universität wien

DISSERTATION | DOCTORAL THESIS

Titel | Title

Kuhn's Harvard and the Resolution of a Historicist Scientific Era in The Structure of Scientific Revolutions

verfasst von | submitted by

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angestrebter akademischer Grad | in partial fulfilment of the requirements for the degree of

Doktor der Philosophie (Dr.phil.)

Wien | Vienna, 2025

Studienkennzahl lt. Studienblatt | Degree programme code as it appears on the student record sheet:	UA 792 296
Dissertationsgebiet lt. Studienblatt | Field of study as it appears on the student record sheet:	Philosophie
Betreut von | Supervisor:	Mag. Dr. Christian Damböck Privatdoz. | Univ.-Prof. i.R. Mag. Dr. Friedrich Stadler

Contents

"In the history of ideas, the resulting tradition is the one which produced both Ernst Cassirer and Arthur Lovejoy, men whose work, however profound its limitations, has had a great and fructifying influence on the subsequent treatment of ideas in history."

Kuhn 1971 / *Essential Tension* 1977

Abstract (English)

This dissertation examines the genesis of Thomas S. Kuhn's *The Structure of Scientific Revolutions* through the lens of historicist scientific thinking, challenging prevailing interpretations by scholars who primarily associate Kuhn's work with Kantian or neo-Kantian philosophy. I argue that Kuhn's historicism developed within Harvard's unique intellectual environment, which synthesized European and American traditions in ways that shaped his historical and philosophical outlook.

In Part I ("The Student Years"), I demonstrate how Kuhn's education unfolded amidst a diverse philosophical landscape. While Kantian-inspired Harvard pragmatism and its logical extensions—through figures such as Sheffer, Russell, Whitehead, and Langer—formed part of Harvard's atmosphere, I trace a more direct lineage of Machian historicism transmitted through George Sarton and James B. Conant to Kuhn. Early reviewers of *Structure*, such as Marie Boas Hall, recognized these influences, particularly the traces of Comte and Sarton. Philipp Frank's "Unity of Science" program further reinforced Kuhn's engagement with historicist themes.

Beyond pragmatism, I highlight the decisive impact of Alexandre Koyré's historicism and A.O. Lovejoy's rigorous intellectual history. Lovejoy's persistent critique of Russellian neo-realism and his influence on Kuhn's historical method and broader philosophical sensibility are underscored by archival evidence, including Kuhn's correspondence with Lawrence S. Kubie.

The dissertation further postulates that there is a family resemblance between Harvard critical realism and Kuhn's conceptual thought and development. In transcending Matthias Neuber's insight into neo-Kantian origins, I argue that American critical realists had substantially moved beyond Kantian foundations—a point underscored by Roy Wood Sellars' definitive rejection of Kantianism in favor of physical realism.

In Part II ("The Teaching Years and the Development of *Structure*"), I trace Kuhn's intellectual maturation, emphasizing his engagement with Piaget's developmental psychology, Ludwik Fleck's concept of "textbook science," and the historicist methodologies of Meyerson, Brunschvicg, and Koyré. Archival sources reveal that Kuhn, early in his career, was familiar with both the early and later works of Cassirer. Although Cassirer's neo-Kantianism contributed to Harvard's milieu, often indirectly, non-Kantian figures—particularly Koyré—proved crucial in shaping Kuhn's approach to the history of science.

Challenging Michael Friedman's Kantian reconstruction of Kuhn, I argue that Kuhn's intellectual formation reflects a broader and more eclectic constellation of influences than is commonly recognized. The evidence suggests that *Structure* arose from a historically grounded, interdisciplinary tradition of scientific historicism that predated and transcended his later philosophical affinities.

Ultimately, this dissertation presents *The Structure of Scientific Revolutions* as the culminating expression of a deep, transatlantic tradition of scientific historicism, synthesized within the vibrant intellectual context of mid-twentieth-century Harvard.

Abstract (German)

Diese Dissertation untersucht die Entstehung von Thomas S. Kuhns *The Structure of Scientific Revolutions* durch die Brille des historistischen wissenschaftlichen Denkens und stellt damit vorherrschende Interpretationen in Frage, die Kuhns Werk primär mit kantianischer oder neo-kantianischer Philosophie assoziieren. Ich argumentiere, dass Kuhns Historismus in Harvards einzigartigem intellektuellem Umfeld entstanden ist, welches europäische und amerikanische Traditionen in einer Weise synthetisierte, die seinen historischen und philosophischen Blick prägte.

Im ersten Teil („Die Studentenjahre") zeige ich, wie sich Kuhns Ausbildung inmitten einer vielfältigen philosophischen Landschaft entfaltete. Während der von Kant inspirierten harvardianische Pragmatismus und dessen logische Ableitungen – vermittelt durch Persönlichkeiten wie Sheffer, Russell, Whitehead und Langer – Teil der Atmosphäre an Harvard waren, verfolge ich eine direktere Ahnenreihe des machschen Historismus, die durch George Sarton und James B. Conant an Kuhn weitergegeben wurde. Frühzeitige Rezensenten von *Structure*, wie Marie Boas Hall, erkannten diese Einflüsse, insbesondere die Spuren von Comte und Sarton. Philipp Franks „Unity of Science"-Programm verstärkte zudem Kuhns Auseinandersetzung mit historistischen Themen.

Über den Pragmatismus hinaus hebe ich den entscheidenden Einfluss von Alexandre Koyrés Historismus und A.O. Lovejoys strenger intellektueller Geschichtsschreibung hervor. Lovejoys unablässige Kritik am russellianischen Neorealismus und sein Einfluss auf Kuhns historische Methode sowie seine breitere philosophische Sensibilität werden durch Archivbelege, unter anderem Kuhns Korrespondenz mit Lawrence S. Kubie, untermauert.

Die Dissertation postuliert ferner, dass es eine Familienähnlichkeit zwischen dem harvardianischen kritischen Realismus und Kuhns konzeptioneller Entwicklung gibt. Über Matthias Neubers Erkenntnis der neo-kantianischen Ursprünge hinaus argumentiere ich, dass amerikanische kritische Realisten die kantianischen Grundlagen weitgehend hinter sich gelassen hatten – ein Punkt, der durch Roy Wood Sellars' entschiedene Ablehnung des Kantianismus zugunsten des physikalischen Realismus bekräftigt wird.

Im zweiten Teil („Die Lehrjahre und die Entwicklung von *Structure*") verfolge ich Kuhns intellektuelle Reifung und betone dabei seine Auseinandersetzung mit Piagets Entwicklungspsychologie, Ludwik Flecks Konzept der „Lehrbuchwissenschaft" sowie die historistischen Methodologien von Meyerson, Brunschvicg und Koyré. Archivquellen belegen, dass Kuhn schon früh in seiner Karriere sowohl mit den frühen als auch mit den späteren Werken Cassirers vertraut war. Obwohl Cassirers Neo-Kantianismus indirekt zum Milieu Harvards beitrug, erwiesen sich nicht-kantianische Persönlichkeiten – insbesondere Koyré – als entscheidend für die Prägung von Kuhns Zugang zur Wissenschaftsgeschichte.

Indem ich Michael Friedmans kantianische Rekonstruktion Kuhns herausfordere, argumentiere ich, dass Kuhns intellektuelle Ausbildung ein breiteres und vielfältigeres Netzwerk von Einflüssen widerspiegelt, als allgemein anerkannt wird. Die Belege deuten darauf hin, dass *Structure* aus einer historisch fundierten, interdisziplinären Tradition des wissenschaftlichen Historismus hervorging, die seinen späteren philosophischen Neigungen vorausging und diese zugleich transzendierte.

Letztlich präsentiert diese Dissertation *The Structure of Scientific Revolutions* als den Höhepunkt einer tief verwurzelten, transatlantischen Tradition des wissenschaftlichen Historismus, die im lebendigen intellektuellen Kontext des Harvard der Mitte des zwanzigsten Jahrhunderts synthetisiert wurde.

Preface

My MA thesis in the history and philosophy of science, supervised by Richard Dawid—a physicist and philosopher of science—explored whether String Theory represents a revolution in the Kuhnian sense of scientific revolutions. After completing the thesis, I continued to delve into Kuhn's intellectual world. The natural next step was to trace Kuhn's ideas back to their origins, uncovering how *The Structure of Scientific Revolutions* itself was conceived.

After extensive preliminary discussions over many months with my dissertation advisor, Christian Damböck, I traveled to Cambridge to examine Thomas Kuhn's archival materials housed in the Distinctive Collections at MIT. My goal was to shed light on Kuhn's early Harvard education and his philosophical convictions. Specifically, I sought to investigate the foundations upon which *The Structure of Scientific Revolutions* was built and to critically examine claims about whether Kuhn harbored Kantian sympathies or whether *Structure* could be said to rest on a neo-Kantian framework. For example, Peter Lipton famously described Kuhn as "Kant on Wheels," while other scholars such as Gerd Buchdahl, Paul Hoyningen-Huene, and Michael Friedman have interpreted Kuhn's work through a Kantian lens. However, many of these interpretations are based on Kuhn's later writings, post-*Structure* and its 1972 postscript.

My aim was to resolve this scholarly debate by focusing on Kuhn's archival sources from the period before *Structure*. I intended to scrutinize the very building blocks of the book itself. This endeavor proved rewarding: it became evident that *Structure* did not emerge in a vacuum and that the "Kantian question" was only tangentially relevant to its genesis. Access to Kuhn's original and unpublished materials led to an unexpected discovery — it was not Kantian philosophy per se that influenced Kuhn most profoundly, but rather aspects of Harvard Critical Realism and the thought of A. O. Lovejoy. Kuhn not only paid tribute to Lovejoy's work in intellectual history on multiple occasions, but I believe I have demonstrated that he also aligned with key tenets of Lovejoy's philosophy.

Another central theme of this dissertation is the concept of the "Harvard Hub," characterized by the prominent backdrop of Pragmatism. I argue that this hub represented an amalgamation of European and American philosophical traditions, and that Kuhn inherited a

historicist scientific tradition shaped by figures such as Ernst Cassirer, Alexandre Koyré, George Sarton, and Arthur Lovejoy. It is my conviction that *The Structure of Scientific Revolutions* is the culmination of this historicist scientific era and an integral product of the Harvard Hub.

My exploration of the Harvard context also revealed the roots of Kuhn's interest in Jean Piaget's experiments on children. Kuhn's concept of the "fringe of vague meaning" reflects his engagement with what I have termed the "intellectual awareness" of his time, drawing on ideas from Piaget's developmental stages, Gestalt psychology, Michael Polanyi's "tacit knowledge," Ludwig Wittgenstein's notion of the ineffable, and Susanne K. Langer's concept of "insight." Among the unpublished archival materials, a particularly notable discovery was an exam question Kuhn posed to his students, where he criticizes Bertrand Russell's failure to attend to the historical dimensions of scientific methodology — a direct challenge to prevailing neo-realist views.

In terms of methodology, I have taken a different path from George Reisch's important work, *The Politics of Paradigms*, which examines Kuhn's archival sources through the lens of Cold War politics and cultural history. In contrast, my approach centers on the history of philosophy, emphasizing the philosophical figures and intellectual currents in Kuhn's Harvard.

As this dissertation neared completion, Friedrich Stadler reviewed the work and provided valuable feedback. I met Paul Hoyningen-Huene — who had worked closely with Kuhn — at a conference and later interviewed him. I am grateful for the opportunity. With Paul's permission, I have included the transcript at the end of this dissertation. Although the interview may appear modest, it addresses precisely the questions that, in my view, demanded historical clarification and to which my work attempts to respond. I also consulted Matthias Neuber, an expert on Critical Realism, who kindly read the dissertation and responded with highly encouraging comments. I presented a chapter at the Institute Vienna Circle, with a second presentation forthcoming. Additionally, I am scheduled to present parts of this research at SSHAP 2025 (Society for the History of Analytic Philosophy) in Jena this summer.

MNB, April 2025

Introduction

Understanding the Genesis of the *Structure* Along the Continuum of Historicist Thought

George Reisch's *The Politics of Paradigm* has become an addition to a continuing debate among scholars such as Alexander Bird, K. Brad Wray, Hoyningen-Huene and others on Kuhn's place in intellectual history and their "yet-untapped" potential in understanding science and scientific progress (Reisch 2019, xxxi). According to Reisch, Bird's account offers a historical account of Kuhn's ideas, but this accounts is "exclusively intellectual"; they deal "sparingly" with Kuhn's biography, while politics and ideology play no role in Bird's account. (xxxi). Reisch's attempt in his book is to remedy how things stand, with a biographical interpretation of Kuhn's thought under the overarching of cold war politics of the 50s and all-encompassing ideology associated with that period. In the introduction to his book, Reisch refers to Hoyningen-Huene and the discussion of Kuhn's paradigm shift as a "conversion" or *Bekehrung* – somewhat akin to a religious experience – and acknowledges Hoyningen-Huene in his attributing to the so-called "conversion" (see 6.1)[1] as not belonging to Kuhn's theory of science, but as a fixture within Kuhn's heritage (xxxii). Reisch writes that John Earman had once referred to these passages in the *Structure* as "purple passages". Reisch then outlines his own thesis in the book:

> This book sees these passages differently […]. Along with *Structure*'s sustained interest in scientific revolutions—a word that rings with widely recognized political implications—its purple passages point to the struggle for men's minds, itself a festival of dramatic, purple prose about the overwhelming powers of ideology and the reality-changing stakes of cold war politics. As Kuhn's interests in "conversions" might suggest, these roots sometimes extend through cold war fears about brainwashing into more distant and original features of American culture, such as the nation's Puritan devotion to religious conversion and purification (xxxii).

Reisch mentions Joel Isaac and his book *Working Knowledge* that had argued that *Structure* is "best understood reflecting the interdisciplinary intellectual landscape at Harvard at mid-century". Reisch attributes to Harvard's Lawrence J. Henderson a "broad interest in conceptual schemes", which he had acquired through the sociological writings of Vilfredo Pareto

[1] In Kuhn's lecture notes of November 10, 1953, we encounter a different sense of conversion, which for Kuhn boils down to strictly appealing to "matters of taste" (see 4.4 & 6.1). So, in that sense, I have proposed a narrative that differs from both Hoyningen-Huene and George Reisch.

and had founded the "Pareto Circle" in the 1930s. Henderson's work laid the foundation, according to Reisch, for Kuhn to "later build his new image of science" (xxxiii). However, Reisch writes that:

> Kuhn himself pointed out that *Structure* was not unique in pursuing a humanized and historical understanding of science. Writings by the American philosopher Norwood Russell Hanson, the Hungarian chemist-turned-philosopher Michael Polanyi, the French historian Alexandre Koyré, and Conant himself had said often similar things about how science works and helped to fuel and ignite this glorious explosion (xxxvi).

Reisch refers to warnings from public anticommunists about citizens facing:

> […] an existential choice between incompatible "ways of life." If all that was true…then Kuhn's insight seemed all the more fundamental. Scientists, after all, are human beings. So why shouldn't the history of science involve dramatic, ideological conversions and revolutionary transformations like those at stake within the struggle for men's mind? (xxxvi)

On the other hand, Reisch also admits that:

> For Kuhn either never realized or never admitted how much his great book, its purple prose, and its eager reception had to do with the dramatic politics of these years (xxxvii).

In short, Reisch's scholarly thesis is carried out by examining *Structure*'s origins against this political backdrop (xxxiv).

In this dissertation, however, I tread a different path. I am indeed concerned with Kuhn's early biographical history at Harvard and the origin of Kuhn's ideas in the *Structure* to the extent that my approach overlaps with that of Reisch. And even though, I also examine, just as Reisch has done, the context of Kuhn's intellectual development in his early period, this will be done with different emphases, questions, and will be carried out along a different narrative. This narrative is Harvard Hub as a melting pot of individuals, history, tradition, and demands in forming or re-routing the intellectual streams of the '40s and '50s. This focus on Harvard and the minds that passed through it, and in turn, influenced the young Thomas Kuhn, is indeed the context that is being treated here and therefore different than that of Reisch's. Nevertheless, as for the methodology, it takes its initiative from Reisch, namely by sifting through unpublished archival materials at the MIT during the '40s and

'50s right up to the publication of the *Structure*, to argue that Kuhn's thoughts in the *Structure* were the culmination of a historicist thinking about science and philosophy that had its genesis in the philosophical thinking of the '30s and '40s at Harvard, which was a mixture of, and was indeed inspired by, primarily European philosophy. With the help of the archival materials that I collected at MIT in Cambridge in 2023, I intend to show that Kuhn delved into European historicist tradition, but he also assimilated it with the forces present at Harvard (figure 1**).** For now, simply put, the young "Kuhn's Harvard" can be thought of an embodiment of such an historist tradition sprawling through Harvard and encompassing different strands of historical thinking before there is any mention of analytic philosophy as a discipline.

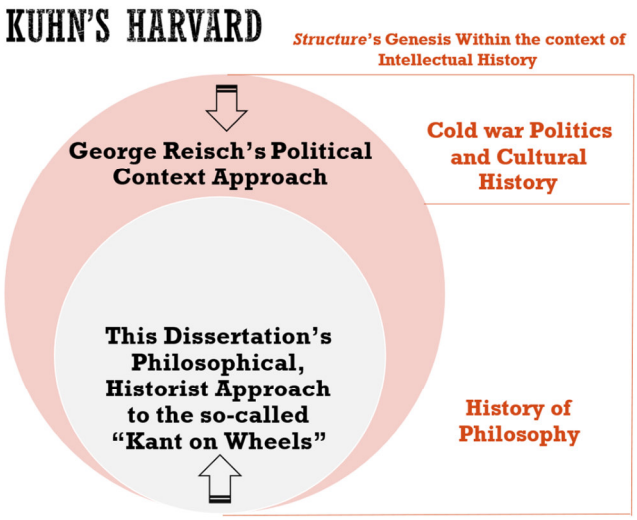


figure 1

Peter Lipton's anecdotal and figurative use of the expression "Kant on Wheels" drew attention to aspects of Kuhn's philosophical position in the *Structure* and its relation to Kant. Lipton writes:

> Scientific theories represent a world, but one partially constituted by the cognitive activities of the scientists themselves. This is not a commonsensical view, but it has a distinguished philosophical pedigree, associated most strongly with Kant. The Kantian view is that the truths we can know are truths about a 'phenomenal' world that is the joint product of the 'things in themselves' and the organising, conceptual activity of the human mind. Kuhn, however, is Kant on wheels. Where Kant held that the human contribution to the phenomenal world is invariant, Kuhn's view is that it

> changes fundamentally across a scientific revolution. This is what he means by his notorious statement that, after a scientific revolution, 'the world changes' (Lipton 2003, 216).

The question is whether we can attach to the figurative notion of "Kant on Wheels", a more inclusive or "global" implication. And the answer may be that in searching for the historicist dimension of *Structure*'s genesis, this may indeed be inevitable. In the final chapter of this dissertation, I shall draw on the rich narrative of Hoyningen-Huene in *Reconstructing Scientific Revolutions*; Thomas Mormann has pointed out that even though Hoyningen-Huene has done without treating Kuhn's philosophy of science in relation to other historical streams, it nevertheless lends itself for such an undertaking:

> Zwar verzichtet auch Hoyningen-Huenes darauf, die Beziehungen der Kuhnschen Wissenschaftsphilosophie zu anderen philosophischen Strömungen, so zum Logischen Empirismus oder zum Neukantianismus zu explizieren. Seine Präsentation des Kuhnschen Oeuvres bietet jedoch eine geeignete Basis für dieses Unterfangen (Mormann 1999, 254).

Mormann's thought is indeed apt since one of the aims of this dissertation is to explain *Structure's* genesis along alternative historical paths (see 2.7).

Michael Friedman's incessant scholarly and consistent contributions to a vision of neo-Kantianism in interpreting Kuhn towards a post-Kuhnian philosophy of science is indeed noteworthy. Perhaps Friedman's early enthusiasm for Kuhn's work, made Friedman himself an integral part of this history. He writes:

> In the years 1969-73 I was a graduate student at Princeton while Kuhn was a professor of philosophy and history of science there […]. I was thrilled (along with everyone else) when Kuhn, following Princeton tradition, served as a teaching assistant for my teacher C. G. Hempel when the latter taught his final course in the Philosophy Department. But I eventually did become seriously interested in both the history of science and the history of philosophy, and I talked with Kuhn about these matters while he was at MIT (Friedman 2012, 77).

Friedman writes that in the '90s, he and others had "called attention to striking similarities between Kuhn's views and those of 'early logical positivists'" (78):

> This historical work highlighted Kantian and neo-Kantian aspects of philosophy of logical positivism, and Kuhn, very late in his career, also

> understood such influences on his own view, characterizing it , appropriately, as a version of "Kantianism with moveable categories […]. (78)

Friedman's narrative of neo-Kantian affinities in Kuhn's work and what Lipton labeled "Kant on Wheels", has primarily been developed systematically. In contrast, this dissertation offers an alternative approach, namely a historical route by highlighting the young Kuhn's Harvard, academic climate and personalities at Harvard, and philosophical problems of his time. This dissertation priorities the genesis of Kuhn's thought prior to the publication of *Structure* or, in short, pre-*Structure* (figure 2).

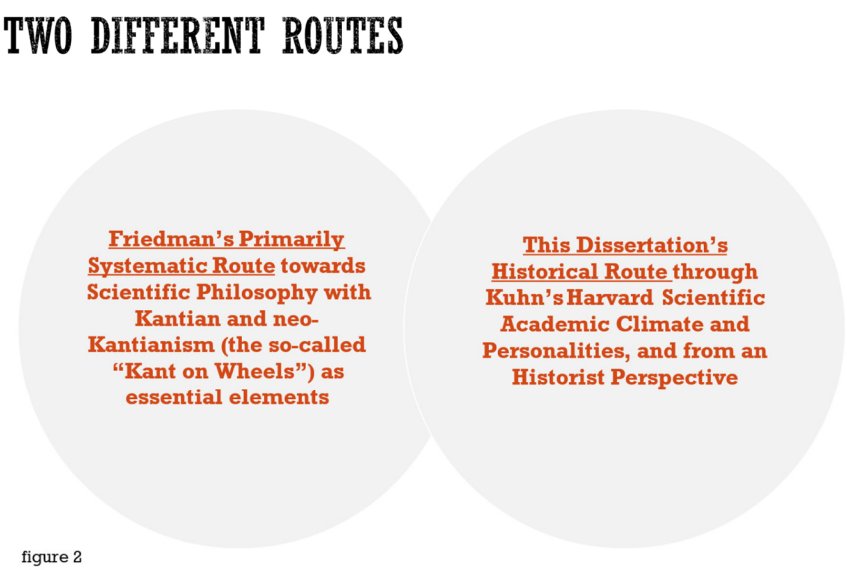


figure 2

To carry out this task I have explored unpublished archival materials, i.e., correspondences both official and private, lecture notes, incomplete memos, and undergraduate papers, covering Kuhn's early period ('40s and '50s), up to the publication of the *Structure,* to throw light on the historicist roots and the makeup of the *genesis* of Kuhn's thought.

In this dissertation, I emphasize the importance of understanding the genesis of *The Structure of Scientific Revolutions* not as the sudden emergence of a radical novelty marked by inconsistencies, but rather as the culmination of ideas deeply intertwined with a continuum of historicist scientific thought. This tradition, which gained momentum through the major scientific discoveries at the turn of the twentieth century, acted as a catalyst, enabling

historicist thinking to solidify across both continental European and Anglo-American philosophical traditions. These strands ultimately converged with a modern—and crucially, science-enthused—*Zeitgeist* that was at once both unsettled by, and drawn to, the vision presented in *Structure*.

I conclude that Kuhn masterfully assimilated what was already latent in the philosophical consciousness of his time. *Structure* thus served not merely to innovate but to elegantly bring to resolution a historicist scientific narrative: one rooted in the intellectual developments following the scientific breakthroughs of the early twentieth century. Among the dominant strands of thought that Kuhn's work synthesized were the philosophies and historiographies of Ernst Cassirer – albeit indirectly – Arthur Lovejoy, and Alexandre Koyré, alongside broader influences such as Piaget's developmental psychology, Fleck's sociology of science, and the rise of evolutionary biology as a new scientific discipline. Equally significant, however, was the parallel influence of Logical Empiricism, which had become increasingly prominent.

By exposing the "thoughts of an era" as foundational to the genesis of *Structure*, I aim to situate Kuhn's work primarily within the continuum of historicist scientific thought, rather than within the neo-Kantian or purely analytic traditions often emphasized in later interpretations.

1

What is Historicism?

1.1 A New Awareness

This chapter demonstrates a historiography in flux, shaped by a new scientific sensibility during the first half of the twentieth century—a development that marked the early stages of a "historical scientific turn." I shall examine the famous Davos debate and its significance in forging a new historicist scientific era. This is followed by an exploration of various forms of historicism, among which I identify a strand encompassing Cassirer's scientific philosophy, as well as that of other figures such as Koyré and Lovejoy, who were advancing historicist scientific ideas. Finally, I raise the question: could the point of departure for Kuhn's 'Structure' be found in the analytic-continental divide? Put differently, did 'Structure' ultimately resolve the philosophical dichotomy that so profoundly shaped Kuhn's generation?

During the genesis of *Structure*, many individuals contributed to aspects of an "awareness" that encompassed, contributed, and integrated historiography and scientific thinking of the time; Ernst Cassirer, Arthur Lovejoy, Alexandre Koyré, Émile Meyerson, Leon Brunschvicg, Ludwik Fleck, Jean Piaget, Ludwig Wittgenstein, Michael Polanyi, Norwood Russell Hanson, and others displayed, in their own unique ways, aspects of an "awareness". Polanyi's 'tacit element', Cassirer's 'neo-Kantian historicism', Meyersonian insights of the 'identities and lawfulness', Koyré's meticulous historiography, Fleck's insights into 'thought style' and 'thought collective', and Hanson's attention to *gestalt* and the puzzles of quantum mechanics, point to an awareness that was the result of scrutiny of novel historical methods that questioned the peculiarity of historical understanding of science among past generations of thinkers. For example, Ernst Mach (1838-1916) "underlined the historical embeddedness of all natural sciences" (Stadler 2018, 65-66). Mach, according to Friedrich Stadler, can be considered "a pioneer of the emergence of the present field History and Philosophy of Science (HPS)" (65-66), and be credited with "paving the way for a "historical turn" in the philosophy of science (67). Thomas Kuhn owned a copy of Mach's *Populär-Wissenschaftliche Vorlesungen* (1923 edition).

As early as 1906, Cassirer had pointed to a "historical turn" – of Platonism initiated by Nicholas of Cusa – which, according to Cassirer, eventually lead to Kepler and Galilei:

> Cusanus bezeichnet hier die *geschichtliche Wendung des Platonismus*, die zu *Kepler* und *Galilei* hinüberführt (Cassirer 1922, 32).

He had also discussed the "knowledge ideal" of an era:

> Es muß der Versuch gewagt werden, aus der intellektuellen Gesamtbewegung eines Zeitalters sein herrschendes und treibendes Erkenntnisideal zu rekonstruieren (10).

In 1937, two years before the publication of Koyré's *Études galiléennes*, Cassirer in an article in *Scientia* titled "Wahrheitsbegriff und Wahrheitsproblem bei Galilei" wrote that Galileo brought about not only a change in thinking about the physical cosmos, but also a new *form of Knowledge*:

> Durch das Werk Galileis wurde nicht nur der *Inhalt* des physikalisch-Kosmologischen Wissens vermehrt oder verändert; es war eine neue Wissens*form* die hier auftrat. Nicht um einzelne Fakten, sondern um die letzten *Massstäbe* des Wissens wurde hier gekämpft, und die prinzipielle Umwandlung dieser Massstäbe ist es, was dem Werk Galileis seine unvergleichliche philosophische Bedeutung gibt (Cassirer 1937, 193).

Cassirer, therefore, occupies a special place for being one of the first, right at the beginning of the 20th century, to prolifically deal with the intricacies of historical thinking about science. The lens through which he organized his thought was no doubt neo-Kantian and reflected his predecessors, particularly Hermann Cohen and Paul Natrop. But someone may object to appealing to Cassirer's historical understanding to throw light on Kuhn's thought pre-*Structure*, especially since there is no record of Kuhn, throughout the early period, ever mentioning him. Nevertheless, Cassirer, I insist, is pivotal for the alternative narrative of the genesis of the *Structure* for the simple reason that Cassiere belonged to the historicist scientific strand Kuhn eventually resolved. Further, Cassirer was a prominent neo-Kantian thinker since Kuhn's *later* philosophy acknowledged affinities with aspects of neo-Kantian thought, some philosophers mused about Kuhn as "Kant on Wheels".

We have now come full circle and can attend to the central question of this chapter: What is historicism? I shall follow Cassirer's own recommendation in his *Problem of Knowledge* (Vol. 4) for "the genesis of historicism" to look at Ernst Troeltsch's *Der Historicismus und seine Probleme* (1922) (Cassirer 1950, 170). According to Troeltsch, historicism is "a term

of abuse" (*Scheltwort*) and "an attack against complex historical science" and a sign of "general rebellion against science" (Adair-Toteff 2017, 146).

> Historicism can be roughly defined as the belief that there is no grand abstract theory of historical occurrences but that "history" is seen differently by different cultures and ages. Or, as Troeltsch provisionally defines it in *Historismus*, "the fundamental historicizing of our knowing and thinking." (146)

According to Christopher Adair-Toteff, Otto Hintze believed that historicism was "a particularly German phenomenon" and that there were two strains:

> One was the strain that Troeltsch represented, which grew out of the rationalism of Leibniz and then Hegel and Ranke. The other was the one that Spengler represented, which developed from Schopenhauer and Nietzsche. Both seemed to lead to skepticism and relativism […]. (158)

Karl Mannheim[2], Adair-Toteff points out, had noted that Kant helped by "proving that objectivity is not given by the object but is determined by the cognitive subject", and that Troeltsch follows Kant in this respect (160). Finally, it is worth referring to the notion of "truth" since Karl Heussi believed that "it was Troeltsch who followed Dilthey, who suggested that "the pure light of truth is only to be seen in various broken streams" (161).

Also revealing, is that Cassirer points to the general intellectual history of the nineteenth century to contrast two ideals of knowledge, one, the seventeenth century ideal of mathematical sciences, and another, what he calls the *primacy of history*. But real "Historicism" Cassirer writes "[…] was almost entirely a product of the nineteenth century" (Cassirer 1950, 170). Cassirer asserted that the theory of evolution proposed questions "that can almost be called 'transcendental' problems in Kant's sense" (170). He emphasized that "Darwinism broke with the long tradition once and for all. The historical, barely tolerated previously, was not actually to *supplant* the rational […] The laws of *real* nature are historical laws, and only through their discovery is it possible to escape a bare logical schematism and get back to the actual causes of phenomena." (173). Cassirer, then concluded that in the controversy over Darwinism:

[2] Incidentally, Kuhn owned a copy of Mannheim's *Ideology and Utopia* (1953 edition) with handwritten notes in pencil and on margins and on flyleaf (TSK-MIT_Inventory).

> […] it was long before it [the thesis of evolutionary history] came to be understood as a "maxim" of research, as a regulative principle of biological knowledge (175).

1.2 A Positive Philosophy of Culture Versus Philosophies of Doom and Gloom

In this section, by recounting the encounter of Cassirer and Heidegger, I intend to add yet another perspective to the strand of historicism under discussion.

In the late 1920s, a tug-of-war appeared on the scene between two philosophical titans. Heidegger's *Sein und Zeit* (1927) had just been published, and Oswald Spengler's Decline of the West (*Der Untergang des Abendlandes*) had appeared in two volumes between 1918 and 1922. These are the years immediately following the First World War (1914-1918). The time literally still reeks of doom and gloom of that brutal war. In a lecture in 1944, Cassirer summarized Heidegger's philosophy:

> In vain we try to build up a logical philosophy: we can only give an "existential philosophy" (*Existenzialphilosophie*). The philosopher cannot strive for an "objective," universally valid truth. He can only give the truth of his individual existence, and this existence has always a historical character. It is bound up with the historical conditions under which the individual lives. To change these conditions is impossible (Cassirer, 1979, 230).

He then refers to political ideas in Germany and speaks of the "cultural pessimism of Spengler" or the "works like Heidegger's *Sein und Zeit*" (230) as having, for the most part, an indirect connection with what eventually happened in Germany:

> But there is an indirect connection between that general course of ideas that we can study in the case of Spengler or Heidegger and German cultural and social life in the period after the First World War […]. A philosophy that indulges in somber predictions about the decline and the inevitable destruction of human culture, a philosophy whose whole attention is focused on the *Geworfenheit*, the Being-thrown of man, can no longer do its duty […]. It cannot teach man how to develop his active faculties in order to form his individual and social life (230).

The "tug-of-war" between Cassirer and Heidegger may have at the time been marginal, but its aftermath was significant. In *A Parting of the Ways* (2000), Michael Friedman argues for the centrality of the Davos debate between Heidegger and Cassirer in 1929. Here, Friedman speaks of the "turmoil taking place in the foundations of historical and cultural sciences" but

points to Cassirer as "a quite central twentieth-century figure" (Friedman 2000, 151-152). Cassirer alone, Friedman suggest, is in the position:

> […] to mediate the increasing intellectual tension between the now notorious "two cultures"—between the *naturwissenschaftliche* and *geisteswissenschaftliche* intellectual orientation (152).

Friedman points to Cassirer's philosophy of symbolic form as "truly remarkable" in attributing distinctive "universal validity" to symbolic forms of diverse origins ranging from mathematics and natural science to history, language, art, and religion, as expressions of the unitary human spirit, striving to "objectify" its surrounding world (152). Friedman then drives the crucial point home that, for Cassirer, it is the theoretical science that represents the "end point" of his dialectical development of the various symbolic forms that has its origins in "mythological thought, governed by the most primitive and least logical" (155). Friedman then concludes if Cassirer's ultimate narrative fails to show the "underlying unity for the totality of symbolic forms", we are left with the dilemma presented by Carnap and Heidegger – with the former adhering and appealing to formal logic, and the latter, severing "exact thinking" altogether (156). This dilemma, Friedman claims, lies at the heart of the twentieth century opposition between "analytic" and "continental" tradition (156). Friedman, nonetheless, sets the signpost straight, for Cassirer:

> Those interested in finally beginning a reconciliation of the analytic and continental traditions, however, can find no better starting point than the rich treasure of ideas, ambitions, and analyses stored in his astonishing comprehensive body of philosophical work (159).

