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Ernest Nagel: Philosophy of Science and the Fight for Clarity

Logic, Epistemology, and the Unity of Science

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Editors

Ernest Nagel: Philosophy of Science and the Fight for Clarity

 Springer

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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 Federico 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 re-issued 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 work¹¹ 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-like¹⁸ 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 Sommerville gave his account: well, since Professor Sommerville 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

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).