't always the best arrangement. That is, when I started teaching at Columbia I think nearly everybody was a graduate of Columbia College. So you belonged to the right caste to be in philosophy. And so as a consequence, what was regarded as important during the college days of members of the department—this was continued. Eventually that was altered. I mean currently as far as I know, there's not a member of the department who is a Columbia College graduate taken from elsewhere. And I think that's important, to have that kind of a mixture.

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[130] Horace L. Friess (1900–1975) was an American philosopher, working mainly on ethical issues.

[131] Robert Cumming (1916–2004) was a Canadian-American philosopher and historian who worked mainly on Continental philosophy and taught at Columbia between 1948 and 1985.

[132] Sidney Morgenbesser (1921–2004) was an American philosopher at Columbia University, known mainly for his intellect and philosophical witticism. Though he published little, he edited a Festschrift for Nagel. See Morgenbesser, Suppes, and White (1969).

[133] Arthur Danto (1924–2013) was an American philosopher at Columbia; he is also known for his art criticism and work on the philosophy of history.

[134] Presumably James J. Walsh (1924–2003), an American philosopher who was a member of the Columbia Department of Philosophy between 1954 and 1990. He was editor of *The Journal of Philosophy* for a while and published on Aristotle, moral and medieval philosophy.

[135] It is unknown whom Nagel had in mind; Thomas Kuhn, for example, was never working at Columbia.

Interview 4

Interview with Ernest Nagel by Remmel Nunn

New York City March 17, 1983

Q: Professor Nagel, we had agreed to talk about your philosophy today. Where would you like to begin?

Nagel: It's really hard to know what is the proper place to begin. When I was still in high school I read some books that would certainly count as being philosophical. For example, I read some of Nietzsche, and I read some of the Stoics. But I can't say that I was really committed to any of these notions. And I think it wasn't until I really got through college that I got a sense of what philosophy was like, and in that case it was really, as I mentioned I think before, the model that I had of a philosopher was my teacher, Morris Cohen. There were other people in the department at City College who were interesting people, but they didn't grip me the way that Cohen did.

So for many years I think I subscribed to the kind of empirical rationalism which Cohen represented. He was a self-proclaimed realist as far as some of the traditional mystics are concerned. For example, he was a Platonist in supposing there were such things as universals which had some kind of a being even when they're not embodied in anything. And I took that view as my own, which was certainly very much supported by the little mathematics that I read in those days where one was faced with: "What are these mathematical objects that one talks about?" And it's very easy to say, "Well, to be sure these things do not exist," but then there's the lingo: "But then they have a different kind of existence. They subsist." And so I took that as my own.

When I started graduate work in philosophy, there were several people at Columbia who influenced me. One of them was Woodbridge, who was a kind of Aristotelian who in part took a view similar to that of Cohen, believing in the reality of universals. But he didn't take a Platonistic view, but rather an Aristotelian view on these matters.

So in a way Woodbridge fortified the kind of ideas that I managed to gather from my exposure to Cohen and to City College.

The other person who, of course, certainly had an influence on me was John Dewey. As a matter of fact, Woodbridge and Dewey were the only people in the department with whom I had any courses.

Now, Dewey I found extremely impressive as a human being, but I'm not really sure that I understood the pragmatism that he represented. He was not a stirring teacher in terms of his eloquence and the care with which he presented his ideas. But he was impressive figure because you saw a man really thinking through the questions he was talking about.

But I couldn't be convinced by anything that Dewey said that this Platonistic-Aristotelian view was mistaken.

As a matter of fact, it's somewhat…well, amusing is perhaps not quite the word for it, but I'm not quite sure what is. But in my graduate years Woodbridge, who

was also editor of the Journal of Philosophy, had received an article from Dewey for publication in the Journal.

Woodbridge showed me the article, which at the time when he was talking about it hadn't been published yet, but he asked me whether I would care to comment on the article.

Q: For publication?

Nagel: For publication. Dewey's article was about the so-called laws of thought, like the principle of noncontradiction or the principle of the excluded middle or the principles of identity. And I eventually wrote the article, and this was a rather wholesale criticism of that position, that Platonist-Aristotelian view, and indicated what I thought were weaknesses in his argument.

The reason why I say it's amusing is that a few years after that piece of mine was published I had a change of heart, and the position that I rejected when Dewey had presented that article, subsequently I embraced for myself.

Now, how did that come about? Well, I think myself it was a combination of two considerations. One was that if one claimed to be an empiricist in philosophy—that we learn about the nature of things by observation, experiment and theorizing about those things—but it was empirical in the sense that the ultimate test for these ideas would have to be some reference to things that one could observe in any sense of observation.

Now, I regarded myself as an empiricist, but then if one accepted this Aristotelian Platonism, one would also have to say well, there are certain general principles, like the so-called laws of thought which presumably also held for existing things. And at least I felt that incompatibility between being an empiricist on the one hand and on the other hand that you really could say, well, these laws of thought they're not generalizations from experience. They're in a real sense a priori. One could certify them as being true on the basis of reflecting on what these things meant.

So I was in a quandary about how to get out of this difficulty, and I didn't get any sense of finding an answer until I went abroad in 1935.[136] I had a Guggenheim Fellowship, and I went on sort of a grand tour of Europe, and eventually I got to know some of the people in Vienna and Warsaw and in Prague, people who were in one way or another related to what was then called logical positivism…logical empiricism.

What I learned from them was I think that these laws of thought—what they really are empty as far as content was concerned—and that they really in a sense, although it's not clear in exactly what sense, represented decisions that human beings make in using language. That is, in a loose sense there were conventions that people made. So instead of regarding them as being a priori truths about existence, I held the view that they were part of the apparatus that human beings themselves bring to their inquiries for the sake of achieving certain results that normally one would say: "Well, these are part of the objectives…".

[136] Nagel went to Europe in 1934–1935.

Q: Would it confuse the issue to ask if they were necessary conventions?

Nagel: Well, I faced that question. Some years after that there was a [missing phrase][137] volume published that was edited by the late Professor Krikorian,[138] who was at City College at that time. I think the title of the book was Naturalism and the Human Spirit. And I contributed an article on logic. And in that article I tried to indicate the reasons for accepting this sort of conventional view, and I had to face the question that you just raised: are there any alternatives to these conventional laws of logic? And I came to the conclusion that there were alternatives, although they were not useful alternatives, because they would destroy some of the objectives that one has in using language, in making inquiries and so on.

For example, if you consider the principle of the excluded middle—that there are only two alternatives, for example: being red or not being red; being hungry or not being hungry; and that this was a kind of fundamental dichotomy and that there was no in-between…

Q: You can't be red and not red.

Nagel: That's right. But I also realized that this is all right in principle, but in fact our language is vague—meaning by that that if, for example, you consider a color spectrum now, you would say, "Well, there are some things that are definitely red, some things are definitely not red, but then there is an intermediate realm where you cannot say, 'This is red or not red.' In a sense it's both. And you can classify it one way or another." So I saw, or at least I thought I saw, very clearly how to answer the question which you raised—namely, to recognize that one uses language very successfully even though in many cases this principle of the excluded middle just breaks down. So that one can communicate not as effectively as one can otherwise, but one can communicate even though one recognizes that some of these logical principles are in point of fact at best only approximates of what goes on.

So that, too, was a help to me to reaffirm this position.

So the positivists—people like Carnap, for example, or the late Professor Frank, who was a physicist, and a number of other people whom I got to know on my trip to Europe—emphasized to a great extent the conventions behind the appearance of a kind of smooth illustration of some of these traditional logical principles.

Now, eventually there was a revolt against the positivists, and people have taken the view, for example, of Professor Quine[139] of Harvard. In a series of papers he challenged what he called the dogmas of logical positivists, and he took a rather strong position that the laws of logic, instead of being a priori truths, are in one sense confirmed by scientific inquiry. That is, instead of saying, "There are alternative

[137] At this point the original transcript's pencil note says: "Get Nagel to fill this in.".

[138] Yervant Hovannes Krikorian (1892–1977) was a Turkish-American philosopher, who taught at City College, New York. He worked on naturalism and Marxism, but he is mainly known for a volume he edited, and Nagel refers to in the next sentence; see Krikorian (1944).

[139] W.V.O. Quine (1908–2000) was an American philosopher and logician, who is known as one of the most influential thinkers among analytic philosophers. Quine was working for almost seven decades at Harvard.

logics," yes, one could conceive of this; but then in point of fact he used one logical system because it is confirmed by repeated experience. So Quine took a position that is a revival of a position that was taken by John Stuart Mill[140] about 150 years ago, who argued that not only are these so-called laws of thought, like the principle of the excluded middle, empirical truths; but that even propositions of mathematics are like that. Mill argued, for example, that $2 + 3 = 5$ is a proposition that has been so fully confirmed by experience that you took it for granted, and this was a position that was abandoned eventually in the nineteenth century, and then Quine had the courage to revive it.

Now, I never felt comfortable with that. It seems to me that one can use a logic that is very different from the traditional logic and organize one's experience in terms of that. The point I mentioned before perhaps illustrates that: that you can in effect use a logic that does not contain a principle of the excluded middle, and that this does not depend upon the content of sensory experience, but that it is part of the resolution to obtain certain objectives in doing science or doing scientific inquiry.

Well, you see, I think I've taken that position pretty much in spite of the fact that so many of my friends have taken the view that this distinction between so-called empirical truth and logical truth—that there's no sharp distinction between them. I mean, for example, that's the view that Quine took, and that's the view that a great many people accepted. Maybe it was old age, but anyhow I stuck with the difference between a proposition that can be known a priori and those that require experience to confirm it.

Well, this is part of my odyssey if you like in philosophy. That is, I started with being a fairly faithful Aristotelian or Platonist that eventually ended up in a halfway position. To be sure, one must have a sort of a criterion, rational criterion, for accepting propositions, but the rationalism is not the traditional, old-fashioned rationalism, where you think that with the aid of reason alone you can discover the structure of things. So that's been sort of a general background for me.

Now, of course, this touches only one point of one's philosophy. After all, there's more to philosophy than is concerned with sort of ontological questions of this sort. And then there were those influences I think I've already mentioned: This semi-Aristotelian view of Santayana that the good life is not something that is established in terms of some Platonic notions of the good, but that people choose between different forms of life, and that there are no absolutes here but that different individuals under different circumstances in different civilizations would have different conceptions of what is a satisfactory or good life. And I've really been sticking to that for a long time. So that in my moral or ethical views I've been a relativist, but not a relativist who thinks that everything is good or bad depending upon what one says, but that there are effective grounds for choosing one form of life rather than another one. So that's been really quite an important view in my own life, although I have written relatively little about ethical matters, except in a somewhat different context.

[140] John Stuart Mill (1806–1873) was a British philosopher and political economist, often noted as a radical empiricist who claimed that logical and mathematical statements are simple empirical generalizations.

Again, when I was at City College, I attended a course given by Cohen on the philosophy of the law, and I didn't cultivate that for a long time; but eventually, when I was still active in the philosophy department at Columbia, I gave an undergraduate course in the philosophy of law; and then I took part in this graduate seminar in the law school with some members of the law school faculty.

Now, there certainly important moral questions arise. If you are considering the objectives of a legal system, clearly to the extent that there are positivistic thinkers who take the view that moral criticism has no place whatsoever in the law, but that what happens to be the positive law is by itself a ground for accepting it.

Well, I couldn't take that position, but I did take a rather negative position towards the notion of a natural law, which, while this view was made explicit by the ancient Greeks—for example, in Plato and Aristotle—and then it was taken up again by Christian thinkers, particularly St. Thomas in the Middle Ages, who thought that there was a categorical commitment to having to subscribe to certain kinds of ethical ideals because they were, so to speak, in the nature of things that you had to somehow do that.

Now, I've written on the subject of natural law and criticized it. And, again, this kind of relativism that I've mentioned before has been part of it.

Now, I never thought that I ever had a system of philosophy, and there are some branches of philosophical investigation about which I have no enthusiasm. For example, I find that the literature in aesthetics is something that leaves me utterly cold. I don't really understand what people are talking about when they deal with that.

I can understand, for example, attempts to philosophize about religious experience. I can understand that. I was brought up, of course, as an orthodox Jew, and I've abandoned that for a long time. And I don't subscribe to any religious notions whatsoever, even though I continue to have a kind of attachment to people who subscribe to views I was brought up in.

I think the only other thing that I may want to say about my philosophy would be I suppose questions of social values. And there really I would again be a relativist in the sense that I've indicated. But on the whole if you have to characterize these positions, ever since I was in high school I think I had a strong preference for socialist ideas of human society. And, of course, this view has been very hard to maintain in the light of what has happened in different parts of the world where countries have gone into some form of socialism. It certainly hasn't worked out. And so in my social views, I think I would regard myself as a kind of rationalist liberal rather than being a socialist or something of that kind.

One other thing I think I ought to mention: Philosophy has had its own problems, like the problem of the universal, the problem of other minds and things of that sort. And I have been interested in those things, but it seems to me that philosophy has also a different task, and that's simply to resolve these problems that are more or less distinctive of traditional philosophy, but that philosophy has also been a critique of not only the ethical values that people hold, but also of some of the other general beliefs that people have. And so I confess I find that being a philosopher interested in only these rather specialized problems is not something that I can take to my bosom.