Sebastian Luft can only agree with Friedman's assessment: "Cassirer *is* indeed a philosopher who can be a model for contemporary work and moving into the future" (Luft 2021, 215). Luft is interested in the *gestalt*, as he calls it, of Cassirer's philosophy as a legitimate candidate for a "post-split" philosophy (215). For Luft, a summation of the way he reconstructs Cassirer's philosophy boils down to what he calls the interdependence of transcendental idealism and cultural pluralism. He thereby rejects any claims to the absolute, but an insistence that "the world is what it is insofar as we experience and, more importantly, shape it in a historical development" (235). Finally, in Cassirer's spirit, Luft emphasizes that it is "the historic and dynamic a priori that constantly evolves, pro- or regresses, and is forever subject to critique, correction, modification, and even radical change" (236).

Massimo Ferrari has also emphasized that the central role in Cassirer's philosophy is played by science, as "the theoretical self-awareness" of a new era of Western civilization – and a specific symbolic form among others (Ferrari 2021, 72).

> The crucial passage from *Substance and Function* to *The Philosophy of Symbolic Forms* clearly shows how Cassirer extends his early epistemological view into a broader philosophy of culture, thereby bridging the gap between natural sciences and human sciences (73).

Ferrari reiterates that Cassirer has not abandoned his Marburg Neo-Kantianism:

> For both Cohen and Natrop, the renewal of Kanian philosophy consisted in shaping a philosophy of human culture, albeit still restricted to the classic Kantian bounds of knowledge, ethics, and aesthetics (88).

The essence of Ferrari's and Luft's assertions is, I believe, a *global* and *malleable* historicist scientific philosophy, which is indeed decisive in the fate of the "gap" since it emphasizes inclusivity rather than exclusivity, and integration rather than division.

Popper, in his 1950 William James Lectures at Harvard, had warned of a "severe crisis" of rationality (KPA-Box 39, Folder 6). Popper had referred in his talks that there are internal and external "attacks against rationalism":

> But the attacks against rationalism comes from two sides – from outside, as it were (i.e. from those who ask whether reason is making us good, is valuable) and from inside – from those who advocate the view that scientific research is more or less a matter of routine production […]. (KPA-Box 39, Folder 6).

Note that both "attacks" involve the rationality of the scientific enterprise, but what he calls the "attack" from outside seems to echo Heidegger in Davos. In the debate with Cassirer in Davos, Heidegger had rejected the idea of any "breakthrough" into an essentially non-finite realm. In essence "holding fast to our essential finitude" – our "hard fate" (Friedman 2000, 3). Cassirer had asked whether Heidegger wanted to maintain that "all truth is relative to Dasein (the concrete finite human being)" (3). According to Friedman, Cassirer had strongly objected to the idea that "symbolic animals" are limited to the "arational" sphere of finitude (2). This, I postulate, can only mean that Cassirer leaves both the scientific rationality intact, and the field of inquiry wide open. Friedman may be right then when he points to Carnap

and Heidegger and the "dilemma" that lies at the heart of the twentieth century opposition between "analytic" and "continental" tradition (156). Finally, even though, Friedman's locating the source of the infamous gap between "analytic" and "continental" philosophies in the breakdown of the intricate and "original Kantian architectonics" (156) seems plausible, the fact is that Cassirer and some of his prominent contemporaries, i.e., historians of science; such figures as Koyré, Sarton, and Lovejoy, each with their own unique contributions, not only held the course, but also defended the historicist course against the "bonfires" of the time.

1.3 Types of Historism and The Strand of Historism *Structure* Resolved

Herbert Schnädelbach has defined three types of "Historism", one of which accords very closely with what we have been discussing. "**Historism$_1$**" is to be understood as "eine bestimmte Wissenschaftspraxis" (Schnädelbach 1974, 20). In other words, collection of historical information simply for the sake of collection of such information. "**Historism$_2$**", according to Schnädelbach, is a philosophical position and it also relates to the debates around the time of the First World War (21) "sie vertritt einen durchgängigen historischen *Relativismus* im Bereich der Erkenntnis und der Moral". Schnädelbach also uses "*Skeptizismus* und *Agnostizismus*" in this connection (21). As for "**Historism$_3$**", Schnädelbach writes that the word is used:

> […] unpolemisch, ja sogar in gewissem Sinne positiv gebraucht. So gebraucht *Ernst Troeltsch* (in:7.41) dieses Wort zu Kennzeichnung des Prozesses der grundsätzlichen Historisierung alles unseres Denkens über den Menschen, seine Kultur und seine Werte […] *Karl Mannheim* geht darüber noch hinaus, indem er den Historismus geradezu die „Weltanschauung" der Gegenwart nennt (22).

Schnädelbach associates this third form with:

> […] *historischen Denken*: zum Erfassen der historischen Dimension von Phänomenen in einem weiter zu bestimmenden Sinne Charakterisieren kann (23).

Following Schnädelbach's scheme, Cassirer clearly belongs to "Historism$_3$". Yet, this assessment is certainly only part of a larger historicist picture, a spectrum, or an era. This era is one in which, Cassirer's historism with its inherent neo-Kantian underpinnings occupies a prominent space. And it is precisely this era, I have postulated, that was eventually

culminated in the *Structure*. Seen through this lens, scrutinizing Cassirer's role becomes inevitable.[3] But what was 'historicism' for Cassirer? In his Rectoral address in Hamburg on November 7, 1929, Cassirer exalts 'historicism' receiving a new mold:

> As Goethe felt bevor the work of Herder, so we feel as we stand before any really great historical achievement. What grips us in it is not the report of what has happened in the past, nor is it the rendering of something merely factual; rather, we feel in it a genuine rebirth—a remoulding and revivifying of the world, something which cannot result from anything but a peculiar spiritually personal center of life (Medicus 1936, 141-142).

And a 'new way of thinking':

> Hegel's system[4] provided the appropriate metaphysical foundation for and expression of historicism. This new way of thinking, thanks to the Darwinian doctrine, penetrated into biology (Cassirer 1950, 170).

In the concluding pages of his book, Cassirer points to 'historicism' as he understood it, namely, as a unifying medium, metaphysics-avert, and aiming at a soberly human enterprise of science:

> […] we do an injustice to "historicism" when we stress only its negative and destructive side and regard it only as the forerunner of skepticism and relativism. […] "historicism" showed its consolidating and unifying power. To be sure, it sought unity elsewhere than in metaphysics and aimed to secure it by other means. Diversity, variety, change, evolution, all these seemed no longer the antithesis to Being [[5]] but its correlative. Historicism found Being no longer in God or in the Absolute Idea and wanted only to hold fast to it in the human mind and the totality of humanity (324-325).

Roughly two decades after the Davos debate, the fourth volume of Cassirer's *The Problem of Knowledge: Philosophy, Science, and History since Hegel* (1950) was published

[3] Except for two comments in *The Essential Tension*, there is no record that Kuhn ever mentioned Cassirer. The two comments are as follows: "That attitude towards past thinkers came to the history of science through philosophy. Partly it was learned from men like Lange and Cassirer who dealt historically with people or ideas that were also important for scientific development" [1968] *(The Essential Tension* 1977, 107). And "In the history of ideas, the resulting tradition is the one which produced both Ernst Cassirer and Arthur Lovejoy, men whose work, however profound its limitations, has had a great and fructifying influence on the subsequent treatment of ideas in history" 1971 (*The Essential Tension* 1977, 149).

[4] Hegel's system, according to Cassirer, "does not stay in the empty spaces of metaphysics. Its intention is to open the way to a very definite and concrete undertaking of scientific knowledge. Its aim is not only to win for history its rightful place at the side of science; it sees in history the realization and true expression of all the knowledge that the mind possesses from its own nature and resources" (*The Problem of Knowledge* 1950, p. 3).

[5] 'Being' is most probably used in the sense Lovejoy also used the word in *The Great Chain of Being* (1936).

posthumously. Whereas Cassirer defined historicism as inseparable from the enterprise of science due to its human dimension, Karl Popper – in the same year – argued for the rationality and "logic" of science' while undermining "relativism". Consider Popper's concerns in his William James Lectures at Harvard (1950):

> […] relativism [is an] accidental character of science" […] [W]ish to criticize this view often asserted in connection with *Principia Mathematica* and Logic and non euclidian geometry which is believed to be characteristic theory of our time. […] the belief that everything is relative to the framework of assumptions: this [is] a misunderstanding […] [The] situation is [that] logic is misinterpreted, not true that [there are] alternative logics (KPA-Box 39, Folder 7).

I suppose Popper is either underlining the logic of science as a rational endeavor – which would amount to truism – or Popper is discarding the remnants of the negative historicism of two decades prior in Davos. What is certain is that what is being "dismantled" by Popper still belongs to the continuum of European historism, traces of which, we will find in Kuhn's address to the student body in 1943. I shall attend to Kuhn's Harvard roots in the following chapter.

To recap, in this chapter I have sought to demonstrate a historiography in flux, shaped by a new scientific sensibility during the first half of the twentieth century—a development that marked the early stages of a "historical scientific turn." I then examined the famous Davos debate and its significance in forging a new historicist scientific era. This was followed by an exploration of various forms of historicism, among which I identified a strand encompassing Cassirer's scientific philosophy, as well as that of other figures such as Koyré and Lovejoy, who were advancing historicist scientific ideas. Finally, I raised the question: could the point of departure for Kuhn's *Structure* be found in the analytic-continental divide? Put differently, did *Structure* ultimately resolve the philosophical dichotomy that so profoundly shaped Kuhn's generation?

PART I

The Student Years

2

Kuhn's Harvard:

2.1 The Harvard Hub And the "Noble Tradition"

Outlining Kuhn's Harvard, his teachers, his contemporaries, and the philosophical challenges within Harvard, I will introduce the concept of Harvard Hub as a melting pot of different strands of philosophies within Harvard (2.1), point to the Kantian- inspired Harvard Pragmatism, and the forerunners of Harvard philosophy, i.e., Hugo Münsterberg, Ralph Barton Perry, William James, and others (2.2). Susanne K. Langer and her clout among the members of the Vienna Circle is the subject of another section (2.4). Then follows Philipp Frank at Harvard and his interactions with James Conant on Conant's General Education program that Kuhn became a part of (2.5). In (2.6) I will look at George Sarton at Harvard and his contacts with, and influence on, James Conant and again the General Education program to postulate that there appears to be a direct link connecting Mach, Sarton, Conant, and Kuhn. And in the final section of this chapter (2.7) I will examine Kuhn's lecture notes to show how Kuhn set himself against Bertrand Russell's neo-realism and his methodology of science, and at the same time positioned himself in favor of Arthur Lovejoy's critical realism; Kuhn showed great appreciation for Lovejoy's approach to intellectual history and his book The Great Chain of Being.

Thomas Samuel Kuhn in his PHI BETA KAPPA Harvard graduation address to the student body on May 26, 1943, speaks about the Harvard tradition. In his speech (TSK-MIT),[6] Kuhn tells the story of his generation that has been "so frequently cursed for its cynicism and lack of faith". He says that "We have been called a generation of cynics […] We are at the edge of nihilism". He refers to both World Wars: "we find ourselves in a world again at war". And yet it is 'the Tradition of the Humanities and the Liberal Arts' which has led us to this revulsion from negation". "Our sense of dignity", Kuhn announced "has come to us in two ways". He then speaks of the Harvard tradition and the 'historical perspective' he has gained through his studies:

[6] "Thomas Samuel Kuhn, '44, was the last undergraduate marshal of Phi Beta Kappa and editorial chairman of the *Crimson* from the Class of 1944. More recently he has been a research associate in the Radio Research Laboratory at Cambridge. He has just returned from a year spent in England and on the continent as technical consultant to the U.S. Army Air Forces." (Source: Harvard Alumni Bulletin September 22, 1945). The previous excerpt is mentioned as Kuhn's credentials under his contribution *Thomas S. Kuhn, on Behalf of the Recent Student, Reflects the Undergraduate Attitude* in which he distinguished teachers from scholars, and speaks of the need for "inspiring teachers". Since the early days of the American independence in 1776, Phi Beta Kappa is considered America's most prestigious academic honor society to celebrate excellence in the liberal and sciences as well as championing the cause of freedom of thought. The Phi Beta Kapp society at www.pbk.org. TSK-MIT, Box 1, Folder 3, Undergraduate Papers.

> From our studies, we have gained what, for lack of a better word, I would call a historical perspective [...] This makes us part of our history, and effectively prevents us from renouncing an issue because we may declare it "not of our making" […] through our studies and through the mere experience of life in Harvard University […] the intimate knowledge of the living tradition which is Harvard. It has shown us the promise of a continuity of the Humane tradition.

He then refers to the Widener Library and its immense collection:

> [T]hey are stored there on the tacit assumption that anything purposefully created by man must sooner or later reenter and become a part of the world's culture.

He then reads an ode to Harvard titled "L'Envoi", which was published in *Crimson* the following day and concludes:

> Those who enter in my departing footsteps will not see the vistas I saw. You have not shuttered your windows, yet vision is obscured; the scene has changed.

In Kuhn's speech, the humanities or *Geisteswissenschaften*, is at the center of Harvard's tradition. But *Geisteswissenschaften* had, no doubt, deep European philosophical roots and therefore must not be associated with Harvard tradition only. Cassirer associated its significance to Hegel:

> Its aim [Hegel's system] is not only to win for history its rightful place at the side of science […] he imparted the most powerful and lasting impulse to the logic of the "Geisteswissenschaften" (Cassirer 1950, 3).

Kuhn is even referring to the two European wars as if intimately tied to his generations' *Weltanschauung*. And it is precisely such a 'historical stance' not only towards the way the young Kuhn and his generation were taught, but also towards the common roots of the liberal arts and humanities, that occupied for Kuhn the centerstage. This stance may have in fact undermined the so-called "divide" in the 'continuity' of thought. But what is the young Kuhn referring to when he speaks of the "continuity"? Is the 'tradition', the very influential American Pragmatism? Is the American Pragmatism an isolated stream of thought at Harvard? Could the 'continuity' have its source elsewhere? Or is the 'continuity' is being amid a flux of various streams of thought? In the sections that follow I will point to Harvard's

"tradition" and its historical anchor and argue not only for a common ancestry of historism, but also its almost tandem and complementary interdependence.

2.1.1 Kuhn's Harvard Education

To start off, I shall briefly refer to one of Kuhn's undergraduate papers to point to Kuhn's early immersion in philosophical logic in search of connection to early analytic philosophy. I mention C. I. Lewis in connection with Quine to show that Kuhn's early educational environment is one imbued by logical analyses and to present the early signs of flux of ideas Kuhn is encountering.

The young Thomas Kuhn's early point of contact with C. I. Lewis can be documented in a paper he wrote for Prof. H. M. Sheffer for 'Philosophy 8' in the Fall Term, 1945-1946. In this paper titled "A Comparison of the Logic of Propositions with that of Ascriptives", he refers to C. I. Lewis several times primarily in dealing with aspects of Russell's assertions in the second edition of *Principia Mathematica* about H. M. Sheffer's mathematical logic. Kuhn refers the reader to Lewis's *A Survey of Symbolic Logic* (1918) and describes the goal of his paper in the following way:

> Primary emphasis is given the comparison of the logic of ascriptives with the propositional logic of the Principia, but mention is made of the significance of the formulation for systems, like that of Professor Lewis, which replace material implication with strict implication […] (TSK-MIT, Box 1, Folder 3).

The undergraduate Kuhn is of course imbued in the new logic throughout his early education at Harvard. Elsewhere and much later, Lewis pointed to the 'pragmatic in Kant':

> I look to a pragmatic basis of the normative […] And perhaps it comes as near to what is pragmatic in Kant and what Royce called his "absolute pragmatism" as it does to James or Dewey—or even Peirce (Lewis 1969, 107).

In his book, C. I. Lewis had "embraced a version of pragmatism to develop a view of the *a priori* as based on meaning […]" (Shieh 2013, 1052). According to Sanford Shieh, Lewis's views were "assimilated, mainly by Quine, to the Positivists' account of necessity in terms of analyticity" (1052-1053). Michael Beaney in "What is Analytic Philosophy?" writes:

> There were new methods and kinds of analysis that were indeed introduced into analytic philosophy beginning with Frege's logical and logicist analyses, his use of contextual definition, and Russell's theory of descriptions (Beaney 2013, 25).

According to Beaney these methods were further developed by Russell himself, by Wittgenstein in the *Tractatus*, by Carnap in *The Logical Construction of the World* (25-26). These ideas, Beaney writes, were introduced to Oxford by Ryle and by Ayer, "and Quine gave them a powerful presence in America through his own logical and philosophical work" along with others such as Lewis, Marcus, and Kripke (26). The early educational environment for Kuhn is therefore one imbued by logical analyses and present the early signs of flux of ideas Kuhn is encountering. They are literally traversing back and forth from the European continent to Britain and from the continent to North America – and ultimately to Harvard. The young Kuhn, in turn, one can surmise, was, in the early 1940s, physically and intellectually amid this flux. Kuhn was, therefore, not only a part of Harvard and its tradition, but was also amid "streams" of continental thought and its tradition. Alexandre Koyré and Arthur Lovejoy in the United States later became the center of Kuhn's focus; they represented historicist themes of European and American historiography. In other words, by scrutinizing Harvard philosophical tradition, one is bound to find, as early as 1913, both interest and a tension in logical as well as historical themes. Such tensions on the part of Harvard educators and philosophers to balance out the latest trends and the older and established ones were, in a sense, a "tug-of-war" between the logical and the historical. Two historical letters confirm the foregoing. On March 3, 1913, a letter from Harvard's Hugo Münsterberg to Cassirer speaks of this very interest in logical themes:

> Wir haben im nächsten Jahre, und zwar in unserem zweiten Semester, vor allem Russell aus England bei uns, der ja in seiner Richtung als allererste Kraft heute gelten kann (Cassirer 2009, 17).

But at the same time, another letter sent on the same day by Harvard's Ralph Barton Perry had communicated to Cassirer that his philosophy would be of great interest to Harvard. Perry had insisted that "your visit would be of genuine interest to philosophers not only here but throughout the country at large" (19). In short, this identification with openness to different strands of thoughts points to ~~the spirit of~~ Harvard's educational tradition that the young Kuhn later proudly puts up for display in his speech to the student body.

As for Harvard's philosophical roots, an examination of profiles of educators during Kuhn's Harvard years will primarily depict an intellectual climate at Harvard where different philosophies coexisted and enriched an atmosphere of debate. Such debates were not exclusively local or indigenous to American thought, but as we shall see, they not only strongly shared the common European roots of *Geisteswissenschaften*, but also reflected the philosophical movements of the continent. It was this very climate, I argue, that eventually gave rise to the *Structure*. In the Harvard arena, what I underscore can be divided into three areas: the early period, the middle period, and the late period, which can be labeled respectively, the old Kantian-inspired Harvard Pragmatism leading to the logical turn at Harvard around 1910, Vienna Circle's ideas in the '30s and '40s, and the resolution of the historicist strand by the end of the 1950s. Numerous examples of each period will indeed follow, but for now consider that for the middle period, for instance, a quick look at historiographical literature shows that strands of thought representing Harvard Pragmatism and the Vienna Circle were already in place. Charles Morris had published various articles to promote the ideological integration of American pragmatism with European logical positivism (collected in Morris 1937) (Tuboly, forthcoming in 2025).

2.2 Kantian-Inspired Harvard Pragmatism: Royce, James, and Peirce

In this section, I will ask two questions: What aspects of Kantianism were attractive to the founders of pragmatism at Harvard, and what were the influences of pragmatism, if any, on the young Kuhn? Regarding the latter, I argue that Harvard pragmatism appears to have been passed on to Kuhn by way of William James, John Dewey, and James Conant.

William James was a central figure at Harvard who initiated "pragmatism" that promoted "different 'pragmatic' philosophies", which allowed for:

> […] changes in belief over time, defining the belief as a habit of action and not as some abstruse mental entity. By arguing a la Darwin, that survival was somehow the test of intellectual as well as biological fitness, these men were able to reconcile the new science to a new religion (Kuklick 1977, xix).

Darwin's ideas had provoked uncertainty, and it appears that the appeal to Kant was an anchor in the face of that uncertainty. We encounter William James in Bruce Kuklick's *The*

Rise of American Philosophy (1977) who wrote from Berlin in January of 1868 to one of his friends (47):

> When I get home let's establish a philosophical society to have regular meetings and discuss none but the very tallest and broadest question (47).

This was the origins of James's Metaphysical Club, which was an informal setting for discussions. Kuklick in his book points to both reaction to Kant, and aspects of Kantian affinities as *raison d'etre* of Harvard philosophy:

> The epistemological problems bequeathed by Kant to succeeding generations of philosophers were central for these men […]. Cambridge thought and Harvard Pragmatism, like all forms of pragmatism, had an idealistic character. The truth of a belief was a function of its practical significance (xx-xxi).

According to Kuklick, "When William James argued that religious ideas enabled us to live well—that they were successful—he meant that they were true" (27). Even the name given to James's doctrine—pragmatism—was Kantian (53). And for the Metaphysical Club, this was a connection to "definite human purposes" as there existed "an inseparable link between meaningful beliefs and practical activity" (54). James wrote that Münsterberg had once told him: "I seem to be ignorant that Kant ever wrote, Kant having already said all that I say" (Carlson 1997, 364). Similarly, James Ward, an English psychologist and philosopher, had informed him "All the worth I see in pragmatism is to be found—don't kick—in Kant, in his 'primacy of the practical reason,' and in his showing that there is 'room for faith'" (364). Thomas Carlson has characterized James's philosophy as an "evolutionary Kantianism". And that "the heart of Kantianism is to be found in:

> [T] he "Copernican turn" of transcendental humanism: the search to establish what are the most reasonable philosophical views for us to hold through examination of our nature as autonomous rational individuals (382).

The consequence of such a Kantian view, Carlson points out, is "democratic respect for the pluralism of perspectives" and "reasoned faith in a higher power" in search of ideals" (382).

> Whatever the substantive relations between the Americans and Kant, his *pragmatisch* became the English word *pragmatic*, and it did so as an extension of Darwinian ideas (Kuklick 1977, 54).

It appears that aspects of Kantianism found a secure place within what was to become Harvard pragmatism:

> From 1870 to 1890 these men searched for their own reconciliation of religion with the new ways of thinking in biological science. They all emphasized the limitations of common-sense empiricism and believed that the philosophic rationale for a defensible religion would have to rely mostly on Kant and the post-Kantians (46).

Kuklick points out that "The outlook of Royce and James derived from Kant's critique of pure reason" (312). We hear of Royce referring to Kant as "the good father" his debt to Kant "most of all" (144). Peirce recalled "he spent three hours a day for two years mastering the text"; in the 1870s he "sported impressive philosophical armor" especially within the Metaphysical Club (106). In 1881, he wrote "we all now live, philosophically speaking, in a Kantian atmosphere" (144).

> Harvard Pragmatism was a form of neo-Kantianism whose adherent drew from a set of connected technical doctrines: a constructionalist epistemology stressing the changing character of our conceptual schemes; […] a recognition of the importance of logic for philosophy; […] (257).

And the ties go even deeper in time; there is evidence that German Idealism was prevalent at Harvard. A. Hunter Dupree portraying Asa Gray (An American and a close friend of Charles Darwin) wrote in his 1968 book on Gray (first published 1959) book [7] :

> But as an American Gray had to reckon with the fact that almost at his own back door transcendentalism was the most vigorous intellectual movement in the country (Dupree 1968, 221)

In 1875-76, when Josiah Royce spent a year in Germany, he had immersed himself in Hegel, Schelling, Kant, and Schopenhauer. He was taught by Peirce (Misak 2013, 81). Cheryl Misak adds:

> Royce had a formidable intellect and became a force on the American philosophical scene. His students included C. I. Lewis, George Herbert Mead, Santayana, Henry Sheffer, Arther Lovejoy, and Morris Raphael Cohen. They tended to be devoted to him but not to follow him down the idealist path, an independence of thought that was encouraged by Royce […]

[7] Here Dupree in Gray's words points to Ralph Waldo Emerson and Henry Thoreau, but this still points to the Kantian atmosphere in 1850s roughly 20 years before James' Metaphysical Club.

> James got him a position at Harvard and he remained intertwined with James and Peirce for the rest of their lives (82).

It is noteworthy that Royce pointed out that both pragmatism and his own absolute idealism "share a kind of Kantian argument against realism" since both take the world to be the construction of human beings rather than a set of facts independently of human experience" (88-89). According to Misak:

> Pragmatism, he says, sticks to "transient, passing, variable" human beings and hence true judgements become "not genuinely true, but only special points of view" (89).

But how are we to understand the "transient, passing, variable" that Royce had spoken of? A type of relativism perhaps? Or simply a liberal tradition of free flux of ideas? In Misak's introduction, we read:

> Pragmatism, as Robert Westbrook says, is like a contentious family of thinkers holding distinct but related positions on the "workmanlike" nature of knowledge, meaning, and truth (3).

"The debate on pragmatism continues to this day" (3) Misak writes.

> But roughly, it is a debate between those who assert (or whose view entails) that there is no truth and objectivity to be had anywhere and those who take pragmatism to promise an account of truth that preserves our aspiration to getting things right (3).

In the light of the above assertions, two associations come to mind. One addresses the "relativism" of the philosophical "taste"[8] or trends of the time – the prevalence of certain ideas in philosophical discourse**;** a clear example is Popper's assertions in his 1950 Harvard's William James Lectures who called "the belief that everything is relative to the framework of assumptions…this [is] a misunderstanding" (KPA-Box 39, Folder 7). Another association points to Philipp Frank's understanding of American pragmatism and its affinity with the logical empiricism. Frank in Harvard had conveyed to Carnap that:

> "What you advocate is pragmatism." This was astonishing to him as it had been to me. We noticed that our group, which lived in an environment of idealistic philosophy had eventually reached conclusions by which we could find kindred spirits beyond the Atlantic in the United States (Frank 1949, 33).

[8] In using the word 'taste', I am taking cue from Kuhn himself. In Structure's first draft and referring to what brings revolutions to a close, Kuhn dealt extensively and gave priority to the concept of "taste" as opposed to 'validation' (TSK-MIT, Box 4, Folder 2).

But what are we to make of the above associations? I submit that these associations point to diversity of thought and pluralism of perspectives at Harvard, which would, in turn, be indicative of meshing of traditions, i.e. Logical Empiricism and Harvard Pragmatism. So far, I have dealt with the first question posed at the outset of this section, namely, What aspects of Kantianism were attractive to the founders of pragmatism at Harvard. I will now attend to the second question about the influence of pragmatism on the young Kuhn.

According to Kuklick "From the mid-1890s to James's death in 1910 Harvard Pragmatism controlled American philosophy" (Kuklick 1977, 257). And According to Misak "the Received view of pragmatism's fortunes is that it shone brightly right through to John Dewey's death in 1952, then nearly burned out" (Misak 2013, 1). But as we get closer to Kuhn and the genesis of the *Structure* in 1954-1955,[9] one encounters references to Conant. Reisch writes:

> Conant liked this empiricist sensibility and attitude—"a habit of meeting problems in a detached, experimental, observing spirit," as the report has it—and he once characterized his own philosophy of science as a "mixture of William James's *Pragmatism* and the logical empiricism of the Vienna Circle, with at least two jiggers of pure skepticism" (Reisch 2019, 142).

Conant's attitude reminds us of Royce's stance towards his students mentioned earlier. And, it appears, that the "transience" Royce had spoken of – a type of relativism of perspective in today's jargon – had indeed persisted at Harvard all the way up to the publication of the *Structure*. And as for the influence of pragmatism on the young Kuhn and the genesis of the *Structure* in the 1950s, it appears, at least on the surface, to allow the postulation that Kuhn's *Structure* may have shared aspects of Harvard's tradition of pragmatism.

However, Bojana Mladenović in her book *Kuhn's Legacy* (2017) and in a chapter titled "Kuhn's Pragmatist Roots", she argues that Kuhn's philosophy is properly appreciated in the light of "creative and fruitful continuation" of American pragmatism but admits that there were also other influences (Mladenović 2017, 167). When tracing philosophical lineage, she writes, "we should situate Kuhn on a branch of American pragmatism" (168).

[9] TSK-MIT, Box 5, Folder 84. In "Plans for Research" for the academic year 1954-1955, Kuhn mentions his acceptance of an invitation by Charles Morris to contribute to the *International Encyclopedia of Unified Science* and his suggestion of the title *The Structure of Scientific Revolutions*. In fact, the date of the acceptance of Morris's invitation precedes the above date. Kuhn writes: "Nine months ago I accepted an invitation…".

Nevertheless, I must point out that Mladenović has explicitly pointed to "Kuhn's mature work, which was published in the 1980s and 1990s" (192). She writes – albeit vaguely – that:

> The influence of American pragmatism on Kuhn's philosophy is detectable but not foregrounded in Structure; it comes into prominence in Kuhn's mature work (192).

Mladenović quotes Peter Carl Gustav (Peter) Hempel as referring to Kuhn's ideas as "historicist, pragmatist approach to problems in the methodology of science" (188). According to Mladenović, Hempel eventually warmed up to Kuhn's ideas and referred to his own position developed under Kuhn's influence as "pragmatic empiricism" (188). For Mladenović, "Kuhn responded to the more Peircian pragmatist legacy in the works of Quine, Goodman, and Putnam" (189). In short:

> Although no tenet is in principle immune to revision, a necessary condition for any revision is that *some* tenets are temporarily *taken* as given. This Peircian idea, revived by Quine, forms the backbone of Kuhn's mature epistemology of science. Of course, Quine's body of work cannot be described as an expression of an undiluted pragmatism. […] It is thus significant that the *only* idea Kuhn borrowed from Quine is precisely this purely pragmatist one (189).

But we also saw in (2.1) that Lewis's views were also "assimilated, mainly by Quine" (Shieh 2013, 1052-1053) and that he had "embraced a version of pragmatism to develop a view of the *a priori* as based on meaning […]" (1052). But for earlier associations, recall Conant's assertions about his own philosophy as a "mixture" of Pragmatism and logical empiricism of the Vienna Circle. Also recall that Frank had enthusiastically reminded Carnap of the affinities of Logical Empiricism and Pragmatism. Other aspects of interest are Kuhn's early education (Baltas, Gavroglu, and Kindi, 257) and the personalities and ideas he encountered and interpreted, among whom, those of Frank and Conant, Piaget and Lovejoy (see 2.5, 2.7, 4.2).

Therefore, in the light of the arguments in this section, I submit that pragmatism was part and parcel of the debate at Harvard during the genesis of *Structure.* Therefore, it is plausible to consider that even though Kuhn's pragmatism may have found distinct expression in his later years, the roots of which I argue, in contrast to Mladenović, must lie in pre-*Structure*.

What is Harvard Pragmatism? Since Pragmatism has been broached so very differently, it is important to ask what is Pragmatism and to determine the extent to which Kuhn may have identified with its tenets. The fact is that Harvard pragmatism was being actively debated by critical realists.

Lovejoy discussing Pragmatism, had written "This kind of instrumentalism I personally believe to be idealistic in its logical tendency" [as opposed to realistic] (Lovejoy 1909, 580). As a student of William James at the beginning of the last century, Lovejoy puts the question of Pragmatism in perspective:

> One of the things that pragmatism of James is, certainly, is a modern expression of the motive which, in certain other expressions, is known as nominalism or positivism. In his original volumes of lectures on the subject, James showed very plainly that he was in the line of the great nominalistic tradition of English thought, a successor of William Ockham, of Hobbes, of Locke and Hume and Berkeley (577).

Lovejoy continues:

> The typical nominalistic motive – the simplifying, clarifying, *denkökonomisch* motive; the typical nominalistic method – the definition of universals as collective names for particular items in experience; the typical nominalistic results—the rejection as negligible, if not demonstrably unreal of all entities incapable of being brought within the compass of concrete experience—these are all conspicuously present in the most authoritative exposition of the pragmatic doctrine (577-578).

First, this is certainly in line with Mach's teaching, which consequently has a closer affinity with the origins of Logical Positivism and for that matter the continuum of historicist scientific thought. Lovejoy continues:

> It is true, however, that pragmatism also means instrumentalism. But its instrumentalism, it seems to me clear, should be construed in the light of its nominalism, of its demand for the reduction of all meanings to concrete particulars of experience stated in their most "economical" terms (578).

Later, in the 1920s, Lovejoy emphatically debated the role of Pragmatism and its position towards Idealism and Realism. In criticizing Dewey, Lovejoy writes "A consistent pragmatism must recognize":

> That, if a real physical world having the characteristics set forth by natural science is assumed, certain of the contents of experience, and specifically

> the contents of anticipation and retrospection, cannot be assigned to that world, and must therefore be called "psychical" (i.e. experienced but not physical entities) (Lovejoy 1920, 76).

Kuhn's philosophy fits the above picture described by Lovejoy since Kuhn did not reject scientists' "unconscious" insights—in fact he defended it. Kuhn did employ both aspects of instrumentalism (i.e., as applied to theories), *denkökonomisch* methods, and nominalism (i.e., how language and meaning play a central role in the evolution of scientific views e.g. incommensurability).

An apt remark made by Massimo Ferrari exposes the early ties between Mach and James's Pragmatism:

> To be sure, they agreed "on finding the meaning of ideas in the sensations that may be expected from their realization", but also on considering the central role played by functional relations within scientific knowledge: an aspect appreciated by James as a typical and unique feature of Mach's philosophy of science (Ferrari 2017, 21).

Further, Ferrari adds that Mach had shown great interest in James' essay on *The Sentiment of Rationality*, "finding in it a confirmation of his own ideas about the concepts as 'economic tools'" (21).

Above, I spoke of Kuhn's philosophy fitting aspects of pragmatic thought. This is, of course, unsurprising, given that the philosophical climate at Harvard in the 1950s had evolved considerably from that of the early twentieth century. One, can therefore, attribute a degree of pragmatism to Kuhn's pre-*Structure* thought—as the enduring "background music" of Harvard's intellectual environment. In this sense, *The Structure of Scientific Revolutions* can be seen as a child of Harvard Pragmatism, nurtured in a cradle where European and American historicist traditions were deeply intertwined.

2.3 Undergraduate Papers and the Logical Milieu at Harvard in the 1940s

This section shows that the young Kuhn of the mid-1940s stood at the crossroads of Harvard's logical tradition and a broader, more eclectic philosophical heritage.

For ‘Philosophy 8’ in the Fall Term 1945-1946, the young Thomas S. Kuhn wrote a paper for Prof. H. M. Sheffer’s course titled "A Comparison of the Logic of Propositions with that of Ascriptives”, a paper dealing with aspects of the logic in Whitehead & Russel’s second edition *Principia*:

> […] the comparison of the logic of ascriptives with the propositional logic of the *principia*, but mention is made of the significance of the formulation for systems, like that of Professor [C.I.] Lewis […] and the older two-valued algebra of Pierce and Schroder [Peirce and Schröder] (TSK-MIT, Box 1, Folder 3).

The citations are mainly from the *Principia*, C. I. Lewis on symbolic logic, along with mentions of M. R. Cohen & E. Nagel, J. C. Cooley, R. M. Eaton, Susanne K. Langer, with “lectures delivered by Dr. H. M. Sheffer’s during his introductory course at Harvard” (TSK-MIT, Box 1, Folder 3). The young Kuhn’ conclusion for this paper:

> […] the most apparent direction for progress in logic is through an analyses of the proposition and a search for more primitive elements. The theory of ascriptives is certainly at least a first step in this direction (TSK-MIT, Box 1, Folder 3).

Another paper for ‘Philosophy 9’ is addressed to Prof. Donald Cary Williams (D. C. Williams) again for the Fall Term 1945-1946 entitled "An Analysis of Causal Connectivity” in which Kuhn discusses causality in general yet associated with questions around physics of the day, namely Quantum physics, simultaneity, and other topics including Boyle’s Law and ‘the nature of scientific law’. Throughout this paper, Kuhn refers to Russell. The paper displays numerous lines of handwritten comments – either by Sheffer himself or by someone else who read his students’ papers. The citations for this paper are Russell’s *Mysticism and Logic*, *Our Knowledge of the External World*, Hume’s *Enquiries Concerning Human Understanding*, *The Treaties of Human Understanding*, and McTaggart’s remarks on C. D. Broad’s *Scientific Thought*, and McTaggart’s *The Nature of Existence* (TSK-MIT, Box 1, Folder 3).

D. C. Williams thesis of 1928 was titled "A Metaphysical Interpretation of Behaviorism”. He has been associated with empirical realism, metaphysics, ontology, and cosmology. A sample of his publications include “The A Priori Argument for Subjectivism” (1933), “The Argument for Realism” (1934), *The Ground of Induction* (1947), and “Professor Carnap’s Philosophy of Probability” (1952).

In short the young Kuhn of the mid 1940s is both much immersed in Harvard's logical milieu and a mixture of philosophical thought a the Harvard hub. This is hardly surprising since according to Adam Tamas Tuboly "[…] all those young scholars who had embarked on a pilgrimage to Europe in order to study the latest logical and philosophical trends – like Nagel, Morris, Quine, Sidney Hook, and Alonzo Church – had already returned home and [had] started to prepare the ground for the analytic 'takeover'" (Tuboly, forthcoming in 2025).

2.4 Susanne K. Langer, the Vienna Circle, and *The Practice of Philosophy*

This section explores Kuhn's points of contact with Susanne Langer and her diverse philosophical interests, suggesting that Langer's acquaintance with Ernst Cassirer may have influenced her gradual shift—toward the end of the Second World War—away from philosophical logic. This examination further underscores Kuhn's deep entanglement within the Harvard intellectual milieu and positions him firmly within the historicist scientific strand that I have been tracing.

According to Sander Verhaegh, symbolic logic had always played a role in Harvard's philosophy curriculum, but it had a limited impact (Verhaegh 2024, 22). In time, "[T]he academic landscape gradually started to shift in the years after the First World War" (21). With that shift, the intellectual climate changed rapidly as well at Harvard:

> The department hired several philosophers who had contributed to the development of symbolic logic—H.M. Sheffer, C. I. Lewis, and A. N. Whitehead—and Harvard quickly began to be viewed as a central hub for analytic philosophy in the United States (21).

Susanne Katherina Langer was born in 1895 (as Susanne K. Knauth) had studied at Radcliffe College under Sheffer and Whitehead. Many instructors of the Radcliffe College also taught at the neighboring Harvard University. In 1926, She obtained her PhD in philosophy from Radcliffe College (established in 1879 as a college for women). Langer had Alfred North Whitehead as her dissertation advisor in 1924. Langer's parents were German immigrants even though she was born and raised in the United States. German language was spoken at home and she had a great command of the language.

Tracing Kuhn's point of contact with Langer in the archival materials, we find in one of Kuhn's papers written for Prof. Sheffer's course (mentioned in the previous section), an acknowledgement of Langer: "I have derived considerable assistance from the clear exposition in Susanne K. Langer's *Introduction to Symbolic Logic* (1937) (TSK-MIT, Box 1, Folder 3). Another point of Contact with Langer is Kuhn's reading [or sifting through] of Langer's *Philosophy in a new Key* along with other books by April 1949 (Reisch 2019, 160). The book was, among others, inspired by Ernst Cassirer "that pioneer in the philosophy of symbolism" (Langer 1948, preface). Kuhn owned a copy of Langer's translation of Cassirer's *Language and Myth* (TSK-MIT_Inventory).[10]

Because of her family background and impeccable command of the German language "Langer's intellectual context would also have included German influences on her thought: philosophers such as Ernst Cassirer, Edmund Husserl, and Ludwig Wittgenstein" (Verhaegh 2024, 26). Langer had advocated a pluralistic conception in her *The Practice of Philosophy* (1930), which may indeed be attributed to Langer's Radcliffe Harvard education. "For Lewis and Sheffer, too, had defended variants of pluralism and were known for their pragmatic conception of the *a priori* […] " (25). Quine attending meetings of the Vienna Circle in 1933 had been surprised that they were reading Langer's book [*The Practice of Philosophy*] (27). Moritz Schlick singling out the book's method "the method of logical analysis, will be the only method of future philosophizing" (27). Schlick had valued the "author's exquisite style, lucid, fluent and brilliant, has been a source of real joy for me" (27). Rudolf Carnap had expressed interest to work with Langer once in the United States and had noted in his diary that Langer "used her first meeting with the by-then German philosopher to ask him about Frege" (27). Herbert Feigl called Langer's discussion group "*Langer Zirkel*" and had "told Schlick that it reminded him of the *Wiener Kreis*". "She is an excellent woman and her versatility is admirable" Feigl writes (27). Elsewhere Feigl wrote in a letter that "She reminds me a bit of Wittgenstein in her demeanor, in her intuitive determination, and in the biblical conciseness of her statements" (28). I venture to say that Kuhn in his acknowledgement of Langer with expressions "considerable assistance" and "clear exposition" points to her unquestionable philosophical prowess observed by those who had either read or met her.

[10] I received a partial book list from MIT staff on April 15, 2025. During my stay in Cambridge, I had found out that such a list existed but I was told that it was quasi a work-in-progress by those who are cataloging books in Kuhn's possession and were given to MIT as gifts.

Langer wrote an article in *Fortune Magazine* (Langer 1944) and had been corresponding with Ernst Cassirer. In a letter on February 15, 1944, Cassirer thanks Langer for the article and writes:

> Ich habe ihn mit großem Interesse studiert und mich an der grundsätzlichen Übereinstimmung unserer Auffassungen gefreut. Die Einwilligung zu Ihrer Übersetzung meiner Schrift „Sprache und Mythos“ gebe ich natürlich mit Freuden (Cassirer 2009, 229).

Langer translated the book into English as *Language and Myth* (1946) one year after Cassirer’s sudden death. In the introduction to the book, she writes:

> Reason is not man’s primitive endowment, but his achievement. […] I offer the translation of this little study […] both as statement of a new philosophical insight and as a revelation of the philosopher’s work: his material, his technique, and the solution of the problem by a final flash of interpretive genius (Cassirer 1946, ix, x).

In conclusion, it should now be clear that Susanne Langer was highly respected in the philosophical circles of the late 1920s and 1930s, not only for her background in philosophical logic and her engagement with members of the Vienna Circle, but also for her intellectual charisma and distinctive personality. Building on Verhaegh’s assessment, one might interpret Langer’s shift in philosophical interests—roughly toward the end of the Second World War—as a move away from the dominant "trends" in philosophical logic. This transition appears to have been influenced, at least in part, by her growing interest in and acquaintance with Ernst Cassirer’s work. The points of contact between Kuhn and Langer, with which I began this section, are thus indicative of a broader intellectual shift. They further highlight Kuhn’s deep connection to the Harvard intellectual milieu and his place within the historicist scientific strand I have been tracing.

2.5 Philipp Frank at Harvard Pitching for the Unity of Science

In this section, I examine Frank’s contributions within the Harvard University setting to highlight how terms such as ‘revolutions,’ ‘meaning,’ ‘vocabulary,’ ‘worlds,’ ‘evolution,’ and alternative conceptions of ‘history’ had become “common currency” by the 1950s. I argue that this linguistic and conceptual landscape reflected a broader “philosophical

awareness" — a distinctive feature of the Harvard intellectual hub. I will then underscore Kuhn's ingenuity in not only absorbing this prevailing awareness but also in channeling it into The Structure of Scientific Revolutions*, ultimately opting for a historicist approach.*

Philipp Frank (1884-1966) had studied theoretical physics with Ludwig Bolzman at the University of Vienna in 1907. Gerald Holton recalls that Frank had arrived in the United States in 1938 for a lecture tour and on a temporary visa, but as Prag fell, his situation changed drastically (Holton 2006, 299). According to Holton, Frank started teaching at Harvard in September of 1939, which coincided with the Fifth International Congress for the Unity of Science on "Logic of Science," an event that Harvard President James Bryant Conant had opened (301). Noteworthy is Frank's very early ties with the Vienna Circle; During 1924–1936, Frank "Participated occasionally in meetings of the Vienna Circle under Moritz Schlick's leadership" (Frank 2021, xiii). Frank was also is also associated with the "First Vienna Circle":

> Between 1907 and 1912, Frank met regularly with the mathematician Hans Hahn and the economist-sociologist Otto Neurath at the Philosophical Society of the University of Vienna and in Viennese coffee houses. (Neurath and Hahn had attended the same Gymnasium, so they knew each other well and for a long time, while Frank joined them presumably during their shared university years.) Rudolf Haller (1991) called this trio the "First Vienna Circle" that preceded the better-known circle that formed around Moritz Schlick in the 1920s (6).

At Harvard (1939-1953) Frank, a former member of the Vienna Circle, continued to emphasize the "unity of science" and found it imperative that "the relation between science and human behavior must be demonstrated" and that scientists need to become "less ignorant" when it comes to history, philosophy, and sociology of science (Frank 1955, 350-351). He wrote:

> If the philosophy of science can be considered the search for the rules according to which concepts and facts interact, then the history of science is a study of how these rules originated and how they have changed. Thus, from the beginning, the history of science and the philosophy of science have been connected very closely with each other and with science as a whole (350).

Therefore, "no science teaching can be satisfactory without historical interpretation" (350). Earlier, in 1949, Frank had written:

> President J. B. Conant urged, in particular, a new approach to the teaching of science. He stressed that science teaching has to be linked up with a presentation of the historical, cultural, and psychological background of the work done by the great scientists (Frank 1949, 50).

For clarifying the context, let us fast forward in time. In an interview in 1990, Kuhn said that he had discovered while reading Aristotle that a text required interpretation (Sigurdsson 2012, 21). Kuhn had then referred to *hermeneutics* "quite well known in Europe (although I didn't know it at the time)" he had said and continued:

> It was a way of reading texts, of looking for things that don't quite fit, puzzling over them, and then studently finding a way of sorting out the pieces. I had never heard of interpretation in that sense, for I'd never read any continental philosophy (21).

This appears to be very close to Frank's thoughts. Abstract scientific presentations are "imbued with a certain philosophical interpretation which is not a result of scientific research" (Frank 1951, 30). On the ideas of positivistic philosophies outlined by A. Comte and J. Littré, Frank writes that "Their idea has been advocated more recently by men like J. Piaget and J. Dewey." [11] The latter, Frank adds, has presented the case in his book *Reconstruction in Philosophy* (1920) for "active positivism" in a strong and impressive way (30). Elsewhere, Frank writes that upon reading Carnap's *The Logical Structure of the World*, he was:

> […] reminded 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 as a direction for human behavior . I wrote immediately to Carnap, "What you advocate is pragmatism." This was astonishing to him as it had been to me. We noticed that our group, which lived in an environment of idealistic philosophy had eventually reached conclusions by which we could find kindred spirits beyond the Atlantic in the United States (Frank 1949, 33).

I strongly suspect that Frank's 'kindred spirits beyond the Atlantic' is another way of emphasizing the relevance of an interconnected "construct" that encompasses many aspect of the intellectual life including prevalent philosophical "taste" or "trends" of the time. The

[11] In the next section, I will point to Marie Boas Hall, one of the first reviewers of Kuhn's *Structure* in 1963, in connection with Comteian influences.

point worth emphasizing is that Kuhn was no stranger to these thoughts. Kuhn's early sketches of the *Structure* in the 1950s and particularly those on "revolutions" depict analogies to the arts as changes similar to changes in artistic "taste".[12] Not only Frank, but other scientists, among them the famous physicist J. Robert Oppenheimer in a contribution titled "Growth of Science and Culture" (1958) proposed the relevance of the *Zeitgeist* in view of radical discoveries and their assimilation. Oppenheimer points to Newton, Darwin, and Freud to isolate the spirit of acceptance of their discoveries: "His [Darwin] sense of the unity of nature fitted wonderfully with the romantic notion of the unity of man with nature". And for Newton, "a proof of man's reason, as a warrant for confidence in its future", whereas in the case of Freud, it was partly due to "the newly vivid idea of the continuity of good and evil, and of illness and sin, was made natural and welcome in the early years of this century by the decline of religious and secular authority" (Oppenheimer 1958, 70-71).

Frank edited an anthology titled *The Validation of Scientific Theories* (1956). In the introduction, he writes that a conference on "Validation of Scientific Theories" organized in 1953 started discussing "the reasons for the acceptance of scientific theories". "This issue was not treated", Franks adds, "from the logical viewpoint only […] the pragmatic reason of acceptance were stressed" (Frank 1956, viii). Frank writes that:

> Since about 1930, the "Unity of Science" movement has attempted to study the traits which are common to all scientific theories. The "Institute for the Unity of Science," in Boston, has organized local, national, and international meetings in which the results of these studies have been presented and discussed by cooperation with scientists and philosophers of various lines and specialties (viii).

In this publication where Frank uses the word "pragmatic", Koyré, who was also a contributor to this anthology, speaks of "trends". Koyré's contributions in this volume is titled "Influence of Philosophical Trends on the Formulation of Scientific Theories". In short, it appears that terms such as "pragmatic", "trends", or "taste" are in common use by physicists, philosophers, and historians of science, and for all practical purposes, they are not very far apart in meaning. There is another apt contribution in the Frank volume titled "The Dual role of the *Zeitgeist* in Scientific Creativity" by Edwin G. Boring:

[12] In this dissertation and in a section titled "Analogy to the Arts: Revolutions as Settling Matters of Taste", I explore Kuhn's unpublished archival materials on the subject, namely during the time of the *Structure's* first draft (this draft incidentally could even be the proto, proto *Structure*, which would date earlier than what Hoyningen-Huene introduced as the proto *Structure*).

> The *Zeitgeist* must be regarded simply as the sum total of social interaction as it is common to a particular period and a particular locale. One can say it is thought being affected by culture, and one would mean then that the thinking of every man is affected by the thinking of other men […] (Boring 1956, 205-206).

According to Boring, the twentieth century, at least since 1925, is physicalistic and behavioristic and that in a physicalistic era, physically minded scientists, choose a physicalistic definition (208). He wrote:

> Between 1910 and 1930 the Zeitgeist changed. Mind gave way to behavior. This transition was eased by the positivists who supplied the transformation equations […] by way of operational definitions of experience […] (208).

I suppose that in hindsight one could speak of Kuhn's novel insight of assimilation, *re*formulation, and *re*presentation of the awareness of his time in connection with how science worked.

One of the central questions of the "unity of science" mentioned by Frank during the 1950s, was the question of meaning as one of the most widely discussed topics. In Frank's own words: "For building up science, the creation of words and their syntax is as important as experiments" (Frank 1958, 62). Percy Williams Bridgman and others had characterized the commonalities of the sciences and the humanities as "far more fundamental and important than the differences" (Bridgman 1958, 83). Bridgman had pointed out that Einstein's relativity and that the concept of "simultaneity" as an agreed upon bit of verbal *behavior*, loses that very characteristic "outside the universe of verbal behavior" (84). Therefore, "conformity and consensus are not enough to ensure that a term can be used in the way we would like to" (84). Perhaps Bridgman is here playing with Wittgenstein's "meaning as use".

In Gerald Holton's edited collection, *Science and the Modern Mind*, Giorgio De Santillana's contribution is about history's role in referring to Galileo or Descartes: "history was a repository of classical examples" (De Santillana, 1958, 46). This is very reminiscent of Kuhn's own remark in the *Structure* (1962) that "History, if viewed as a repository for more than anecdote or chronology, could produce a decisive transformation in the image of science" (Kuhn 1970, 1). Gerald Holton in the introduction to the above collection, writes: "The

revolutions in science in our times have destroyed not only the validity of the old outlook but even the old vocabulary itself" (Holton 1958, viii). This in turn is very reminiscent of Kuhn's 'science destroys its history' (see 3.4), revolutions, and incommensurability.

Even though there are scarce references to Frank in Kuhn's early archival materials, it is important to note that Kuhn's ideas during the genesis of the *Structure* were very much informed by a mixture of philosophical ideas that encompassed among others, those of the Vienna Circle. As evidenced in the "Athens Interview,"[13] the later Kuhn said that he had "read some Philipp Frank" (TSK-MIT, Box 28, Folder [Athens]). Further, the archival materials for the early period under consideration in this dissertation, reveal that there are two references to Frank. One reference is in a document titled "Plans for Research" for the academic year 1954-55 in which Kuhn mentions an invitation from Professor Charles Morris of the University of Chicago to write the "history of science" volume for the International Encyclopedia of Unified Sciences, published by the University of Chicago Press under the editorship of Morris, Rudolf Carnap (Chicago), and Philipp Frank (Harvard) (TSK-MIT, Box 5, Folder 84). A second reference to Frank is for a course 'Natural Science 4' during 1954-55 where he among some other writers, lists chapter XIII of Frank's *Between Physics and Philosophy* (TSK-MIT, Box 1, Folder 16).

I conclude this section by emphasizing that terms such as 'revolutions,' 'meaning,' 'vocabulary,' 'worlds,' 'evolution,' and alternative concepts of 'history' were indeed integral to the philosophical awareness — or "common currency" — of the 1950s. In this light, it seems hardly far-fetched to suggest that Kuhn, as a bright and talented student and later an instructor at Harvard, assimilated and reformulated ideas already embedded in his intellectual environment. He wove them into a comprehensive narrative of the "mechanics" of scientific development — an assimilation and reinterpretation that was, nonetheless, distinctly his own.

2.6 Mach, Sarton, Conant, Kuhn

[13] In this dissertation, I have used the original materials that were copy-edited by Kuhn himself and have only compared it with the published version in *The Road Since Structure* (2002). The advantage of the original materials is of course access to comments that were designated as "off the record". If a comment happens to be "off the record", I shall consistently draw the attention of the reader to it. Otherwise, the archival material on this are almost the same as the interview conducted by Baltas, Gavroglu, and Kindi and which appeared in *The Road Since Structure*.

Beyond introducing Sarton and outlining his role at Harvard, the central aim of this section is to shed light on the presence of Machian historicism in Sarton's work and its availability to Kuhn in the 1950s. I will also address the later Kuhn's seemingly contradictory — or at best, misleading — characterization of Sarton as a Whig historian, a judgment that, I suggest, calls for historical revision.

George Alfred Leon Sarton (1884-1956) was a Belgian-born U.S. historian of science at Harvard University. He received his Ph.D. in mathematics (1911) from the University of Ghent and in 1915 arrived in the United States the international quarterly review *Isis*, which he had founded in 1912, the first periodical to coordinate the results of historical research in all the sciences. In 1936 he founded a second journal, *Osiris*, devoted to lengthier papers on the history and philosophy of science, editing both periodicals until his death.[14]

In 1912, Sarton had written to Ernst Mach[15] and had encouraged him to both become a member of the advisory committee of the *Isis*, and to occasionally collaborate with the new journal (Stadler 2018, 67-68). Interesting enough, in the editorial of the first issue of *Isis*, Sarton refers to Mach's *Mechanics* – among other Mach's resources (69). Many of Kuhn's syllabi throughout his teaching at Harvard included Mach's *Mechanics*. As I also mentioned earlier, Kuhn owned a copy of a 1923 edition of Populär-Wissenschaftliche Vorlesungen (TSK-MIT_Inverntory).

In 1916, Sarton's "The History of Science" appeared in *The Monist* where there is "a reference to Mach's method which inspired his own genetic-historical methodology" (69). In fact, according to Sarton, The *Monist* and *Isis* had "identical aims" and that both provided "a forum for the early dissemination of Mach's ideas in North America" (69).[16] In connection with the pragmatist movement, Stadler writes:

> Referring in particular to the history of natural sciences since the end of the 19th century Sarton illustrates a strong exchange between science and philosophy, but he proposes a sort of "holistic" view on this development covering the "positivist schools" of Comte and Spencer which had

[14] Britannica, The Editors of Encyclopaedia. "George Alfred Leon Sarton". *Encyclopedia Britannica*, 27 Aug. 2024, https://www.britannica.com/biography/George-Alfred-Leon-Sarton. Accessed 1 October 2024.

[15] Many years later, Sarton also invited A. O. Lovejoy to join *Isis* to conduct his own branch of intellectual history but Lovejoy had reservations and did not accept Sarton's invitation (see Daniel J. Wilson's (1980) *Arthur O. Lovejoy and the Quest for Intelligibility*).

[16] One must also note that surprisingly in Sarton's *The Study of the History of Mathematics & The Study of the History of Science* (1936, 1954, 1957) there is no mention of Mach. He mentions instead August Comte (1798-1857), Paul Tannery (1843-1904) and William Whewell (1794-1866) as the forerunners of History of Science.

> influenced the sciences. This Sarton argues, also promoted the emergence of the pragmatist movement (H. Bergson, W. James, F.C.S. Schiller) arguing for the independence of philosophy from the sciences via intuition (72).

Sarton had proposed to bridge the gap between "neo-positivists and pragmatists" with "the need for thinking *and* acting", which according to Stadler, converged nicely with the "hidden pragmatism" of Mach and the later convergence of Logical Empiricism and pragmatism (72).

Recall George Reisch's remark on the way Conant described himself as a "mixture of William James's *Pragmatism* and the logical empiricism of the Vienna Circle, with at least two jiggers of pure skepticism" (Reisch 2019, 142). Add to this, Conant's opinion and interaction with Sarton as early as 1916-1917 – to be discussed at the end of this section – at which point Conant reports meeting George Sarton for the first time (Conant 1957, 301).

Sarton was a professor of history of science during Kuhn's studies at Harvard. A position the later Kuhn said was held only by half a dozen people in the United States. Sarton retired in 1951 and died five years later. So any meaningful contact with the young Kuhn must fall within the early 1940s during Kuhn's undergraduate studies and after 1945 until 1951. From 1943-1945, Kuhn was helping with the war efforts working with Radio Research Laboratory.

"It would be difficult to exaggerate the importance of history to Mach's philosophy" writes Thomas Uebel (Uebel 2021, 87). Uebel points out that it was Paul Feyerabend "who noted that 'Mach develops the outlines of a knowledge without foundations' (1970, p. 178). Rudolf Haller (1988) combined the appreciation of Mach's historicism with that of his naturalistic anti-foundationalism" (87). Sarton indeed thought along Machian lines and his 'principle of economy': "Even so [like the artist] the scientist cannot study every fact and attack every problem; he must choose and choose and choose again.[17] His activities are continually dominated by the need of selection[18] […] The artistic creation is far more arbitrary than scientific creation, the difference between them is quantitative rather than, as is generally believed, qualitative (Sarton 1936, 1954, 1957, 51). On the same page, in a footnote, Sarton

[17] Kuhn in a letter to L. Kubie (March 17, 1955) assert that both knowledge and perception demand the "organized selectivity" (see excerpt of the letter in 2.7).

[18] Philipp Frank writes that according to Mach this desire to make use of a unified mode of expression in all fields of science is a consequence of the economical design of science. This design implies the comprehension of as many facts as possible by the simplest possible system of propositions [thereby Sarton's 'choose and choose again'] Frank, P. (1949), *Modern Science and its Philosophy*, p. 82, Harvard University Press.

tells an anecdote[19] about Einstein, the essence of which is that "science destroy its history" (see 3.4), which is the idea that Kuhn builds on to argue *against* cumulativeness and subsequently *for* revolutions. The comparison of art and science was critical to Kuhn's narrative; he not only mentioned these connections in early notes (see 4.4), but also made use of them, and together with the problems of "textbook science", tried to strengthen his case *against* "cumulativeness". For example, in the final chapter of *Structure* titled "Progress Through Revolutions" and on "science's continuing evolution", Kuhn writes:

> But those are also the years, particularly during the renaissance, when little cleavage was felt between sciences and the arts. […] that while artists aimed at representation as their goal, both critics and historians chronicled the progress of the apparently united group (Kuhn 1970, 160-161).

Kuhn concluded:

> Scientific education makes use of no equivalent for the art museum or the library of classics, and the result is a sometimes drastic distortion of the scientist's perception of his discipline's past. More than the practitioners of other creative fields, he comes to see it as leading in a straight line to the discipline's present vantage. In short, he comes to see it as [cumulative] progress (166).

It appears then, that Kuhn was aware of comparisons between the sciences and the arts – perhaps even through Sarton's work. The fact that he makes use of such comparisons to argue against the notion of cumulativeness is certainly Kuhn's contribution.

When in mid-1944, the young Kuhn was back at Harvard and signed up for a physics course (TSK-MIT, Box 28, Folder [Athens]), a book offered in homage to Sarton was published on the occasion of Sarton's sixtieth birthday titled *Studies and Essays in the History of Science and Learning* (31 August 1944). Among the contributors are Koyré and Cassirer. One of the philosophical debates in this volume is Galileo and Platonism – a theme very dear to both Koyré and Cassirer.[20] Koyré's manuscript and those of other contributors were

[19] Einstein once remarked that infinitesimal calculus would certainly have been discovered even if there had been no Newton and no Leibniz, but without Beethoven we should never have had a C minor symphony […] it remain true that Beethoven's work is absolutely individual and as such irreplaceable, while Newton's would have been replaced sooner or later (51).

[20] Incidentally, another author who is at the center of early Kuhn's attention, and one mentioned in the *Structure* is Leon Brunschvicg who wrote and article titled "History and Philosophy" on the occasion of Ernst Cassirer's Festschrift *Philosophy and History* (1936) edited by Raymond Klibansky and H. J. Paton.

eventually left out of the completed volume due to reasons mainly organizational as Sarton explains.[21]

Kuhn first began Conant's General Education and History of Science course in 1952, a year after Sarton's retirement. When writing an outline of a research proposal (TSK-MIT)[22], he pointed out that his "formal connections with the history of science were not established until 1947"; we can assume that any window of interaction between Kuhn and Sarton is further reduced, while also considering the fact that the same research proposal mentioned not only I. B. Cohen[23] but also George Sarton:

> […] I have been continually guided by the bibliographical advice of Professor I. B. Cohen of Harvard, and I have profited from the presence in Cambridge of Professor George Sarton, though unfortunately he has been relatively inaccessible during the time I have been engaged with the history of science (TSK-MIT, Box 5, Folder 84).

For the sake of thoroughness, there are two points to mention here. First, Kuhn dates the Society of Fellows on his CV from 1948-51 (TSK-MIT, Box 1, Folder 1) so this even further reduces a meaningful contact "window" to less than 3 years. Second, it may have been strategically wise to mention a prominent scholar in history of science at Harvard when writing a proposal. Gerald Holton remembers:

> We [Holton and Kuhn] also publicly acknowledged our intellectual debts to many of the same powerful scholars (among contemporaries, Koyré, Sarton, Meron, Nagel, etc., among those from whom we had courses or consulted, Quine, P., Frank, P., W. Bridgman, van Vleck, Reichard von Mises, Raphael Demos, etc.) (Holton 2012, 32).

Incidentally, Kuhn owned a 1951copy of von Mises' *Positivism: A Study of Human understanding* along with several of Frank's books as well as Sarton's *Six Wings: Man of Science in the Renaissance* (1957) (TSK-MIT_Inventory). It is important to ascertain whether Sarton's conception of history accorded or collided with that of Kuhn's, sometime during these years. Stadler has pointed out that Hayo Siemsen, in discussing differences between Mach

[21] A review written by George Sarton of *Studies and Essays in the History of Science and Learning*, edited by Montagu, M. F. Ashley, in The Philosophical Review, 1947, Vol 56, p 458, Boston: Ginn and Co.
[22] TSK-MIT, Box 5, Folder 84. The research outline is titled "QUESTION 3: ADVANCED STUDIES AND RESEARCH". On another sheet "PLANS FOR RESARCH" academic year 1954-55 has been mentioned.
[23] Leaflet and copied materials of I. B. Cohen's "Astronomical Work of Galileo Galilei" is in archival materials for 'Natural Science 2 (a)' (materials included handwritten pages dated September 24, 1947) (TSK-MIT Box 1, Folder 11).

thought that "Sarton was skeptical of psychological methods as opposed to mere historical ones and missed specific empirical experiments" (Stadler 2018, 80). Siemsen had nevertheless referred to "Sarton's evolutionary view on genesis and development as introduced in the first issue of *Isis* (1913)" (80). Kuhn had emphasized the "revolutionary". Holton remembered that:

> While lauding many aspects of Tom's writings, Steve [Weinberg] called the description of scientific revolutions "seriously misleading", insisting that changes in understanding nature "have been evolutionary, not revolutionary," and then asked: "What in Kuhn's life led him to his radical skepticism, to his strange view of the progress of science? (Holton 2012, 32).

It is puzzling, therefore, that the later Kuhn thought that "he [Sarton] certainly was a Whig historian" (TSK-MIT, Box 28, Folder [Athens]). He adds that he could have learnt "a lot of data" from Sarton, but not the sorts of things he wanted to explore (TSK-MIT). "Sarton drove people away", Kuhn said, those who wanted to study with him by telling them that they had "to learn Arabic, and Latin, and Greek, and so forth" (TSK-MIT). This is the image the later Kuhn is providing of Sarton. I am referring back to the period Kuhn could have potentially known of Sarton's attitude towards prospective students, or the possibility of any interaction he could have had with him. But let us keep in mind that this is the later Kuhn at the end of his career speaking. The early Kuhn, at least as the archival materials show for his 'H of S 105' bibliography dated Oct. 31, 1953, mentions Sarton under resources in his lecture notes:

> Medieval Science in general: Sarton: Introduction…" and "Lynn Thorndyke: Magic & Experimental Science (TSK-MIT, Box 1, Folder 21).

Also, we have Sarton's *A Guide to the History of Science*, which appears among "Books reserved for History of Science 212" in 1954 (TSK-MIT, Box 1, Folder 22). And later for the 'History 105-A' in Fall 1959-60 under Assigned Reading (pages), there are four authors: M. Clagett's *Greek Science in Antiquity*, R.J. Forbes *Man the Maker,* Sarton's *A History of Science* vol. I, and Kuhn's own *The Copernican Revolution*. And Under Recommended References, we find Sarton's *A History of Science*, vol. II among others (TSK-MIT Box 1, Folder 25). In *A History of Science* (vol. I & II), the assigned sections in Sarton's books involve Ancient and Hellenistic Sciences. So, it appears that up to the publication of the *Structure*, Kuhn was using Sarton's books and references both on Mediaeval and Ancient

Sciences on his syllabi! Unfortunately, it appears that Kuhn's strong assertions about Sarton elaborated above is at best misleading. Kuhn's practice of including Sarton's rich "data" while excluding the historical "method" of acquiring such data seems puzzling.[24] Taking an alternative route to resolving this puzzle, I ask, how important was Sarton to Harvard and the field of History of Science? James B. Conant has provided a definitive answer, which aims at Sarton's interest in what we would call 'interdisciplinarity' today. Conant writes:

> I recall his saying emphatically that the history of science could not be written by historians - one must first of all have had a scientific training. […] (Conant 1957, 301).

Elsewhere in Conant's article, referring to Sarton's letter and proposal dated 3 Nov 1938 to the President of the university, we read: "As far as parts I and II are concerned this could not be accomplished without the active cooperation of the classical and medieval department" (303). Conant continues:

> His writings were among those which brought glory to the University during a period of nearly forty years. He was one of the rare jewels in the academic crown During the second World War. […] Professor Sarton's course on the history of science, for undergraduates in Harvard College, grew in popularity. Starting with an enrollment of about a hundred, it grew until in the immediate post-war years over 300 attended his lectures. […] I recall that a number of Nieman Fellows (journalists in residence at Harvard for a year) over the years expressed great satisfaction with Professor Sarton's lectures. This judgment of older, rather sophisticated students is of interest, not only as throwing light on the success of one particular professor but also on the relevance of the historical approach to science (302).

Some scholars have pointed to "Conant's expansive vision for history of science" (Dennis, et al. 2020, 595). "The postwar era produced novel pedagogical projects aimed at integrating history and science teaching. Above all, these were shaped by the pedagogy of James Bryant Conant, who placed history at the heart of general science education" (596). But as we saw, it appears that Conant's 'expansive vision' may have been shaped, or at the least influenced,

[24] In fact, I once posed this question to Paul Hoyningen-Huene (see my interview with Paul at the end of this dissertation as appendix to which he granted me permission to include): "Not necessarily" he responded, "because the materials themselves that a Whiggish historian collects may be quite useful". Even though this response allots justification for Kuhn's assertion, it does not stand up to the historical fact of Sarton's important contributions.

by Sarton. Nevertheless, Kuhn's Structure is an individual effort at reformulation of available history of science.

According to Gerald Holton, "As we look into history, it would seem that sometimes progress in a field of learning depended on one man's incisive formulation of the right problem at the right time" (Holton 1952, 56). Holton in his *Introduction to Concepts and Theories in Physical Science* (1952) had cited Alexandre Koyré to prove that very point namely offering a way of understanding historical puzzles:

> Therefore what the founders of modern science, among them Galileo, had to do, was not to criticize and to combat certain faulty theories, and to correct or to replace them by better one. They had to do something quite different. They had to destroy one world and to replace it by another. They had to reshape the framework of our intellect itself, to restate and to reform its concepts, to evolve a new approach to Being, a new concept of knowledge, a new concept of science—and even to replace a pretty natural approach, that of common sense, by another which is not natural at all (21-22).

One could imagine, I suppose, that this could qualify as the blueprint for the *Structure*, which is Koyré's in spirit. Holton writes that Galileo will speak to us as a scientist "after the historical setting of his work has been clarified" (17).

Referring to Copernicus, Galileo, Einstein, and Bohr, Holton suggests: "perhaps we must reserve a special name for those rare mutations in science", for the "heroic effort" "to use Mr. Sarton's phrase" of one individual, without which, "the normal evolutionary progress in science might almost be as slow as that of natural evolution" (249).

In fact, Sarton's contributions not only predate but resemble Kuhn's discoveries. The fact is that during the years of the genesis of *Structure*, Sarton's work is being assimilated in Kuhn's course syllabi and also at least their mention in the context of his lectures.