I always thought that philosophy ought to be in a position to dirty its hands in other people's business and offer some sort of a critical evaluation. So, unlike some of my friends and contemporaries, I think of philosophy as a critical occupation and that you can make some sort of a contribution to life even in a specialized society such as ours, provided that you didn't make philosophy a rather narrow discipline—another specialty among other specialties.

So I think I've practiced, to a degree at any rate, this conviction in the kind of teaching that I've done. I mean I had a course in the philosophy of mathematics. The subject matter is not primarily philosophical, but there are broad issues that are raised by mathematical disciplines.

I have been interested in some of the problems in the social sciences—in particular sociology—and I collaborated for a great number of years in taking part in a graduate seminar in sociology. I collaborated with the late Professor Lazarsfeld.

I collaborated with some people in the law school in a graduate course in the philosophy of law. One year I think I collaborated—yes, I did—with Professor Feinberg[141] in the physics department. We discussed some issues of space and time and some of the paradoxes of quantum theory.

All these things—I don't even boast about it. I'm not boasting, but I'm trying to express that I've tried to live in accordance with my conviction that philosophy is an important discipline not because it solves problems of its own, but because it deals with problems that are really of general concern to people in all areas.

That I think is the way I would want to talk about my philosophy.

There's one other point. And, again, it's hard to know just how to express one's conviction about this. I never had a very thorough training in the sciences. I did get a master's degree in mathematics. I took a number of writers' courses in physics. And I suppose if I had learned about these matters when I was still in school, I may have gone into some discipline such as physics or mathematics. But when I became aware of these disciplines, it was really too late for me to do anything about it. But I have retained a great respect for the sciences. I've tried to read a little bit on my own and continue to be informed about what is happening. But in this general sort of outlook upon things, the revolt against any idea that you have to produce evidence for what you believe and that you don't settle things simply by indicating what your preferences are, but that you must indicate what the reasons are for these preferences, I've always regarded myself as sort of a champion of the rational life as illustrated by the various sciences, both natural, and to the extent that they've been successful, the social sciences. So this has been really a very strong conviction. And I suppose it's really a part of… I don't know whether it's so much a part of what philosophy is as part of one's general attitude to the contemporary scene. And so in that sense I feel myself being a very old-fashioned person, you see.

[141] Gerald Feinberg (1933–1992) was an American physicist, working at Columbia, who worked on quantum physics; he is often associated with the hypothetically faster-than-light particles, called tachyon. Feinberg thanks the help of Nagel in many papers of his; see also his paper in Nagel's Festschrift, Feinberg (1969).

Q: You said at one point a moment ago that as long as philosophy did not allow itself to became a narrow discipline among other narrow disciplines, it would remain healthy and could maintain its position as a discipline that could criticize many others, almost the mother of the sciences. Do you feel like philosophy is in a healthy situation in terms of the way it's progressing now?

Nagel: Well, let me come to a different end of this problem. When I was still a student both on college and in graduate school, certain problems were very much discussed, and then I and many others thought that well, these problems have been settled. They're gone. There are different answers to them.

Well, I learned at first to my dismay that if you live long enough, all these old problems come up again, and people continue dealing with them. And this I think is an illustration of the narrowness of a conception of philosophy which thinks it has its own particular problem—specialized problems which can be pursued by a person who knows nothing about anything else.

So I found myself very often out of the thick of things as far as the contemporary philosophical scene is concerned, because people were doing just the sorts of things that I thought were not very fruitful.

And so in response to your question I would say that I thought what many of my contemporaries were doing did not make me feel that philosophy was a worthwhile enterprise.

But now coming to another aspect of this thing: I also found that after a while people began to tire of these very specialized problems and that in point of fact there had been a sort of rebirth of an interest in other areas. That is, there would be a philosophy of something rather than just philosophy in the blue.

So while I've had senses of frustration at one time, I began to think that well, things are not as bad I once thought, and that there is a growing concern with concrete problems to which philosophy can offer at least kind of a critical attitude. So on the one hand I would say well, in the main most philosophers today are concerned with fairly specialized, narrowly specialized issues, but I think it's also true that there's a substantial number of philosophers who have been concerned with issues that arise out of some concrete inquiry, whether it's in physics or in the social sciences or whatever.

As a matter of fact, in some cases I think there are more extreme…. For example, one takes the philosophy of science as an illustration for this. There are quite a number of very learned philosophers who have mastered some branch of natural[142] social science to the point where I think some of them are trying to do the job that physicists do: become extremely technical in dealing with problems in let's say quantum mechanics. And what I find that's happened is that in many cases what they're really doing is repeat what you can find in the textbooks of physics, and I think this is a pity, because it seems to me there should be some kind of a philosophical payoff which is not just something that is an echo or a repetition of what a competent physicist can do. I can read physicists for that rather than philosophers.

[142] The original typescript does not have anything here between "natural" and "social".

So the picture I think is sort of mixed. A lot of it is hopeful, but then some of it is still a continuation of traditional views, and still another one is where people go overboard and just try to become better scientists than the scientists themselves.

One illustration of that is: I know a young man who's very much interested in the philosophy of language and very able, but he has no patience whatsoever with anything that is not geared directly to technical questions that arise in being a linguist. And I think this is a pity because he's an able person, but he's gone overboard I think.

Q: To the point where he not only specializes in these very specific questions but also doesn't attempt to extrapolate from them to more abstract ideas?

Nagel: That's correct, and is extremely critical of people who do.

Q: Professor Nagel, you have written more, and more coherently, than anyone I know, on the issue of reductionism in the sciences. I wonder if you could say something about your view of the reductive quest and its feasibility and what's going on in that area.

Nagel: Well, I think you raise an extremely important question. And if I may start answering you by making a comment upon a view that's sometimes been inculcated by people who have been very knowledgeable in some of the sciences, particularly physics: there has been a view which certainly goes back many many centuries, that the so-called commonsense world with all its furniture, including the colors and the smells and the sounds, are really superficial aspects of nature and that they really have no strong foothold in nature, and that ultimately what is really real are—to put it in terms of Democritus—"atoms in a void."

This is not just an ancient view. It's a view that's been restated by contemporary thinkers. The first one I was thinking of as illustrating this in terms of figures that have a conspicuous position in the contemporary scene is Professor Wilfrid Sellars.[143] He accepts this dichotomy—that there is reality, but then the human scene is just kind of the psychological appearance of things. And the reason why I think he and others take that position is that they think that one has, to use the current language, reduced colors {to} electromagnetic vibrations. Something analogous were smells and the tactile quality of things. And I think that people are really deluded in supposing that by effecting something they call reduction, one has eliminated these things from the domain of nature. I think that's a mistake.

And if I may use other kind of illustrations: it's often said currently that well, physics has really made tremendous strides. It has reduced chemistry to physics, because, so the claim goes, the laws of chemistry can be derived from the assumptions of quantum mechanics; and similarly that biology is no longer an independent domain—it's simply a branch of physics perhaps, because allegedly we now know something about the chemical composition of various parts of the body, including

[143] The original typescript said "Wilfred Sellars," but Nagel had in mind Wilfrid Sellars (1912–1989), an American philosopher, associated with many American universities (Iowa, Minnesota, Yale, Pittsburgh). Sellars approached philosophy quite critically and historically, and though he was always influential, during the last decade his work was rediscovered and reconsidered again in many volumes.

the genes. And so we have to come to the rather pervasively held picture of the nature of things: that there are these certain fundamental elements and the laws connecting them, and everything else is just a derivation of that.

I think that's a mistake. And perhaps the best way of showing this or presenting some argument for denying that this has been the case: if you consider exactly what has been done in biology. To be sure, we know now a good deal about the chemical composition of various parts of living organisms. But if you ask—take some biological law, such as one finds if one discusses the genetic properties of human beings and one makes all sorts of discoveries about cell division and how the components of a cell arrange themselves and eventually they split and so on, these are elementary but fundamental laws in biology, particularly in that part of biology that deals with genetics. But no one has thus far shown how one might be able to derive these laws from physical chemical ones.

Now, my own position would be: you cannot show a priori that this could not be done. But at the same time I also believe that this has not yet been done, and the prospect of doing this—I don't think that it's particularly bright.

So I think in the polemics that one carries on, one directs criticisms against those who think: "Well, there really is a fundamental division between different branches of science," and that it is in principle impossible to reduce, say, biology or even psychology to something that is assumed when one does biological research.

I think that the number of places in the various sciences where one can actually show that a reduction has in fact been effected seems to me rather small. And, again, while I would not take the position that further progress cannot be made, I think in point of fact there are important difficulties and one has to face them.

Now the additional point that one has to make in this connection is, as you know, that in order to effect a reduction in many cases, one has to introduce what have been called bridge principles or corresponding principles. If one thinks, for example, of the famous remark of Laplace that an intelligence that is greater than ours would know the instantaneous positions and motions of the fundamental particles, everything in the history of man could be deduced…well, the answer—the retort—to that seems to be perfectly plain. If you start out with, let's say, as Laplace did, with the assumptions of Newtonian physics, and you consider what terms appear in such sciences … well, in the case of mechanics it's simple. You have to have the notions of space, of time, of motion and of mass. Now, if you think you are going to be able to obtain from fundamental laws which introduce these terms, and if you're going to try to derive a statement about—let's say—the color that one sees, obviously you couldn't do this, because you can't have suddenly a new term appearing in your conclusion where there's nothing in the premises which warrant this conclusion. So you need bridge principles. But once you admit that, once one sees that, one has to say that even if the reduction were as complete as you like, it couldn't be done without the introduction of bridge principles; and in consequence, if you think that atoms in a void are real but everything mistaken, because the bridge principles themselves were contained in some of the crucial terms.

Q: One other thing that I thought of: you mentioned, speaking of the reduction in chemistry, biology and even psychology: one of the fascinating things about the reduction of psychology to physics is that it raises the issue that we were discussing much earlier about the necessity and universality of the laws of thought. And it raises the old issue of psychologism. One would think that someone who held to the reduction of thought to matter in motion would have to be an empiricist about the laws of thought, perhaps a "psychologist", or a holder of psychologism I should say.

Nagel: Right. Well, you see, there are a lot of pursuits currently about which it's hard for a person who's not directly involved in them to know what is the likely outcome. I'm thinking, for example, of the tremendous amount of thought and energy that's been devoted to artificial intelligence. I'm not very well read in this domain, but the little that I've read: it seems to me that people have a program but thus far mighty little to show for that … I mean that you really have indicated in what way one can sort of analyze all of the so-called rational processes in terms of these rather simple assumptions involved in making a machine.

I don't know whether you happened to see some controversy between proponents of artificial intelligence and philosophy in Berkeley Professor Sears[144] has been a very stout critic of artificial intelligence, in effect arguing that none of the results that students of artificial intelligence have obtained have taken care of some aspects of human thinking that seem not to be caught by the assumptions that are made in artificial intelligence.

I have an old friend who's pursuing artificial intelligence. Perhaps you know him. This is Douglas Hofstader,[145] who eventually became quite of world-wide renown because of his book on Gödel and Escher and Bach.[146]

Now, he's a firm believer that eventually you can really reconstruct the character of sensate organisms in terms of ideas that come from artificial intelligence. But this still is a hope, and why not pursue it? That's perfectly all right. But I don't think there's been convincing evidence that you could do this.

Interview 5

Interview with Ernest Nagel by Remmel Nunn

New York City, March 24, 1983

Q: Last week, Professor Nagel, you said that philosophical system building was not really something that you were particularly interested in.

Nagel: Yes.

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[144] Nagel presumably had in mind John Searle (1932-) who is an American philosopher, who worked at Berkeley and was indeed a noted critic of strong artificial intelligence. Presumably Nunn heard "Searle's" as "Sears".

[145] Douglas Hofstadter (1945) is an American cognitive scientist and popular science writer.

[146] See Hofstadter (1979).

Q: I wondered whether that was a personal preference or whether there's something in principle about that sort of philosophy that you objected to.

Nagel: I don't think it's personal. Now just what principle is involved in that, I'm not quite sure. Just a sense of what the function of philosophy is. Really, that's the main point. I think that a lot of people must share that view that, well there was a certain time when every philosopher who thought he had anything to say would have to have a discussion of the whole series of things. Has a book on religion, has a book on aesthetics one on theory of knowledge and logic and so on and I don't see much point in that unless of course it just happens that a man has something to say about these different areas. I suppose if one lives long enough, as Dewey did, in a way he did have a system because he wrote on all these things we just mentioned.

In that sense I don't think it's very useful these days to have that kind of a thing. What happens is that some of the sections of systematic philosophy are not very deeply thought through and I think this is certainly true for Dewey. His views on aesthetics, that's probably a very useful volume.[147] But his little book on religion, his little book on political theory,[148] they leave me cold.

Q: One of the philosophers you've mentioned several times, Alfred North Whitehead, came up with a sort of monumental metaphysical system.

Nagel: Yes, yes.

Q: Monumental in its own scope. And yet he also delved into at least physics to a great degree.

Nagel: Surely.

Q: What did you think of Whitehead's work?

Nagel: Well, his earlier books, I thought they were really extremely stimulating and they held out such a great promise.