Below is an excerpt from Sarton's *The Guide to the History of Science*. I clearly find a definite sense of some of the elements that Kuhn emphasized in *Structure*, namely, the 'tacit element' and the 'rule-following':

> Teachers of science […] are satisfied to insist upon the rules and precautions of definite experiments. After having completed a cycle of , say, physical experiments, students ae aware of general methods (in addition to special ones), but their awareness may remain largely unconscious or unformulated (Sarton 1952, 82).

In a chapter titled "The Tradition of Ancient and Mediaeval Science", Sarton writes about the necessity of historical retrospection as well as its problematic aspects. In his words "easy" and "superficial" historical 'retrospection':

> The historical difficulties of such superficial retrospect are not great, the sources are easily obtainable. The chronological basis is relatively easy to establish. The fundamental questions "when did that happen? where?" are easy to answer. The questions "why?" and "how?" are more difficult of course […]. (17)

Or in the chapter titled "Science and Tradition" he writes that "The main 'cuts' [revolutions] in social history are due to inventions and discoveries" and that "These changes can be symbolized by a series of revolutionary discoveries" (3). Sarton emphasized "the integration of scientific teaching in all its forms" and spoke of interconnectedness of "the unity of science" and the "spiritual life" (64). "Seventeenth century men of science were standing upon the shoulders of mediaeval giants", despite some believing that they were following "Greek antiquity" (35). Sarton in his books on science and history had often used the term 'prejudice'[25]. The citation below involves the very points Kuhn had raised, namely lag in theory acceptance, and non-cumulativeness:

> Young historians of science, know only the bare outline as may be read in a short primer, may fancy that the development of science was much simpler than it really was; that it was logical, continuous, straight forward. Nothing is further from the truth. […] The notion that the trajectories of planets must be circular retarded Kepler's discovery for centuries, though Apolonios had prepared the mathematical basis of it. That is the classical example of inertia due to prejudice, but there are plenty of others (38) Each discovery was delayed by a kind of intellectual inertia, and when it was finally made, its acceptance was delayed by the same inertia, the refusal to experiment (or even to observe) and to abide by the experimental results (35).

Kuhn also used in numerous places in the Lowell Lectures the term 'prejudice':

> There is always a divergence between the results of scientific experiment and the predictions of scientific law, and our orientations, which in this case appear quite literally as prejudices […] (Reisch 2021, 82).

Reisch, in the introduction to Kuhn's Lowell Lectures given in 1951, also points to this term used by Kuhn:

[25] See for instance *The Life of Science: Essays in the History of Civilization* (1948)

> Over the course of his eight lectures, Kuhn calls them "preconceptions," "prejudices," "theories," "points of view," "orientations," "metaphors," "meaning systems," "mental frameworks," and "behavioral worlds." They drive science forward by shaping how scientists think and reason, how they perceive nature and events […] (xvi)

The above are just instances where Kuhn seems to have adopted Sarton's terminology.

Sarton also spoke of scholasticism and its "tendency to exaggerate deductive reasoning" and mocked the schoolmen: "The experimental basis of mediaeval schoolmen was pitifully, ridiculously, small, but the main point is this, that no matter how large that basis be its fertility and efficacy are limited" and in the absence of experimental means leads to "fallacies and nonsense" (Sarton 1952, 36). In the Structure, citing A. R. Hall, Kuhn pointed to this very issue albeit for a different period:

> The state of Ptolemaic astronomy was a scandal before Copernicus' announcement (Kuhn 1970, 66).

In the following decisive quotation, Sarton in the introduction to his 1952 book, appears to be speaking both of revolution ("cuts"), and "incommensurable worlds"; worlds divided by size, structure, and meaning:

> Think of the "Weltanschauung" or scientific outlook before and after COPERNICUS, before and after GALILEO, before and after NEWTON, before and after DARWIN. Each of these men made a new gigantic "cut" in our fundamental conceptions. They did not change the world, but they changed so profoundly our viewing of it, that it was as if they had moved us into another one. [...] The change might be one of size, or structure, or meaning (3).

It may also be the case that scientific progress is, for Sarton, the progress in the way we view the world, rather merely in technical or specialized know-how. In other words, scientific progress is a function of the conception of truth at any given time. Sarton warns that "in spite of the revolutionary nature of science, or rather because of it" we should be wary of breaking with the past if we wish to live "good and noble" lives (7). Otherwise, we are paving the way to "fall into scepticism and cynicism"[26] (66).

[26] Recall also Kuhn speaking of cynicism in his graduation address in 1943 as part of Harvard "noble" tradition, if you will.

In conclusion of this section let me add that in the Athen's interview, following a question by Kindi and Baltas related to "historicist turn" specifically on those who followed Kuhn, he says:

> […] but it struck me very forcefully that all of them entirely dropped the problem of meaning, when they made that turn, and that they therefore dropped incommensurability, and that they therefore were back having eliminated the philosophical problematics (TSK-MIT, Box 28, Folder [Athens].

But as we saw, Sarton not only mentions what echoes scientific revolutions, or the associated "cuts" between them, but he also unmistakably points to the resulting world as one distinct in meaning. Sarton, certainly did not assimilate these ideas in a whole as Kuhn did after him. Illustrative of Sarton's influence on Kuhn is reflected in one of the very earliest reviews of Kuhn's *Structure*, by prominent historian of science Marie Boas Hall[27]. In 1963, only one year after the publication of the *Structure*, she wrote:

> THIS closely reasoned monograph (prepared for and also issued as a fascicule of the International Encyclopedia of Science) will undoubtedly remind historians of Crane Brinton's [[28]] Anatomy of Revolution [1938].[29] Since the origins of this work lie, as Mr. Kuhn tells us in his preface and his friends will remember, in the years when he was a junior fellow at Harvard, the resemblance is undoubtedly the result of the remarkable impression that Brinton's book made on most young historians in the postwar years. The origin of The Structure of Scientific Revolutions explains why the questions there raised are similar to those asked by recent converts to

[27] A biographical detour of the reviewer reveals that Marie Boas Hall attended Ratcliff College (an all-female college taught by Harvard professors) where Susanne K. Langer had also studied. In 1952, she wrote her dissertation on Robert Boyle. Boas Hall was an American historian of science known for her studies of 16th and 17th centuries science and the author of *Robert Boyle and Seventeenth Century Chemistry* (1958). During the 1950s and early '60s, sections of both Marie Boas Hall (pp. 48-74) and Alfred Rupert Hall's (to whom she was married) books were on Kuhn's syllabi under assigned readings. A. R. Hall had written *The Scientific Revolution 1500-1800: The Foundation of Modern Scientific Attitude* (1954). (the course syllabi are located in TSK-MIT, Box 1, Folders 1, 25, 30).

[28] It does not appear that Kuhn used Brinton's *Anatomy of Revolution*. But he did, in fact, listed in 1953-54, C. Brinton's book *Ideas and Men* (1950) (together with J. H. Randall, Jr.'s *The Making of the Modern World*) 'On Social, Cultural, and Intellectual Backgrounds of Modern Science", under Useful References. And again in 1954-55 (this time he assigns chapter 18 [11] of this book) and in 1955-56. (TSK-MIT, Box 1, Folders, 14, 16 & 17). Finally, a biographical note on C. Brinton reveals that he was in the history department at Harvard from 1923 and became a full professor in 1942, and also the Chairman of the Society of Fellows (Kuhn finished his undergraduate studies in 1943 and became a Junior Fellow at the Society of Fellows in 1948-51). But what was Kuhn looking for in Brinton's *Ideas and Men*? In this connection, Kuhn referring to Newton's world-machine, was asking his students: "Is it justifiable to develop a "world-view" from a scientific theory: are there respects in which such a development is invalid and even dangerous?" (TSK-MIT, Box 1, Folder 16).

[29] In *Anatomy of Revolution*, Brinton acknowledges Lawerence Joseph Henderson: "(A fact in the natural sciences, we define with L. J. Henderson as 'an empirically verifiable statement about phenomena in terms of a conceptual scheme')" p. 11, Vintage Books, 1938/1952, New York.

> the history of science who have been trained as scientists and by historians strongly influenced by sociology; indeed Kuhn regards this book as a contribution to the sociology of knowledge (Boas Hall 1963,700-701).

She then refers to a point of mutual departure between Kuhn and Sarton:

> The author will not mind, I feel sure, if I say that I detect the influence of George Sarton as well. Not that Sarton practiced this sort of analysis, but he was, like so many continental historians of science of his generation, strongly influenced by Auguste Comte and often stressed the Comtian view of the triform development of the sciences (from theological to metaphysic to positive) as well as their essentially cumulative nature. Kuhn here offers a replacement to Comte: a view of science as developing not in a smooth and gradual ascent, but in a series of discontinuous jumps, named scientific revolutions (700-701).

The "Sarton element" in connection with Kuhn is hardly researched and the question one must ask is whether Sarton's influence on Kuhn was much more profound than he ever admitted or is known to date. It also appears therefore that Kuhn calling Sarton Whiggish is solely a strategic move to distance himself from those who failed to devise a systematic philosophy of science from available rich scientific historicist data as Kuhn himself believed he eventually did. In fact, we may find precedence with such strategic moves in philosophical literature. Martin Kusch has associated one such move with Husserl's and psychologism. Kusch writes:

> Husserl also employed a clever strategy to distance himself and of choosing his allies. As presenting himself as a converted earlier proponent of psychologism, he visibly and clearly separated himself from his academic teachers like Brentano and Stumpf, who were anything but hostile towards experimental psychology. […] As concerns allies, Husserl expressed sympathies with Neokantians […] Yet there too his approval was mixed with challenge and provocation, since Husserl suggested not even the Neokantians were immune to psychologistic disease (Kusch 1991, 53).

In a similar vein, Kuhn indeed used this very strategy when speaking of Sarton; Kuhn said that "[…] in some very important sense, he was a great man but he certainly was a Whig historian […]" (TSK-MIT, Box 28, Folder [Athens]). In fact, such a strategy – and associated statements – may even roughly satisfy Kusch's 'Philosophical Facts'. [30]

[30] Statements in philosophy that Kusch has called "philosophical facts" must satisfy the following criteria: "(i) being widely accepted, i.e., being incorporated into the standard textbooks, (ii) being such that they cannot be ignored or bypassed whenever one works in the respective field, and (iii) can be used without further arguments to support new statements" (Kusch 1991, 46).

2.7 Kuhn's Tug of War: Arthur Lovejoy and Bertrand Russell

In this section, I argue that at the heart of the genesis of Thomas Kuhn's landmark ideas in The Structure of Scientific Revolutions *lies a philosophical tension between Arthur Lovejoy and Bertrand Russell: the former championing a historical treatment of science, the latter embodying a purely logical conception of the scientific enterprise. Drawing on previously unpublished archival materials at MIT, I make the novel claim that the early Kuhn's thought can be associated with the critical realism espoused by Lovejoy. I focus on A. O. Lovejoy (1873–1962) and Bertrand Russell (1872–1970) as two pivotal figures in Harvard's American realism debate, aiming to flesh out the two poles of philosophical interest that shaped the young Kuhn during the 1940s and early 1950s, prior to the publication of* Structure *(1962). I demonstrate that Kuhn exhibited a strong affinity for Lovejoy's thinking—particularly as reflected in* The Great Chain of Being *(1936)—while showing marked antipathy toward Russell's views, especially regarding the methodology of science. I conclude that Kuhn's historical approach to science did not emerge in isolation but was deeply rooted in his Harvard intellectual background.*

Arthur Oncken Lovejoy (1873-1962)[31] was a professor of philosophy at John Hopkins University from 1910-1938. He was born in Berlin, Germany, to a German mother and an American father from Boston. Lovejoy's parents soon returned to the United States as he was still a child. He studied in Berkeley at first and then in 1895 signed up to study at Harvard where he studied under William James and Josiah Royce. According to Daniel J. Wilson, Lovejoy had taught twice at Harvard in the thirties, in 1932-33 and 1937-38 (Wilson 1980, 190).

Kuhn had a strong affinity for Lovejoy's thinking, especially for his *The Great Chain of Being* (1936), the content of which was indeed presented at Harvard as part of William James Lectures in 1933. There is direct mention of the importance of A. O. Lovejoy to Kuhn's thought and the genesis of the *Structure.* There are also references to Lovejoy in Kuhn's first book, *The Copernican Revolution* (1959). In the preface to the *Structure* Kuhn writes:

> […] their work [Koyré, Meyerson, Metzger, and Maier's] together with A. O. Lovejoy's *Great Chain of Being*, have been second only to primary

[31] [Sarton's announcement of Lovejoy's journal in ISIS 1940] *Journal of the History of Ideas*, edited by ARTHUR O. LOVEJOY. Volume I, no. I, I28 p. College of the City of New York, I940. ISIS "It is one of the purposes of this journal to contribute, so far as its resources permit, towards such a more effective liaison among those whose studies have to do with the diverse but interrelated parts of history, in so far as history is concerned with the activities of man's mind and the effects of these upon what he has been and has done-or (to change the metaphor)to assist towards more cross-fertilization among the several fields of intellectual historiography. It is hoped that the journal will serve-among other things-as a useful medium for the publication of researches which traverse the customary boundary-lines, or are likely to be of interest and value to students in other fields than those in which they primarily lie" (Isis 32, 483).

> source materials in shaping my conception of what the history of scientific ideas can be (Kuhn 1970/1962, Preface).

My own research of Kuhn's archival materials shows that over the years while teaching Conant's history course, Lovejoy's book regularly appeared on Kuhn syllabi, and several of its chapters were frequently assigned for reading.

With Henry Sheffer and D. C. Williams, Kuhn appears to be taking courses at Harvard within and as part of a particular trend, which for the most had its emphasis on logic, inspired by Russell. It is exactly at this juncture that Matthias Neuber's work on the origins of analytic philosophy in the United States and particularly at Harvard is instructive:

> Russell was instrumental in preparing an analytic-continental divide within American philosophy itself (Neuber 2023, 127).

2.7.1 Harvard Critical Realism

The later Kuhn, particularly in *The Essential Tension*, singles out Lovejoy and especially Koyré as those he had learnt the most from: "The search for the integrity of a discarded mode of thought" (Kuhn 1968/rev. 1976, 11). In *The Great Chain of Being* and in exploring the history of ideas, Lovejoy introduced his concept of the "unit idea", but here we shall neither explore this concept, nor the critique Lovejoy's 'unit idea' has endured [32]. A brief mention, however, may be in order, only to move on to examine aspects of Lovejoy's work that appear to have influenced Kuhn and the genesis of the *Structure*:

> In dealing with the history of philosophical doctrines […] it cuts into the hard-and-fast individual systems and […] breaks them into their component elements, into what may be called their unit ideas (Lovejoy 2001/1936, 3).

In an article titled "Russell and American Realism", Neuber discusses two varieties of early 20th century American realism, namely, neo-realism and critical realism. Principally, they addressed the theory of perception. While neo-realists defended a "direct theory of

[32] See Daniel J. Wilson's article "Lovejoy's The Great Chain of Being after Fifty Years" (1987).

perception, the critical realists promoted a representational account of perception" (Neuber 2023, 127). Neuber tries to show that:

> Critical realists' reservations pertained primarily to a more traditionalist—'continental'—account of philosophy, whereas the neo-realists proved instrumental in setting the stage for later full-blown analytic philosophy in the United States (127).

Indeed, Russell's own take on Harvard philosophy and its neo-realists as well as other philosophical competitors confirm this point. Russell's lectures were delivered as the Lowell Lectures in Boston in March and April of 1914:

> Among present-day philosophies, we may distinguish three principal types, often combined in varying proportions by a single philosopher, but in essence and tendency distinct. The first of these, which I shall call the classical tradition, descends in the main from Kant and Hegel; it represents the attempt to adapt to present needs the methods and results of the great constructive philosophers from Plato downwards. The second type, which may be called evolutionism, derived its predominance from Darwin, and must be reckoned as having had Herbert Spencer for its first philosophical representative; but in recent times it has become, chiefly through William James and M. Bergson, far bolder and far more searching in its innovations than it was in the hands of Herbert Spencer. The third type, which may be called "logical atomism" for want of a better name, has gradually crept into philosophy through the critical scrutiny of mathematics. This type of philosophy, which is the one that I wish to advocate, has not as yet many whole-hearted adherents, but the "new realism" which owes its inception to Harvard is very largely impregnated with its spirit (Russell 1914, 3-4).

A key point is that the tradition of Kuhn's teachers shows them as students of the old pragmatists and the tradition of pragmatism at Harvard. Royce's advisors were William James, Hermann Lotze, Charles Sanders Peirce, Wilhelm Windelband, and Wilhelm Wundt. The students of Royce were Henry Sheffer, and C, I. Lewis, and George Santayana. In this older tradition, there is Morris Raphael Cohen (M. R. Cohen) who studied under Royce and William James. We have Ernest Nagel whose doctoral advisor was John Dewey. William James and Royce were also advisors to A. O. Lovejoy. Nevertheless, it appears that among the students of the old Harvard pragmatists, a split, or a gap began to form, which eventually made them defenders of two distinct 'realists' camps. According to Neuber:

> While neo-realism, with its (Russell-inspired) emphasis on logic and formal analysis, may seem downright 'progressive', critical realism, with its emphasis on epistemology and (anti-Russellian) representationalism, appears rather 'regressive'. To be sure, this should be taken with a grain of

> salt. But there are indications that such a divide existed (Neuber 2023, 131).

It seems, then, that with the emergence of these two distinct camps, we have already moved at least a step beyond the old Harvard pragmatism and entered a new era. In this period, remnants of both the old pragmatism and the continental heritage—present at Harvard since its early days (see 2.2)—continued to survive, taking up the challenge of developing an "early brand" of analytic philosophy at the beginning of the last century.

Neuber rightly points to a first wave of realists and their manifesto "The Program and First Platform of Six Realists".[33] Among the six names mentioned by Neuber, three are from Harvard, namely psychologist Edwin B. Holt (1873-1946). William Pepperell Montague (1873-1953) and Ralph Barton Perry (1876-1957) (128).

> The second realist wave in early twentieth-century American philosophy culminated with the volume *Essays in Critical Realism: A Co-Operative Study of the Problem of Knowledge*, published in (1920) (128).

2.7.2 The Positions Advocated by Critical Realists

Interestingly, we now encounter the name of Arthur O. Lovejoy (1873-1962) as one of the authors of the above volume associated with *critical realism*:

> From the beginning there were problems. Although the group was formulating a position in opposition to the platform of the new realists, they could not all agree to call themselves 'epistemological dualists' or 'representative realists'. Indeed some (C. A. Strong and Drake) were entirely opposed to properly stated version of epistemological monism. By the time the volume appeared they had split into two groups, the essence wing (Santayana, Drake, Strong and A. K. Rogers) and the reference wing (Lovejoy, R. W. Sellars and J. B. Pratt) (Evans 1981, 285-286).

Let me now interject a caveat that while discussing critical realism as described by Sellars, we must be aware of its nuances:

> The former group believed that the data of perception had the nature of essences or universals (in occasional formulation, propositions), and as such, could be identical with characteristics of physical objects (or, in the case of propositions, states of affairs). The reference wing took a more conceptualistic stance; they advocated directness of reference, but not literal identity between mental content and external object (286).

[33] Edwin B. Holt, Walter T. Marvin, W. P. Montague, Ralph Barton Perry, Walter B. Pitkin and Edward Gleason Spaulding.

Lovejoy thus belongs to the reference group, which, as I intend to show, aligns closely with Kuhn's ideas expressed in an important letter to Lawrence S. Kubie dated March 17, 1955 (see 2.7).

For now, let us explore critical realism as formulated by Roy Wood Sellars who was himself part of the debate:

> We may say that the gist of critical realism is an analysis of the nature and mechanism of knowing. Its thesis is that human knowing is a direct knowing of objects-this against representative realism as ordinarily interpreted-and yet that this knowing is mediated by logical ideas (Sellars 1929, 440).

Now, we need to follow Sellars a bit further to be able to place Lovejoy's position in this atmosphere of debate:

> In other words, the critical realist is convinced that a more careful analysis of the mechanism of knowing enables him to accept the transcendence of the object known. It is this transcendence which differentiates cognitional givenness (knowing) from existential givenness (experiencing). The human mind has developed methods and mechanisms by which it can know that which is genuinely external. The very nature of man as a living organism necessitated this feat.

Above, Sellars is apparently appealing to a type of *naturalism* in dealing with perception.

> The defeat of traditional representative realism [of Lock] led to the belief that the category of substance must be relinquished. […] It has always been my thesis that the influence of Berkeley led to a neglect of the total experience of perception. The *Gestalt* or configuration with its categories was abstracted from. It has always been this total configuration with its meanings and distinctions which the critical realist has had in view (443-444).

J. B. Pratt, in turn, spoke of "extreme dualism in Lockian epistemology" (Pratt 1920, 89) to go beyond it:

> Critical realism differs from both [Lock's realism and neo-realism] in insisting that the quality-group which one finds in perception is not the object of perception but the means by which we perceive. By adopting this view the critical realist is able to avoid the difficulties about perception and error which (as other contributors to this volume will make plain) render neo-realism altogether untenable, and at the same time escape from the falsely subjective Lockian view that we perceive only our perceptions and are thus imprisoned within our ideas (97).

Here, Pratt defines 'concept' for critical realists:

> The concept is thus not the object of one's thought but *the means* [italics mine MNB] of one's thinking. (97)

Pratt underlines the position of critical realists by making two illuminating points, namely, 'opinion' and 'common sense', which bring us a step closer to Kuhn:

> […] Before leaving the critical realist's view of knowledge I should add that he agrees with Plato, not only in maintaining that knowledge is opinion, but in insisting that it must be " true opinion with reason." (98) And as to the means of judging whether or not an opinion is true, the critical realist has nothing novel or ingenious to suggest, but merely points to the common methods of experience and reasoning which scientists, historians, judges, juries, and business men regularly use (99).

But this is also the gist of Kuhn's thought and his emphasis on both 'opinion' or 'belief', and common sense in his correspondence with L. Kubie on March 17, 1955:

> […] I doubt that there is (even as an ideal) an "undistorted" view of a unique "given" world. I find it more useful to suppose that either: i.) The world itself is shaped by our beliefs, and without beliefs there is no perception and no world (except, perhaps, James' "Bloomin' buzzin' confusion); or ii) There is a fixed world, but it is too vast to be an object of knowledge or even of perception, both of which demand the organized selectivity of a partial perception. I prefer (i) because, despite its paradoxes, its doesn't introduce any purely metaphysical "extras." But for most purposes the two statements are equivalent, and (ii) is certainly more compatible with common sense (TSK-MIT-Box 5, Folder 67).

I now turn to the interpretive aspects of perception, with which, I argue, Kuhn identified closely with the critical realists. Here, Sellars introduces a critique of Russell—an argument that, as we shall later see, forms the essence of Lovejoy's famous *The Revolt Against Dualism* (1930). According to Lewis S. Feuer, Lovejoy's aim was "[…] to show that when Russell, Whitehead, and Dewey tried to overcome or obliterate the distinction between mental and physical existents, their devices were specious" or empty (Feuer 1977, 358). This critique of Russell becomes a focal point for Kuhn, and I harbor the thesis that it was this very critique that propelled Kuhn further toward Lovejoy's mode of historical thinking, particularly as articulated in *The Great Chain of Being* (1936).

An encounter underscoring further parallels with Kuhn's notion of 'common sense' is one between Lovejoy and Carnap during the American Philosophical Association (Eastern Division) meeting of December of 1935 is noteworthy. Carnap presented Testability and

Meaning, and Lovejoy gave a response to Carnap with "An Examination of Logical Positivism".[34] On the encounter, Carnap had made a note in his diary:

> Zuerst unwichtiger Vortrag Burnham, Ursache in der Geschichte Dann mein Vortrag „Testability and Meaning" (einige Gedanken aus dem großen MS). Dann Spricht Lovejoy (Programmänderung) ausführlich über mein großes MS Testability. Er meint, nun ist alles Konventionell, und die Metaphysik können wir nicht mehr als sinnlos hinstellen. Ich antworte dann ausführlich auf ihn, sowie auf Bemerkungen von Damos, Weiss, Savery, und anderen (Carnap 2022, 733).

Lovejoy's sarcasm in his response is indicative of his philosophy. According to Lewis Feuer (1912-2002), Lovejoy as a student of William James in the 1890s "had joined with his generation at the turn of the century against absolute idealism" (Feuer 1977, 358). In short, for Lovejoy, a new philosophical "stance" was replacing cherished reason and common sense:

> Several incidents took place that year and later in the mid-thirties, when Lovejoy was again at Harvard, that poignantly underscored the rift between the "critical realist" and the new generation of philosophic militants. […] (359) A philosophy was becoming a stance rather than one's most sincere reasoned standpoint (361).

This is further related to the 'interpretive response' that R .W. Sellars as a critical realist had advocated as early as 1929:

> But, surely, perception is more than this. It is an interpretative response to an object. The stimulus aspect is just the beginning of true perception. The empiricist tradition never did justice to the act of cognition because it could not understand it. It broke down the function into mental elements. I must frankly confess that the psychology of many neo-realists including Mr. Russell has appeared to me very faulty (Sellars 1929, 452).

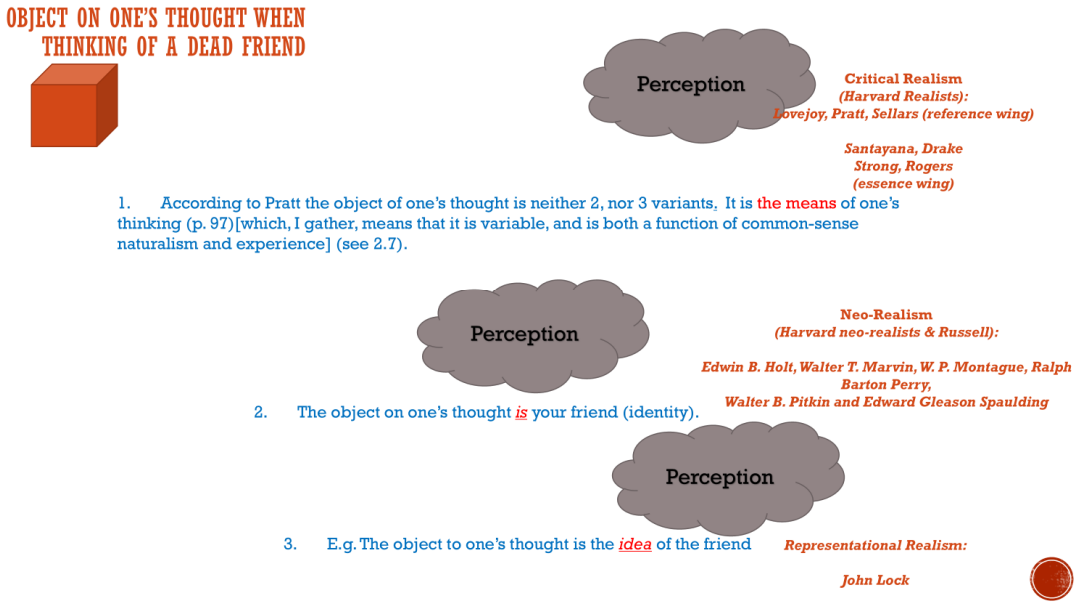
But Sellars also emphasized that critical realists were not Kantian in any shape and form:

> The critical realist believes in transcendent, physical objects which are known. These objects are present to the mind (known) but not in the mind. They are not noumena in any Kantian sense. The whole position is a frank physical realism very different from Kant's construction (451).

Finally, to complete our discussion of "common sense," we turn to the views of contemporary philosophers such as Alexander Bird. Bird characterizes Kuhn's "naturalistic approach to the philosophy of science" as a "truly innovative and radical contribution" (Bird 2004,

[34] I have not been able to locate the content of this talk, but it could perhaps be available through John Hopkins University Archives for Arther O. Lovejoy. I have reflected, however, the observations of Lewis, S. Feuer who was apparently present during the encounter.

68). Yet in light of the foregoing, Kuhn's achievement may be better understood as emerging from the broader philosophical sensibilities of the period prior to *Structure*.



2.7.3 Critical Realism and its European Roots

According to Neuber, critical realism "had its origins in Europe as well, particularly in the writing of thinkers such as Alois Riehl, Oswald Külpe, and the early Moritz Schlick" (Neuber 2023,132). Lovejoy's *The Revolt Against Dualism* (1930) meticulously outlined the tensions epistemological monism, that Lovejoy attributes to Russell and Whitehead, had levied on those who argued for critical realism. He writes:

> Lovejoy in his later—quite famous—*The Revolt Against Dualism* (1930), emphatically justified and strongly defended what he saw as the unbridgeable gulf between the epistemological dualism of the critical realists, on the one hand, and the epistemological monism of the rivaling neo-realists, on the other. (128).

In a sense, then, this narrative depicts the very origins of American philosophical thought going back to the old pragmatists starting with William James in its problematic philosophical tug-of-war with the British empiricism. Rudolf Metz, a professor from Heidelberg who reviewed Lovejoy's book in 1932 shortly after its publication in the United States in 1930, highlights this point as "die von uns zu wenig gekannte erkenntnistheoretische Problemlage des angelsächsischen Denkens der Gegenwart" (Metz 1932, 150). This, I suspect, confirms, the existing inner tensions of homegrown *American* philosophy primarily at Harvard with

its counterpart in Britain. At first sight, it may appear that early in the century, the "continentalists" were defending the remnants of the European philosophical tradition they had inherited from their European fathers. But it is important to keep in mind that even though, the roots of critical realism may have lied in Europe, *American* critical realism was certainly non-Kantian as Sellars has emphasized.

2.7.4 Critique of Russell in Kuhn's Lecture Notes

Lovejoy in his *The Revolt Against Dualism* (1930), dedicates two chapters (out of 9) to rebuttals of the Russellian neo-realist position; the chapters are titled "Mr. Bertrand Russell and the Unification of Mind and Matter: I" and "Mr. Bertrand Russell and the Unification of Mind and Matter: II". In Kuhn's lecture notes labeled "Natural Science 4, 1952-56" we encounter the following four-page document, an excerpt of which is cited below:

> Bertrand Russell is trying to establish a cause and effect relationship between the material world and the mental world of the scientist. His basic fault is that he assumes both of these can exist in some absolute form; that is, they can both be defined. He also assumes that all scientific conceptual cases develop the same way. In order to examine his theory more closely, a reference to several historical situations might be helpful (TSK-MIT-Box 1, Folder 16).

It appears that this document is Kuhn's own response – over four typewritten pages – involving a question he had posed as, an exam question allotting 60 minutes for response, to his students on several occasions in the early to mid-part of the 1950s. Below, the entire exam question.[35]

(60 minutes) Bertrand Russell has described the development of scientific conceptual schemes in terms of "the three main stages: the first consists in observing the significant facts; the second in arriving at a hypothesis which, if it is true, would account for these facts; the third in deducing from this hypothesis consequences which can be tested by observation.
Russell's "scientific method" has been criticized as one which never has and never will work –

> The first stage: there is no conceivable way of determining which "facts" are "significant facts" before arriving at a hypothesis or theory. Facts become significant only in their relation to a theory, and they change their significance with a change of theory.

[35] The question relates to chapter II of Russell's *The Scientific Outlook* (1931) titled "Characteristics of Scientific Method".

> The second stage: there are always a number of hypotheses that will account for a given array of facts; there is no known way of determining how closely each must "account for these facts."
>
> The third stage: no major hypothesis can be "tested" as such by observation. The "consequences which can be tested by observation" are associated with the original hypothesis only through a chain of reasoning involving a number of untested and often unrealized auxiliary hypotheses.

Documenting your essay with suitable illustrations from the case histories, discuss the validity of these criticisms of Russell's scientific method. How should you amend Russell's definition to improve it? (TSK-MIT, Box 1, Folder 16).

His own type-written response touches upon each and every point of the questions asked. In fact, the points of objection are given in the formulation of each stage. I am not going to refer to all of the points he addresses, but it is worth noting that the entire page 3 is for the most part on "the social and historical dependence of both scientist and the theory", or "The truth of a theory is relative to its usefulness". We also encounter in this document: "By this time one begins to suspect that Russell's description is too easy. It is inflexible; and does not allow for enough variation." On the final page of this document, we can read:

> [it would perhaps be better] not give a pattern which would be applicable to any conceptual scheme, because I don't think there is one. *Any attempt to categorize a repeated development almost invariably ends up being too restrictive* [my emphasis MNB]. The relation between thought and matter is also much more complex than Russell suggests. The most that anyone can do is study several different developments and not generalize. It is almost sure to be fatal. The only way this can be avoided is by making the generalization as broad as Conant's or as narrow and modest as Priestley (TSK-MIT, Box 1, Folder 16).

At this point, I would like to add two minor speculations and two important observations. First, what I have marked in italics in the citation above likely reflects how Kuhn conceived of Whig history. Second, the reference to Conant may allude to the superiority of the case study method—or to case histories more generally—though this remains speculative. Third, in the Athens interview and toward the end of his career, Kuhn stated that he had read Russell's *Knowledge of the External World* along with "quite a number of others of the quasi-popular, quasi-philosophical works" (TSK-MIT-Box 28, Folder [Athens]). To this list, it is now possible to add several additional titles: *Mysticism and Logic* (1929), the *Introduction* to the second edition of *Principia Mathematica* (1925), *Principles of Mathematics* (1938), *An Introduction to Mathematical Philosophy* (1924), and *The Scientific Outlook* (1931). All

except the last were cited as references in Kuhn's undergraduate papers. Fourth, and finally, it is significant that Kuhn did not assign any of Russell's works—except *The Scientific Outlook*—either as required or recommended reading during the early 1950s (1951–1956), when he was teaching Conant's course. Notably, around Chapter II of *The Scientific Outlook*, Kuhn formulated a critical examination question for his students, as discussed above. In short, Kuhn, like Lovejoy, Sellars, and others in the critical realist tradition, maintained a critical stance toward Russell.

Kuhn used Lovejoy's *The Great Chain of Being* (1936) especially the first four chapters repeatedly. On at least two occasions, for example, for the course in 1953-54 (Natural Sciences 4), he describes chapter 4 of the book, which he lists under term paper 'Suggested Topics': "The most stimulating and original, but also the most abstract and difficult of these sources" (TSK-MIT).[36] On another occasion in the Fall of 1955, he even assigns a large chunk of the book (pages 3-143) for the Reading Period, which was time set aside by Harvard University for students during the semester to do readings of selected works (TSK-MIT-Box 1, Folder 23).