Certainly there's been one problem that interested me for a long time and I'm not sure that the problem has even been solved, at least completely, namely how are these abstract, theoretical concepts related to matters of every day experience? I mean this seems to me to be a really central problem in what I call the theory of knowledge. You take different views. Some people think that the every day view of the world is just an appearance and the reality lies somewhere else, or the other way around.

So the early books of Whitehead namely The Concept of Nature and The Principles of Natural Knowledge,[149] at least they suggested a way of establishing connections between theoretical notions and the things that we learn from our mother's knee, so to speak.

One example of Whitehead's procedure was what he called his "principle of extensive abstraction." The principle was intended to show how, specifically he used

[147] See Dewey (1934).

[148] Presumably Dewey (1934) and Dewey (1935).

[149] See Whitehead (1919 and 1920).

examples like this, how the concepts of geometry can be related to the physical world. If you think of the fundamental concepts like point, line, plane; how a point is something which has no dimensions at all, a line has one dimension but no thickness. On the face of it, these do nothing to be referred to anything that actually exists. What he tried to show in those early books is how you can really tie up these notions with something that is really in existence. At least when I first read those books, and I think it must have been when I was in college, I felt very excited about that.

Later on I had some doubts about the success of his principle of extensive abstraction, but at any rate, he was to many of us a very fascinating and promising thinker. When his later works came out, particularly Process and Reality,[150] it was a great disappointment to me as it was to some of my colleagues. Not to people who make a kind of central place for process philosophy, I know Dewey was very sympathetic to that. Professor Randall was. But I didn't cotton to that and I still remember when Bertrand Russell was here at Columbia, he was giving a series of lectures and we had a dinner for him and he was asked what he thought about the later Whitehead. He said, "Well, it's not worth anything at all." Maybe that's strong, but on the whole that expresses my sentiments.

Q: What sort of issues are you thinking about and working on these days?

Nagel: Well, I'm working on a number of things rather sporadically because I manage to get diverted from doing these things. I don't remember now whether I mentioned the fact that I gave the Carus[151] lectures about ten or more years ago.

The Carus lectures required that you deliver three lectures and eventually make a book out of them and publish them by the Carus Publishing Company. The Carus lectures were instituted by Paul Carus who was a German who came to this country—I believe it must have been in '48 when the failure of the revolutions in Europe, in central Europe and Germany and Hungary and so on—he came over and established this publishing firm that was located I think in Chicago or some suburb of Chicago, I forgot now. He published some of the philosophical classics in paperback very cheaply and then he published other things as well and in his will he wanted to establish these lectures that were called the Carus lectures.

There was enough money at that time to make the honoraria worth something. I think the honoraria was something like a thousand dollars. And you received three hundred dollars when you read your lecture, another three hundred dollars when you did the manuscript for the publisher and the remainder when the publisher printed the book.

I gave three lectures, it must have been in the mid-60's I believe, it's a long time ago now. One of them was on essentially the philosophy of the natural sciences, the second one was on the philosophy of law and the third one was on the philosophy of history. And as I say, I just let those things ride and didn't do anything about them

[150] See Whitehead (1978). Originally delivered as the 1927–28 Gifford Lectures.

[151] The original typescript had "Carris lectures", but Nagel was definitely talking about Paul Carus and the "Carus lectures" he delivered in 1963, "The Dimensions of Critical Philosophy." "Carris" has been corrected to "Carus" in the text everywhere.

until a few years ago when suddenly I realized that time was getting late and I better do something, if I wanted to have it.

So, I made a commitment to myself that that is what I wanted to be doing. But for one reason or another I got diverted, partly for reasons of health but partly I believe because other things seemed to me more tempting. Like giving a lecture or writing a paper for this or that. It was only about three years ago that I made up my mind about what I wanted to do but then I didn't get very much done.

One of those lectures I've completed, I've completely revised. This was the one on the philosophy of law. So the original lecture was something like twenty type-written pages and now it's about fifty type-written pages. And I plan to do the same thing for the others and add other materials too.

I had some interest in, for example, economics and some interest in some schools of psychology and I thought it would be no problem at all to really make a decent book out of it. But so far I haven't done it.

The proper reply, I think, to your question is that I have been thinking on these matters but it's been sort of sporadically. I think I have a kind of difficulty in saying no.

Q: Professor, among your many works, I just wondered which part of it you feel the best about or the proudest of.

Nagel: I think myself that the book which was published in 1961, The Structure of Science,[152] is perhaps the most ambitious and systematic thing that I've done. I've published also some essays but they were pretty much independent of one another even though they had a good deal of repetition in them.

Now the book on the structure of science, I had a contract to do the manuscript in 1941 and the book wasn't published until 1961. So there was a long delay. In addition to that, the original plan was to have either a tremendously large one volume or a two volume book. Now in the second volume what I planned to do was to deal mainly, although not exclusively, with the problem connected with probability and induction. I felt really very regretful that I haven't been able to do that and now it's too late for me to do that because the literature in that area has grown tremendously and I couldn't control it any more. It would be a considerably younger man than I am who would have the energy to go through it and make some systematic presentation of the main ideas and of my own views on this. So it would have to be one of these truncated affairs, just a one volume rather than a two volume book.

I suppose a substitute for writing the second volume is the Carus lectures and there again I let time slip by without paying attention to the things as soon as—many people who are Carus lecturers pretty much had the book written before they gave the lectures. I mean this I think was probably true for Dewey who was the first Carus

[152] See Nagel (1961).

lecturer.[153] It was also true of the late philosopher at Chicago, George Mead.[154] It was certainly true for Professor C.I. Lewis.[155] He gave just three lectures and they were on rather abstract things and they were already written, they are now chapters in this book called Analysis of Knowledge and Valuation. It's a tremendously written book. It's probably over-written, but he had no problem in publishing it pretty shortly after he gave the lectures.

Whether there's enough time for me to do the edition, Lord knows and He won't tell.

Q: Of the three topics, you mentioned law, history, as well as your work in science, of the three areas, does the science still sort of hold a special place for you? Is it something that you think more about?

Nagel: Well, there too I think I let things slip by. I know a little bit about what's going on in physics today. Not very much but some. But there too, the developments have been so fantastically huge that an outsider like myself can't really penetrate some of this material. So that a lot of people have of course {?}[156]some of these paradoxes of quantum theory. I find that a very fascinating subject but then while I have certain prejudices on this, I don't really know enough to write about that kind of material with any confidence.

So I suppose the answer to your query would be that I don't think I'm currently spending much time on thinking about the natural sciences with the possible exception of biology where I continue to be concerned with some of these issues such as teleology or of behavior and so on.

Since I have made a good deal of effort to re-write this chapter on natural law, I suppose that's about the most intense activity I've had in the past two or three years, I suppose. I've been reading a fair amount of that and I've managed to continue my association with people in the law school after I've formally retired. So that's been a great help.

(Tape interruption).

When I came to Columbia—after having gone to City College for my undergraduate degree and I was very much influenced by Morris Cohen—I came to Columbia kind of a scoffer as far as Dewey's philosophy was concerned. I acquired that disposition, I think, from my teacher. Eventually I began to see what he was trying to do and that it wasn't quite as unimportant or as unphilosophical as Cohen sometimes seemed to hold, to believe.

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[153] John Dewey delivered the first Carus lectures in 1925. See Dewey (1925).

[154] George Herbert Mead (1863–1931) was an American philosopher, sociologist and psychologist, working at the University of Chicago. He delivered the 1930 Carus lectures; they were published in 1932 posthumously as Mead (1932).

[155] C.I. Lewis (1883–1964) was an American philosopher, associated to pragmatism and modal logic. He taught at Harvard almost during his entire career. He delivered the 1945 Carus lectures on "An Analysis of Knowledge." They were published, as noted by Nagel in the next sentence in 1946; see Lewis (1946).

[156] The original typescript contains the question mark to note the missing phrase.

I could never regard myself as being very close to Dewey. There were certain temperament differences, I mean his whole outlook upon things was kind of an activist who thought a philosophy should deal with problems of men, that it should occupy itself with making an effort to improve the state of mankind. And while I certainly respected that and in a way I shared that view, I couldn't really give myself whole-heartedly to that. And so I couldn't do what some of my friends did to become almost friendly with Dewey and I didn't [have] too much respect at all for him.

But I had a recent experience—the Southern Illinois University Press is publishing all of Dewey's writings from soup to nuts, everything. They must have easily ten volumes already published, the early works that were essentially before 1900 and the middle works which were somewhere around the early thirties and the last set of volumes are going to be reprinted, the so-called later works. One of the later works is Dewey's Logic.[157] All of these re-publications for the most part, if they were books rather than a collection of essays, had an introduction by somebody. And I was asked whether I'd do the introduction to Logic. I agreed with a certain amount of misgiving because I thought well, again, I'm diverted from doing these Carus lectures. But I finally agreed and I re-read the book and it was a very difficult task for me to go through that; it's not very well written, but that's neither here nor there, I mean he's not a great stylist but also the absence of a really detailed treatment of a lot of issues which he touches upon.

So I was really in a great quandary. I didn't want to just make this a paean of praise; at the same time I have a great deal of piety towards Dewey. So this was sort of a struggle between being honest and being pious. Well, somehow I managed to do that, I finished the job. It's about twenty-eight pages long and again, you see, that indicates how long it takes me to do that because one would write an introduction of this kind I suppose, for the most part, in a day or two but it took me several months to do it.[158]

And this I think is now kind of a—that is, my opinion of the Logic book has certainly gone down from what I think was the opinion I once held shortly after the book came out. I must say I feel somewhat that way about a lot of other things by Dewey. He was certainly a, I don't think I'd want to say great man, but he was certainly a man who was an important man and he did a good deal for education in this country. But that's not a subject that I could ever feel very much interested in. But if you think of some of his non-logical writings, such as his views on ethics, he again did not work out—as far as my own opinion is concerned—he didn't work out the details of the theory that he held. In retrospect it's rather disappointing to find this so. And I begin to see the point of some of the English critics of Dewey who can't quite understand why he's regarded to be a tremendously important philosopher. Because the books don't have this kind of finish or this sort of virtuosity that one would anticipate.

[157] See Dewey (1938).

[158] See Nagel (1986).

I don't know whether you've ever seen the book by an Australian philosopher Passmore. He has a book called One hundred Years of Philosophy.[159] It's a fantastically interesting book—Passmore must have read everything. And he reports on all of these things that have happened in Anglo-American philosophy in the past hundred years. Now he—I forget now whether he said that in a book or whether it was in conversation with him—he said, "Well, I just don't understand why you Americans think so highly of Dewey." And I begin to see the point of that. And it's a little bit disappointing that I should feel that way about it.

But then I suppose that's true of so many other people of Dewey's generation. Compared to the virtuosity which philosophy exhibits, it doesn't stand up to that kind of thing. He even—philosophers today who really have very little to say, but then they can say it very well. So I think too, for example, Cohen's writings, if you re-read them—when I read them as a student, they were terribly impressive, of course he was a very learned man. But I had occasion to re-read some of his things and I don't see why one felt that his critiques were really incisive.

One of the things I did in connection with this introduction to Dewey's Logic, Cohen had a controversy with Dewey, this was a great occasion—I forget now whether it was Dewey's seventieth or eightieth birthday—there was a meeting of the American Philosophical Association here at Columbia. There were two commentators on Dewey, one of them was Cohen, the other was, he's no longer alive either, he was a Harvard man—it'll come to me in a minute—and then Dewey replied to that. These things were published in the Philosophical Review,[160] I forget the year now, and Cohen I don't think did justice to Dewey and his criticisms weren't really as keen and incisive as they could have been.

So that I have this sense of being unbalanced in my feeling towards Dewey. He was a fine human being and fought for the right things, but the quality of his philosophical works is not as good as I tend to think of that. And that's a little bit saddening, this kind of experience where you think the man is really at the top of his profession and he's doing all the important things and then when he dies usually people no longer read him and if they come back to him, they don't find what they're looking for.

Q: To me it raises another question though. Can you think of any philosophers from that period for which the reverse was the case? Their names never really became known but perhaps in retrospect what they did was better than Cohen or Dewey.

Nagel: Yes, well, I'm not sure that I can think offhand of some people in recent American thought.

Of course it's in general true that there are fashions in philosophy and it was a fashion to regard Dewey as an outstanding American philosopher. When Cohen

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[159] See Passmore (1957). Passmore (1914–2004) was a historian and a philosopher as well as it is reflected in his mentioned work.

[160] Nagel presumably has in mind volume 49, number 2, 1940 of *Philosophical Review* that published a symposium on Dewey's *Experience and Nature*. It contained a paper from Morris Cohen, "Some Difficulties in Dewey's Anthropocentric Naturalism" (pp. 196–228), one from William Ernest Hocking (the "Harvard man", noted by Nagel in the main text), "Dewey's Concepts of Experience and Nature" (pp. 228–244), and Dewey's reply, "Nature in Experience" (pp. 244–258).

was alive he was regarded as being an acute critic. Admittedly not an outstanding constructive thinker, but a very acute critic. During his lifetime he had this tremendous reputation as a person who could penetrate through a complex argument. Now in the case of Cohen, I say as far as I can tell, nobody reads him except possibly his essays dealing with problems in the philosophy of law. {—?—}[161] is, the book that he regarded as his book the Reason and Nature—I've had some students who were fascinated by Cohen and they wanted to write a doctoral dissertation on him and two of them did.[162] And they come away pretty disappointed. You see they didn't find what they had hoped to find.