It is well known that Kuhn taught historical case studies of science during the early 1950s. Aspects of his purely philosophical thinking from this period, however, appear elsewhere, particularly in his correspondence with friends and acquaintances. What follows is a significant example of such correspondence. In reviewing a letter Kuhn wrote to Lawrence Kubie—mentioned earlier—I will draw parallels to Arthur O. Lovejoy's *The Revolt Against Dualism* (1930) in order to show that the early Kuhn, as represented in *The Structure of Scientific Revolutions*, appears to share a similar philosophical stance. Specifically, this concerns the central tenet of the book: the notion of "world changes," or incommensurability. The letter in question outlines the assumptions underlying the concept of the "real" prior to any investigation, revealing important aspects of Kuhn's realism. This realism, I argue, is inspired by Lovejoy's philosophy and the broader Gestalt of critical realism.

[36] TSK-MIT, Box, Box 1, Folder 16. Specifically, on the topic of religious reactions to the Copernicus system, he assigns 3 books: The other two books are D. White's *History of Warfare of Science with Theology* (Chapter 3), and D. Stimson's *The Gradual Acceptance of the Copernican Theory of the Universe.*

2.7.5 Kuhn's Tug of War Resolved

In what follows, the essence of the resolution I will be arguing for, centers on reconciling two seemingly opposed approaches to understanding science and knowledge: the strictly rational, empirically grounded methodology and the recognition of intuitive, non-rational processes as essential to scientific discovery. On one side, there is the traditional view—exemplified by figures like Russell—that emphasizes the importance of objective, rational, and historical data to capture an un-distorted picture of the world. On the other side, influenced by Lovejoy's critical realism, there is the view that emphasizes non-rational, intuitive processes (or "unconscious symbolic processes") as fundamental to creative scientific thought and as shaping how we come to know reality. In detail, I will argue that this tug-of-war plays out in several key ways:

Rational Validation vs. Intuitive Insight:
Kuhn's letter to Kubie expresses skepticism about the possibility of an "un-distorted" view of a given world, suggesting that what we perceive is already mediated by non-rational, intuitive processes. This contrasts with the idea that only the rational, consciously validated processes can yield truth. Lovejoy's stance, as cited, argues that the intuitive grasp of reality—which might be independent of, or even prior to, rational thought—still plays an indispensable role in our understanding.

The Role of "Non-Rational" or "Unconscious" Processes:
Both Kuhn and Lovejoy argue for the necessity of acknowledging non-rational elements in scientific thought. Kuhn's emphasis on intuitive processes being later checked by rational thought reflects a move away from the strict empiricism of the neo-realist perspective (as typified by Russell) toward an appreciation of the creative, non-linear ways in which scientific ideas emerge. In this respect, Kuhn's ideas about the shaped, belief-dependent nature of the world prelude his later concept of incommensurability in scientific revolutions.

Historical and Philosophical Context:
The tug-of-war is also evident in the contrast between a static, linear conception of scientific progress and a dynamic, historically contextualized process where discontinuities (or "crises")—long acknowledged by Lovejoy—are the very catalysts of revolution in scientific paradigms. Kuhn's incorporation of these insights into his work demonstrates his resolution of this conflict: instead of dismissing the non-rational or intuitive, he integrates them into a broader understanding of how science progresses.

I shall now trace the parallels between Kuhn's thought and Lovejoy's philosophy in detail, arguing that Kuhn may have incorporated key elements of Lovejoy's intellectual framework during the early to mid-1950s. Let us first consider how Lovejoy delimits the question regarding consciousness and conscious processes:

> Neither James's denial of the existence of consciousness as psychic process nor G. E. Moore's assertion of the existence of a consciousness which is pure psychic process had, in strictness, any implications whatever as to the

> "independence" of immediate data or their identity with parts of the "physical world" (Lovejoy 1955/1930, 11).

In the same tone, Kuhn's "metaphysical extras" also do not seem to be any hindrance to understanding the world – or even enter the field. Lovejoy continues:

> It is perfectly possible to conceive of the existence of psychic content dependent on the physiological act of perception without admitting any "impalpable inner flowing" that never has the status of content. Indeed, even a phenomenalist might without inconsistency agree with James's rejection of a *Bewusstsein iiberhaupt* – might, in other words, adopt the theory that experience, and therefore what is knowable of reality, consists of "thoughts" in their concreteness, and includes no such insensible stuff as pure thinking (11).

The point Kuhn raises in his letter to Kubie regarding his doubt about an "undistorted" view of a "given world" would be a snug fit with that of Lovejoy as elaborated in the above citation. Lovejoy writes that the "dualist" does not deny "distortion":

> That there may be in one place physical effects of the presence of an object in another place, and that some of these effects may resemble the object i.e., that there may be, as common sense assumes, physical images, more or less distorted—he does not, of course, deny (50).

So, one could safely assert that Kuhn's thought expressed in the letter to Kubie reflects that of Lovejoy. Elsewhere in the letter, we encounter "intuitive" thinking as having no relation with the validity of the thought processes. In Kuhn's own words:

> It seems to me that the rational thought processes of the conscious mind are used to check and validate the result of intuitive thinking, but that the intuitive *processes* themselves need have no relation whatsoever to the conscious *processes* by which their result is validated (TSK-MIT-Box 5, Folder 67).

Lovejoy would have agreed. Lovejoy, quotes Bridgman (in *Science* 1930, p. 22) to make a very similar point on intuition:

> our intuitive grasp of an elementary situation may be tested by our ability to describe what to expect in terms of conceptual experiments." This seems to imply that we have some reason to believe that at "the frontiers of physical exploration" we actually come upon "underlying elemental processes and entities," and that, [italics mine MNB] *different as they are from the entities of perceptual experience, they are still of an intuitable nature* […] (Lovejoy 1955/1930, 369).

In the above citation, it appears that Lovejoy is expressing a similar concern with very minor differences in depiction. Again, in the same letter, we find:

> I wonder whether non-rational symbolic processes do not perform essential function in intuitive thinking […] I wonder, though, whether processes intrinsically irreducible to rational processes may not be necessary to creative [scientific] thought […] The model of science outlined above demands that the non-rational (and I suspect I now mean unconscious) play a double role in research (TSK-MIT-Box 5, Folder 67).

Kuhn is apparently equating the 'unconscious' with the 'intuitive'. If we only substituted Lovejoy's 'entities of an intuitable nature' (see italics) with Kuhn's 'non-rational symbolic processes' (see above citation) we arrive at the same point. Lovejoy appears to have been convinced of the importance of the "non-rational". He writes:

> Let us, then, turn to that class of experiences which first gave rise to the belief in a realm of being consisting of apprehended but non-physical, and at least in that sense "mental," particulars the experiences of illusion, dream, hallucination, creative phantasy. The content of these also must, of course, in a realistic philosophy untainted with metaphysical dualism (with respect to content) be given a place among "real" and "physical" existents (Lovejoy 1955/1930, 85).

So, the non-rational not only does play a role in Lovejoy's philosophy, it must also be acknowledged. Pressing our comparison forward into Lovejoy's pragmatism of the commonsense, we could place both items mentioned by Kuhn in his letter namely i) and ii), in Lovejoy's depiction of commonsense category: "[…] the existence of differences of opinion as to the precise extent of what is thus known cannot affect the validity of the conclusion which we have reached as to the *generic* nature and natural function of knowing" (397).

Note the same point expressed differently:

> Contemporary physics, then, does not seem as yet to have given the deathblow to man's natural belief that he lives in a physical world which is, in its general structure and the modes of relatedness of its components, somewhat like the world which he perceptually experiences. That it conclusively vindicates this belief I am not maintaining, though I think it still lends some support to it. But the belief in question is at worst a natural and almost universal prejudice of mankind; and I can not but think that the burden of proof rests upon those who demand that we abandon a prejudice of this sort (370).

In the above citation, Lovejoy very clearly expresses Sellars *naturalistic* definition of critical realism (see also Mach's *naturalism* in previous section). Further Lovejoy puts the ball in the opponent's court simply through its pragmatic weight. Consider that in what follows, he attributes malleability common to different accounts of knowledge and epistemologies – a malleability anchored in commonsense experience of the world:

> The account of knowing both accepts and qualifies, it will be seen, certain epistemological theses characteristic of idealism, of monistic realism, and of (in one sense, of that ambiguous designation) pragmatism (397).

Sellars has similarly harped on this point:

> It is the claim of critical realists that all the valid insights of realist, idealist, and pragmatist, will be found in this position (Sellars 1929, 455).

Then Lovejoy justifies his assertion above:

> And the type of pragmatist who insists upon the necessity of postulation, of belief which outruns empirical evidence, is also, in an important sense, right […] Since our knowledge is characteristically concerned with beyond, we know by faith (Lovejoy 1955/1930, 397).

In other words, what 'outruns empirical evidence' for Lovejoy is no doubt the 'intuitive' – for which he demands acknowledgement. In concluding our comparison, one is bound to find tremendous agreement and affinities. I would even go beyond these affinities to suggest that this may even be the springboard on which Kuhn later justifies scientists not requiring "rules" (see 4.2-4.3) to venture beyond their existing "world". Below consider Lovejoy's remarks, which again resonate in Kuhn's letter:

> He [a person] assumes that, in so far as he ever succeeds in one or another form of the cognitive enterprise, some very curious things must be true about the known fact's or object's relation to himself and his act of knowing. These assumptions are no far-fetched inventions of the philosopher; they are ill manifestations of the primary and most universal faith of man, his inexpugnable realism, his two-fold belief that he is on the one hand in the midst of realities which are not himself nor mere obsequious shadows of himself, a world which transcends the narrow confines of his own transient being (14).

The inexpugnable (*unbezwingbar*) realism is *critical realism*, espoused by Lovejoy and others in the early decades of the last century. Therefore, Kuhn may well have—in fact it is likely—anchored his philosophical stance in some form of acquaintance with Lovejoy's philosophy. This acquaintance may have been prompted by Kuhn's deep interest in Lovejoy's historical method in *The Great Chain of Being*. Kuhn's "The world itself is shaped by our belief" then appears as a prelude to Kuhn's later notion of 'incommensurability' in *Structure* (see 4.5). One must note that the crucial building blocks of Kuhn's thought in the early to mid-1950s, do not only come to the fore in the letter to L. Kubie. Kuhn's lecture notes clearly reflect an emphasis on Lovejoy's historical thinking, and a rejection—or at least wariness—of the Russellian scientific methodology.

so far, I have demonstrated that, throughout the early to mid-1950s, Kuhn consistently praised Lovejoy and, in particular, Lovejoy's intellectual history as developed in *The Great Chain of Being*. During this same period, Kuhn actively positioned himself critically in relation to Russellian thought. Although Kuhn never explicitly stated that he adopted Lovejoy's philosophy, his correspondence suggests that he may have absorbed certain strands of it.

There can be little doubt that *The Great Chain of Being* reflects Lovejoy's philosophical outlook, even though it is primarily a work of intellectual history. Lovejoy's philosophy and historical thinking are deeply intertwined in *The Great Chain*, making Kuhn's appeal to the latter, at least implicitly, an engagement with the former.

To further clarify this point, we must ask whether Lovejoy's philosophy and his historical method can truly be separated. Daniel J. Wilson, one of Lovejoy's archivists in the 1970s, addresses this question in his *Arthur O. Lovejoy and the Quest for Intelligibility* (1980). Wilson concludes that Lovejoy's philosophical and historical approaches developed in unison. Notably, Wilson shows that as early as 1902, Lovejoy had already outlined the historical problem that would eventually be fully elaborated in *The Great Chain of Being* (Wilson 1980, 140)

> In the principles of evolution man had a means whereby a world of becoming could be made intelligible. […] This revolution in thought lay behind Lovejoy's own philosophy. Just as the nineteenth century had rejected a

> static absolute conception of the world, so, too, he had rejected the absolute idealism of his philosophical mentors in favor of philosophy (142).

Lovejoy's "historical inquiries, by revealing the contradictions inherent in an absolute, helped Lovejoy reject the absolute philosophies of his day. The moral of this history was thus central to Lovejoy's intellectual enterprise, and he returned to it at the end of The Great Chain of Being" (144-145).

> He [Lovejoy] concluded that the history of the chain of being was the history of its failure, but a failure with a moral: no static, absolute, and completely rational conception of the universe could render the actual world intelligible (149).

The reason for the failure was that:

> It revealed "the hypothesis of the absolute rationality of the cosmos to be unbelievable" because it conflicted with "one immense fact," "the fact that existence as we experience it is temporal" (156).

Referring to *Chain of Being* [pp. 242-45], Wilson quotes Lovejoy in what appears to echo Kuhn's notion of "crisis":

> Difficulties and contradictions in the conception of plentitude, continuity, and gradation caused the "static and permanently complete Chain of Being" to break down "largely of its own weight" (153).

'The Principle of Plentitude'[37] a term given by Lovejoy[38] was the title of one of Kuhn's lectures as part of the eight-part Lowell Lectures of March 1951; these lectures discussed many aspects of the notions of continuity and progress (see 3.1). The fact that Kuhn omitted references to the 'principle of plenitude' in his talk is telling. The roots of Kuhn's

[37] Here Kuhn did not mention Lovejoy and in fact announced that " I should like this evening to omit con sideration of the historical continuity of the development of the idea of a subtle fluid in favor of a more detailed examination of one of its more recent applications. In particular I shall omit all further remarks about that aspect of subtle-fluid thinking referred to in the title of this lecture as the principle of plenitude." In G. A. Reisch (2021), *Thomas Kuhn's Lowell Lectures of 1951, edited with an introduction by George A. Reisch*, p. 59, MIT administrated.

[38] Name given by Lovejoy to a principle he detected in much Greek and medieval thought, that the existence and abundance of creation must be as great as the possibility of existence, commensurate therefore with an infinite and inexhaustible source. […] The principle infused many versions of the view that there is a great chain of being, stretching from the lowest invisible worm to the highest seraph (for it also ensures that there are creatures intermediate between us and God); it also infuses the optimistic view, found as late as the 18th and 19th centuries, that the soul will, or at least could, rise through a perpetual progress of degrees of perfection (an idea effectively criticized by the American Samuel Johnson as giving rise to a version of Zeno's paradox). Source: https://www.oxfordreference.com/display/10.1093/oi/authority.20110803100331692.

thought on progress may indeed be traced to both Lovejoy's philosophy and historiography. Note that "Lovejoy believed that the idea of "unending progress" had emerged "as a consequence of reflection upon the principles of plentitude and continuity" (153-154) and recall that overcoming continuity and "cumulativeness" was a major difficulty for Kuhn. In fact, it was through overcoming this generally accepted "obstacle in thinking" that Kuhn's concept of revolutions as 'punctuated equilibrium' was born. However, Lovejoy had already acknowledged "discontinuous mutations" when contemplating unified sciences:

> Lovejoy's interest in the nature of the scientific enterprise did not result in a full-scale theory of scientific development. He did, however, examine the possible base for a unified science […]. Lovejoy felt that the largest barrier to the unification of the sciences was the evidence that evolution had produced "discontinuous mutations and genuine, not-wholly-predictable innovations". (160)[39]

What Lovejoy viewed as a hindrance to the unity of science ultimately became, for Kuhn, the very basis and driving force behind scientific revolutions. This marks the essence of Kuhn's reinterpretation. Thus, Lovejoy's historical and philosophical ideas were deeply intertwined, and given Kuhn's alignment with Lovejoy's historical thinking, it follows that he must have, by extension, adopted the core principles of Lovejoy's philosophy.

To conclude, I briefly examined in this section aspects of Neuber's research to argue that Kuhn's thought can be linked to the critical realism championed by Arthur O. Lovejoy and other realists. In doing so, I aimed to uncover yet another pattern in the intellectual fabric of 'Kuhn's Harvard' and the broader Harvard hub. While Kuhn's background was undoubtedly shaped by Pragmatism, I contend that his philosophical commitments can be traced to strands of critical realism originating at Harvard and its neighboring universities.

I further argued that, given Russell's alignment with neo-realist thought at Harvard (see Neuber 2023), and Kuhn's affinity for Lovejoy's philosophy, it is unsurprising that Kuhn rejected the neo-realist perspective. It was Lovejoy, along with like-minded thinkers, and

[39] Wilson refers to Lovejoy's "The Unity of Science" contribution, p. 32. [*The University of Missouri Bulletin: Science Series,* Vol. I, N°. 1, January 1912].

eventually Kuhn himself, who resisted the Russell-inspired trend at Harvard. This positions Kuhn squarely within the tradition and intellectual diversity of the Harvard hub.

Finally, I approached Kuhn's philosophy pre-*Structure* from two distinct angles. First, I examined Kuhn's correspondence with L. Kubie, his acknowledged affinities with Lovejoy and *The Great Chain of Being*, as reflected in both *The Structure of Scientific Revolutions* and his unpublished lecture notes. I also explored his opposition to Russell's unhistorical approach, as well as the Lovejoy-Carnap encounter, to highlight the shared emphasis on 'common sense' and the significance of 'interpretation' for both Kuhn and critical realists. Second, I provided additional evidence to demonstrate that Kuhn not only engaged with Lovejoy's historical thinking, as outlined in *The Great Chain of Being*, but also adhered to Lovejoy's broader philosophical framework.

2.7.6 Conclusion

Recall Alexander Bird's remarks in the conclusion of 2.7.2, where he describes Kuhn's "naturalistic approach to the philosophy of science" as a "truly innovative and radical contribution." I would now suggest a reformulation of Bird's assessment, emphasizing that the innovation in Kuhn's work was not necessarily the "naturalistic approach" he inherited from his contemporaries, as I have demonstrated. Rather, the true innovation in Kuhn lay in his creation of a fundamentally new conception of the philosophy of science. This helps explain why, in his Athens interview, Kuhn remarked, "Carnap, staying within the tradition, had been driven to this—I had rebelled already and come to it from another direction, and in any case we were still different" (TSK-MIT-Box 28, Folder [Athens]). The 'tradition' Kuhn refers to is undoubtedly the scientific historicist tradition, and the 'different' reflects the distinctive nature of his own conceptual approach.

Throughout this chapter, I have equated the Harvard hub with a meta-concept, referring to it as a "philosophical awareness" characteristic of the time. However, this need not remain merely a meta-concept. When considered more concretely, one might argue that what Kuhn and others acquired through their Harvard education—rooted in the tradition of Geisteswissenschaften—was, in essence, the 'textbook knowledge' available to them. Nonetheless, Kuhn's key contribution lies not just in acquiring this body of knowledge from science historians and other scholars at the Harvard hub, but in transcending it. He

reorganized and reinterpreted this knowledge within a new, historically oriented philosophy of science. In doing so, Kuhn was able to break free from the established thinking of his contemporaries, redefining the philosophy of science and shifting it away from the somewhat static historicism of his predecessors.

Venturing outside conventional interpretations, one might even suggest that the story of *The Structure of Scientific Revolutions* is, in itself, a revolution—Kuhn's own method of "breaking out" from the dominant historicism. In this sense, this "revolution" can be seen as the "resolution" of the historical thread referenced in the title of this dissertation. Ironically, however, Kuhn ultimately helped preserve the "continuity" of that very historicism. This narrative may provide a solution to the puzzle of why Kuhn regarded most science historians before him—if not all—as Whiggish:[40] They failed to develop their great ideas into a comprehensive philosophy of science, which *he* eventually did. Another mystery lies in Kuhn's strategic omission or reluctance to fully credit certain authors, such as Fleck, Sarton, and even Frank, for the very "textbook knowledge" he assimilated. This strategy can now be understood as Kuhn's way of distinguishing himself and his work from his contemporaries. On the final page of section 2.6, I drew parallels between this strategy and Martin Kusch's concept of "philosophical facts," suggesting that such a strategy—and the associated statements—may align with Kusch's criteria for "Philosophical Facts" (see footnote in 2.6).

However, beyond what Kuhn admits, omits, or even disavows, scholars like Joseph Rouse have highlighted an important aspect of Kuhn's philosophy of science—its role as a challenge to logical empiricism. As Rouse notes, "Although Kuhn's book in 1962 was only one of several prominent challenges to logical empiricism, it has retrospectively come to symbolize its own revolution, marking a transition to a post-empiricist era in the philosophy of science" (Rouse 2003, 101). In this light, Kuhn's philosophy of science can be seen as a complementary addition to, rather than a rejection of, logical empiricism, particularly by emphasizing the historical treatment of science. Reisch also highlights this point, observing that "the introduction of a historical picture of science, which emphasizes 'practical' aspects of scientific reasoning, had already received parallel treatment from the leading figures of logical empiricism" (Reisch 1991, 276). These "practical" aspects—what I've

[40] Kuhn owned a copy of Butterfield's *Whig Interpretation of History* (1951) (TSK-MIT_Inventory).

referred to throughout as ‘common sense’—align closely with Kuhn’s philosophy of science. Thus, rather than viewing Kuhn’s work as merely a history of science, we can recognize it as a distinct philosophy of science that offers a nuanced and complementary perspective—almost an “upgrade” that builds upon and refines the existing conception.

PART II

The Teaching Years and the Development of *Structure*

3

Lowell Lectures and the Formation of Kuhn's Early Convictions

3.1 Lowell Lectures

Thomas Kuhn's Lowell Lectures of March 1951 (sponsored by The Lowell Institute)[41] titled "The Quest for Physical Theory" is one of the elements crucial to understanding Kuhn's project in the *Structure*. In connection with the lectures, this chapter explores three interconnected and recurring themes in Kuhn's early thought: The goal of science, cumulativeness, and the notion that 'science destroys its history'.

Let me begin with the 'Lectures' that had, in many ways, personal and philosophical significance for Kuhn, which offered the young Kuhn an intellectual arena for putting up his very early ideas to public scrutiny. Many ideas in *The Structure of Scientific Revolutions* published 11 years later in 1962 find their source in these lectures. In the *Structure* Kuhn admits that at that time, he was still a novice, and the lectures provided him with "[…] a first chance to try out my still developing notion of science" (Kuhn 1970/1962, vii).

> In the next year, I began to teach history of science proper, and for almost a decade the problems of instructing in a field I had never systematically studied left little time for explicit articulation of the ideas that had first brought me to it (vii).

Kuhn adds that that "proved a source of implicit orientation" (vii). In a proposal introducing a "Plan of Study", Kuhn outlined his activities for the academic year 1954-55. Here, eight years before the publication of the *Structure*, he mentions a monograph as his main project:

> My principal project for the year is to write (by invitation) the "history of science" monograph for the ENCYCLOPEDIA OF UNIFIED SCIENCE, published by the University of Chicago Press. The ENCYCLOPEDIA provides a very appropriate forum for the presentation of ideas about the nature of scientific research which I first discussed in the Lowel lectures in 1951 and which have since provided a basis for much of my teaching and

[41] The Lowell Institute was established in 1836 when "John Lowell Jr. dedicated a significant portion of his estate to the establishment of a trust dedicated to the "maintenance and support" of free public lectures for the citizens of Boston, regardless of race or gender. Even today, "The Lowell Institute funds public lectures and educational programming in partnership with several Boston leading institutions […]" from https://lowellinstitute.org/about/ on 13.10.2024).

> research. I have suggested "The Structure of Scientific Revolutions" as a title for the monograph and have proposed the following subject matter which the editors have now approved (TSK-MIT-Box 5, Folder 84).

Kuhn added that the monograph will be devoted to "the role of established scientific theories as *ideologies*" (TSK-MIT-Box 5, Folder 84). These "ideologies" he later called "paradigms".

The original flyer advertising the lectures is itself a telling document. It reads: What are the problems of scientific research today? (Reisch 2021). The question bears an oversized question mark in front of it. The thought behind the words on the flyer is sober and apparently aims at dealing with a practice or an activity, namely, the activity of research. The question posed is hardly philosophical. It does not ask, for example, what is the concept of science? It asks what scientific activity is about? So, it is meant to be practical, informative, and supposedly accessible to a public enthused about matters related to science. The talks are held at Boston Public Library at Copley Square, in the city center and right across Charles River from Harvard and MIT universitates. In fact, Boston Public Library may be unintimidating, but the lectures held there were on specialized topics, which apparently catered to an interested and educated public (vii). The lectures are listed I-VIII.[42] Kuhn had conveyed to Lowell that the series of talks "might appropriately be titled "The Creation of Scientific Objects". According to Reisch, the latter title puts the idea of creation close to Ludwik Fleck's "The Genesis and Development of a scientific Fact" (*Entstehung und Entwicklung einer Wissenschaftliche Tatsache*) to whom he had found at the time an intellectual affinity (xv-xvi). Reisch further points to the notion of the "infinite" complexity of experience that is at the center of Kuhn's "creation" as opposed to "discovery" (xvi). Reisch mentions in one of the footnotes that the infinite is central to a sophomore-year essay Kuhn wrote titled "The Metaphysical Possibilities of Physics" (xxxii). Now, Reisch is right and in fact Kuhn mentions the "infinite" many times over, but as for the roots of the "creation" at work here, it is instructive to consider that in the very same paper, we can witness a feisty young Kuhn making a stance against metaphysics:

> No one has succeeded in discarding reason and logic as methods, while those who have attempted to derive a metaphysic from the nature of the method alone have been led to the scepticism of Hume and the Antinomies of Kant. […] The concepts thus derived [time, cause and effect, mass and

[42] I. Introduction: Textbook science and Creative Science, II. The Foundation of Dynamics, III. The Prevalence of Atoms, IV. 'The Principle of Plentitude': Subtle Fluids and Physical Fields, V. Evidence and Explanation, VII. The Role of Formalism, VIII. Canons of Constructive Research.

> energy] are not the concepts of Kant's *Critique of Pure Reason*. They are not imposed upon sense data by the nature of the mind itself […] Above all these concepts remain fiction […] Furthermore, all these concepts are constantly being changed to fit new data [...] But while the concepts of physics expand and the narrow fictions are replaced by broader ones, the structure of physics remain unshaken, for the basis of this structure is data, not concepts [...] The bricks remained, the mortar was renewed (TSK-MIT).[43]

Kuhn had made notes of the books he was reading and those he had finished reading (*in toto*). Kant's *Critique of Pure Reason* is listed as one of the books that he had not read in its entirety.[44] Even though the quotation above clearly illustrates early Kuhn's critical stance, it does not deny Kant or the Kantian conception of knowledge. In this connection, let me very briefly point to A. C. Crombie's *Augustine to Galileo* that appears on many of Kuhn's syllabi for his students at Harvard in the 1950s; Crombie clearly emphasized Kant's appeal to "creative activity":

> With Kant natural science is seen to be a creative activity, not a mere dissection of Nature; scientific experiments and theories are seen to be the creations of an active reason attempting to explain facts of observation by showing that they can be deduced from a theory. Science attempts to construct a whole interlocking system of theories from which more and more diverse forms of experience can be deduced (Crombie 1952, 400).

If we agree with Crombie, it may be legitimate to conclude that, in some respect, Kuhn's early thoughts – as they related to creative thought – may find the young Kuhn "sparing" with Kantian thought. In this vein, the relatively straightforward "The bricks remained, the mortar was renewed" in the undergraduate paper, therefore, anticipated what some 15 years later became the com*9-plex notion of incommensurability of paradigms as formulated in *Structure.* In other words, Kuhnian early thought seems to have its source not only in the dominant "logical" mindset at Harvard during the early 1940s (see Kuhn's teachers in the previous chapter), but – without attributing to Kuhn, Kantian affinities – also in simply

[43] TSK-MIT, Box 1, Folder 3, (there are two remarks on top of the last sentence penciled as: *Merely that? Is this a sound figure?* (I can only make out the last word to be 'figure' in the last sentence).

[44] No doubt, Kuhn was very familiar with the weight of Kantian thought and the *Critique.* We can also find a handwritten summary on general tenet of Kant's *Critique* apparently written by Kuhn during a presentation by a certain Father Vendler. Kuhn often asked his students to prepare materials for presentations. It is not however clear that Father Vendler was a student of Kuhn or Kuhn simply knew that Father Vendler was versed in Kantian thought and wanted to expose his students to these thoughts.

sparring with the "Kantian element". On what I have called the Kantian element, Gorege Reisch writes:

> […] the lectures amply document Kuhn's familiarity with the writings of other historians and philosophers. Lecture V, for example, shows Kuhn adapting Kantian epistemology to his methodological picture of science. (Reisch 2021, xix).

Reisch's reference to Lecture V stems from Kuhn's assertion that "[…] I wished to emphasize that although these *prejudices* [italics mine MNB] legislated by experience like the mental categories proposed by Kant, they were not, like Kantian categories, necessary or *a priori*" (78). Shortly before this citation, we encounter Kuhn talking about the "prejudices" he has in mind. Kuhn says: "Finally I hoped that the choice of the word prejudice would suggest that the elements under discussion were individual and *alterable*" [italics mine MNB] (77). So, Kant and the alterable or "moving" categories may have been present in Kuhn's thought at least as early as early 1951. With the foregoing as the background, I think it is justified to conclude that interaction, or what I referred to as 'sparring' with Kantian thought – albeit rudimentary – is one of the very first strands of formation of Kuhn's thought in the genesis of the *Structure*.

3.2 The End Goal of Science

Another strand in Kuhnian early thoughts is reflected in the following quotation on the goal of science. The young Kuhn in an undergraduate paper writes:

> But we may still ask, to what will this lead? Do the fictions of physics approach nearer and nearer to the truth, and will the ultimate fiction thus correspond to the truth? Albert Einstein once said that the scientist is a man examining a watch. He may observe the dials and the hands; he may listen to it ticking; but he must never open it […]. But if this investigation correctly performed [infinite data and finite concepts to which the data may or may not apply], yielded the possibility of but one concept, we would believe that science could in time arrive at a picture of the universe, and that that picture would be an image of the reality (TSK-MIT-Box 1, Folder 3).

This is the early Kuhn contemplating the end goal of science. Here, if I understand Kuhn correctly, he appears to suggest that it may be possible that the ultimate fiction could

correspond to 'an image of reality' or for that matter, the truth. This is surprising since Kuhn was aware of the debate on the end goal of science through reading Russell's *Mysticism and Logic* (collection of essays 1901-1915) which he cites under references in one of his undergraduate papers in 1945 on the topic of 'causality'. Russell had addressed this question in his book:

> Not only the aspirations, but the ideal too, must change and develop in the course of evolution: there must be no fixed goals […]. All our thinking consists of convenient fiction (Russell 1957, 23).

Not only Russell, but also A. C. Crombie had broached this very concept when discussing the condition of knowledge in the 14th century as one displaying different degrees of skepticism as well as speculative mysticism; Crombie is indeed referring to the human condition in this period. For Crombie, Nicholas of Cusa, for example, held the thought that even though "[…] it was possible to approach closer and closer to truth, it was never possible to grasp it finally…" (Crombie 1952, 235). As I mentioned earlier, Crombie's book was on Kuhn's syllabi for his students in the 1950s, so one can assume that he was thoroughly familiar with it. Kuhn at the end of the *Structure* points to a similar human condition regarding knowledge in that he asks:

> We are accustomed to seeing science as the one enterprise that draws constantly nearer to some goal set by nature in advance [...]. Does it really help to imagine that there is some one full, objective, true account of nature and that the proper measure of scientific achievement is the extent to which it brings us closer to that ultimate goal? (Kuhn 1970/1962, 171).

Reisch's point on the proposed earlier title of the Lowell Lectures, namely, "The Creation of Scientific Objects" is well taken, but it may go even deeper than "a book with that title" (Fleck 1979/1935, viii) that the later Kuhn writes in the preface to Fleck's book in English translation of the book (1979). Kuhn explicitly points to R. K. Merton, Jean Piaget, and Hans Reichenbach. I am suggesting that there might be more to the reference to Reichenbach's *Experience and Prediction* (1938) and the discovery of a footnote (Reichenbach 1938, 224) on the title of Fleck's book (in German). More specifically, In Reichbach's *Experience and Prediction* (1938), the sentence cited by Kuhn in the preface to Fleck's book starts with "Must we renounce the possibility of ever obtaining a true picture of the world?" (224). In other words, throughout the '40s, Kuhn is occupied with asking questions on science, creativity, and social influences.

> To the best of my recollection, I first read the book during the year 1949 or early in 1950. At that time I was a member of the Harvard Society of Fellows, trying both to prepare myself for the transition from research physics to history of science and also, simultaneously, to explore a revelation that had come to me two or three years before (Fleck 1979/1935, vii).

I am able to confirm that the dates Kuhn is referring to are in line with what the archival materials show, namely during the time Kuhn was a Junior Fellow (1948-51). In a document, apparently an incomplete essay (not dated but filed under 1949).[45] Fleck's name appear in handwritten form on top of 'text book science' on the margin of page 2 of the document.[46] However, it is perhaps of historical interest that Kuhn never directly attributes 'text book science' to Fleck – neither in Structure (1962), nor in the Lowell Lectures (1951) even though the introductory talk of the lectures is titled: "Introduction: Textbook Science and Creative Science".

On the same page that Reichenbach has a footnote on Fleck, Reichenbach delicately points to the crucial social dimension at play, which we might perhaps even attribute to Reichenbach's reading of Fleck. Reichenbach had also developed a very informative review of 'apriorism' as well as 'the problem of induction' in his book. Reichenbach writes:

> The social milieu into which we are caught adds pressure to the stronger influence of the physical and historical milieu. Our modern eyes, familiar with rectangular houses and steel constructions, see the richer forms of nature within the frame of our architectural style; modern drawings, in comparison with ancient drawings, betray this influence (Reichenbach 1938, 224).

The last sentence in the above citation then carries a footnote to Fleck's book and Fleck's discovery of the differences between modern and ancient drawings of the skeleton. Kuhn in the introduction to the English translation (1979) of Fleck's book quotes Reichenbach:

> Intellectual operations have shown us the way to overcome the limitations of our subjective intuitional capacities [...]. Every picture may, besides

[45] This unfinished essay comes out of a response addressed to Sandor Rado, M.D. on a reprint titled "Psychodynamics as a Basic Science" (1946) sent to Kuhn by Rado. The response is not dated, so it is not clear whether a letter with that content was ever sent back to Rado. The essay deals with the question of "How" and "Why" in science that Rado had addressed in his article and with which Kuhn respectfully disagreed. Now, the reference to Fleck in this letter, specifically on 'textbook science' may be 1949 or perhaps even earlier, namely around the time the article was published in 1946, which would date Kuhn's acquaintance with Fleck even earlier. It is not, however, possible to determine this point with certainty.

[46] TSK-MIT, box 1, folder 6 labeled 'INCOMPLETE MEMOS & IDEAS', 1949 [the year on the folder is handwritten].

> containing false traits, introduce some true features into the composition. (Fleck 1979/1935, viii).

Kuhn then writes: "Acquaintance with Fleck's text soon confirmed that intuition […]" (viii). On January 6, 1951, in a letter to the Chairman of General Education Committee, David Owen, Kuhn, in a seven-page letter, outlines his upcoming research plans and their background. In this context, he not only employs expressions drawn from Fleck's lexicon (an excerpt follows below) but also integrates them into his forthcoming research. It is important to note that the Lowell Lectures are scheduled to begin in March—just two months from the date of this letter—indicating that he has been contemplating these themes for a considerable period. For example, Fleck's vocabulary frequently encompasses terms such as psychological "predisposition" and the "facts" of science, as well as the problem of induction, which Reichenbach eloquently reformulated in terms of experience and prediction.

The following is an excerpt from the longer letter Kuhn wrote to Owen in that January of 1951:

> Any particular set of observations in science (or everyday life) presupposes a predisposition toward a conceptual scheme of a corresponding sort; the "facts" of science already contain (in a psychological, not a metaphysical, sense) a portion of the theory from which they will ultimately be deduced […]. The recognition that experiment and observation are not independent of theoretical orientation narrows or may possibly eliminate the methodological gap labeled "the problem of induction"[…]. Also useful is the limited amount of earlier research which proceeds from a similar orientation toward scientific method, particularly the work of Brunschvicg, Fleck, Meyerson, and Koyré (TSK-MIT-Box 5, Folder 84).

Allow a quick detour on the origins of conceptual scheme followed by a reference to Kuhn's correspondence with G.F.F. Lombard. In the above citation the reference to conceptual scheme is pivotal in Kuhn's thought formation. According to Stephen Turner "Two of the ideas that animated Harvard thinking in the mid-twentieth century, conceptual schemes and systems, were associated with him and associated with each other. Both terms first gained currency at Harvard through a famous seminar run by Henderson … But he could not regard science as devoid of nonlogical elements" (Turner & Nichols 2024, 33 & 48). In fact, the archival materials show that Kuhn in a letter to Prof. George F. F. Lombard on July 26, 1949 writes:

> The Henderson lectures are of considerable interest, I find, and their emphasis upon the necessity of learning to live within a new viewpoint, of deliberately reorienting oneself toward the perceptual field, can supply an important missing ingredient to contemporary methodological studies (TSK-MIT, Box 1, Folder 6).

It appears that Kuhn highly valued Henderson's views. Elsewhere, Henderson had written: "Then the theory and previous conclusions yield and suffer modification or destruction as the case may be" (Henderson 1970, 18).[47] This is indeed reminiscent of Kuhn's own views in the *Structure* (see p. 82).

Returning back to the previous citation (the Owen letter), Kuhn wrote:

> In fact, as you know, I expect that attention to historical material can produce an important reorientation in methodological thinking, and I suspect that such a reorientation cannot but affect our notions as to the sources of our knowledge (TSK-MIT-Box 5, Folder 84).

Kuhn is obviously still far away from the ideas advanced at the end of *Structure* regarding progress along the lines of biological evolution – a conception that the later Kuhn adhered to. But in these early documents, Kuhn appears to follow several strands of thought associated with, among others, with Reichenbach, Fleck, historical methodology as a source of knowledge, and away from the very central question of whether science is cumulative. I am therefore suggesting that the encounter with Fleck deflected Kuhn's thought on cumulativeness to a point that it lost its centrality for Kuhn.

3.3 Cumulativeness as Fiction

It is in lecture 1 of the Lowell Lecture series titled "Introduction: Textbook Science and Creative Science" that we encounter a strong statement of commitment to cumulative as if the issue is not up for discussion. In the entire Lowell Lecture series, there are only 3 mentions of the word 'cumulative' two of which are included in the citation below:

> […] I am committed to the belief that scientific knowledge is good knowledge, that it is useful knowledge, and above all that it is cumulative knowledge … [a] more basic task of these lectures to provide an alternative description of the method employed by scientists, and to show how science conceived as a body of cumulative knowledge can proceed from this alternate description (Reisch 2021, 3-4).

[47] See also footnote on page 50 on Crane Brinton and L. J. Henderson.

I suppose, one can speculate that the last sentence in the above citation, namely, the peculiar notion of an 'alternative description' is apparently cumulative but perhaps not construed along established traditional philosophy – perhaps, not even Kantian in any shape and form.

In any case, it is worth mentioning that *Structure* puts more emphasis on the 'cumulativeness' as the Lowell Lectures did, but nothing compared to the first draft of the first chapter of the book. There are 21 mentions of the word 'cumulative' for example in the published *Structure*. But a first draft of the *Structure*'s first chapter illustrates the difficulty Kuhn was having on the use of the concept of 'cumulativeness'. Chapter 1 was to be called "Discoveries as Revolutionary" and started with section 1 titled "Cumulativeness and Revolutions in Science".

> As a correlate of the more famous idea of progress, the concept of cumulativeness has been implicit in discussions of science and its method at least since the days of Francis Bacon […]. Simultaneously, we have ceased to find meaning in the statement that successive scientific theories provide closer and closer representations of reality (TSK-MIT).[48]

The chapter is marbled with innumerous references to 'cumulative and 'cumulativeness'. It appears that Kuhn originally wanted to dedicate the bulk of the chapter to cumulativeness and, interestingly, not only in science but also in comparison with the arts; Kuhn later uses the arts to speak of revolutions as 'matters of taste' (see 4.4).

Kuhn did shuffle sections around and he eventually got 'cumulativeness' out of the first chapter – and almost entirely out of the book. The *Structure* did refer, however, to the notion of 'cumulative' and its connection to art – much in line with the original text. And, the intention in the original first chapter to enter a philosophical discussion of the cumulative process remained a statement of doubt for the published book; Kuhn instead put the emphasis on historiography:

> […] difficult to see scientific development as a process of accretion […]. The results of all these doubts and difficulties is a historiographic revolution […] (Kuhn 1970, 3).

This is clearly a strategic decision on the part of Kuhn to cast doubt on the 'cumulative' thesis to make room for the 'non-cumulative' perspective, which later became Kuhn's main thesis in *Structure*. Kuhn's strategy is apparently to shift the emphasis. Normal science, the

[48] TSK-MIT, Box 4, Folder 2 (MONOGRAPH: Chapter 1 – 1st Draft), p. 3.

puzzle-solving is highly cumulative and when successful, finds no novelties (52). In other words, when Kuhn claims that outdated theories are not in principle unscientific because they have been discarded and that we nevertheless use the latest science and not the outdated one, and casts "profound doubts about the cumulativeness process" (3), he is simply applying a strategy to shift the perspective to the 'non-cumulative'. A handwritten note reads:

> Can I and ought I get cumulativeness out of the 1st chapter entirely? So it stays out until 495 [?], but that's also a reason for emphasizing it less at the start (TSK-MIT).[49]

And in the words of the later Kuhn:

> […] I had wanted to write *The Structure of Scientific Revolutions* ever since the Aristotle experience. That's why I had gotten into history of science – I didn't know quite what it was going to look like, but I knew the non-cumulativeness; and I knew something about what I took revolutions to be […] (Kuhn 2000, 293).

But to what end? Why the emphasis on the non-cumulative? The origin of this thought may lie in the notion that science destroys its history.

3.4 Science Destroys its History: Rosenstock and Koyré

I shall now refer to Reisch's research and Kuhn's interest in literature dealing with revolutions, albeit political revolutions. A section in Reisch's book titled "Ideology and Revolutions" illustrates very eloquently Kuhn's interest in Eugen Rosenstock-Huessy (1888-1973). At this point Kuhn was collecting notes on revolutions in general and had come across Rosenstock-Huessy's book *Die Europäischen Revolutionen* (1931), a 554-page-strong, the book is almost entirely on political revolutions in Europe. Kuhn's selections, Reisch tells us, emphasize sharp breaks between the old and the new and that revolutions inaugurate a new life principle and a different logic (Reisch 2019, 223).

Rossenstock-Huessy's book harbors other similarities with the thoughts later expressed in *Structure*. For instance, „Wenn die neue Syntax jeder Revolution einmal gesprochen wird, so liegt ein *Sinnbruch* vor" (Rosenstock 1931, 23). We also find:

> Jede neue Revolution erzwingt eine Revision der vorhergehenden Geschichtsbilder, Sie vernichtet eine Teil der alten Überlieferungen, weil

[49] TSK-MIT, Box 4, Folder 2 (MONOGRAPH: Chapter 1 – 1st Draft), handwritten note. (Approximate date of the first draft could be late 50s or even early 60s right before the publication).

> diese ihre Geschlossenheit einbüßen und aus „der“ Geschichte zu einem Teil der Geschichte werden (52).

In the *Structure*'s first draft, we encounter:

> Science destroys its history […]. The sciences embrace their history (and thus qua history [handwritten addition]) render it functionless, and thus destroy it […]. These shifts of canon and of problem field are usually unrecognized because of what I previously described as the way science destroys its own history (TSK-MIT).[50]

And in the *Structure,* we read:

> "Copernicus destroyed a time-honored explanation of terrestrial motion without replacing it; Newton did the same for an older explanation of gravity, Lavoisier for the common properties of metals, and so on." (Kuhn 1970/1962, 157).

It is worth pointing out that Alexander Koyré had also written of the notion of the "destruction" of the Cosmos. On the founders of modern science including Galileo, Koyré writes:

> They had to destroy one world and to replace it by another. They had to reshape the framework of our intellect itself, to restate and to reform its concepts, to evolve a new approach to Being, a new concept of knowledge, a new concept of science – and even to replace a pretty natural approach, that of common sense, by another which is not natural at all […]. [And] to make clear that the apparent continuity in the development of medieval and modern physics (a continuity so emphatically stressed by Caverni and Duhem) is an illusion (Koyré 1968/1943, 21).

For Kuhn, this may resonate with the non-cumulative. Nevertheless, not only Koyré, but again Rosenstock-Huessy further resonates in other areas of the *Structure*'s first draft: „Jede Revolution beginnt daher mit der Ideologie, mit der die vorhergehende geendet hat.“ (Rosenstock 1931, 177). We encounter the word 'ideology' nine times in the *Structure*'s first draft describing the way new ideologies get revised after a revolution:

> During the debate that usually follows, concepts, standards, and values are reforged and redefined. Simultaneously, past history is reformulated in new texts so that the scientific enterprise may seem to continue as before. What emerges, as the revolutions ends, is a new ideology that points to new problems and discards old, and ideology that once again permits professionals to see the world "scientifically" […]. (TSK-MIT).[51]

[50] TSK-MIT, Box 4, Folder 2 (MONOGRAPH: Chapter 1 – 1st Draft). The first two sentences are in comparison with the art, so science versus art.
[51] TSK-MIT, box 4, folder 2 (MONOGRAPH: Chapter 1 – 1st Draft).

Again, why the emphasis on the non-cumulativeness? Kuhn's argument is based on the conviction that if since science destroys its past, cumulativeness must be a fiction!

> The textbooks that emerge from a scientific revolution characteristically do contain just a bit of history either in an introductory chapter or, more often, in scattered references to the work of the great heroes of the pre-revolutionary period. Such references give both student and professionals an important sense of their participation in a long-standing tradition […]. Yet the textbook tradition in which scientists come to sense their participation is a strange one, because it never existed […]. Partly by selection and partly by distortion, the scientists of earlier ages are implicitly represented as having worked upon the same set of problems […]. No wonder science comes to seem entirely cumulative (TSK-MIT).[52]

Therefore, for Kuhn cumulativeness is only a useful fiction, and it can only be non-cumulativeness that, rationally considered, deserves the emphasis. Further, by the time *Structure* goes into print, the discussion of cumulativeness becomes almost a non-issue.

Nonetheless, the archival materials show that the early Kuhn had begun forming clearer ideas about why cumulativeness is puzzling and how science destroys its history. In fact, in January 1956 in a course titled 'Natural Sciences 4' this very topic is one of the questions put to his students on a mid-year examination. The question is as follows:

> Scientific Knowledge is often described as "cumulative" because the precision and scope of scientific theories are observed to increase with the continuation of productive research. But research is often destructive; a significant scientific investigation, almost by definition, is one which demolishes the conceptual scheme upon which it was originally based. Therefore, if cumulative knowledge is knowledge which grows by addition to a stockpile which is itself preserved, scientific knowledge cannot be cumulative (TSK-MIT-Box 1, Folder 16).

He also instructs his students to utilize illustrations drawn from the course materials (particularly from those concerning the Copernican and Chemical Revolutions) (TSK-MIT-Box 1, Folder 16). It appears then that Kuhn's most relevant sources involving this question are the Copernican and Lavoisier's Chemical revolution. In this context, the later Kuhn's Athens interview is illuminating:

[52] TSK-MIT, box 4, folder 2 (MONOGRAPH: Chapter 1 – 1st Draft).

> There was an increasing influence of a different approach to historical examples among the philosophers of science gradually […]. I don't think that the people who were doing history by and large saw everything in it that I was seeing it in it. They were not coming back asking "What does this do to the notion of truth, what does it do to the notion of progress," or if they did, they were finding it too easy to find answers that seemed to me superficial (Kuhn 2000, 311).

As we saw right at the beginning of this chapter in a citation from one of his undergraduate papers, the notion of "fiction" may have sprung from an initial encounter with the Kantian thought that may have acted as a catalyst:

> […] The concepts thus derived [time, cause and effect, mass and energy] are not the concepts of Kant's *Critique of Pure Reason*. They are not imposed upon sense data by the nature of the mind itself […] Above all these concepts remain fiction […] (TSK-MIT).[53]

I have argued in this chapter that the early Kuhn actively devised a strategy to shift the focus from a cumulative view of science to a non-cumulative one—an approach that ultimately paved the way for his concept of scientific revolutions. In doing so, he drew on Rosenstock's ideas on political revolutions and Koyré's assertion that science, by its very nature, obliterates its own history.

In concluding this section, I suggest that during his teaching years—reinforced by his encounter with A. C. Crombie's observation that "With Kant natural science is seen to be a creative activity, not a mere dissection of Nature" (Crombie 1952, 400)—Kuhn came to recognize that creative reinvention within an established scientific tradition is not only fruitful but essential.

[53] TSK-MIT, Box 1, Folder 3, (there are two remarks on top of the last sentence penciled as: *Merely that? Is this a sound figure?* (I can only make out the last word to be 'figure' in the last sentence).

4

Developmental Psychology and Reaffirmation of the Philosophical Awareness

4.1 Kuhn's 'Fringe of Vague Meaning'

In chapter 2, I equated the Harvard hub with a meta-concept—what I termed the "philosophical awareness" of the time. However, this concept need not remain abstract. One can concretely argue that what Kuhn and his contemporaries acquired through their Harvard education and the tradition of *Geisteswissenschaften* was essentially the 'textbook knowledge' available to them. This body of knowledge included influential ideas such as Polanyi's notion of tacit knowledge, the "ineffable" at the end of Wittgenstein's *Tractatus*, Langer's insights, and the concept of gestalt—many of which are discussed in the secondary literature and, in fact, were referenced by Kuhn in *Structure*.

By examining archival materials, my contribution here is to show that Kuhn's engagement with these concepts led him to recognize that science is not an entirely rational enterprise dependent solely on logical and formal concepts. Instead, he discovered that it also relies on inherently vague, "fringe" elements—a phenomenon he termed the "fringe of vague meaning." I illustrate this critical realization by reconstructing Kuhn's thought on Piaget, arguing that *Structure* did not appear spontaneously in the early 1960s but was the culmination of ideas already present in the available literature.

I further contend that elements such as analogies to art, incommensurability, and the evolution of scientific theories were the building blocks of the construct that eventually became *The Structure of Scientific Revolutions*. Ultimately, scientific progress, as Kuhn saw it, boils down to the creative reshaping of readily available concepts—often via purely psychological and even non-rational connections. Consider the following example.

Philip Frank writes that Einstein had once provided the following response to the French mathematician, Jacques Hadamard in 1945 when asked about the psychology of his own creative work:

> The *psychical entities* which seem to serve as elements in thought are certain signs and more or less clear images which can be "voluntarily" reproduced and combined […] taken from the psychological viewpoint, this combinatory play seems to be the *essential feature in productive thought* [italics my emphasis] (Frank 1949, 280).

In Frank's words:

> The concepts which arise in our thought and in our linguistic expressions are all—when viewed logically—the free creations of thought which can not inductively be gained from sense-experience (279).

It appears then that for both Einstein and Frank, the psychical entities (tacit elements) play decisive roles in scientific creativity. A similar spirit is present in Kuhn's conviction. According to Lorraine Daston:

> That reasoning from exemplars can indeed be cashed out as rule-following. Kuhn insisted that it could not […] (Daston 2016, 130).

In *Essential Tension*, Kuhn formulated this basic thought in different ways:

> Shared examples can serve cognitive functions commonly attributed to shared rules. When they do, knowledge develops differently from the way it does when governed by rules (Kuhn 1977, 319).

This conviction is echoed as early as March 17, 1955 in a letter to Lawrence S. Kubie:

> My point is that when seeking a new mold to impose upon the world, rational principles of connectivity between symbols and the world are no use, because all such principles are part and parcel of the old mold (or partial perspective). Non-rational Principles, like a propensity to certain sorts of free association, seem all that remain (TSK-MIT-Box 5, Folder 67).

This thought is later affirmed in the postscript to the *Structure*:

> When I speak of acquiring from exemplars the ability to recognize a given situation as like some and unlike others that one has seen before, I am not suggesting a process that is not potentially fully explicable in terms of neuro-cerebral mechanism. Instead I am claiming that the explication will not, by its nature, answer the question, "similar with respect to what?" That question is a request for a rule [...]" (Kuhn 1970, 192).

On the same page, he seems to reinforce his claim by a digression that echoes themes in Wittgenstein's *Philosophical Investigations (PI)*. Kuhn writes:

> But people do not see stimuli; our knowledge of them is highly theoretical and abstract. Instead they have sensations, and we are under no compulsion to suppose that the sensations of our two viewers are the same (192).

For Kuhn, not only Ludwig Wittgenstein and Michael Polanyi, but also certainly to a greater extent Ludwik Fleck are at the center of unfolding of the ideas about paradigms – Kuhn's later 'shared exemplars'. In the preface to *Structure* (vii), Kuhn does recount his indebtedness to Fleck. George Reisch reminds us that Fleck's scientists, like Kuhn's, are deeply immersed in sociological dynamics (Reisch 2019, 220). "[E]ven the simplest observation is conditioned by the thought style and is thus tied to a community of thought," Reisch quotes Fleck's reference to *Denkkollectiv* (220). And in a sense, the circle closes when we are reminded by Allan Janik that there are numerous parallels to Wittgenstein's mature philosophy that we find in Fleck (Janik 2006, 180). In other words, the unfolding of ideas about paradigms in Kuhn's thought are very much related to what Kuhn is acquiring through available 'textbook knowledge'.

As for the notion of paradigm, exemplars, and rules, I think it is worth mentioning that in a debate on theory choice, and in a heated response to Karl Popper, Kuhn writes that neither party has access to an argument which resembles a proof in logic or formal mathematics, and that:

> Only if the two discover instead that they differ about the meaning or applicability of a stipulated rule, that their prior agreement does not provide a sufficient basis for proof, does the ensuing debate resemble what inevitably occurs in science (Kuhn 2000, 156).

Kuhn is suggesting that one of the techniques by which the members of a group learn to see the same thing when confronted with the same stimuli is by being shown examples of situations that their predecessors in the group have already learned to see as like each other and as different from other sorts of situations (194). Let me further clarify by drawing a parallel: In two sections of Wittgenstein's *PI (Philosophical Investigations)*, Wittgenstein askes how do you *undertake* the use of a private definition of a word you have given yourself? "Is it to be assumed that you invent the technique of using the word; or that you found it ready-made?" (*PI* 262) (Wittgenstein 1989, 93) and "Once you know *what* the word stands for, you understand it, you know its whole use" (*PI* 264) (93). In a similar vein, Kuhn writes:

> […] once we have learned to do it, recognition of similarity must be as fully systematic as the beating of our hearts. But that very parallel suggests that recognition may also be involuntary, a process over which we have no control. If it is, then we may not properly conceive it as something we mange by applying rules and criteria (Kuhn 1970, 194).

Before the Postscript (1969), Kuhn had expounded the issue in a chapter of *Structure* titled 'The Priority of Paradigms', which unequivocally – albeit in passing – refers to family resemblances and tacit knowledge associated respectively with Wittgenstein and Polanyi. According to Kuhn, the existence of a paradigm need not even imply that any full set of rules exists (44). In this connection, he mentions that "Michael Polanyi has brilliantly developed a very similar theme, arguing that much of the scientist's success depend upon "tacit knowledge"(44). He then asks: "In the absence of a competent body of rules, what restricts the scientist to a particular normal-scientific tradition?" (44). Reformulated by Reisch:

> What do the members of a scientific community share that explains how they work together effectively, how they understand each other's work and its goals, and why their professional judgments about scientific matters fundamentally agree? His new answer he explained, was not "a paradigm or a set of paradigms," but a disciplinary matrix (Reisch 2019, 348).

Before writing the Postscript, Kuhn who was groping with the problem, suggested that Wittgenstein had developed partial answers to questions like these (Kuhn 1970, 44). According to Kuhn, "For Wittgenstein, in short, games and chairs, and leaves are natural families, each constituted by a network of overlapping and crisscross resemblances (45). Kuhn concluded that what various research problems arising within a single normal-scientific tradition have in common is not that they satisfy some explicit or even some fully discoverable set of rules, but instead they may relate by resemblance to other scientific corpus which the community already recognizes as among its achievements (45-46). In short, I have argued that Polanyi and especially Wittgenstein's texts, or "style of thinking" if you will, with its ineffable character had an appeal to Kuhn and can be considered one of the propensities or one of the characteristics of *Structure*.

4.2 Developmental Psychology

This section begins by examining the notion of gestalt alongside Piaget's developmental stages and broader developmental psychology, before turning to N. R. Hanson's application of gestalt experiments in his work. I then offer a reconstruction of Kuhn's interpretation of Piaget's experiments with children. In doing so, I aim to demonstrate that the significance of Piaget's experiments in Structure *extends well beyond a mere invocation of gestalt; they underpin what Kuhn terms the "fringe of vague meaning." This concept finds clear parallels in Polanyi's tacit element, Langer's idea of "insight," and even in Wittgenstein's discussions on language use—an insight that even an early Russell might have endorsed.*

In *Mysticism and Logic* (Essays 1901-1915), he writes:

> Instinct, intuition, or insight is what first leads to the beliefs which subsequent reason confirms or confutes; but the confirmation, where it is possible, consists, in the last analysis, of agreement with other beliefs no less instinctive. […] Even in the most purely logical realm, it is insight that first arrives at what is new (Russell 1957, 12).

In short, the "fringe of vague meaning" enabled Kuhn in the Structure to state that in the absence of rules, it was still possible for working scientists to actively move forward. In short, I intend to show in this section that "the fringe of vague meaning" was a pillar and another propensity of the genesis of *Structure*.

In the Postscript (1969) to the *Structure* and in discussing translation, persuasion, and conversion in scientific communities, Kuhn touches upon what has been associated with *Sapir-Whorf hypothesis*, "a term coined by Harry Hoijer (1954) to a conference on the work of Benjamin Lee Whorf in 1953" (Scholz 2023). In the preface to the *Structure,* Kuhn indeed mentions both Jean Piaget and the Gestalt psychologists as well as "B. L. Whorf's speculations about the effect of language on worldview" (Kuhn 1970, vi). According to Whorf, the grammar of a language "is not merely a reproducing instrument for voicing ideas but rather is itself the shaper of ideas" and the "guide for the individual's mental activity" (Scholz 2023). It appears that the example of Whorf, later flows in the *Structure* into a discussion of Wittgenstein use of language, i.e. "games" and "family resemblance" (Kuhn 1970, 44). To clarify aspects of *gestalt*, Kuhn writes in the Postscript:

> To translate a theory or worldview into one's own language is not to make it one's own. For that one must go native, discover that one is thinking and working in, not simply translating out of, a language that was previously foreign (204).

For Kuhn, the process appears spontaneous, that is, one is in a sense delivered into a new translation or as Kuhn himself puts it one "has slipped into the new language without a decision having been made" (204). This "conversion process" Kuhn likens to "a gestalt switch" and that he claims is "at the heart of the revolutionary process" (204). This is nevertheless a very general notion of *gestalt*. But what were Kuhn's earliest concrete thoughts

on the *gestalt*? The archival materials for 1949 show that in a response to Dr. Rado [54] Kuhn describes:

> […] the Gestalt school which indicate that we tend to see things first as wholes, that our perceptions of the parts are affected by the manner in which we view the wholes, and that cultural and educational factors may alter our perceptual groupings (TSK-MIT).[55]

This definition involving parts and whole, in fact originates from Goethe,[56] and is mentioned in Heinz Werner's *Comparative Psychology of Mental Development* (1948) [first published in 1940] (Werner 1957, 40-41) with an introduction by Gordon W. Allport, a professor of psychology at Harvard. According to David Kaiser "Reading Piaget and Werner clearly left their mark on young Kuhn" (Kaiser 2016, 79). Kuhn had also recommended the 1948 edition of the book "to a correspondent who had asked for reading suggestions about the psychology of perception" (79).

In Connection to Piaget (1890-1980)[57], Werner cites Ernst Cassirer's *Sprache und Mythus* (1925), *Philosophie der symbolischen Formen* (1923-29), and *Begriffsform in Mythischen Denken* (1922). Werner writes: "Observation and experimental evidence point to the conclusion that the formation of concrete groups based on a realistic similarity develops early in *childhood*" (Werner 1957, 230). Within this context, he refers to Cassirer discussion of South Sea Island languages that "Expressions for the sun and moon are classified together with those for the human ear, for fish of a certain shape, for canoes, etc. On the other hand, the nose and tongue as long objects may be brought under the same heading" (230). Werner then mentions Piaget's experiments:

[54] For additional detail on Dr. Rado, see Galison, P., in R. J. Richards, L. Daston (2016), *Kuhn's Structure of Scientific Revolutions at Fifty: Reflections on a Science Classic*, p. 56, edited by Robert J. Richards and Lorraine Daston, University of Chicago Press.

[55] TSK-MIT, box 1, folder 6 labeled 'INCOMPLETE MEMOS & IDEAS', 1949 [the year on the folder is handwritten].

[56] The more perfect the creature becomes, the less similar become the [morphological] parts to one another. On the one genetic pole the whole is more or less similar to the parts. The more nearly equal the parts, the less are they subordinated one to another. Subordination of the parts indicates a perfect creature (Werner 1957, 40-41).

[57] "Successively or simultaneously, Piaget occupied several chairs: psychology, sociology and history of science at Neuchâtel from 1925 to 1929; history of scientific thinking at Geneva from 1929 to 1939; the International Bureau of Education from 1929 to 1967; psychology and sociology at Lausanne from 1938 to 1951; sociology at Geneva from 1939 to 1952, then genetic and experimental psychology from 1940 to 1971. He was, reportedly, the only Swiss to be invited at the Sorbonne from 1952 to 1963. In 1955, he created and directed until his death the International Center for Genetic Epistemology" (from: Jean Piaget Society for the Study of Knowledge and Development, https://piaget.org/about-piaget/).

> Some investigators, Piaget in particular, have demonstrated the vital concrete connection between the name of a thing and the thing itself during the earlier stages of childhood. At an initial stage—up to five or six years, according to Piaget—the child believes that the name is inside or attached to the thing. In consequence he believes that he can recognize the name of an object simply by looking at it (255-256).

The point I am emphasizing is Kuhn's early interest and acquaintance with certain aspects of Piaget's work and how that acquaintance had germinated. Before reconstructing Kuhn's thought on Piaget, it is important to briefly mention Norwood Russell Hanson and how he was, in a sense, a catalyst for Kuhn to seriously deal with Piaget's ideas and to go beyond Hanson.

4.2.1 Going Beyond N. R. Hanson

In *Structure*, Kuhn acknowledges Hanson and his "gestalt demonstrations". Hanson was not only a prominent philosopher, but also someone who had expressed in *Patterns of Discovery* – specially the chapter titled "Observation" – aspects of a new way of thinking that Kuhn himself was very much attracted to (TSK-MIT-Box 28, Folder [Athens]). Hanson had had the opportunity to referee Kuhn's manuscript for the University of Chicago very early. In referring to Hanson, Kuhn writes:

> Yet, though psychological experiments are suggestive, they cannot, in the nature of the case, be more than that. They do display characteristics of perception that could be central to scientific development, but they do not demonstrate that careful and controlled observation exercised by research scientists at all partakes of those characteristics (Kuhn 1970, 113).

When discussing Kuhn's opinion of the "inadequacy" of *gestalt* experiments to the practice of science, a comparison of Kuhn 1962 and Kuhn 1949 is in order. Let me refer you back to Kuhn's "response" (briefly elaborated in chapter 3) to Dr. Rado in 1949, where we are bound to find only a slightly different emphasis on this very point; the citation that follows is quite similar in intent with the above citation from *Structure*:

> The large residue of objectivity of the perceptual function is further and drastically reduced during the conscious reporting process, an essential intermediary in all research, for an individual will fail to "see" (or at least to consciously note that he has seen) an aspect of a given phenomenon which

> at a later date will prove crucial to the conceptual construction of scientific law (TSK-MIT).[58]

Besides these similarities, the comments on Hanson mentioned a few lines earlier, can be interpreted as Kuhn downplaying Hanson, but Kuhn was highly interested in the psychological phenomena affecting how they may apply to science. In the Athens interview, Kuhn expresses his great frustration at reading Hanson's book *Patterns of Discovery* (1958). The passage below was meant to be 'off the record' and was eventually left out of the printed version in *The Road Since Structure* (2002).

> I had been wrestling with this thing, and this is a number of years later. And somebody called my attention to N. R. Hanson's *Patterns of Discovery*. And I read it, and I was upset, because I wanted to talk about these things, and I also said to myself, "But if I could have let go of these ideas in this form, I'd been out many years ago." […] it tells you something about where I was at with respect to these ideas (TSK-MIT-Box 28, Folder [Athens]).

Again, one may misinterpret Kuhn as downplaying Hanson's ideas but I strongly believe that Kuhn wants to go beyond them because he finds them, albeit crucial, inadequate to the practice of science. He in turn appeals to his own brand of narrative namely one involving historical considerations:

> If historical example is to make these psychological experiments relevant, we must first notice the sorts of evidence that we may and may not expect history to provide. […] In the sciences, therefore, if perceptual switches accompany paradigm changes, we may not expect scientist to attest to these changes directly (Kuhn 1970, 113-115).

It appears then that Kuhn of the *Structure* continued to adhere to his *earlier* views (as far back as 1949) namely that language, psychological factors, and interpretation were crucial. In 1955, Kuhn wrote:

> […] I doubt that there is (even as an ideal) an "undistorted" view of a unique "given" world. I find it more useful to suppose that either: i.) The world itself is shaped by our beliefs, and without beliefs there is no perception and no world (except, perhaps, James' "Bloomin' buzzin' confusion) […] (TSK-MIT-Box 5, Folder 67).

[58] TSK-MIT, Box 1, Folder 6 labeled 'INCOMPLETE MEMOS & IDEAS' [1949 is the year on this folder in handwritten, but there are book loans from the library that are from 1950. There are also some undated writings, which I suspect could go back as early as 1946].

In *Structure*, Kuhn repeats this very same thought:

> What a man sees depends both upon what he looks at and also upon what his previous visual-conceptual experience has taught him to see. In the absence of such training there can only be, in William James's phrase, "a bloomin' buzzin' confusion (Kuhn 1970, 113).

Bear with me now for a quick detour on a possible source of Kuhn's assertion. The source of this thinking may indeed be William James who in his *The Principles of Psychology* (1890) asserted this very view:

> *The psychologist's attitude towards cognition* will be so important in the sequel that we must not leave it until it is perfectly clear. It is a *thorough-going dualism* (James 1890, 218).

Here James has a two-page quotation of Bordon Parker Bowne's (B. P. Browne) *Metaphysics*; in the footnote, there is also a mention of Herman Lotze's *Logik* on the same topic.[59]

> But in order to pass beyond these signs into a knowledge of the outer world, we must posit an interpreter who shall read back these sign into their objective meaning. But that interpreter, again, must implicitly contain the meaning of the universe within itself [compare with Kuhn's assertion in the citation above "experience has taught him to see"] (220).

Following a long citation, James wrote:

> The dualism of Object and Subject and their pre-established harmony are what the psychologist as such must assume, whatever ulterior monistic philosophy he may [...] have in reserve. I hope that this general point is now made clear, so that we may leave it, and descend to some distinctions of detail (220-221).

Later, in 1920, James Bissett Pratt's (one of the authors of *Essays in Critical Realism*) observation about idealism and realism further demonstrates the philosophical tensions of the time:

> For nearly a century idealism in some form or other dominated philosophy. Almost all the thinkers of to-day were brought up under its influence. Realism was banished from text-book and class-room except as a " terrible example," […] (Pratt 1920, 88).

Therefore, we might now be able to see that Kuhn's thought very clearly reflected James' conviction – as related to psychology of perception. Stadler has pointed out that according

[59] Bordon Parker Browne (1847-1910) was an American Christian philosopher associated with Theistic Idealism. Hermann Lotze (1817-1881) was a prominent representative of German Idealism.

to James' biographer, Harmann Lotze was among three German philosophers James was indebted to besides Fechner and Mach (Stadler 2017, 12). In a similar vein, Philipp Frank, speaking of his early years at the turn of the century as a young graduate in Physics, writes:

> We saw much truth in Kant's statement that the recording of observations is not a purely passive act but that a great deal of mental activity is necessary in order to formulate general statements about observation. […] Our whole group understood and fully agreed that the human mind is partly responsible for the content of scientific propositions and theories (Frank 1949, 7).

The above is to show that Kuhn's "previous visual-conceptual experience" (Kuhn 1970, 113) may have its roots in a version of a mind-dependency present in James, Frank, and particularly Lovejoy.

4.3 Reconstructing Kuhn's Thought on Piaget

So far, I believe I have established Kuhn's sustained interest in the psychological debates of the late 1940s and 1950s. But how do Piaget's discoveries fit into Kuhn's broader project? I aim to show that Piaget's influence on Kuhn extends beyond the simple notion of gestalt. In other words, there must be more to the story. Recall, for example, Kuhn's reference to Hanson, where he suggests that scientists' mere "shifts in perception"—if they occur at all—are insufficient to fully account for scientific change.

. In the Athens interview, late in Kuhn's career, Kuhn says:

> […] I guess in reading Merton's thesis, somehow or other, I think it was there that I discovered Piaget, And I read a good deal [...] I read beginning with the [*Les notions de movement et de Vitesse chez l'enfant*]. And I kept thinking, my, these children develop ideas just the way scientists do, except - and this was something I felt Piaget did not himself sufficiently understand, and I'm not sure that I realized it that early – they are being taught, they are being socialized, this is not spontaneous learning, but learning what it is that is already in place. And that was important (TSK-MIT-Box 28, Folder [Athens]).