The man who had a tremendous following for a while was Santayana. That was certainly true at Columbia. Frederick Woodbridge was perhaps the most influential person in the Department of Philosophy for a while. He was dean of the graduate school. He had a tremendous admiration for Santayana that was shared by some of the younger people like Irwin Edman. Of course, this really was not shared by many people outside of a certain locality. Cohen had a high opinion of Santayana, very largely because of this five volume book that Santayana wrote, The Life of Reason. And we read four of those volumes when I was an undergraduate taking Cohen's course in the philosophy of civilization. Now, this high regard for Santayana has pretty much evaporated. In England, to the best of my knowledge, he was never regarded as being an outstanding philosopher. I remember a remark that—I wish I could remember the name of the—yes, Warnock[163] was in the country and he thought that Santayana was the Puccini of philosophy.

So I've really been mentioning things that are contrary to your question, they don't answer your question but I'm talking about people whose reputation have declined. But the others, I know there are certain philosophers who deserve to be better known but they haven't been. For example, there was really a most interesting philosopher at the University of Wisconsin when I was still a student and he gave the Carus lectures and they were eventually published posthumously.[164] He had very interesting things to say about problems in epistemology which certainly today are not known at all. I find in talking about it to people, they don't know a thing about it. He held a position that he himself referred to as "objective relativism." To simply show that things could be dependent upon some condition, but nonetheless they are genuine parts of reality. I mean he wrote what I think was an extremely good paper, it appeared in General Philosophy around 1938 or something of that sort, '32 maybe, called "Perceptive and Memory Perspectives."[165] And there he dealt, among other things, with the question of what are we going to say about these so-called secondary qualities such as colors

[161] The original typescript contains the question mark to note the missing phrase.

[162] It is unknown whom Nagel had in mind.

[163] Geoffrey Warnock (1923–1995) was a British philosopher and Vice-Chancellor of Oxford. Warnock was often associated to the ordinary language movement, and he is mainly known for his book, *English Philosophy since 1900* (Warnock 1958).

[164] Nagel has in mind Evander Bradley McGilvary (1864–1953) an American philosopher, born in Bangkok. He is often associated to idealism, Hegelianism and worked often on theology. He delivered the 1939 Carus lectures on "Toward a Perspective Realism", see McGilvary (1956).

[165] Nagel had in mind McGilvary's paper from 1933, see McGilvary (1933).

and sound and small and taste? He argued very effectively I thought for the genuine reality of them, despite the fact that they developed on the presence of an organism. Now, that view was never developed by anybody.

Q: Interesting.

(Tape Interruption).

Nagel: When I came to Columbia the department really consisted of people who were products of Columbia, Columbia College and Columbia Ph.D.'s. They had a certain outlook upon things which was sort of more or less characteristic of Columbia and eventually it was recognized that this kind of inbreeding was not a desirable thing. And eventually, of course the department is now made up, pretty much, at least half of the members of the department are not Columbia graduates. Edman was a Columbia product, so was Professor Gutmann[166]—I don't know whether you've met him, he's retired now and Professor Schneider who was primarily interested in ethics and political theory, he was a Columbia man. Professor Friess, Horace Friess, he was Columbia. And as a matter of fact, this group of people, for years they had a date to meet one day a week and have lunch in the faculty club. Anybody who came here and didn't have this background was an outsider.

Q: Really? You said earlier that there was a certain attitude that they had. Was that part of it, just this sort of cliquishness?

Nagel: Yes, yes. You see, the sense you had about the group of people was that everything that was in some way connected with Columbia, that was fine, everything outside was foreign.

But part of it—I'm not so sure whether this is so but sometimes you have that kind of suspicion—Professor Randall's father was a minister, Professor Schneider's father was a minister, Professor Friess' father-in-law, Felix Adler,[167] was a minister and there was a kind of sympathy towards religious philosophy that I could never share. That too in a way was, but then I'm sure that must be true for a great number of other people of that generation where it was customary for a minister to give courses in philosophy.

Q: I realize you're talking about something subtle and just about a few men but it still contrasts somewhat with the sort of liberalism you've also referred to at Columbia.

Nagel: Yes, yes.

Q: I believe one thing I'm not sure we have talked about, that you mentioned before we began taping was the whole period of—

Nagel: The uprisings.

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[166] James Gutmann (1917–1988) was an American philosopher, who worked at Columbia, and who was the Director of the University Seminars between 1970 and 1976.

[167] Felix Adler (1851–1933) was a German-American political and moral philosopher, founder of the Ethical Culture Movement.

Q: No, not the uprisings but the period back in sort of "red-baiting" days when certain Ivy League schools were much more demanding than Columbia.

Nagel: Yes, that's right, that's right. Now Columbia, I think, has an excellent record on that and—

Q: What was going on at the Ivy League Schools? I mean, what were they asking people to do?

Nagel: Well, I know of one case, he happened to be on the faculty at Columbia. He had been a member of the Communist Party and was taking graduate work at Harvard. This was essentially sort of the McCarthy period and he was called in by the dean of Harvard, at that point McGeorge Bundy.[168] Bundy asked him to tell about his experience as a member of the Communist Party and my friend agreed to do that but he said "I decline to say anything at all about other people who were in the Communist Party." And as a result of his refusal he was dismissed from a small position he had at Harvard.

There are comparable things I know that happened elsewhere. Nothing like that happened at Columbia, even though there were people who were sometimes difficult in their political positions.

Q: Do you think this was a result of who the president of Columbia was then? Or do you think it was a general liberalism?

Nagel: Well, I think it was certainly connected with the administration. During the First World War, when Butler was president, he forced the resignation of some people. For example, both the historian Harvey Robinson,[169] was critical of the American position in the First World War and he was, as I remember this he was fired. Charles Beard[170] resigned in protest. There were other men too who lost their jobs.

Let's take a different sort of example. There was a man in the Classics Department, this was also during, roughly, the First World War period. He was a member of the Classics Department, a very able man connected with Longfellow,[171] the poet. He was divorced and remarried and as a consequence he was fired. This was also Butler.

Now nothing like that happened during the Second World War. There were some people, I don't know whether actual members of the Communist Party, but they were pretty far to the left and there was no interference with their continuation. The only

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[168] McGeorge Bundy (1919–1996) was an American academic, and advisor to John F. Kennedy and Lyndon B. Johnson. He became the youngest dean of Harvard's Faculty of Arts and Sciences in 1953.

[169] James Harvey Robinson (1863–1936) was an American historian, one of the founders of "new history" that aim to open up the subject-field of history, including the social sciences as well.

[170] Charles Beard (1874–1948) was an American historian, a professor at Columbia, working on, among others, the economic interpretation of the American constitution. He resigned form his post at Columbia in 1917.

[171] Nagel has in mind, presumably, Henry Wadsworth Longfellow (1807–1882), an American poet and educator. He was also the American translator of Dante's *Divine Comedy*. It is unknown who was the member of the Classics Department who was fired.

case that really arose was a woman anthropologist, Jean Weldfish.[172] She—again I don't know whether she was ever a member of the Party—she was pretty far left and she accused the—this was during the Vietnam War—that Americans were using poison gas in Vietnam. Well, as far as I know that isn't true but despite this sort of thing she was never taken to task for making that statement. She was eventually, not fired, but she was not re-appointed in the Anthropology Department. And there the reason given, and I see no reason for doubting it, was that she wasn't that good anyway. There used to be a committee, there still might be, a kind of committee on conference which consisted of members of various departments who met in order to discuss some kind of a problem about a member of the faculty and I served on that committee much later. Jean Weldfish wasn't even asked to appear before that committee.

So the record is really extremely good. The person who was president during those years, I think was Grayson Kirk. It just happened that it was in that period that I was asked to serve as chairman of the local A.A.U.P. committee, let's see, the American Association for the University Professors. And so we introduced resolutions in the faculty meetings and so on. Sometimes the faculty didn't approve of these things but the administration never raised any questions about that.

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[172] We were unable to find anyone by that name.

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Chapter 14
Ernest Nagel's "The Philosophy of Science" Lecture at the Delaware Seminar



Matthias Neuber and Adam Tamas Tuboly

Abstract Among the "Ernest Nagel Papers 1930–1988" at Columbia University's Rare Book & Manuscript Library, there is a file at Box 7, entitled "The Philosophy of Science: University of Delaware 1961". This is the edited text with some editorial comments and polishing.

14.1 Editorial Introduction

Among the "Ernest Nagel Papers 1930–1988" at Columbia University's Rare Book & Manuscript Library, there is a file at Box 7, entitled "The Philosophy of Science: University of Delaware 1961".[1] Nonetheless, to that material, our attention was called at first by the "Hist-Analytic" webpage (www.hist-analytic.com). There, it was indicated at the front cover of the untitled talk that it was passed over to Steve Bayne by Prof. Stefan Bernard Baumrin. As many would recognize the name, Baumrin was one of the leading organizers of a famous seminar series about philosophy of science at Delaware University between 1961 and 1963, known simply as "Philosophy of Science: The Delaware Seminar". Most materials were published in two volumes (Baumrin 1963a, b) at Interscience Publishers. As it is stated in the series introduction,

[1] We edited this talk and reproduce it with the kind permission of Yvonne, Sidney, and Alexander Nagel. All rights reserved. We are also grateful for the help of Steve Bayne. Adam Tamas Tuboly was supported by the MTA BTK Lendulet Morals and Science Research Group and by the MTA Premium Postdoctoral Scholarship.

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the seminar was designed to build bridges between "the scientific and humanistic communities".

Just a few years after C.P. Snow's notorious diagnose about "the two cultures" (Snow 1961b)—namely the separation of the natural and the human sciences that were supposed to rely on different approaches, aims, goals, and values—people were eager to do something about the situation in America's education system.[2] As we stated in the introduction to this volume, Nagel was deeply interested in disseminating knowledge about how science works and how the precise, but still deliberate use of its method delivers well-balanced results. It is not that surprising then, given also Nagel's fame in the intellectual circles of the 1950s and 1960s, that Nagel was one of the prominent invited speakers at this conference to give a general lecture about philosophy of science.

Nagel's lecture could be taken as a summary of his general philosophy of science at the time of the publication of his most famous *The Structure of Science*. It could be also read as '*Structure* in a nutshell'. The talk was followed by a short discussion, which is also included here as it sheds some further light on Nagel's thinking and approach.

All footnotes are ours, and the included secondary literature, along with Nagel's own intended but not revealed references are to be found at the end of the chapter.

Nagel's paper was not published before, and we have no information about why Nagel's talk was not included in the published volumes.

14.2 Ernest Nagel: The Philosophy of Science

The title of this talk obviously leaves me a great deal of room for scope since it is so very indeterminate as to what the subject is that I am going to talk about. And I think partly in order to place the themes upon which I do want to talk in some detail, and partly in order to outline the range of the subject that one calls the Philosophy of Science, I thought it would be appropriate to say something about how I conceive the structure of science—what is it. And clearly to talk about the structure of science would first of all require one to indicate roughly what one means by science itself. Now, I think we know enough about the controversies that have been going on for a very great number of years as to whether something is or is not a science. These controversies, on the whole, have been exercises in name calling; they have been profitless. Certainly, you may take the position that it is only mathematical physics which is a science and every other sort of investigation is just stamp-collecting. But I had rather not engage in such recriminations, so I would like to think of science

[2] One might recall here the attempts of one of Nagel's close friends from the logical empiricists, namely Philipp Frank who formulated a similar thesis to that of Snow's although years earlier. With his teaching, lecturing, and publications Frank made a lot to find bridges, or as he called them, "links", between philosophy (human sciences) and science (natural sciences). For him, it was the philosophy of science (see Frank 1957; 2021).

in a much more catholic fashion, and I would like to delimit it as being identifiable as an institutionalized human activity which conforms to certain institutional traditions and which is to be distinguished from other institutions in three respects; namely, in respect to its aim. And here I think the aim is, in most general terms, the acquisition of explanations or understanding through a systematic and comprehensive determination of fact, but the emphasis would be upon the attempt to achieve understanding or explanation. This is the general aim. Secondly, not only an aim but certain products, and the products are primarily intellectual, which consist of more or less systematic explanations, the degree of systematization will vary in different disciplines that are counted among the sciences; primarily intellectual, but secondarily consisting of effective control over various kinds of events. And thirdly, in addition to aim and product, there are certain procedures, procedures of inquiry, in which I think one can distinguish, certainly, certain special techniques, each having a fairly limited range of application to specialized inquiries into certain specialized subject matters. But not only special techniques but something which is perhaps less easy to identify, namely—certain logical methods whose scope coincides more or less with the entire domain of science, and these logical methods include, certainly, principles for assessing evidence for the various conclusions that are obtained by inquiring. And allegedly, and certainly more debatably, principles or rules for the actual conduct of research and for the making of scientific discoveries. It is in terms of aim, product and process that I would want to identify something that I was afraid to call a science. And clearly, this account allows for a great deal of variation.