In the previous section (see 4.2), I spoke of the roots of Kuhn's *acquaintance* with Piaget, but if I am not mistaken, Kuhn, never really elaborated on the roots of his *interest* in Piaget's experiments on kids. In the above interview, Kuhn attributes social interactions to kids in Piaget's experiments, but I propose that the roots of these interactions in Kuhn's early

thoughts on the subject, lie elsewhere. I intend to answer this crucial question by reconstructing Kuhn's very early thought on the subject. Archival materials for 1949 show several references to Piaget, particularly fragments involving kids in Piaget's experiments. Kuhn first proposed a model:

> This model has both logical and psychological consequences. In the first place, since the connotations of our words are pragmatically abstracted (as the particular connotations most likely to distinguish the members of this block from those of another) and are therefore not logically complete for a world of all possibles (in which a language would be of no use, for it could not determine our behavior but only describe the past - if that), our words must have a relatively large fringe of possible objects to which their application is uncertain - requires a further choice. *This is the fringe of vague meaning* [italics my emphasis MNB] (TSK-MIT-Box 1, Folder 6).[60]

It appears that it is this "fringe of vague meaning" that Kuhn is after, but why? Now, enter 'Kids in Piaget' experiments. Piaget had proposed that at certain early stages of development. According to Iris Levin "[…] the child believes that more velocity covers more space, that more space takes more time to be covered, and hence, that more velocity takes more time […] and hence, as long as the child continues to confuse time with speed, he believes "faster" implies longer duration (Levin 1979, 469-477).

Kuhn writes:

> (Let me mention in passing – since it occurs frequently to kids in Piaget's experiments – that it is on the above model perfectly possible to choose a set of connotations adequate to your past experience which will still be logically inconsistent in the sense that in a future experience the connotations will <u>demand</u> your placing the object in two mutually exclusive boxes. I am thinking particularly of "faster" in Piaget […])(TSK-MIT-Box 1, Folder 6).

There are a couple of more steps to see what Kuhn is after. Kuhn continues:

> But giving all this and a good deal more to logical language, they seem inevitably to suffer from the following shortcoming (at this point I go metaphysical – can't help it). They apply only to a world which you know all about in advance […] What I'm getting at is that natural language provides a finite means of mediating an infinitely complex universe. The consideration of a particular cut chosen is entirely pragmatic, and since pragmatic grounds change it is necessary that any language with the characteristic of a natural language be self-correcting to some extent […]. But a logical

[60] TSK-MIT, box 1, folder 6 labeled 'INCOMPLETE MEMOS & IDEAS', 1949 [the year on the folder is handwritten].

> language can't help but sacrifice this fluidity which is an essential of natural language, and in sacrificing this property it loses its ability to the acquisition of knowledge in which the fluidity plays an essential role (TSK-MIT-Box 1, Folder 6).

We can then see that it is the "fringe of vague meaning" that Kuhn is after:

> But the "fringe" lying between the narrow and broad choices is characterized (metaphorically) by a weighting function, differing a good deal from individual to individual, which represents our expectations about the likelihood of an actual occurrence in that part of the fringe (TSK-MIT).[61]

I strongly suspect that the later Kuhn's remark in Athen's interview cited a page earlier, namely, '[…] learning what is in place' refers to nuances of "interpretations" that in some sense also remain universal.

In *Structure*, Wittgenstein and Polanyi not only reinforce Kuhn's discussion of the "rule following", but they also buttress Kuhn's conviction that one of the central elements of the logic of science, what may even seem "illogical" or "irrational" is vague, malleable, and as far as science is concerned, entirely open to revision. Interestingly, whether Kuhn was aware of this or not, Lovejoy's had defined this psychological phenomenon[62] very neatly:

> That, pragmatically considered, knowledge is thus necessarily and constantly conversant with entities which are existentially " transcendent " of the knowing experience and frequently with entities which transcend the total experience of the knower (Lovejoy 1920, 76).

A. R. Hall's [63] final sentence in his book *The Scientific Revolution 1500-1800* (1954) interestingly refers to the same point:

> […] we cannot write the full history of science save by reflecting the operations of original thought, which we do not understand; and that we

[61] TSK-MIT, box 1, folder 6 labeled 'INCOMPLETE MEMOS & IDEAS', 1949 [the year on the folder is handwritten].

[62] The „fringe of vague meaning „echoes Wittgenstein's "ineffable" and Polanyi's "tacit" knowledge.

[63] A biographical detour (already mentioned in 2.6) of the reviewer reveals that Marie Boas Hall attended Ratcliff College (an all-female college taught by Harvard professors) where Susanne K. Langer had also studied. In 1952, she wrote her dissertation on Robert Boyle. Boas Hall was an American historian of science known for her studies of 16th and 17th centuries science and the author of *Robert Boyle and Seventeenth Century Chemistry* (1958). During the 1950s and early '60s, sections of both Marie Boas Hall (pp. 48-74) and Alfred Rupert Hall's (to whom she was married) books were on Kuhn's syllabi under assigned readings. A. R. Hall had written *The Scientific Revolution 1500-1800: The Foundation of Modern Scientific Attitude* (1954). (the course syllabi are located in TSK-MIT, Box 1, Folders 1, 25, 30).

> cannot exclude from science, which is rational, the influence of factors which are irrational (Hall 1954, 367).

In fact, there are numerous examples reflecting this vey thought in *Structure*, but perhaps one will suffice. Kuhn writes:

> Scientists can agree that a Newton, Lavoisier, Maxwell, or Einstein has produced an apparently permanent solutions to a group of outstanding problems and still disagree, sometimes without being aware of it, about the particular abstract characteristics that make those solutions permanent. They can, that is, agree on the *identification* of a paradigm without agreeing on, or even attempting to produce, a full *interpretation* or *rationalization* [64] of it (Kuhn 1970, 44).

Finally, and in the very same vein, in the concluding sentence of the final Lowell Lecture series in March 1951 (Lecture VIII titled "Canons of Constructive Research") Kuhn says:

> So continuing progress in research can be achieved only with successive linguistic and perceptual re-adaptation which radically and destructively alter the behavioral worlds of professional scientists. (Reisch 2021, 152).

The above reconstruction of Kuhn's engagement with Piaget's findings is intended to show that, through his interaction with Piaget's experiments on children, the young Kuhn moved beyond the gestalt model to uncover what he called the "fringe of vague meaning." This shift was not merely inspired by the philosophical trends or the prevailing "textbook knowledge" of the time, but more importantly, it revealed that Piaget's experiments served Kuhn's broader strategy: to demonstrate that scientific activity was not simply a purely logical endeavor, but one that involved elements that eluded clear, quasi-rational definition altogether.

In this connection, it is worth noting that Susanne K. Langer (see 2.4) in her book *The Practice of Philosophy* (1930) had dedicated the entire chapter VII to "Insight". In fact, one could say that this chapter, in a sense, vindicates Kuhn's appeal to harping on the concept. As mentioned in chapter 2 Kuhn had, at least, read two books by Lagner, namely, *Philosophy in a New Key* (1942) and Langer's *Introduction to Symbolic Logic* (1937). It is not known to me that Kuhn had read Langer's *The Practice of Philosophy*. Langer wrote:

> Through the symbolic act or image, an important past event plays into the later history of the organism, without being discursively known. But I maintain that such formulation of an idea is knowledge in a wider sense—

[64] Here Kuhn inserts a reference to Polanyi's "tacit knowledge" in *Personal Knowledge* (1958). Immediately following these remarks on the following page, there are references to Wittgenstein's language games, family resemblance, and rule following.

> it acts as a stimulus, puts us into rapport with a past event just as a proposition might be (Langer 1930, 164).

She justified the above in the following way:

> There is no knowledge without form; and probably no form is unique; therefore all knowledge can find symbolic expression. But it is the types of expression rather than the organ of recognition that determines the difference between insight and inference. […] A scientist of genius is a person who can apprehend a new concept through some natural medium for whom there are unprobed patterns […] which some enlightened psychologists have recognized as the principle of "Gestalt," and which is "form" in a really general sense (165-166)

She then concluded the chapter by saying that there is nothing "alogical" about this process:

> I do not see why, in the case of intensive [as opposed to extensive] symbolism, where the meaning is sometimes incommunicable, the process of understanding should be classed as "alogical" […] the personal discovery of meanings through myth, ritual, and art, highly individual and awe-inspiring by its subtlety, is the very acme of logical procedure, and the refinement of intelligence. (166)

No doubt, the source of language of myth for Langer is Cassirer. There are several references to Cassirer's *Philosophie der symbolischen Formen* (1923-1929). She writes:

> The kernel of a myth is a remote idea, which is *shown*, not stated, in the myth. It is only the myth that is stated in words. Ernst Cassirer has made this point excellently clear in his *"Philosophie der symbolischen Formen"* […]. (158) [65]

Further, Langer also dedicated the bulk of chapter V titled "The Logical Basis of Meaning" to Wittgenstein's Tractatus (1921). The point I am making at the conclusion of this section is that Kuhn's "fringe of vague meaning" as well as similar concepts such as the "ineffable" or "tacit" or relating to "insight" was either a "craze" [66] of the time, or the "craze" was

[65] Here Langer freely translates a segment of over a page of Cassirer's book but I shall only refer to a sentence or two: "Thus the philosophical 'rescue' of Myth, which is at once its philosophical dissolution, consists in conceiving it as a form and a stage of actual cognition—and, in fact, as a form of cognition which is peculiar and appropriate to a definite subject-matter, to which it gives adequate expression. Plato assigned a certain conceptual content to the mythical sphere: for he regards it as the only symbolic form where in the world of Becoming can be expressed" (159).

[66] Feyerabend in a letter to Kuhn speaks of "Wittgenstein, at least, is an original source for some craze" and adds "And if they ask, 'shall we read PHILOSOPHICAL INVESTIGATIONS?' I reply if you want to waste your time, yes." (Letter Feyerabend to Kuhn 1961 in *Feyerabend's Formative Years. Volume 2. Feyerabend*

already part and parcel of the philosophical awareness of the time when discussing how science worked. For Kuhn, the question remained, however, that if science had such an "unspeakable" element, how did scientists chose among relevant theories? This is the subject of the next section.

The Building Blocks of *Structure*

4.4 Revolutions as Settling "Matters of Taste"

I intend to show in this very short section that Kuhn by using examples in the arts and with it "taste" reasons for himself – as reflected in archival notes, and later in 'Structure' – that theory choice is in principle arbitrary.

For Kuhn and *Structure*, the analogy to the art plays decisive role in turning "non-cumulativeness" into scientific revolutions. For a section labeled 'NEW OUTLINE – CHAPTER 1', under 'II. WHAT IS A REVOLUTION', we can read:

> i. Vocabulary that gives general description of what I want to call scientific revol[ution] is close to that for change in artistic taste.
> ii. Many of techniques relevant to bringing revolution to close are closer to those relevant to settling matters of taste than to what would normally be called validation. [handwritten] Persuasion far more than proof (TSK-MIT-Box 4, Folder 2).[67]

According to Kuhn and in the same document, the difference between science and art is "where we go to learn about them". He then enumerate: "Art from museums, libraries, etc." He then compares that with science: "Popular writings on science's achievements, technology, texts, and teachers. What science has achieved vs. what artists have done." Kuhn concludes with a phrase he had used on past occasions, namely "Science destroys it's [its] history" (TSK-MIT-Box 4, Folder 2).

> This difference is, in fact, immensely consequential. May come to see possibility of presenting science this way as main key to apparent cumulativeness of enterprise. Reason for rapid progress, etc. (TSK-MIT-Box 4, Folder 2).

on Logical Empiricism, Bohm & Kuhn. *Correspondence and Unpublished Papers*, eds. Matteo Collodel and Eric Oberheim, p. 311. https://doi.org/10.1007/978-3-031-57519-8, Springer 2024.

[67] TSK-MIT, Box 4, Folder 2 [SSR: Monograph: Chapter 1-1st Draft].

In other words, Kuhn is arguing that cumulativeness is misleading; it misleads, among others "about important problem of scientific decision making" (TSK-MIT-Box 4, Folder 2). In *Structure*, it takes the following form:

> Copernicus destroyed a time-honored explanation of terrestrial motion without replacing it; Newton did the same for an older explanation of gravity, Lavoisier for the common properties of metals, and so on. In short, if a new candidate for paradigm had to be judged from the start by hard-headed people who examined only relative problem-solving ability, the sciences would experience very few major revolutions. Add the counter-arguments generated by what we previously called the incommensurability of paradigms, and the sciences might experience no revolutions at all (Kuhn 1970, 157).

The references to the arts in *Structure*, are primarily in section "XIII. Progress through Revolutions". within the context of our discussion, perhaps one of these references is worth noting here. Kuhn refers to the "textbooks education" in the arts versus natural sciences, where he emphasizes the much bigger role of reliance on textbooks for the latter (165). In short, Kuhn addresses "the professional scientific community and the nature of its educational initiation. In music, the graphic arts, and literature, the practitioner gains his education by exposure to the works of other artists" (164-165).

> Scientific education makes use of no equivalent for the art museum or the library of classics, and the result is a sometimes drastic distortion in the scientist's perception of his discipline's past. More than the practitioners of other creative fields, he comes to see it as leading in a straight line to the discipline's present vantage. In short, he comes to see it as progress. No alternative is available to him while he remains in the field (167).

Therefore, one must note that Kuhn recognizes two historical realization, first, "science destroy its history", a phrase Kuhn had acquired from Rosenstock-Huessy (see the last section of previous chapter) and Koyré, and second, "cumulativeness" is a fiction that has come about as the result of the misleading phenomenon of "textbook science", which Kuhn had acquired from the writings of Fleck.[68] The answer to the question posed at the end of last section, therefore, is that Kuhn by using the example of the arts and the notion of artistic "taste" argues that theory choice is in principle arbitrary.

[68] As mentioned earlier there is a handwritten reference to Fleck and textbook science as early as 1949.

4.5 Incommensurability

This short section is to show that another building block in Kuhn's 'Structure' is the concept of incommensurability.

Gerald Holton in his personal recollection of the later Kuhn, refers to his "reconceptualization" as Kuhn had called it, and that Kuhn needed to reinterpret "the main parts of his previous thought" – "[T]he answer is incommensurability", Kuhn had put it (Holton 2012, 34). For Kuhn, the ambivalence of incommensurability, or "reconceptualization" was born out of a wariness towards both the slippery slope of "cumulativeness" and again for Kuhn its often accompanying "Whiggish" component. The controversy over *Structure*, Richards and Daston point out was "provoked by Kuhn's insistence that successive scientific paradigms were in some sense 'incommensurable' (Richards & Daston 2016, 4). I showed in the previous chapter, Kuhn first struggled with "cumulativeness" but eventually left the bulk of it out of an early draft of *Structure*[69] in favor of non-cumulativeness – and consequently – 'abruptness' at the right moment as in revolutions, and incommensurable in character like many revolutions.

Briefly fast-forwarding, it is worth noting that in 1982, in a PSA article, Kuhn wrote that it has been twenty years since Paul Feyerabend and I "first used in print what we had borrowed from mathematics to describe the relationship between successive scientific theories. 'Incommensurability' was the term" (Kuhn 1982, 669)., referring to "the meaning of scientific terms and concepts […] that often changed with the theory in which they were deployed" (669), which led according to Kuhn, to the impossibility of defining all the terms of one theory in the vocabulary of the other (669). Kuhn emphasizes that he had "repeatedly likened theory-changes to Gestalt switches" (669). He admits and, in a way, complains that no one has fully faced "the difficulties" that had led Feyerabend and himself to incommensurability (669). In notes to the above article, he writes:

> I believe that Feyerabend's and my resort to 'incommensurability' was independent, and I have an uncertain memory of Paul's finding it in a draft manuscript of mine and telling me he too had been using it (684).

[69] Among the archival materials I have explored for this chapter, I have apparently come across the earliest version of *Structure's* first draft (In a conversation with Hoyningen-Huene, he told me that this draft could even be the proto, proto *Structure*, which would date earlier than the version Hoyningen-Huene himself had introduced as the proto *Structure*).

Kuhn then lists his use and Feyerabend's first use of the term as early as 1962. However, the archival materials, specifically the lecture note for History of Science 105, point to 1955, in discussing Copernicus "[one] Can trace one tradition to this point, other from it. But two ends are incommensurable" (TSK-MIT).[70] Also, there are three references to 'incommensurable' in the outline of the first chapter, and the draft of a first chapter that changed almost entirely in the published version in 1962 (unfortunately not dated).

In the same PSA article, Kuhn's example is that the theoretical term 'phlogiston' is "not eliminable from any text that purports to be a translation" of the term and if so "they seem also not to be replaceable individually by some set of modern words or phrases", i.e., "the stuff given off in combustion as a chemical principle". He then concludes that such phrases cannot be translations (Kuhn 1982, 676). Kuhn emphasized later in life what eventually became the core of the *Structure*:

> I knew the non-cumulativeness and I knew something about what I took revolutions to be (TSK-MIT-Box 28, Folder [Athens]).

Therefore, incommensurability is another building block in Kuhn's *Structure*. Let us now move on to a final building block, namely Kuhn's appeal to evolution.

4.6 Appeal to Evolution for Scientific Progress

In my estimate, appeal to evolution' constitutes yet another building block of *Structure*. On the very last pages of *Structure*, Kuhn, in a very distinct manner, appealed to evolution by addressing the "goal" of science, and scientific progress. He took issue with pre-Darwinian "German *Naturphilosophen*" in their approach to evolution as a "goal-directed process" (Kuhn 1970, 171). Kuhn pointed to the "most significant and least palatable" of Darwin's ideas as "the abolition of that teleological[71] kind of evolution" (172). On this point, *Structure*

[70] TSK-MIT, Box 1, Folder 23 (H of S 105-Lecture 11-12/20/55).

[71] In his book, Eiseley also refers to a book by Lois Whitney titled *Primitivism and the Idea of Progress*. Here Whitney too referring to Lovejoy writes: "Now both the idea of final causality and that of immutable species were quite compatible with, in fact necessary to, the principle of continuity, which, as Prof. Lovejoy has pointed out, came into great popularity as part of the philosophy of optimism of the late seventeenth and early eighteenth century" (from Whitney, L. (1934), *Primitivism and the Idea of Progress in English Popular Literature of the Eighteenth Century*, p. 139, John Hopkins Press).

directs the reader to A. Hunter Dupree's *Asa Gray* (1959).[72] The book is also listed for 'suggested reading' under 'The Non-Scientific Implications of Darwinism' in Kuhn's course for Spring 1960 (History 105B) with selected chapters XIV – XVIII. What is interesting and relevant to progress here, I gather, is Dupree's reference to William James and pragmatism:

> Gray knew he was making a joke when he said that Darwin had reintroduced teleology into natural history. But the kind of teleology he pointed to here, the purpose which an organ served for the organism which developed it, is pure pragmatism as the younger Americans developed it. Gray is the most clearly identifiable link in American intellectual history between the Providential utilitarianism which was so pervasive in the early nineteenth century and the standard of usefulness which was the keystone of pragmatism at it end. Gray provided a straight highway from [William] Paley to Chauncey Wright and William James (Dupree 1968, 382-383).

Therefore, Gray belittles teleology[73] in evolution and in a sense, in Harvard's early days, evolutionary theory and intellectual history come in contact to produce a pragmatic and non-teleological background at Harvard – the way evolution of scientific progress is depicted in *Structure*. Incidentally, Kuhn owned a copy of Friedrich Albert Lange's *History of Materialism* (1950/1865) (TSK-MIT_Inventory) (see the footnote).

There is also a reference to Loren Eiseley's *Darwin's Century: Evolution and the Men Who Discovered It* (1958) (chapters ii, vi-v) in *Structure*. Kuhn had also used Eiseley's book in 1960 among assigned readings (pp. l-26, 35-55, 65-115, 117-204) (TSK-MIT, Box 1, Folder 30). It is highly interesting since there appears to be a connection between Eiseley's book and A. O. Lovejoy. In the words of Marston Bates who reviewed Eiseley's book for *Science* in 1958:

[72] The other book was R. Hofstadter's *Social Darwinism in American Thought* [no suggested chapters]. Further, the biography of Asa Gray, a professor at Harvard is of particular interest. Gray is perhaps the most important American botanist in the 19th century and his close friendship with Charles Darwin has been documented through their correspondence.

[73] Briefly and as a background, one must keep in mind that the goal referring to teleological thinking in science was not only a concern for Kuhn. Going back to the heydays of neo-Kantianism, Frederick Beiser points out that Darwin's inroads "seemed to vindicate Friedrich Albert Lange's "steadfast naturalism, mechanism and nominalism, his long-standing opposition to the teleology and (conceptual) realism of Platonic-Aristotelian tradition." (Beiser 2014, 429).[73] "Anthropomorphic conception of teleology" for Lange, a neo-Kantian, "had been completely defeated by the theory of natural selection." (430) He believed that "the old model of teleology is deeply misleading" in that it makes an "exception into a rule, as if nature somehow followed a norm or plan" (430). Nevertheless, here Lange takes issue with Kant's regulative doctrine and "thinks that Kant went astray" in regarding "teleology to be a *necessary* manner of thinking for the human mind"(430).

> One is led, almost inevitably, to compare Eiseley's book with Arthur Lovejoy's *The Great Chain of Being*. Eiseley's book in a real sense is built on Lovejoy's book (Bates 1958, 1493-1494).

There are indeed numerous references in the book to Lovejoy and *The Great Chain of Being* (1936). Eiseley's book has also references to other books that mention Lovejoy, e.g. *Primitivism and the Idea of Progress* (1934) by Lois Whitney with a forward by A. O. Lovejoy. Eiseley writes:

> There can be little doubt that the rise of comparative anatomy is inextricably linked to the history of the Chain of Being concept with its gradation of complexity in living forms. […] The whole scheme was as rigidly fixed as the medieval social world itself (Eiseley 1958, 7).

One can imagine, therefore, that for Kuhn this may have been another instance of confirmation that similarly, and in the same vein, the goal-oriented cumulativeness, could just easily be thought of as something malleable – and consequently revolutionary. Eiseley writes:

> Charles Darwin's later contribution lay, not in the application of the struggle for existence to the entire animal creation, but rather in his discovery that biological variation combined with the pruning hook of selective struggle might be the key to endless organic divergence (53).

As far as the method is concerned, this would provide a nice analogy to Kuhn's own method. In Kuhn's system, "normal science" and "crisis" can easily parallel the above analogy (biological diversity and natural selection) and on a meta-level, scientific revolutions would not only parallel political revolution (see chapter 3), but such parallels would now extend into the organic world. Beyond the fact that Kuhn made ample references to political revolutions, he also made it clear that he has been assimilating what had already been out there in the "textbook knowledge" of his contemporaries. In the Postscript (1969), Kuhn wrote:

> To the extent that the book portrays scientific development as a succession of traditional-bound periods punctuated by non-cumulative breaks, its theses are undoubtedly of wide applicability. But they should be, for they are borrowed from other fields (Kuhn 1970, 207).

In *Structure*, Kuhn speaks of "the most difficult and disturbing aspect of Darwin's theory", and prompts the question "What could 'evolution,' development,' and 'progress' mean in the absence of a specified goal?" (172). On this very last page, Kuhn even tops that off with a "Kantian style" transcendental question: "What must nature, including man, be like in order that science be possible at all?" (173) For Kuhn this is a significant conclusion to which he held on until the end of his career. In Athens, he said:

> […] I think I would now argue very strongly that the Darwinian metaphor at the end of the book is right, and should have been taken more seriously than it was; and nobody took it seriously. People passed it right by (TSK-MIT, Box 28, Folder [Athens]).

In the context that Kuhn examines the concept of 'progress', it is worth noting that according to Lois Whitney, Adam Ferguson (1723-1816) in his *Principles* [74] (1792) and also in *Essay on the History of Civil Society* (1773) "[…] has found in this principle of change an adequate basis for a theory of progress for man. The 'law of nature' has become for him the law of progress." (Whitney 1934, 139). The point I am making is that there has been a long history of association of progress with understanding the laws of nature. And Kuhn's use of the concept of scientific progress is analogous with evolution. According to Kuhn himself, the Darwinian metaphor mentioned in the above citation was crucial:

> The question of stopping to see us [or] ceasing to see us getting closer to something, but see us instead as moving away from where we were […] (TSK-MIT-Box 28, Folder [Athens]).

This is, if I am right, Kuhn's very definition of 'scientific progress.' The very last sentence of *Structure* (1962) reads:

> Any conception of nature compatible with the growth of science by proof is compatible with the evolutionary view of science developed here. Since this view is also compatible with close observation of scientific life, there are strong arguments for employing it in attempts to solve the host of problems that still remain (Kuhn 1970, 173).

It is therefore inevitable to conclude that, for Kuhn, the appeal to evolution was not merely significant but also pragmatic. As I have argued, it must be recognized as one of the

[74] *Principles of Moral and Political Science* (1792).

foundational elements of *The Structure of Scientific Revolutions*—a component that provided Kuhn with a sense of confirmation and helped consolidate many of the book's central ideas. [75]

In this chapter, I have outlined the principal components essential to the genesis of *Structure*, tracing both the origins and the development of Kuhn's early theoretical commitments. These components were rooted, above all, in the prevailing "textbook knowledge" of the period.

[75] It is perhaps also interesting to note that both Eiseley's and Dupree's book on evolution that Kuhn used on his course syllabi were published pretty much right before the publication of *Structure* namely in 1958 and 1959 respectively. He may have been thinking about the topic and about how to conclude *Structure*.

5

The Resolution of Historicism

5.1 Kuhn's "Honor List" for Historical Methods of Choice

In an article in *The Essential Tension* titled "The Relations between the History and the Philosophy of Science" (published in 1968 and later revised in 1976), Kuhn names Meyerson and Brunschvicg:

> There have been philosophers of science, usually those with a vaguely neo-Kantian cast, from whom historians can still learn a great deal. I do urge my students to read Emile Meyerson and sometimes Leon Brunschvicg (Kuhn 1968, 11).

He then adds what he approves and disapproves of in their work:

> But I recommend these authors for what they saw in historical materials not for their philosophies, which I join most of my contemporaries in rejecting (11).

What I call Kuhn's "Honor List" refers to the philosophers whom Kuhn frequently names and holds in special regard for their influence on the development of his work. In evaluating these figures, I will adopt a particular approach: because my focus is on the early Kuhn of *Structure*, I will rely on archival materials to guide my selection of which philosophers to emphasize and how deeply to engage with them. In other words, I will follow Kuhn's own practice by highlighting not their philosophical systems in full, but rather the historical insights Kuhn himself drew from their work.

As mentioned in chapter 3, Kuhn in a letter to Chairman of the General Education Committee, Prof. David Owen on January 6, 1951, wrote a seven-page outline of his research plan on the methodology of the physical sciences. On page 3, he writes:

> Also useful is the limited amount of earlier research which proceeds from a similar orientation toward scientific method, particularly the work of Brunschvicg, Fleck, Meyerson, and Koyré (TSK-MIT, Box 5, Folder 84).

For example, note that in *Structure*, Kuhn mentions Fleck only in passing. Yet it is significant that Fleck appears alongside thinkers whom Kuhn would later reference more extensively. In the following pages, I will examine the individual perspectives of these four figures, closely following Kuhn's interest in "what they saw in historical materials." What follows, then, are four brief descriptions of the historical outlooks each of these individuals espoused. We begin with Brunschvicg.

5.2 Leon Brunschvicg (1869-1943)

In "History of Science" an article published in 1968 Kuhn wrote:

> And partly it was learned from a small group of neo-Kantian epistemologists, particularly Brunschvicg and Meyerson, whose search for quasi-absolute categories of thought in older scientific ideas produced brilliant genetic analyses of concept which the main tradition in the history of science had misunderstood or dismissed (Kuhn 1977, 108).

In this very brief section on Brunschvicg, I intend to look at Brunschvicg's historical method. According to Bernard Elevitch historical interpretation, which constitutes the bulk of Brunschvicg's work, is his method (Elevitch 1961, 11). Elevitch asserts that Brunschvicg treats historical evidence as his principles because for Brunschvicg "Philosophy […] sums up the experience of thinking humanity" (11). Therefore, "an examination of the various doctrines that history has produced" (Brunschvicg 1897, 42) becomes an imperative for Brunschvicg. "His method, he said, reflects the philosopher's obligation to read the works of the past with an open mind rather than with the rigid conviction of dogmatism" (Elevitch 1961, 13). In *Ecrits philosophiques*, Brunschvig speaks of "infinitely boundless resources of the intelligence" with the following underlined in the original text: "Our positive and precise objects will then be the discovery of an intellectual consciousness" (14). Brunschvicg's view of history may be associated with:

> his essential *mobilisme* or vitalism: the refusal to commit himself to one norm for the indefinite future, since the direction of the mind's invention cannot be grasped before its actual accomplishment [*Ecrits philosophiques* II, p.119]. (15).

Elevitch writes that Brunschvicg devoted entire books or sections of books to Descartes, Spinoza and Kant (also Montaigne and Pascal) and returns to them time and again (15). Brunschvicg's sees his tasks, Elevitch explains, in terms of the ideals of Western 'Cartesian'

thought and in illuminating reason's mathematical accord with nature and in delineating the "realm of the mind" (21). Elevitch formulates the move from Leibniz to Kant succinctly:

> Kant endeavor to account for the conditions of knowledge is seen as a recovery of the "heritage" of Spinoza, regarded by Brunschvicg as having been deflected from its course by Leibniz's ultimate reliance on a transcendent God (23).

I submit that the quotation right before the last very clearly points to freedom of thought and creativity – a vital element in Kuhn's early thought – which was also one of the main points I emphasized at the beginning of the previous chapter.

5.3 Émile Meyerson (1859-1933)

Imagine, it is the 1950s and Kuhn has just returned from a trip to Europe and has started teaching Harvard University's "General Education Program". At this time, Karl Popper is presenting in the William James Lectures at Harvard. After he had met him there, Kuhn recalls that Popper kept talking about how the older theories embrace the earlier one and that was something Kuhn did not agree with at all because "It was too positivist for me" (TSK-MIT, Box 28, Folder [Athens]. He also recalls that Popper later sent him a book – from an unusual "suspect" so to speak since he had not expected that. "But Popper did me a tremendous favor […] getting the books that meant something to me…". The book was Émile Meyerson's *Identity and Reality*. "And that was […] I didn't like the philosophy at all. But boy, did I like the sorts of things he saw in historical materials. He went into those briefly and I mean he didn't do it as a historian but he was getting it right in ways that were different from the ways that history of science was being written." (TSK-MIT, Box 28, Folder [Athens]).

Meyerson's seminal work *Identité et Réalité* was first published in 1908 in France; the first English translation *Identity and Reality* appeared in 1930 in the United States. Meyerson writes in the preface to his book about a procedure; it is with the help of the history of science, he says, that we shall search for the solution of problems concerning common sense (Meyerson 1930, 8). The foundation of this procedure is the identity of the "progress of conscious and unconscious thought", which is to function as a "*heuristic* principle, a *working hypothesis*" (8). His aim in the book, he says, is to investigate "whether it is true, as Comte and, later on, Mach affirm, that all science is established only for the purpose of action and

prevision". He introduces right at the outset the two important principles of "lawfulness" and "scientific causality" (10). According to Meyerson, the book is to state that "the world of common sense is created by a process strictly analogous to that which produces scientific theories" (10).

Let us follow an example of Meyerson's style of looking at history. This example involves "the unity of matter" and the chemical revolution:

> For Descartes it is a fundamental and self-evident truth: "There is but one and the same matter in the whole universe." After him everyone seems agreed upon this point. [….] how attribute one and the same property to material fundamentally diverse? (234).

Both partisan and adversaries, writes Meyerson, were convinced of the unity of matter. Chemists share the same opinion; Boyle a firm adherent of corpuscular theories "openly affirms the unity of matter" (235). Meyerson says that there was a crisis in the eighteenth century, "a crisis which becomes more and more accentuated" (235). According to Meyerson:

> Nothing, it seems, is more essential in chemistry than the concept of the element, and it is precisely this concept which is in the act of being completely transformed. And yet the historians of chemistry have never taken this transformation as a point of departure of a new epoch. This is because it is so slow, imperceptible, and, as it were, spontaneous (234-235).

Meyerson then points to the crux of the issue namely the existence of "a kind of extremely powerful undercurrent of which even the protagonists of chemistry are not conscious, which carries them along, so to speak, in spite of themselves" (235). Kuhn's terminology also suggests that the protagonists—representing scientists engaged in "normal science"—may not be fully aware of the underlying forces driving their work forward, forces that will eventually lead to a crisis.

Meyerson briefly expounds on the chemistry of *phlogiston* and some of the characters involved with it. He points out that even Stahl as the first person to use the term phlogiston is not known to have:

> […] ever declared that he considered metallic calces veritable elements. Yet little by little this last opinion (prepared in the seventeenth century by the writings of Boyle […]) tends to prevail amongst his successors. It is certainly found already solidly established on the eve of Lavoisier's discoveries (235).

Meyerson concludes, that "the 'chemical revolution' of which he [Lavoisier] was the author, found the way prepared" (Meyerson 1930, 235-236).

I will take the liberty of making a brief detour here, involving both Kuhn and Friedman. On the one hand, in Meyerson's description of the chemical revolution, we can clearly see elements that Kuhn would later isolate in *Structure*—namely, the concepts of "normal science" and "crisis," as mentioned earlier. The key difference in Meyerson's account, however, is that the scientists appear largely unaware of the "undercurrents" beneath the surface of their professional activity. On the other hand, I would suggest that we also find a connection here to Friedman's notion of the "incremental" and "interdependent" development of science and "scientific philosophy," leading eventually to major upheavals, or scientific revolutions. But what does this mean? I would cautiously propose that Friedman's claims reflect Meyerson's view. Likewise, I would suggest that Meyerson's perspective helped shape Kuhn's concepts of "crisis" and "normal science." Kuhn's contribution, in contrast to Meyerson's, would then lie in elaborating a step that Meyerson left underdeveloped: Kuhn's emphasis on the accumulation of "anomalies," which ultimately prompts scientists to question some of the basic tenets of their profession.

Note what Meyerson writes:

> […] the history of atomism is that it is not a question of a simple renaissance, but of a true and an enormous progress. [...] Let us notice that this evolution did not take place with constant progression, but by fits and starts, a sort to flux and reflux. During a part of the nineteenth century atomic theories were somewhat discarded, discredited at least in appearance. Ostwald proclaimed the "defeat of atomism." Duhem wished to direct science toward a return to peripateticism (90-91).

It is worth noting that Ludwik Fleck in *Entstehung und Entwicklung einer wissenschaftlichen Tatsache* (1935), English translation with a forward by Kuhn (1979) illustrates a different aspect of Meyerson's thought related to sociology and psychology:

> Even if a particular statement is contested, we grow up with its uncertainty which, circulating in society, reinforces its social effect. It becomes a self-evident reality which, in turn conditions our further acts of cognition. There emerges a closed, harmonious system within which the logical origin of individual elements can no longer be traced (Fleck 1979/1935, 37).