Now, what would an account of 'the structure of science' be, if science is conceived in this way. Well, it seems to me that an account of 'the structure of science' will include the following components.

First, bearing in mind that science, as I conceive it, is an institutionalized activity, directed toward certain aims—because it's an institution—such an account must include a discussion of the influence of other social institutions and society at large upon the development of science, both in respect to the substantive content of science and also in respect to the canons of standards of scientific procedure. I might say right off that this is an aspect of 'the structure of science' about which I will have nothing to say.

Secondly, an analysis of the ways in which what I have called the intellectual achievements of science are organized; that is, an analysis of the kinds of explanations that one is able to obtain, and an account of their internal patterns. And also, a discussion of the manner in which explanations become altered through new experimental discoveries or the introduction of new ideas. So, the second aspect that I would want to include in the account that is sufficiently adequate of the 'structure of science' would be a consideration of the organization of the intellectual products. And the intellectual products would include, certainly, a good deal of factual detail, but in terms of the primary aim, it would certainly include an account of the kind of explanation or the sort of understanding one obtains, or seeks to obtain, in various areas of inquiry.

Thirdly, an account of the ways the explanatory premises are related to matters of observation or experiment and of the requirements which such premises are generally

expected to meet if they are to function properly in inquiry. And this, of course, would include, among other things, the consideration of the various ways in which scientific ideas may be defined in terms of each other but more specifically, of the routines or routes which connect ideas which may on the face of it be abstract and rather remote from the materials of observation and experiment to the subject matter that is being investigated.

And fourth, and finally, an account of 'the structure of science' would have to include a reasonably complete codification of, and rationalization or justification for, the logical principles that are employed in evaluating or in warranting the conclusions that are adopted at various stages of scientific inquiry.

Now, as I mentioned, I shall have nothing to say about the impact of society upon science and the way in which scientific activity might be influenced by social considerations. And I will have very little to say, although I will have something to say in passing, about the relation of explanations to matters of observation. And what I will want to concentrate the time I have available upon are certain specific issues that arise in connection with a discussion of the procedure of science or the methods of science and secondly, specific questions that I would like to discuss dealing with the way in which certain kinds of explanations in science might be related to others. But I will indicate more specifically what I want to talk about in this connection when I come to it.

Let me come, then first of all to the issue that I wish to discuss in connection with the principles of scientific method. And I thought it would not be entirely inappropriate since, a week ago, Professor Hanson made, I'm sure, a very vigorous plea (perhaps more than that, an argument) for there being such a thing as a logic of discovery.[3] I thought it would not be inappropriate if I commented on the question whether there is such a thing as a logic of discovery. And I thought the best way of doing this would be to really comment upon the arguments that Professor Hanson uses. I didn't hear his lecture last week but I assume that the things he said in print about these matters would be at least in substantial agreement with what he said here.[4]

Let me first of all say something to place the problem. What is the problem about there being a logic of discovery? Now, some of us are very well familiar with the fact that historically, a great number of thinkers, some of them scientists; others, commentators on science or philosophers, have sought to find certain rules, such that if a man were properly instructed in those rules, then he would be able to advance a science by making substantial additions to the content of science. Some of the very well known names in this long tradition are certainly Francis Bacon, of the seventeenth century, John Stuart Mill of the 19th, and early nineteenth century,

[3] Norwood Russell Hanson's lecture was published in the conference proceedings, see Hanson (1963).

[4] See, for example, Hanson (1958); see further Hanson (1961).

Herschel,[5] who was a distinguished astronomer, who also believed it might possible to formulate such rules.

In the image that Francis Bacon used, just as a person who is a highly gifted painter or draftsmen would be able to draw freehand a circle which lesser men would be unable to reproduce, but if they are given a pair of compasses, then you don't have to be a Giotto in order to draw freehand something that approximates to the geometrical canon or standard for a circle, so Bacon argued, just as in the past it had been great men who made important scientific discoveries, if you could find certain rules making discoveries, and if these could be taught, then much lesser men could make important advances in various branches of science.

Now, as far as this particular quest is concerned, the record, I think, is without qualification uniformly a failure. No one has been able to devise any rules for making discoveries or for making inventions. Many of us have thought that this was a dead issue; that no one who was familiar with the history of thought would try his hand again at resuscitating what seems like a lost cause. However, there has been a revival of interest in this subject and Hanson has been a vigorous exponent of the claim that there is such a thing as a logic of discovery; not a set of rules for making inventions but a logic in the sense if you examine what are admittedly tentative groupings of the great figures in the history of science, one can find that there is a method in their procedures. And while familiarity with their methods may not enable the rest of us to make scientific discoveries, nevertheless, it isn't just a hit or miss procedure; that the business of making important discoveries is not, as Bertrand Russell once said, just a matter of guesswork, but that there are certain more or less invariant canons or rules by means of which one could formulate why it is that man comes to think of one hypothesis or why he prefers to take seriously one hypothesis rather than another.

Now, the argument as I understand it, and certainly the one that Professor Hanson advances, is that there may be good as well as bad reasons for a scientific inquirer adopting one kind of hypothesis rather than another. And these reasons, so he claims, may but also do differ in type—namely, the reasons for his initially adopting, for the purpose of further exploration, one hypothesis or another; that the reasons which lead him to accept one type of hypothesis rather than another, that these reasons are different from the reasons that one advances in order to accept a hypothesis as being the conclusion of a determinate inquiry. And I think the point could be made clear in terms of a favorite illustration of Prof. Hanson. Kepler convinced himself that on the basis of the observed positions of the planet Mars, instead of assuming now that the planet has a circular orbit, that the planet moves on an elliptical orbit with the sun at one of the foci of the ellipse. Leaving aside the question of how he came to entertain this hypothesis before concluding that the detailed, factual observational evidence supported beyond possibility of reasonable doubt—leaving aside that question—assuming that Kepler had established on the basis of detailed inquiry that this was really a satisfactory hypothesis to account for the observed position of the planet

[5] Frederick William Herschel (1738–1822) was a German-British astronomer. He discovered Uranus in 1781and became a well-known scientist and Fellow of the Royal Society.

Mars, Kepler turned his attention to other planets, in particular to the planet Jupiter. What kind of an orbit did Jupiter have?

Professor Hanson argues that if Kepler concluded that the likely hypothesis, although you couldn't be sure that this would be, was that Jupiter also had an elliptic orbit, it was because Kepler must have argued something like this: not only is Jupiter like Mars, a planet, but Jupiter, like Mars, is an exterior planet; that it exhibits certain variations in the apparent velocity as it moves through different parts of the heavens. In short, there were certain pronounced analogies between Mars and Jupiter and because of these analogies, he had good reason for supposing that the orbit of Jupiter was also of a noncircular kind; that is the type of hypothesis which he should entertain was one which would lead him to say that the orbit was non-circular and more specifically, that the orbit was an elliptical one.

Thus far, of course, Kepler simply arrived at a suggestion as to what specific hypothesis to explore. He reasoned by analogy to the hypothesis that the orbit of Jupiter was elliptical. This kind of reasoning would not be sufficient, so Professor Hanson argues, and I think probably correctly, this kind of reason is not sufficient to establish the claim that the orbit of Jupiter is elliptical. To establish this, one would have to make detailed observations on the various observed positions of Jupiter to see whether they do or do not fit in within certain limits of the requirement that these positions should fall upon an elliptical orbit. And so, in short, the argument is this: in extending the elliptical hypothesis from Mars to Jupiter, Kepler was reasoning by analogy. In establishing the Jupiter hypothesis that it was elliptical, he was not reasoning by analogy, he was reasoning from a set of examined instances, namely observations on the position of Jupiter—finding that these fit into the hypothesis and in concluding that since the observed positions fall upon the orbit, therefore, all further positions which had not yet been observed, would also fit on to that orbit. And so, he makes what seems like the prima facie reasonable claim that the type of reasoning which Kepler used, and which investigators use, in making discoveries is different from the type of reasoning that they use in establishing the suggestion that they have arrived at the preliminary stage of the inquiry. And I think he rests his case upon examples of this sort.

Now, let me just make some very brief critical comments on this. Let me first of all say what my conclusion is and then advance reasons for it. I don't think that the argument that Professor Hanson advances to show that there is something that none might identify as a logic of discovery as distinct from a logic of proof, shall we say, or logic of validation—I don't think the argument is one that carries very much weight and for the following reasons.

Let's grant that analogies play important roles in the construction of hypotheses. This, it seems to me, to be indubitable. Various kinds of analogy, and if there were time, it would be really very instructive to examine the variety of analogies that are employed in different domains of scientific inquiry, but my point would be that though there are analogies, which lead a man in one direction rather than another, it is the case that the type of hypothesis to which the analogy leads is controlled by the more or less tacit assumptions which are part of the accepted body of beliefs at the time. So, for example, when Kepler, according to Professor Hanson, arrived at the

suggestion that the orbit of Mars was non-circular, he opted for a type of hypothesis which required him to say that the likely hypothesis would be one which would say that the orbit of Mars would be a closed plane curve, smooth in shape, and that Kepler would have rejected the assumption or the suggestion that a likely hypothesis to account for the different velocity—that apparent velocity which Mars exhibits to different points of its orbit is in some way controlled by variations in the apparent color of the planet; so the color hypothesis is excluded; the type of hypothesis which was required to say that it is a non-circular orbit is regarded as being reasonable. But why is the color hypothesis excluded? Well, I think the answer is very simple—because of the general beliefs that Kepler had which led him to say that color is not a relevant circumstance to account for the behavior of a planet. Kepler had all sorts of curious ideas about the influence of various sorts of forces and emanations from the sun. Color was not one of the things that played a relevant role in this, and so certain hypotheses are excluded because of the assumptions that a man has at the time.

Just to make the point perhaps a little more emphatic, suppose an investigator today is confronted with the data that Rhine at Duke,[6] or some other people working in parapsychology, produce to indicate that some people can correctly call the configuration on a card before they see it and you want to explain this. What hypothesis would seem likely? This will depend very largely upon the kind of a training a man will have had and what he will regard as being antecedently reasonable on the basis of his antecedent scientific experience. I venture to say, although this is an extrapolation from the responses of a few statisticians whom I know, that if a statistician were to be asked to offer a type of hypothesis which would be a reasonable one to adopt, he would say: Well, this unusual success in calling cards, this might be accounted for in terms of the general assumption that the shuffling of the cards, although it is maybe done in accordance with some kind of a randomizing procedure, that no actual randomizing machine is completely random; every randomizing machine, at least over a finite run, exhibits some kind of a bias. Individuals who say that they are making guesses—whether individuals who have different patterns of guessing would be able to call the cards as successfully as other individuals who have a different pattern of guessing, in that the correlation that is to be made is between patterns of guessing and the pattern of bias which the particular randomizing machine, which shuffles the cards, introduces into the shuffling.

Now, as you know, Mr. Rhine did not think that this was a reasonable hypothesis. He introduces the hypothesis of extrasensory perception. For him, this was the reasonable hypothesis. But what seems reasonable—what seems credible—what seems irrelevant is, in a certain sense, controlled or dictated by the set of more or less explicit assumptions a man has as to what factors are to count in giving an explanation.

And so, instead of attention to a logic of discovery, I think that all Professor Hanson has successfully called attention to is the direction in which, given a certain intellectual framework, investigators will prefer to explore one type of hypothesis

[6] Joseph Banks Rhine (1895–1980) was an American botanist, known, however, for his work on parapsychology. He founded a parapsychology lab at Duke University after he moved there in 1927.

rather than another because of the tacit or sometimes explicit assumptions with which they operate as being experienced practitioners in a domain. And so I conclude, although to be sure much more needs to be said in order to make my argument conclusive, that the claim that there is a logic of discovery is a claim that is without any substantial support. What there is, according to me, is that there are principles for assessing evidence for conclusions or for hypotheses that are advanced and for which evidence has been produced; that there are no rules for finding conclusions. There are no general rules for finding evidence but, given a certain set of evidential statements, given certain conclusions which are based upon that evidence, there are, so I would submit, certain general principles which control the evaluation or the weighing of the evidence to see whether the evidence supports, strongly supports, powerfully supports, supports beyond reasonable doubt the alleged conclusion.[7] The conclusion, of course, in this case is not something which follows necessarily from the evidential premises. Conclusion, to use a well-worn phrase, is only made probable on the basis of the evidence and so the issue is whether there are any rules for weighing the degree of probability. And here, though clearly this is a subject which would require several talks in order to even skate over the surface, I can only say two things which seem to me to be pertinent. First, that though in recent years there have been very ingenuous attempts to develop a calculus for weighing evidence for conclusions that do not logically follow in such a way that it would be possible to assign a numerical value to the degree of evidential support, thus far, at any rate, there is no reason to suppose that these attempts are more than a bit of scientific fiction: that they have no relevance at all to any thing that plays a role in actual scientific practice. I mean scientific practice in a sense of scientific inquiry even when scientific inquiry is concerned with fundamental theoretical questions rather than with practical ones. So, I think at present one has to say that, at best, we can introduce a kind of a scale, a rough qualitative of a scale of stronger evidence, less strong evidence without being able to assign a numerical degree of probability. And, of course, this would certainly mean that what is known as the mathematical calculus of probability, and which has obviously very important uses, is not applicable to this particular domain when what one is asking for is the degree or the weight of the evidence which supports a particular hypothesis in some area of inquiry.

The second point I would like to make is this; that many attempts that have been made, again in recent years, to codify principles of evidence or principles for weighing evidence have placed a tremendous emphasis upon the sheer number of bits of supporting evidence and there has been a tendency to believe that the more—the larger is your evidential basis, the stronger is the support that the evidence supports the supposed conclusion. Now, I think that this is not an adequate account. It is partially correct and that it is not an adequate account I think becomes clear if we remember that perhaps one of the elementary points in any sort of a scientific inquiry

[7] One might recall here Feyerabend's early review of Hanson's *Patterns of Discovery*, which shows interesting similarities with Nagel's criticism. Nonetheless Feyerabend is not mentioned in the lecture; neither did he pop up in Nagel's *The Structure of Science*. See Feyerabend (1960, pp. 250–251).

is to introduce controls in any observation or experiment. I recall a very distinguished physician telling me that until perhaps something like 20 or 30 years ago, patients in hospitals who suffered from typhoid fever were uniformly plunged into a cold bath. And it was believed that this was very salutary. It had a good effect. When this was done, it apparently had not occurred to those who were in charge of the hospitals to ask the question: Well, look, we are confident that this is so, but do we really have any control; that is, have we tried to do the following very simple things. Have two more or less matched groups of individuals who are suffering from typhoid—some of them we are going to submit to this cold bath treatment—others, we are not going to submit to that treatment. See if there is any differential effect that is significant and decide upon the basis of introducing a control group.

Now, what is the control in point of fact indicate? It means that what you are interested in is not simply the sheer number of items of evidence but that you also want to introduce some kind of variety in the evidence; that if you have the same experiment performed over and over again without essential variations in it, that this, in a certain sense, does not increase the degree of support that the evidence gives to a conclusion. Certainly it increases it much less than if you had a fewer number of experiments but they came from different areas; that is, for example, the quantum hypothesis which Planck introduced in order to account for the distribution of energy in the spectrum of black body radiation—well one could continue making fine experiments about the distribution of energy and find that Planck's formula was in accordance with it. But people really didn't begin to feel confident in the hypothesis until, for example, Einstein was able to show that the photoelectric effect could also be accounted for in terms of the Planck assumption; or when Einstein showed that the specific heats of solids could be explained in terms of the quantum hypothesis or when later, Bohr could account for the Balmer Series in the spectrum of elements—again in terms of the quantum; that is, what is important is the variety or, at any rate, it is an essential point in estimating the weight of evidence and the sheer number without variety seems to me to count very little.

Beyond that, I think there is no time to go, particularly if I do want to say something about one other aspect of this thing that I am calling 'the structure of science'; namely, in connection with the analysis of explanations. Now, I suppose from the point of view of a systematic presentation of the subject, one ought to—or at least I ought to, since as far as I know, I am the first one to talk about the subject in the series and there will be others who will be talking about it, I ought to start at the very beginning and indicate first of all, something about the types of explanations that one can identify, raise some of the issues that have been considered by students as to whether there is only one type, or whether there are a plurality of different types; that is, raise formal questions about the logical structure of explanations. Similarly, one could ask apart from formal differences, such as for example, some explanations seem to be of a deductive sort where what is being explained follows necessarily from the premises. As, for example, when Newton explained Kepler's law that a planet moves on an elliptical orbit with the sun as the focus, he explained this by showing that this was a consequence of his assumptions about the laws of motion and universal gravitation. This was a deductive explanation. Then, there is the kind of explanation

that one calls probabilistic, where the conclusion doesn't follow necessarily from the premises but the premises, in some ways, stand in a relation of probability to the conclusion. For example, if a historian explains the action of some one individual in the past, for example: if a historian seeks to explain Roosevelt's attempts to pack the Supreme Court,[8] why did he do this? No historian, to my knowledge, is able to produce as explanation a set of propositions from which Roosevelt's proposal follows necessarily. All a historian does is to offer statements such that if those statements are true, it makes Roosevelt's action probable.

And so there are a variety of different kinds of explanations which differ in their formal structure. Then there are explanations, and again I want to hasten over things which I am sure will be covered in greater detail later on—explanations which differ in the kind of explanation which is offered. What I mean by this is that sometimes we wish to explain a singular occurrence such as, for example, we wish to explain why it is that there was a rainfall on a particular day at a particular place. And so what we explain then is a singular occurrence. Sometimes what we wish to explain is a general feature of something and we do this by subsuming it under something that we might call a regularity—an empirical law.

Let me offer what you might think is an absurd illustration because it is so elementary and primitive. If I find that a certain object, which is placed into some electric circuit, conducts electricity very well, as compared certainly with something else that I might introduce into the circuit, then I ask myself, why does it? And suppose I say, well, of course, you see the reason is that this substance that I have put into the electric circuit is made of silver and silver is a good conductor. So, I have explained the general characteristic by indicating that it can be subsumed under a simple law like silver is a good electrical conductor.

Now, sometimes, of course, we wish to account for things like, why is silver a good electrical conductor? Now, in this case, I think you will find that after a certain point, the explanations, when they are satisfactory, no longer consist of simple empirical laws like the one that I just mentioned, but that the explanation will be given in terms of what we generally call a theory. For example, the theory which is formulated in terms of the atomic structure of the elements, which will account for differences in the electrical conductivity. And so my point is that one can distinguish one group between explanations which use as explanatory premises empirical laws. If, for example, I want to explain why it is that ice floats; well, you know the answer could be given in terms of Archimedes' law. Archimedes' law is sort of a low level generality. It's empirical—it could be tested, so to speak in a laboratory. But now, you say, well—now, but why should Archimedes' law hold. Well, eventually it led to the principles of mechanics, to Newton's laws of motion, to certain assumptions about forces operating in a liquid; and you no longer have a simple empirical law which

[8] In 1937, President Franklin D. Roosevelt proposed a legislative initiative to add more justices to the U.S. Supreme Court. As the Court had ruled the New Deal unconstitutional previously, he hoped to obtain more favorable rulings with the increased number of justices. The legislative initiative failed after all.

explains Archimedes' law, you have something one calls a theory. And so there are differences in explanations of the kind that I have been indicating.

I want to turn, for the remainder of the time that is available to me to a discussion of theories and to certain problems it seems to me arise in connection with theories. Problems that I think are often perplexing, perplexing, I will venture to say, not only to the layman in science but most frequently to the scientist himself.

First, let me say what I do mean by theory. I have given illustrations but I think I would like to be a little bit more specific as to what I do mean by theory, particularly when I contrast it with an empirical law. One of the things that I think is characteristic of those systems of premises that we call theories is that first of all, they can explain a large variety of different material. If, for example, you think of what Newtonian mechanics can explain. You can explain the behavior of a freely falling body; you can explain the behavior of a planet; you can explain the behavior of the tides; you can explain, with some specialized assumptions, the rise of liquids in thin tubes; that is, these are qualitatively different phenomena and yet, all of them can be subsumed, can be explained, in terms of this theory that we call mechanics. Similarly, with electromagnetics; similarly, with the kinetic theory of gases.

There are a tremendously large number of qualitatively distinct phenomena which can be explained. Empirical laws don't do this. I mean, Archimedes' law, for example, what will it explain? It will account in general for the behavior of solids in liquids, but that is the limit; that is the range of explanation. Newton's theory will account for Archimedes' law but will also account for Kepler's law, for Galileo's law and for a great number of other things. So first of all, one of the things that characterizes a theory is this large range of coverage that it has. Secondly a theory frequently, if not always, tends to correct an empirical law. For example, Kepler said that all planets—more particularly Mars—moved on an elliptical orbit and now we say, well, yes; his observations certainly confirmed that. On the basis of Newton's theory we say yes, of course, Kepler's law follows from Newton's assumptions provided that there are just two bodies, but if you introduce a third body, then the behavior of Mars—the orbit of Mars—is no longer that of an ellipse. It must depart from an ellipse and so we know then that on the basis of theoretic considerations, that Kepler's conclusion cannot be quite correct; it might be a first approximation, so that one important function, a characteristic of theory is that they are frequently used in order to correct what are called empirical laws.

On the basis of theoretic considerations, we correct Boyle's law, which holds only for so-called ideal gases and show how by introducing more complicated assumptions, we can get Van der Waals equation and things of that kind. This is all, I take it, very familiar.

Thirdly, I think it is one of the important functions of theories that they provide directives for experimental research. For example, the kinetic theory of gases, among other things, you see—you say, well, according to the kinetic theory of gases, each molecule of the gas has a mass. What is the mass of the gas? According to the kinetic theory, a given volume of gas under standard conditions of temperature and pressure will have a certain number of molecules in it. What is this number? Well, the theory

raises questions of this kind and then the experimental physicist tries to set up some experiments which will enable him to make these determinations.

So, first of all, theories are characterized by the fact that they have this variety of functions that experimental laws in general do not have. Secondly, it is one of the features of theories that they have certain analytical components. And I would like to mention three—many others could be identified but for my purposes, these three will suffice. First of all, I think it is the case that most terms that occur in what we are prepared to call theories are rather remote from experience; that they cannot, so to speak, be defined in terms of observational terms. This is evident even, I think, in the case of some of the ideas that enter into Newtonian mechanics. Sometimes, Newtonian mechanics is regarded as not being theoretical in this sense which I am indicating because many people claim that the theoretical terms, that the basic terms, really are very close to experience because we are going to talk about position, talk about velocity, talk about mass. But if you look at the way in which Newton proceeds—of course, Newtonian mechanics is particle mechanics—supposes that the things he is dealing with have positions but no dimensions. Well, point masses—point mass is not an experimental concept, point mass is a theoretical concept. Again, when Newton supposes now that point masses can have instantaneous velocities, instantaneous velocity is not an experimental concept, it is a theoretical concept. And so on down the line.

In the kinetic theory of gases, we don't define what is to be a molecule in what is sometimes called operational terms. We do this in terms of certain postulations upon the character of molecules without defining them in terms of some sort of an overt laboratory procedure. So one of the features of theory is that the terms that play fundamental roles in the theory in general are not defined overtly, the way, for example, the late Professor Bridgman insisted that all terms should be given operational definitions.[9] This does not seem to me to be the case for theoretical terms.

The second point to observe about theory is that because the terms are not defined operationally, it is an essential step in using a theory that one should introduce what people have called by various names, but I am going to call them rules of correspondence; that you have to have rules which will associate the theoretical term with some kind of a well recognized experimental situation.

Let me illustrate again what I mean. There are no rules of correspondence for the term molecule, for the term velocity of the molecule; that is, we don't know how to associate the term velocity of the molecule with something that we can identify in a laboratory. What we can do is to define certain theoretical notions in terms of the basic theoretical notions, such as, for example, the mean kinetic energy of molecules and then introduce some kind of a rule of correspondence as well, 'mean kinetic energy of molecules' that we are going to associate with temperature, as temperature is defined by some kind of a laboratory procedure. So you have some sort of a range between the theoretical term or a complex of theoretical terms, and

[9] Percy W. Bridgman's most important work regarding operational definitions is his early book, Bridgman (1927).

something that we might recognize as an experimental notion, which is associated with a fairly definite laboratory of operational procedure. And so, it is required then for a theory, rules of correspondence. Professor Northrop, I think, in the first lecture, called them epistemic correlations[10]; other people have called them coordinating definitions; other people call them semantical rules. I think I use the term rules of correspondence in the way in which Professor Margenau, about whom I want to say something else, uses it.

The third thing I would like to mention in connection with theories is that by and large, every theory that is used fruitfully will not only have rules of correspondence but will also have something that we will call a model. For example, the kinetic theory of gases, well, it is familiar to say that the kinetic theory of gases has been built on the image of billiard balls, but, of course, that image of billiard balls is not essential to the formulation of the kinetic theory because what does the mathematical work in the kinetic theory are simply the various equations from which further deductions can be made. Instead of formulating the equations explicitly, sometimes it is convenient to say, well, look; let's forget about the finer details; let's think of the theory in such a way that there are certain—in some cases—visualizable models which satisfy the postulates of the field. Similarly, we have this in quantum mechanics, although there are difficulties in finding a suitable model but certainly some model, either a wave or a particle or some fusion of them, and then you try to get over difficulties by introducing such things as complementarity or what not. All these are devices by means of which you can find something that you will recognize as a model. Now, I think that models are important not simply because they are psychological aids, but because models suggest analogies and in this way, tend to unify different areas and also because models help us to indicate just how I am going to use the formalism of a theory in dealing with some experimental situation.

Now, assuming that this is so, assuming that theories are of this character, the feature of theories to which I would like to address myself for the remainder of my time is this. It is, I think, a well known phenomenon in the history of science that at various stages in the development of science, one theory is replaced by another, and the things that one theory is used to explain are then explained by another theory; that is, what sometimes happens is that one branch of a science becomes somehow absorbed into another one.

As for example, when Maxwell absorbed optics into physimagnetism or when, on the basis of the work of Maxwell, Boltzmann and others, what is called thermodynamics became absorbed into mechanics or statistical mechanics, that here you explain certain phenomena or certain laws of phenomena which are explored in one branch of a discipline in terms of theories which have been originally introduced and exploited in connection with a quite different domain of investigation. And now what I would like to say is that whenever this happens, many people feel very uncomfortable—many people feel very uncomfortable because it appears to them, and I say many people including scientists, that somehow the material which, so to speak, has been sort of reduced to this more fundamental and pervasive theory; that somehow

[10] See Northrop (1963, pp. 16ff.).

it has lost its footing in the universe or in the nature of things. And that in particular, that when we somehow give an explanation for the familiar world around us in terms of the refined and abstract concepts of modern physics, that the distinctions we make in connection with the familiar world are really not fundamental distinctions, and that they have been somehow wiped out of existence by the explanation that is given.

Now just to indicate that I am not inventing this problem, let me offer a few quotations from the most recent book, at least as far as I know, of Professor Margenau, a book which came out, I think, in—maybe—January, February—or March of this year, a book called OPEN VISTAS.[11] In this book, among other things, he tries to show that modern physics is no longer materialistic. He tries, also, to show that modern physics, as contrasted with classical physics, somehow is more favorable to democracy.

I should like to consider only one part of Prof. Margenau's claim, namely, that modern physics has shown that this bugbear of materialism, which loomed over the imagination of many nineteenth century thinkers, has somehow been eliminated by modern physics. And so Professor Margenau says, among other things, he says:

> "Modern physics has come to recognize the reality of empty space and along with it, the possibility of an influence pervading space, an influence which is not material."[12]

Then later on, he says:

> "Is everything that exists material? A simple but perfectly proper answer would be: matter is no longer material in spite of the literal contradiction in terms. But since the ultimate constituents of matter have resolved themselves pretty much into mathematical singularities haunting space, materialism is no longer the comfortable doctrine it used to be, and one may dismiss it as having lost its major point."[13]

Then, several pages after this, he says:

> [[The]] electron cannot be said to have a determinate position at every [[instant]] of time. [[...]] Widely [[espoused]] by scientists in the early decades of the century has now been sublimated into the current concept of complementarity. This term was introduced by Niels Bohr; its use animates his writings for it regards complementarity, the need for dual types of descriptions of human experience, as inevitable, as grounded in the nature of things, and the limitation of man's understanding. The appeal is to the deeper concerns of our being, somewhat reminiscent of a modern trend in theology which suggests that the knowledge of divinity is possible only through myths, allegories and paradoxes.[14]

And now one final quotation, if you will permit me, in order to indicate how significant current physics is in changing our whole conception of the place of man in nature. Margenau has the following to say:

> Consider [[... the]] neutron, which is on its way to a uranium bomb. *If classical physics were true*, a single set of observations on a position and velocity of the neutron at a suitable time

[11] See Margenau (1961).

[12] Margenau (1961, pp. 114–115).

[13] Margenau (1961, pp. 126–127).

[14] Margenau (1961, pp. 161–162). Double brackets are our corrections to Nagel's quotations.

> would decide whether an explosion occurs. It would leave room for action only to the very limited extent that if the neutron is found headed for the bomb, we can try to intercept and deflect it before the impact.[15]

But then he says: Well, of course, it moves so fast that before we can move, there is not much we could[16] about it. So, we couldn't in effect prevent the explosion. But he says "according to classical mechanics, the very decision to intercept the neutron must be taken as a physico-logical determined consequence of all pervading physical realities.[17] Our very decision is determined by all sorts of antecedent conditions.[18] Therefore, decision as such is an impossibility on the basis of classical mechanics. The new physics leaves greater room for action and avoids this difficulty. Even if a set of observations reveal[[s]] the neutron to be headed for the bomb we can still hope and act for our survival, because what is now dynamically determined is a probability of collision, not a necessity. There is less cause for fatalism but accentuated need for action. [[…]] According to the new philosophy of nature,[19] man has been transformed from a spectator to an active participant in a drama of becoming. Room has been made for decision and choice which had no place in the older scheme of things."[20]

Now, this is all very nice but the curious thing is that Professor Margenau himself, also says, and rather disarmingly, having said all these things, what these important consequences are—and how different we can really now conceive our position in nature and the surroundings around us because we have now shown that classical mechanics somehow is not adequate; then, he says:

> Although statistical reasoning is now recognized as the last resort in quantum mechanics, practically, very little has been changed in the visible world around us, for it happens that the probabilities of quantum mechanics, when calculated for the large and heavy objects of our common experience, congeal to certainties, much as they would for a die so biased that it must always fall one way.[21]

So, the point I would like then to discuss in the few minutes I have left is this contrast which is so apparent in Margenau and, of course, it is apparent in a great

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[15] Margenau (1961, pp. 199–200); emphasis in the original, Nagel did not reproduce it.

[16] A word seems to be missing here from the typescript.

[17] Nagel's talk contained the word "reality" but was crossed out and corrected to "realities"; in fact, Margenau's book says "reality", and continues with "whose historicity can be but an illusion" (1961, p. 200).

[18] Margenau's text does not contain the sentence, "Our very decision is determined by all sorts of antecedent conditions." The last two words are pencil additions to Nagel's typescript. "Therefore" is also Nagel's addition.

[19] Margenau's text says, "Along with these developments".

[20] Margenau (1961, pp. 200–202).

[21] This is a quite free quote from Margenau. In *Open Vistas* (1961, pp. 167–168) we read the following: "[S]tatistical reasoning, long regarded as appropriate and necessary in the social sciences, has now been recognized as the last resort for physics as well. In saying this we are speaking about fundamental matters of principle. Practically very little has been changed in the visible world around us. For it happens that the probabilities of quantum mechanics, when calculated for the large and heavy objects of our common experience, congeal to certainties, much as they would for a die so biased that it must always fall one way.".

number of other writers who insist on the one hand on the tremendously great changes in our conceptions of the world which modern physics has contributed, who then say, well, now, what quantum physics has discovered are the fundamental realities; if these are the fundamental realities, then the ordinary way of looking upon various things in our environment—this is, at least partly, in illusion and the distinctions that it is customary to make cannot really be taken as being fundamental or basic.

Those of us who have lived long enough may recall the great vogue that some of the late Sir Arthur Eddington's popular writings have had, and the very brilliant image with which he starts his book on The Nature of Physical World[22] and of the setting down of two tables. One is the common, ordinary table and then there is the scientific table. The common, ordinary table is solid and substantial and so on. The scientific table, well, it is chiefly nothing. I mean, it is made of a lot of particles but then chiefly separated by an empty space and so on; and so he contrasts them. And then he asks, which is the real table?

Now, the point I want to make is that this contrast is really the result of a mistaken analysis; and that if it is a mistaken analysis, the contrast should never be made and more specifically, the supposition now that human freedom is possible, granted that quantum mechanics is true but it is not possible if you suppose that classical mechanics is true; that this again is simply a consequence of making a contrast where none should be made. And why should none be made? Well, for the reason for which I have essentially done nothing more than lay the groundwork; namely, the theoretical concepts which operate in a theory, which are present in a theory, these, by themselves—if you just have the postulates of the theory which asserts various kinds of connections between the terms that occur in a theory, nothing can be done with that theory until you introduce what I call rules of correspondence. Rules of correspondence between what? Well, certainly between expressions that one calls theoretical and which are unfamiliar and which are not applicable to experimental situations per se; that is, there are no experimental situations to which you can say: well, now this is an atom, or now I am measuring overtly the velocity of an atom. What you have to do is to establish rules which connect expressions like molecule—velocity of molecule—field strength—or what not, with certain terms that refer to gross macroscopic situations, and that the theory itself makes no sense until such rules of correspondence, that is—makes no sense as a viable scientific theory in terms of which you can explain phenomena. Explain phenomena, namely those on which you make observations but that nothing can be done with the theory apart from the rules of correspondence. Clearly, if such rules of correspondence are essential to operate the theory, one never can, without self-stultification then turn around and say: if I accept the theory, then I really must be skeptical as to whether the terms between which I make the correlations represent something that is a genuine part of nature. In particular, those characteristics—those bodies—which we recognize in ordinary, common sense experience; that is, the contrast can't be made because in

[22] See Eddington (1928). Nagel reviewed Eddington's *The Nature of the Physical World* in 1930 and his *The Philosophy of Physical Science* in 1940; they were jointly republished in 1954 as "Eddington's Philosophy of Physical Science," see Nagel (1954).

order to show the relevance of the theory to anything that goes on in the laboratory or the field, rules of correspondence must be introduced which take for granted the very thing which presumably now, after you have gotten the theory you exhibit a certain amount of skeptical doubt. Or, put it in a somewhat different way, those who raise a problem in the way in which I think Professor Margenau raises it, and which a great number of other people raise it, are, in effect, failing to observe that the language of theory is the language of theory. It is not the language of the laboratory. If you want to identify the language of the laboratory as something which deals with macroscopic objects, macroscopic processes, things which, in an obvious sense can be said to be observed, although to be sure, the division between what can be observed and what is not is murky, quite clearly we may make gross distinctions; and the mere fact that you cannot draw a sharp distinction between what is observed and what is not observed no more wipes out the distinction between what is observed and observable and what is not, then is the fact that there is no sharp line which separates what we would call the front of my head from the back of my head. I don't quite know where to draw the line but it doesn't follow that there is no difference between the front and the back, and similarly in this domain.

Now, what I am suggesting then is that the error consists in—I am really saying the same thing but putting it in slightly other words, in order to try to convey my point: the language of theory is not the language of the laboratory and the mistake—or the language of ordinary life—and the mistake consists in supposing that you can use the language of theory in applying it to the things for which the language of the laboratory is the appropriate language.

Now, you have to have both languages. Theories cannot function if you didn't have theoretical terms; that is, you wouldn't have what we call theories if they did not employ terms which were remote from experience, which dealt with objects that are in an obvious sense unobservable, and whose status might be uncertain. The whole point about offering an analysis of the structure of explanations is to clarify difficulties of this kind and not to be caught in the trap of on the one hand, recognize that you have to make distinctions; on the other hand, using those very distinctions in order to undercut the distinctions themselves. Or, if I may make one final application of Professor Margenau's comments upon this problem of freedom, Professor Margenau claims that well, in a universe in which the laws of classical mechanics would be true, there would be no decision, but that in the universe where the quantum mechanical laws are operative, there is decision. I think the only correct thing to say to that is that it is nonsense; and why is it nonsense? Well, for the following reasons: no matter whether quantum mechanics is true or classical mechanics is true, Lord knows what the final answer of physics is going to be about the ultimate constitution of the human body. I don't suppose any of us has any reasonable doubt that in 200 years, the type of explanatory theory is going to be very different from one which is fashionable today. Perhaps there is going to be a return to classical mechanics. Who knows? Certainly the return is not going to be in quite the way in which it was abandoned but something like that. Some kind of a deterministic theory might become the fashion once again, as Einstein believed it would, as Planck does and as De Broglie still continues to believe and so on. But what my point would be—that we distinguish

between acts—human acts that we call free—from acts what we call, say that they are not free. I take it everybody would say that a person who is bound hand and foot has not freedom in an obvious sense. A person who is under various kinds of economic or physical pressure, he hasn't got freedom. A person who is ill; again, say he has some dimension of freedom denied him. But in an ordinary sense, say we do recognize choices that we regard as being our own from choices that are not our own. We didn't really choose this; we were hypnotized; we assented only under duress; that is, no matter what the fundamental explanation is for the mechanism which carries on this activity that we call choice, we distinguish between acts that are the outcome of deliberate choice and acts which are not. I can move my fingers at will; I cannot wiggle my ears. What the ultimate explanation for this is, I don't know and perhaps nobody knows the whole story. But the real point, however, is that things are as they are. There are differences between acts that we call free, acts that are not. If it should turn out that the fundamental physical theory—the deterministic theory—that difference will still be present and the only thing that we will have to do is to adopt the language of free and unfree to whatever might be the fundamental, physical assumption.

And so, I conclude that one of the consequences—or one of the results of engaging in an analysis of the structure of science, particularly when the analysis is directed to considering the way in which scientific explanations are organized and the way in which scientific explanations are rated to others; and the way in which the history of science—one finds the elimination or the reduction of one type of explanation to another, is that throughout all of this, there is something that remains permanent; that is certain logical relations hold. And when one talks about 'the structure of science', obviously one must be talking about something which is structural; which isn't something which is true now and it doesn't appear there. It has a kind of an invariance over time in the history of science and that in particular, in connection with this particular problem to which I addressed myself so very largely this evening; that by recognizing that a theory and a theoretical explanation involves certain indispensible components, certain to be sure perhaps logically elementary but pervasive, and I think from the point of view of our general culture, very serious confusions can perhaps be eliminated.

14.3 Discussion

Question by Prof. Maxwell Primmack of Lincoln University

Professor Nagel, we know in principle that there can be no direct observation of the properties of theoretical entities; e.g. the velocity of a molecule. And if we do not know this in principle, then how does this effect your argument concerning the separateness of the language of theory and the language of the laboratory.

Ernest Nagel

Now, of course, it is quite clear that the answer would have to depend upon how one—sort of theoretically—conceives of the velocity of a molecule. Suppose, for example, one were to say, well, now, you can take pictures of molecules through an electron microscope and would one call this an observation or a seeing of the velocity of a molecule. Of course, here, I think the question would be what sort of things are you prepared to count as having observed, observed in a sense in which one would say, you observe the height to which a column of mercury rises in a thermometer and so on. Would one be prepared to say this is a case of observation? Would one be prepared to say that you have observed—that you have counted the number of molecules in a liter of a monatomic gas under standard conditions of pressure and temperature? If, on the basis of various kinds of experiments, you then calculate the value of Avogadro's number, is this a case of counting? That is, I think I will be able to answer the question only after having come to some agreement as to how these terms are being employed. But this sounds a bit like a dodge, and so I had rather not proceed in that way. Let me put it more unambiguously, that in some cases, there might be some doubt as to whether something that is represented by a theoretical term is observable. And one might say, perhaps, it becomes observable. In other cases, I think one would say—in principle one would say—this is not observable. As, for example, if it should turn out that the closer the way in which certain theoretical notions are defined and given certain—let's suppose—experimentally or observationally determined limits of human perception, that those things couldn't be seen. That is, if—take perhaps an absurd illustration. Could one see one wave length? And I think here I would not be risking very much if I were to say, well, this is, in principle, impossible. Similarly for terms like an electron; could one see an electron? And I think again I would be able—I would feel fairly confident to say this, in principle, is impossible by an analysis of how an electron is defined and the kind of rules of correspondence that are used in translating the theory—not in translating—in coordinating the theory with matters of observation. So I think, to be quite precise, that what I would have to say is those terms that occur in theories which, on the basis of procedures such as that I have indicated, would be shown to be in principle incapable of being observed, those terms cannot be used for matters that are subjects of observation. That, I think would be a more careful reformulation of what I actually said.

Question by Dr. Baumrin

On this same subject, what about Hoyle's hypothesis of the existence of bodies, beyond the bounds of the observable universe, travelling faster than the speed of light? Which hypothesis is deduced from an observed phenomenon, namely the red shift. Here we have something which is in principle unobservable and yet something which requires some to talk about unobservable entities whose existence is to be inferred from an account which is an explanation of an observed phenomenon.

Ernest Nagel

Well, I am not sure you know against what—maybe this is a wrong assumption and that your question is not directed against something—maybe this is in connection with this notion of a theoretical. Let me say—first of all, let me see if I can first reformulate the question. What do I make of Hoyle's view that some of the galaxies are receding with speeds that are greater than light and ultimately vanish, at least in the sense that they are no longer capable of being seen through the telescope, and the supposition that they continue. And perhaps; I don't know what the proper formulation would be. Do they continue to exist but no longer are observed, or do they simply disappear from existence? I am not quite sure what would be the correct formulation. But then, the further point is that, well, now—look: this result is simply the—I mean—Hoyle's conclusions are something he arrives at because he is driven to this by data of observation, namely, certain—I mean—the red shift in the spectrum of some of the galaxies. Well, of course, one would have to certainly say it isn't just the red shift itself which leads to this conclusion, but the assumption that the red shift itself is to be understood in terms of the Doppler principle; and if the Doppler principle can be extended beyond the areas in which we have made experimental determinations. Now, I talk here certainly only as an interested layman; a question is whether the only explanation for the red shift is in terms of Doppler's effect. Certainly, it is a debatable one and I understand it continues to be debated, so that alternative interpretations can be placed on this and while I take it the tendency is to use that interpretation, there have been people who refused to go that way. What I am not entirely sure about, Dr. Baumrin, is what the implication of this is for my attempt to distinguish between the language of theory or for my emphasizing the importance of the language of theory and the language of sorts of observation. I mean, clearly the theoretical terms that are introduced in any theory are often suggested by matters of observation. I mean, even take the notion of a point-mass, or the notion of an instantaneous velocity, or the notion of a wave packet; these—even the language we use—indicate that these theoretical notions have been suggested by things that we have observed but it seems to me that there is a tremendous difference between the cash value of the theoretical term, if I may be permitted to use Jamesian language here—the cash value of theoretical terms and the cash value of the observation terms. That is, what you can do with the observation term is something very different from what you can do with the theoretical term, and though psychologically I might find the theoretical term almost inevitable, this might be nothing more than a testimony as to how I happen to think. I mean, other people think differently. To use my previous example of Rhine; Rhine's theoretical notion is extra-sensory perception. The notion of Professor Fellar at Princeton,[23] who is a statistician, is that explanation is in terms of biases in series. Now, you are not driven to either of them in one sense, except in terms of some supplementary assumptions. Or, take one other illustration; take the so-called Freudian theory to account for slips of tongue. Well, on the Freudian theory, you know, you have the complicated mechanism of the conscious, the unconscious.

[23] William Srecko Fellar (1906–1970) was a Croatian-born American statistician who worked on probability as well.

Freud, for some reason, found this is a congenial way of accounting for behavior of humans—certain aspects of the behavior of human beings. Other writers were not driven to this sort of hydrostatic mechanism that Freud invented by analogy from water pressure, you know, but have a more physiological mechanism to account for this. But the theoretical terms are certainly not determined by observation and I would have supposed now that this is fairly well established; that two things are, I suppose, fairly well established. If I may mention or quote an authority here in support, I mean—I think Einstein was perfectly right in saying that theories are free creations of the human mind[24]; that they are not obtained simply by sort of grubbing in the facts and somehow abstracting from the factual details the theoretical notions; that is, I would have supposed now that first of all, that this is so. And secondly, I would argue that the theoretical terms cannot, at least as far as I know, be defined in terms of observation expressions. That is, I do not know how to define, literally define, in any usual sense of definition a term like molecule in terms of expressions which deal with matters of observation.

One of the things that I had supposed that time would always be long enough to talk about is a different interpretation of theory and you see, one of them has a long tradition behind it. 'Well, theoretical terms and the sort of thing that I have called theories, they really are dispensable; they are kind of a luxury that we grant ourselves in order to facilitate certain kinds of jobs—make certain kinds of jobs easier, but that theoretical notions can be eliminated because all theoretical notions can be defined in terms of observation terms.' Now, this is a point of view that has been advanced and advocated by—repeatedly—in the history of thought and by extremely clever and brilliant people and they tried their hand at defining, as far as I know here as in the case of trying to define rules of discovery, the record is that of uniform failure. No—but no one has yet given a definition of any theoretical terms in observational matters. Always a promise—a pie in the sky—a hope and a prayer, but never the goods delivered. Now, I don't know of any theoretical proof that it is impossible to do this, but I think the presumption is that it can't be done. And there are not unreasonable arguments of another kind, not simply that well, nobody has done it; therefore, it is impossible. But other kinds of considerations which would lead one –[25] Now, I am assuming that this is one of the bedrocks, at least it is one of the bedrocks upon which my own discussion rests; if that can be shown to be really just sand, then I am afraid I am lost, but I hope it cannot be shown.

Question by Mr. Gray

You have talked about rules of correspondence and I wonder if there are rules for defining what are proper rules of correspondence. Would it be fair to say that a rule of correspondence would have to be operationally meaningful in Bridgman's terms?

[24] See, for example, Einstein (1921/1954).

[25] Break to change tapes.

Ernest Nagel

The question is that I have used the phrase rules of correspondence. What are the requirements for a rule of correspondence? Are there, so to speak, rules for indicating what is an appropriate rule of correspondence and more specifically, must rules of correspondence be the kind of rules which Bridgman called operational definitions?

I think the answer is a little bit complicated, depending upon the state of the field that you are dealing with. Let me put it this way. Sometimes what you do, in order to make a theory relevant to some domain of inquiry, where the terms of the theory now presumably have this characteristic that I have indicated—what you do is to establish rules of correspondence between terms of that theory and terms of another theory. In this case, the rules of correspondence would not be between theoretical terms and experimental terms, or between things that could be operationally defined, but would be between theoretical terms and other theoretical terms where presumably those theoretical terms eventually are tied down to something else. So, as a general answer to the question, I would say that rules of correspondence are not in general of the sort that Bridgman called operational definitions. I think in some cases, they are; on the other hand, my own hesitation in this connection is I am not entirely sure what the scope of an operation is in Bridgman's sense, because you remember that although in his first book, The Logic of Modern Physics,[26] he insisted upon the fact that all concepts in physics, if you are not going to get into trouble must be operationally defined and the concept is synonymous with the operation. And then later on, when it was pointed out to him, but, look here, Professor Bridgman, how do you operationally define molecule or what not. He says, well, you have to remember that there are two types of operations; I mean, the kind of operation that one calls the overt laboratory operation and the kind of operation that you call pen and paper operations—or mental operations—and then everything becomes operational and then the term no longer has a distinctive reference. But you see, I want to leave this question somewhat open—whether rules of correspondence would conform to what Bridgman intended by what he called overt operations, I am not entirely sure—even in those cases where you can't have some sort of a connection, because it seems to me, in different areas, the thing is so extremely different. Look, let me indicate why I hesitate. Take terms which you use in geometry like point. Now, in some cases, of course, we say: Well, point what is that? The term point is something that has position but no dimension. How can you use this theory you call geometry in order to do physics with, or do carpentry with. And in some cases, you know, you say: well, I have a heap of chalk, a small heap of chalk, and although it isn't quite a geometrical point, it is something you could call a point—well, in this case—one could say that this is kind of an operational definition. In other cases, you say: Well, my point is going to be a planet. And now, how about a planet; I mean, is a planet an operationally defined concept? Frankly, I don't quite know because in part it depends upon a theory. In other cases, I take it you will take an entire galaxy as a point. Is a galaxy an operational concept—an operationally defined concept? Certainly it is

[26] See Bridgman (1927).

not entirely operationally defined. I mean, you have all sorts of really theoretical assumptions built into the notion of a galaxy. It makes me hesitate in saying that all of these definitions, even those which are not of the kind which correlate one theoretical concept to another, would have to conform to Bridgman's operational criteria.

Unknown questioner

Do you subscribe to Born's view that laboratory observational apparatus constitute extension of our senses, and objects so observed are just as much directly sensed objects as those observed with the unaided senses?

Ernest Nagel

Well, certainly, I mean in general, I would certainly entirely subscribed to that—that to suppose now that so to speak—the unaided senses bound the limits of what is real, seems to me an extreme parochialism; that nobody who has ever had a sense of liberation, even from reading science, could fail to recognize that there are certainly much more things for which we have excellent evidence to suppose they are real than we would, suppose we were to fall back upon the kind of naive I'm a man-from-Missouri sort of attitude. I thought that perhaps you were going to ask another question about Born's views which is again a theme I would like very much to talk about. But let me just mention what it is and then say why I find it interesting.

In the preface to a collection of many of his essays, some of them sort of semi-popular, which has the title My Years in Physics or Fifty Years in Physics[27]—I forgot now the precise title—he has a preface which was to me, when I read it, a surprising and in a way a dismaying experience. He said: When he started out as a young man, he turned to physics because he thought this was a way of achieving objective knowledge; that he turned away from metaphysics, from art, from poetry. These were wonderful things, to be sure, but they didn't give him objectivity. And now, at the end of his life, he realizes that he was profoundly mistaken; that physics does not yield objectivity and at any rate, it is no more objective than are art and literature and metaphysics, and what not. And it is sort of—I say it was sort of dismaying, an almost tragic tone in this. A man spent so many energetic years in advancing science in the hope that it will finally put a little salt on the tail of reality and it all turns to nothing, according to him.

And I think that this is a disease now from which many physicists suffer. There is kind of a failure of nerve almost about this; that somehow the fact that in modern quantum physics, we have to introduce some sort of so-called statistical assumptions or indeterminacy assumptions, that this somehow cuts the ground from under the claim that one has objective knowledge. Of course, this is also associated with this claim that you cannot in principle really make a sharp difference between subject

[27] Nagel has in mind, perhaps, Max Born's *Physics in my Generation: A Selection of Papers*, see Born (1956). Cf. page vii of the "Preface". Born's volume, which is a collection of his previously published essays, contains an article, titled, "Physics in the Last Fifty Years," see Born (1956, pp. 109–122).

and object. This is another area, it seems to me, of confusions of which scientists like laymen are extremely prone. As a matter of fact, if I may end on this bit of a sour note. I have been sort of impressed by the claims made by a great number of very distinguished scientists, certainly whose scientific achievements I have nothing but profound respect for, that things in politics would be very much different if only scientists got into that game. And you recall, Sir Charles Snow, in his book Science and Government,[28] argues for the importance of having scientists active in government, not only there to answer questions when they are asked a technical question, but that they should really be in the position to ask the questions, and not simply to answer them when some professional politicians raise them; that they themselves become, so to speak, sort of professional politicians. And this sounds awfully, awfully good except for the assumption that seems to be so—so prevalent, at least among some of my scientific friends that well, sure, you have to be a great expert in doing physics but you don't have to be an expert in doing politics; that you don't need a know-how in doing that. And that a scientist, if he is a good scientist, can bring a good deal of wisdom to public affairs. I certainly wouldn't deny that some scientists might, but I frankly doubt very much whether in matters of political wisdom, anymore than in matters of philosophical wisdom; that is, in matters where it is a question of having a training and a patience to analyze ideas which don't deal specifically with technical problems in a science, that scientists, without suitable training, are particularly equipped to legislate or pontificate. That's the sour note.

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[28] See Snow (1961a).

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