In Meyerson, the emphasis falls on the "evolution" of thought and its "non-linear" progression, often occurring in "fits and starts." In Fleck, a similar theme appears, though it takes a different form: it resembles what we might today call a modern-day misinformation bubble—if the analogy is apt—where Fleck stresses the "disappearance" of the logical origins of ideas. A further contrast emerges during a period that, as Fleck describes, is marked by contestation and debate—a phase equivalent to Kuhn's "transition to maturity." If this line of interpretation holds, all three thinkers reveal aspects of the same phenomenon: for Meyerson, it is the inevitability of the progression or evolution of an idea; for Fleck, it is the peril of losing sight of an idea's "crucial" or logical origin; and for Kuhn, it is the role of retrospection or hindsight in understanding the development of scientific concepts.

Now, we can deepen these comparisons by considering what Kuhn writes in the Postscript to *Structure*, namely, "What changes with the transition to maturity is not the presence of a paradigm but rather its nature" (Kuhn 1970, 179). And according to Fleck, "what remains to be learned, and those who apprehend it, go to ensure harmony within the system" (Fleck 1979/1935, 38). Therefore, for Kuhn, the 'transition to maturity' is geared towards eventual change. For Meyerson, the change occurs by natural evolution of an idea albeit within given social constraints including the community of scientists, and for Fleck, whatever the change, it is to "[...] preserve the harmony of illusions, which is quite secure within the confines of a given thought style" (38).

In *Structure*, citing Chapter X of Meyerson's *Identity and Reality*, titled "Non-Mechanical Theories," Kuhn points out that "During much of the nineteenth century failure to explain the qualities of compounds was no indictment of a chemical theory" (Kuhn 1970, 107). In other words, it is precisely these kinds of "irregularities"—or uneven contours, if you will—in the development of science that make Meyerson's style of historical account particularly appealing to Kuhn. Numerous examples of such irregularities can be found in *Identity and Reality*. For instance, Meyerson notes: "[...] Chemists, while firmly believing in its [phlogiston's] existence, do not seem to make any attempt at isolating it. This was because phlogiston was at bottom a heritage of previous ages" (Meyerson 1930, 332). Similarly, he observes that "Lavoisier had not completely freed himself from all attachments to the old ideas. The name of oxygen indicates that he considered the new element the bearer of a quality" (Meyerson 1930, 333). In short, it is precisely this style of historical narration—emphasizing the

uneven development of science and instances of partial breaks with past norms—that resonates with Kuhn. For Kuhn, the dominant norm he resists is the Whiggish style of historiography.

5.4 Ludwik Fleck (1896-1961)

My intention throughout the previous section has not been to compare Fleck and Kuhn in order to determine who first developed particular ideas or initial insights. As stated at the beginning of this chapter, the aim of this dissertation is to account for those thinkers whom Kuhn himself identified as stimulating his thought prior to and during the genesis of *Structure*. Despite certain contextual similarities, however, Kuhn and Fleck ultimately differed in their approaches. To support this, let me quote Rainer Egloff here:

> Während Kuhn Wissenschaft grundsätzlich von anderen gesellschaftlichen Beriechen losgelöst betrachtet und exogene, soziokulturelle Einflüsse als eigentlich Krisenphänomene konzipiert, steht Wissenschaft bei Fleck stets mitten im Gesellschaftlichen und Kulturellen drin, schöpf ihre Entwicklung aus einer fortwährenden Interaktion zwischen esoterischen und exoterischen Denkkollektiven wissenschaftlicher und nichtwissenschaftlicher Art (Egloff 2005, 7).

We know that Kuhn engaged with Fleck's ideas in *Entstehung und Entwicklung einer wissenschaftlichen Tatsache* (1935) with great interest. Like Meyerson, Fleck illustrates common and often accepted "irregularities." Discussing Haeckel's *History of Natural Creation*, he writes that the book "abounds with biased illustrations appropriate for his theory. […] the intelligent faces of the old chimpanzee and the old gorilla […] with the exaggeratedly gruesome ones of the Australian aborigine and the Papuan" (Fleck 1979/1935, 36).

> If we want to investigate an earlier thought style, we must examine the original sources, not modern summaries of old viewpoints (126).

Fleck writes that it was believed that "A dead person is also much heavier than a living person, because the latter is full of little spirits whereas the former is deprived of them." Here, Fleck reminds the reader that:

> The feeling of heaviness (sluggishness) as well as the modern concept of physical weight, heavy-heartedness, and even the difficulty (unwieldiness) of lifting a corpse are here regarded as identical phenomena." [...]The "heaviness" in the last example is a concept totally different from that of

> our physical "weight." […] comprehending objects and phenomena in a way similar to our own was completely alien to their way of thinking. Our physical reality did not exist for them (127).

Again, setting aside the question of how closely Kuhn's ideas align with Fleck's, it is worth emphasizing that it was precisely this engagement with "irregular," "offbeat," or "different" historical materials that must have both fascinated and influenced the young Kuhn. Let us now delve deeper into Fleck's book by looking at the subject of "the formulation of the modern concept of the chemical element" involving Lavoisier, which was also a topic I briefly referred to when discussing Meyerson's work.

Fleck quotes Ostwald and a "strange psychological phenomenon, which occurs very often at moments of important progress in science" (122). Ostwald, he says, had spoken of "the ultimate step, which confirms a new idea and rejects old one" often remains unnoticed by the creator of the new idea (128). Fleck, on the other hand, and in contrast to Ostwald postulates that this phenomenon can be explained by the fact that "the true creator of a new idea is not an individual but the *thought collective* [my emphasis]", which leads to "the changes in *thought style* [my emphasis]" (128). Now, Fleck concludes that what he calls the modern version of the 'vademecum' – here the modern concept of chemical elements – has its source in "alien collectives" and exoteric and esoteric communication. "Concepts originating in this manner become dominant and binding on every expert" (128). It appears, then, that the lines of thought connecting Meyerson's 'lawfulness' and 'causality' or his general hypothesis of the human mind's search for 'Identity' intertwine with that of Fleck's, and in an odd and remarkable sense, become the "vademecum" for some of Kuhn's major concepts in *Structure*. I believe this is a worthwhile and important insight, which can potentially be pursued further, but for the purposes of this dissertation, it will perhaps suffice to note that the "thought collective" clearly reflects Meyerson's "undercurrents" running below the surface, and that the scientists or experts are not fully aware of such undercurrents. Nevertheless, it is not farfetched, to consider that the concepts of "thought collective" and "thought style" respectively represent, the Kuhnian "community" of scientist doing what they do, and the "paradigm" in which they operate.

Going even farther back, Hoyningen-Huene has pointed to the potential origin of the word "paradigm". In a footnote, we read:

> None of them [contemporary authors] mentions the fact that Neurath, Schlick, and Cassirer also discuss paradigms. […] But Schlick's use of

> "paradigm" may also have been mediated by Cassirer's (1910) *Substanzbegriff und Funtionsbegriff*; on p. 243 he speaks of "paradigms" (emphasized in his text) in the sense of exemplary illustrations (of certain priciples and theorems of pure mathematics). Schlick cites cassirer's work in chap. 40 of his *Erkenntinislehre*. (Hoyningen-Huene 1993, 133).

As I have mentioned earlier, I have found no evidence in the archival materials for early Kuhn (pre-*Structure*) that Kuhn had read Cassirer. However, Hoyningen-Huene's point implies that even the word "paradigm" must have been "common currency" used by prominent philosophers as early as 1910. As shown earlier with reference to Fleck and Meyerson, it is not difficult to imagine that Kuhn drew on many overlapping concepts in developing his own ideas. Furthermore, Paul Hoyningen-Huene's footnote assertion that "Kuhn reports Conant's open-mindedness regarding the "paradox" of Fleck's title" [*Genesis and Development of a Scientific Fact* (1935)] (204) is noteworthy. On the same page, Hoyningen-Huene refers to "startling quality" of Kuhnian "world changes". He writes:

> Precisely this startling quality is what aroused Kuhn's interest in Fleck's book […]. (204)

What Hoyningen-Huene has called "startling", I have called "irregular" or "off the beat" in the above. One might find even better words such as 'peculiar' [*eigenartig*] or 'strange' [*seltsam*]. In discussing Fleck, I have been trying to convey the idea that that (the above) is what appealed to and fascinated Kuhn, and that following this analysis, it also appears that beyond the title of Fleck's work, the core and its content is equally crucial to understanding Kuhn's thought.

Now, let me also briefly review Hoyningen-Huene's position on the Kantian influence on Kuhn to show that they primarily refer to the later Kuhn (see also my interview with Hoyningen as an appendix to this dissertation). On occasion of interviews, including in his 1993 book on Kuhn's *Structure*, Hoyningen had pointed to Gerd Buchdahl (1969) as perhaps the first to have spoken of a connection between Kant and aspects of Kuhn's ideas in *Structure*. In a footnote, Buchdahl had written that Kant:

> […] holds that 'nature' (i.e. 'the order of nature') is a function of this constructive activity itself: 'Reason does not here beg but command' (Buchdahl 1969, 511).

The footnote on Kuhn appears at this very point, namely, *Structure's* contention that central scientific 'paradigms' are not tested, verified or falsified in any straightforward way, but govern the very direction of such test […] (511). Later, Hoyningen-Huene drew his own comparisons between Kant and Kuhn in his 1993 book (German ed. 1989) that:

> For both Kant and Kuhn, epistemic *subjects* are (albeit in different ways) constitutive of this world […]" (Hoyningen-Huene 1993, 33).

Hoyningen is right, but "albeit in different ways" is indeed historically not necessarily Kant anymore; according to the narrative I have depicted, the credit, if any, is indeed due to Lovejoy and American critical realism (see 2.7).

Another instance of such comparison is based on Kant's "pure object-sidedness" whereas:

> Kuhn by contrast, is chiefly warding off the possibility of empirical access to the world-in-itself, that is, the naïve realist interpretation of science (35).

But here too, Kuhn's position is consistent with Lovejoy, since:

> While the new realists sought to 'emancipate' ontology from epistemology and defended a direct theory of perception, the critical realists promoted a representationalist account of perception and thus argued for an epistemological dualism. (Neuber 2023, 127).

Elsewhere, Hoyningen-Huene writes that Kuhn, in contrast to Kant, holds:

> […] *all* subject-sided moments of reality to be learned, and thus potentially variable (125).

This, indeed, is what Kuhn consistently emphasized, and it aligns closely with Lovejoy's account. However, it is important to remember that most of Hoyningen-Huene's comparisons, while valuable, primarily concern the later Kuhn—post-*Structure*—and therefore, for the purposes of this dissertation, cannot always be used to construct an alternative narrative of the genesis of Kuhn's ideas as I have pursued here. The exceptions are the overlapping instances I have documented above, which may also be applicable to the pre-*Structure* period.

5.5 Alexandre Koyré (1892-1964)

In discussing Koyré's supposed influence on Kuhn, my aim is not only to examine Koyré and his historiography of science but also to engage more deeply with his ideas by first contrasting them with those of one of his most significant contemporaries, Cassirer, and second, by drawing parallels with the thinking of Arthur Lovejoy, whose work Kuhn greatly admired. This juxtaposition serves to underscore the particular "style" of historiography that resonated with Kuhn.

Koyré in an article titled "Influence of Philosophic Trends on the Formulation of Scientific Theories" (1955) directly addresses Philipp Frank and announces that he is "in perfect agreement with Frank". According to Frank, Koyré writes:

> [T] he reasons for, or against, the acceptance of certain scientific theories are not always restricted to the *technical value* of the theory in question—that is, to its ability to give us a consistent explanation of the phenomena it is dealing with; instead, these reasons involve a series of other factors (Koyré 1955, 107).

Koyré then explores the relevant "philosophical background" and speaks of a "mutation" in thought "with a shift in the evaluation of intellectual knowledge" that is associated with the birth of modern science (109). Newton's famous admittance that he has not "been able to discover the cause of these properties of gravity from phenomena" and Galileo's "unprofitable" to engage in discussing "the explanations of gravity proposed and developed by his predecessors" has led to the "legalistic trend" i.e., the fruitfulness of the search for the "law" as opposed to 'causation', of modern science "just because nobody knows what gravity is—merely a word" (110). In other words:

> […] instead of trying to find out *why bodies fall*, it is much better to content ourselves with determining the mathematical law of their downward motion (110).

What Koyré is doing is putting a bigger question mark on the 'phenomenal' part of the 'law' and 'phenomenon' equation. In other words, and according to Koyré:

> As a matter of fact it is not the *phenomena*, but the *noumena* or the *noeta* that find themselves bound together by the causally unexplained or even unexplainable laws (110).

Similarly, Koyré points to the tradition in astronomy that "the purpose of astronomical science, Alexandrine astronomers explained, is *not* to find out the real mechanism of planetary motion" but it was to "save the phenomenon" by "putting up a system of circles" as a mathematical device to do relevant calculations as to the position of the planets and so forth (Koyré 1955, 110).

Koyré's conclusion is that *mathematical realism* was the very source of inspiration of the post-Newtonian development of scientific thought (111). Therefore, and it is exactly here that we encounter in Koyré's a final conviction that "the positivistic phase of renouncement, or resignation, is only a kind of retreat position, and it is always a temporary one" (111) and that "there is in man the desire not only to know but to understand" (111). Although vague, Koyré seems to suggest that first, 'mathematical realism' as a philosophical "trend" has somehow changed the emphasis of the questions to 'why' sorts. I suppose one could also read Koyré as saying that while theory formulation is part and parcel of any scientific activity, it is nevertheless adopting a particular perspective, or as the title of this paper implies, a philosophical "trend" may result in drastic changes in formulation of those theories.

Koyré in the introduction to his influential work, *Études galiléennes* (1939) (*Galileo* Studies), which is dedicated "To the memory of Émile Meyerson" praises Duhem, Meyerson and Brunschvicg, but also Cassirer:

> Fortunately it is no longer necessary nowadays to insist on the interest of the historical study of science. It is no longer even necessary—after the magisterial work of those such as Duhem and Emile Meyerson, Cassirer, and Brunschvicg—to insist on the fruitfulness of this study (Koyré 1978/1939, 1).

The central point I wish to emphasize is that understanding the early Kuhn, both prior to and during the genesis of *The Structure of Scientific Revolutions*, requires recognizing his sustained interest in ideas that diverged from mainstream historical narratives. This focus likely explains why Kuhn seemed to favor certain authors while largely rejecting others, such as Sarton. To be fair, it is possible—perhaps even likely—that Kuhn never read Cassirer

directly. However, as I noted earlier in this dissertation, the texts Kuhn assigned to his students—such as Lovejoy's *The Great Chain of Being*, Burtt, and selections from Koyré—do reference Cassirer. The later Kuhn's interview in Athens refers to Hélène Metzger and Anneliese Maier as "A sort of history, and an approach to history that I admired and which I encountered fairly early" (TSK-MIT, Box 28, Folder [Athens]). Noteworthy in this context is the way Kuhn himself enthusiastically volunteers to talk about the encounter with Popper and becoming acquainted with Metzger's and Maier's work. In Athens, as the interviewer Baltas asks Kuhn about how he met Koyré, Kuhn starts talking about him and almost right at the beginning says "Hold it, just a minute, because there is one other story and I think I really should tell you […]" (TSK-MIT).[76] My take on this is that the later Kuhn is highlighting elements that were closely connected with the genesis of *Structure*. These elements introduced through Metzger, Maier, and Koyré are the "different" ways of thinking about history. Kuhn's awareness of this point is informally communicated during the Athens interview; but also in practice, Kuhn's early notes, for example, for the course 'H of S 105,' Lecture 13 on 10/31/53 tell a similar tale:

> I. BIBLIOGRAPHY
>
> **Medieval Science in general**: Sarton: Introduction . . .
> Lynn Thorndyke: Magic & Experimental Science
> **Medieval Dynamics**: P. Duhem: Leonardo da Vinci
> A. Maier: Die Vorläufer Galileis in 14. Jahrhundert
> M. Clagget: Giovani Marliani & Late Medieval Physics
> Boyer: Calculus
>
> [Authors' names above and books are highlighted by Kuhn in red] (TSK-MIT).[77]

Kuhn had read Koyré's III essay, namely, *Galileo's Treatise "De Motu Gravium"*: The Use and Abuse of Imaginary Experiment" in French (the English translation was first published in 1968) and it appears that Kuhn had made use of Koyré's references in his lectures. For example, three out of four of Kuhn's bibliography comes directly out of notes on pages 54 and 56 of the above essay (Koyré 1968, 54,56). Even Thorndyke's book is mentioned by

[76] TSK-MIT, Box 28, Folder [Athens] (unpublished)
[77] TSK-MIT, Box 1, Folder 21 (H of S 105-Lecture 13-10/31/53)

Koyré on page 22 in connection with the topic of Kuhn's lecture namely the problem of *Impetus*. Kuhn's lecture begins with "Last time we considered (two dynamical contributions) [handwritten] some aspects of the Impetus Theory and concluded with Bradwardine's new form. BOTH DYNAMICAL [in red]"[78]

Thomas Bradwardine (1295-1349) is instrumental to Anneliese Maier's contributions. Maier had pointed to "the correct interpretation of a key element" in Bradwardine's mathematics, an interpretation that indeed makes Bradwardine's *Tractatus* as one of the major monuments of fourteenth-century natural philosophy […]. Further, she had referred to the "Oxford school" to distinguish it from the "Parisian school" (Maier 1982, 62). The lecture notes show that Kuhn had discussed this distinction, marked in red, in a lecture 4 days earlier (TSK-MIT).[79] So, at the least, Kuhn was tapping into Koyré and Maier for his lectures in late 1953.

In the archival materials, we also find a short article, which was originally published in *Encounter* (Kuhn 1970, 67) titled "Alexander Koyré & the History of Science" by Thomas S. Kuhn. In this article, Kuhn welcomes the re-publication of six essays by the late Alexander Koyré, some of them, he points out, are appearing in English for the first time (TSK-MIT).[80] The book Kuhn is referring to is Koyré's *Metaphysics and Measurement: Essays in Scientific Revolution (1968)*. One of Koyré's articles in this book is titled "Galileo and Plato" published in *Journal of the History of Ideas* 4 (1943), pp. 400-28.

Incidentally, this article was a centerpiece of a debate between Koyré and Cassirer, which I think is important in our discussion of elements of Koyré's thought that Kuhn tapped into and incorporated into his lectures. I will open up this debate for the sake of thoroughness and to show that even though Cassirer is not in Kuhn's focus, he is indirectly part of the equation Kuhn uses to develop *Structure*. I will further show that Koyré's and Cassirer's seemed to agree on relevant points of the debate and that their occasional differences remained a matter of nuances.

Cassirer's response to Koyré's article mentioned above was titled "Galileo's Platonism" appeared in Sarton's *Festschrift* for 1944 (but published in 1946, a year after Cassirer's death in 1945). Also, there is a reference to Koyré's article in *Structure* – albeit in passing – when

[78] TSK-MIT, Box 1, Folder 21 (H of S 105-Lecture 11-10/27/53)
[79] TSK-MIT, Box 1, Folder 21 (H of S 105-Lecture 11-10/27/53)
[80] TSK-MIT, box 25, folder 21 (Alexander Koyré the History of Science, 1970).

discussing the Pendulum motion by Galileo. In any case, in this article, Koyré is referring to Cassirer in *The problem of knowledge* (Berlin, 1911) as well as to Mach, and Lasswitz, and states that some historians of science and philosophy have tried to characterize physics as physics, by some of its traits, i.e., the principle of *intertia* (Koyré 1968, 19) "In my opinion", Koyré continues:

> […] it is not enough simply to state the fact. We have to understand and to explain it – to explain why modern physics was able to adopt this principle; to understand why and how, the principle of inertial motion, which to us appears so simple, so clear, so plausible and even self-evident, acquired this status of self-evidence and a priori truth whereas for the Greeks as well as for the thinkers of the Middle Ages the idea that as body once put in motion will continue to move forever, appeared as obviously and evidently false, and even absurd (19).

The citation above ends with a reference to E. Meyerson's *Identité et Réalité* (Paris: Alcan, 1926).

Let us now briefly delve into Koyré-Cassirer "debate", which revolves around Galileo's Platonism, to pursue supposed points of contention between them. Koyré had apparently read Cassirer's *The Problem of Knowledge* and at least on the point just mentioned in the above citation, he disagreed with him. Elsewhere in a footnote in the article, Koyré cites Cassirer for insisting "upon the Platonism of Galileo's ideal of knowledge" (40) arguably in support of his own claim that "I have just called Galileo a Platonist. And I believe that nobody will doubt that he is one" (40). Referring to this very point stated by Koyré, Cassirer in his contribution to Sarton's *Festschrift* writes[81]: "[T]here have always been considerable doubts about this very point. How was this possible—and how could these doubts persist in the minds of so many modern readers and students of Galileo's works?" (Cassirer 1946, 280). Cassirer continues:

> In order to understand this fact we must bear in mind that Galileo's Platonism was, indeed, of a new and very paradoxical nature. Never before had such a Platonism been maintained in the history of philosophy and science. Galileo did not simply continue the classical Platonic tradition, nor did he accept the Platonism of the Renaissance, of the Florentine Academy. His

[81] According to Sarton (Isis, Vol. 38, No. 1/2, Nov., 1947, pp. 127-128), Koyré had presented a contribution to Sarton's *Festschrift*, but it was left out in the final version. I am speculating that perhaps Sarton left out Koyré's contribution to his *Festschrift* since Koyré's article was very recent (1943) and a response to Koyré by Cassirer made more sense in terms of a continuing debate on Platonism by the two prominent historians of science.

> was not a metaphysical but a physical Platonism. But a physical Platonism was a thing unheard of (280).

Koyré in his article had referred to Archimedes as the "true forerunner of modern physics" (Koyré 1968, 22). Cassirer in turn writes: "But even Archimedes had to pay his tribute to classical Greek thought. He studied statical problems: but he hesitated to make the last and decisive step. He stopped in the middle of his way; he did not build up a science of dynamics". (Cassirer 1946, 291). And in an apparent response to Koyré, Cassirer continues:

> Galileo simply transferred the method of "problematic analysis" that had stood its ground in the history of geometrical and astronomical thought to physical thought. Yet he could not have done so if he had not first changed the *meaning* of this analysis. That the movements of terrestrian bodies could be reduced to the same simple laws as we find in the movements of the stars, that they possess the same uniformity and regularity as those of the celestial bodies was concept that was quite alien both to Aristotelian and to Platonic philosophy of nature (291-292).

Indeed, Cassirer on the first page of the introduction to his *Problem of Knowledge* (1950) had written:

> To Galileo the "new Science" of dynamics, which he founded, meant first of all the decisive confirmation of what Plato had sought and demanded in his theory of ideas. It showed that the whole of Being is pervaded through and through with mathematical laws and thanks to that is really accessible to human knowledge. The first definite proof of that harmony of truth and reality upon which, in the end, all possibility of knowledge rests was afforded not by mathematics but by natural science (Cassirer 1950, 1).

Elsewhere in the book, Cassirer emphasizes again the philosophical thinking in Galileo; it was Galileo's "[…] philosophical conception of truth that opened the way to his establishment of physics and reform of cosmology" (81). Cassirer is albeit putting the emphasis on methodology, of justifying concept formulation:

> Thus philosophy is tied up with all the efforts of research and it also brings them into unity. It attempts to represent them systematically and then critically to justify them. Kant's *Critique of Pure Reason* grew out of this ambition (81).

But Koyré must agree with Cassirer on this very crucial point, since he writes that he had shown in *Études* that Galileo's *Dialogue* is not so much a book on *science*, but on philosophy:

> As a matter of fact the *Dialogue*, as I believe I have shown in my *Études*, is not so much a book on *science* in our meaning of the term as book on philosophy—or to be quite correct and to employ a disused but time-honoured expression, a book on *natural philosophy* – for the simple reason that the solution of the astronomical problem depends on the constitution of a new Physics; which in turn implies the solution of the *philosophical* question of the role played by mathematics in the constitution of the science of Nature (Koyré 1968, 35).

I am not trying to equate Cassirer's emphasis on concept formation and justification with that of Koyré's emphasis on philosophical questions in mathematics and 'natural philosophy', but the message coming from both, I am pointing out, is the priority of philosophy. Massimo Ferrari in an excellent article titled "Koyré, Cassirer and the History of Science" has dealt with a comparison of Koyré and Cassirer in detail and has emphasized such a priority (Ferrari 2018, 175). Ferrari points to an interesting and crucial aspect of Cassirer and Koyré's approach:

> For Cassirer, by contrast, mathematics essentially amounts to the quantification of natural phenomena through infinitesimal calculus, a topic largely overlooked by Koyré. Thus, in Cassirer's mind, the main feature of mathematics is that of applied mathematics in the broader, transcendental Kantian sense, rather than belonging solely to a realm of pure *mathemata*, as Koyré seems to believe, in agreement with Husserl (173).

In 1937, two years before the publication of Koyré's *Études galiléennes*, Cassirer in an article in *Scientia* titled "Wahrheitsbegriff und Wahrheitsproblem bei Galilei" wrote that Galileo brought about not only a change in thinking about the physical cosmos, but also a new *form of Knowledge*:

> Durch das Werk Galileis wurde nicht nur der *Inhalt* des physikalisch-Kosmologischen Wissens vermehrt oder verändert; es war eine neue Wissens*form* die hier auftrat. Nicht um einzelne Fakten, sondern um die letzten *Massstäbe* des Wissens wurde hier gekämpft, und die prinzipielle Umwandlung dieser Massstäbe ist es, was dem Werk Galileis seine unvergleichliche philosophische Bedeutung gibt (Cassirer 1937, 193).

Building on the above analysis, I have emphasized that Koyré and Cassirer appear to have agreed on several key points, with their occasional differences being largely a matter of nuance. For instance, recall that in his 1955 article, Koyré speaks of a "mutation" in thought,

marked by a shift in the evaluation of intellectual knowledge, which he associates with the birth of modern science. I would argue that this concept is essentially equivalent to Cassirer's 1937 assertion of "eine neue Wissenform," as noted in the earlier citation. It is important, in our discussion of Koyré's thought—which Kuhn drew upon and integrated into his lectures—to recognize that while Cassirer is not directly the focus of Kuhn's work, he nonetheless plays an indirect role in the conceptual framework Kuhn uses to develop *The Structure of Scientific Revolutions*.

5.6 Koyré and Lovejoy – and Cassirer

At this point, and in the interest of further clarifying the types of thought that appealed to Kuhn and their intellectual origins, it is useful to briefly consider Arthur Lovejoy (see 2.7), to whom Kuhn consistently paid tribute, particularly for his work The Great Chain of Being *(1936).*

Throughout this dissertation, I have used Cassirer not only to throw light on the historical thinking he and Koyré shared, but also Relating to scientific change. Whereas Lovejoy's book and specifically his 'principle of plenitude' in that book deals with the *psychology* of scientific change, Cassirer, seems to deal with the *mechanics* of scientific change of an intellectual revolution.

While Burtt's *Metaphysical Foundation of Modern Physical Science* was a regular on Kuhn's syllabi for his students at Harvard in the '50s, he widely acknowledged both Meyerson and Brunschvicg, and specifically Koyré, as those who had contributed to his understanding of history. I mentioned earlier that in Kuhn's "field of vision", Cassirer is present albeit in an indirect way in the writings of Burtt, Koyré, and Lovejoy. Burtt writes:

> […] a rather extensive transformation of the major concept of scientific thinking, furthered on the one hand by radical physical hypotheses of a gifted student of nature like Einstein, and on the other hand by the attempted reshaping of scientific methods and points of view by philosopher of science such as Whitehead, Broad, and Cassirer (Burtt 1925, 14).

Burtt refers to Cassirer as "an acute German scholar" and one is "to gain a magnificent historical perspective" while also blaming not only him but also Whitehead and Broad of "sins" of certain shortcomings in their historical accounts (15). This leads to an historical puzzle,

namely, why Kuhn hardly mentions Cassirer? [82] First, the early Kuhn did speak both French and German, but the archival materials also show that he had a much better command of French and that his German was probably rudimentary – for example, he freely translated French texts for his students and read Koyré's texts that had not yet been published in English. Second, Kuhn as mentioned above, had only indirect encounter with Cassirer through the writing of others Kuhn was reading. However, my latest findings (April 2025) confirm that Kuhn did own some of Cassirer's books. He owned *Language and Myth* (1946) [*Sprache und Mythos* 1925] (translated by Susanne K. Langer), *Philosophy of the Enlightenment* [*Philosophie der Aufklärung* 1932] (1955), and *An Essay on Man: An Introduction to a Philosophy of Human Culture* (1948/1944) (TSK-MIT_Inventory). So, at this point in time, this settles my second point and shows that Kuhn early in his career until mid-50s was indeed familiar with both Cassirer's early and later works—and this is only a preliminary result.

and third, Cassirer's philosophy was hardly represented at Harvard. The latter is evident through a letter from Harvard's Hugo Münsterberg to Cassirer on March 3, 1913 (I have referred to Harvard's formal invitation to Cassirer in chapter 2 of this dissertation in the section on Susanne K. Langer in more detail). Münsterberg wrote to Cassirer:

> Es schien uns aber wünschenswert, auch einen Deutschen in der Reihe unserer Gäste zu haben, und unsere Wahl ist auf Sie gefallen. Ihre Richtung ist hier im Lande wenig vertreten und in unserer philosophischen Abteilung ist gegenwärtig keiner, der sich in Ihrer Richtung bewegt. Da schien es uns denn höchst wünschenswert, dass Sie einmal den Studenten die Grundgedanken Ihrer Philosophie darstellen. [...] Mir liegt sehr viel daran, dass deutsche Philosophie hier zu lebhafter Geltung kommt (Cassirer 2009, 17-18).

Lovejoy's texts on the other hand, and on the history of ideas were readily accessible to him, and that from the beginning of his career and while teaching history of science. Kuhn's numerous references to Lovejoy's *The Great Chain of Being* on his Syllabi for his classes throughout the years at Harvard – materials that he probably deemed sufficient for his purposes – is indicative of this fact. But what did Kuhn encounter through Lovejoy besides

[82] As of mid-2023, the MIT archives (Distinctive Collections) had not yet completed cataloguing some 40 boxes of books of Kuhn's private collection. It is possible that books by Cassirer are among them. **Update: upon completing this dissertation on April 16, 2025 MIT staff kindly responded to my request whether the list of books in Kuhn's possession has been completed. I then received a list for the content of 25 from 40 books and was told that this is 'a work in progress'. I have labeled these books as TSK-MIT_Inventory in this dissertation.**

finding Lovejoy's appealing for his "different" way of doing history? [83] To answer this question, we will need to take a closer look at Kuhn's lecture notes.

The Significance of Cusanus in Kuhn's Lecture Notes

Cusanus' intellectual contribution serves as a clear example that crystallizes both points of intellectual proximity and areas of contention—not only between Cassirer and Koyré, but also between Lovejoy and his direct points of contact with Kuhn. In this section, I will demonstrate that, despite both similarities and differences in content and interpretation among the ideas advanced by the intellectual triangle of Cassirer, Koyré, and Lovejoy, Kuhn's lecture notes align most closely with Lovejoy's perspective.

In the early lecture notes for History of Science 105 (1953), Kuhn introduces Cusanus and the Neo-Platonists:

> Crombie and Duhem treat Cusa as another scholastic and take these remarks as having literal physical sign. As they would in a 14th century scholastic. ABSURD. THE man's a Christian Mystic.[84]

The remarks regarding Crombie and Duhem are not to found in the Book References on the syllabus for this course.[85] So, it is not clear where Kuhn's remark originated, though Kuhn does mention *De Docta Ignorantia* and *De ludo globi* in the course.

Also, in Crombie's book assigned for this class, namely, *Augustine to Galileo*, we find references to Cusa and Eckhart as having "[…] a period of speculative mysticism seen in Eckhart […] and of empiricism and scepticism seen in Nicholas of Cusa […]." (Crombie 1952, 235).

There are ample references in Kuhn's early thought on Medieval history of philosophy that point to congruences with the pioneers of European historiography and historicism and

[83] See chapter VI of Lovejoy's *The Great Chain of Being* that Kuhn systematically assigned to his students for reading. Lovejoy wrote about Kant's cosmology and Kant's "wisdom of God" argument, which included the 'principle of plenitude'. This principle played an important role for Kuhn and was the title of one of his Lowell Lectures.

[84] TSK-MIT, box 1, folder 21 (handwritten next to this entry: Example. Inertia on Rolling Sphere)

[85] TSK-MIT, box 1, folder 21. The book authors on Kuhn's syllabus for this course are: H. Butterfield, Aristotle, Ptolemy, A.C. Crombie, M. Clagett, Copernicus, Burno (this book is crossed out), Galileo, Epicurus, Descartes, Newton, Hooke, and E.A. Burtt (crossed out and replaced with Lovejoy to be read during reading period days).

consequently the rise of modern thinking – *a new form of thought* – as Cassirer called it. In this connection, consider Cassirer's *The Individual and the Cosmos in Renaissance Philosophy* (1927). Here, one of the points being debated involves Nicholas Cusanus (Nicholas of Cusa). Cassirer had dealt with Cusanus in depth and was noticeably clear about Cusanus' contributions as a modern thinker:

> The position towards the problem of knowledge makes Cusanus the first modern thinker. His first step consists of asking not about God, but about the possibility of knowledge about God […]. All Knowledge presupposes comparison, which, in turn, more precisely understood, is nothing but measurement (Cassirer 2020/1927, 10).

For Cassirer, Cusanus certainly heralds modern thinking. Let me now bring this very point home by showing the proximity of this historicism about a new way of thinking in both Cassirer and Lovejoy and, in turn, to propose that Kuhn's thought in this particular debate is formed by Lovejoy.

Cassirer writes:

> To be sure, the concept of *docta ignorantia*, and the doctrine of the 'coincidence of opposites' based upon it, seem to do nothing more than to repeat thoughts that belong to the solid patrimony of medieval mysticism. Cusanus constantly refers to the sources of this mysticism, especially to the writings of Meister Eckhart and the Pseudo-Dionysius. And so, it would seem difficult, if not impossible, to draw a definite line of separation between them (8).

Cassirer puts it even more plainly:

> In the mystical theology of the 15th century two fundamental tendencies stand sharply opposed to each other; the one bases itself on the intellect; the other considers the will to be the basic force and organ of union with God. In this dispute, Cusanus sides emphatically with the former (13).

However, lecture notes for this section of class teaching reveal Kuhn's difficulty, or at least his mulling over the concept. His notes show:

> I assume this [Cusanus and Neo-Platonism concepts] isn't very clear.
>
> a) If I were a mystic, I WOULD SAY, GOOD, you're not supposed to understand it, you're supposed to feel it. Part. In Emot. Exp.
> b) Fairer to say, I don't quite understand it either. CAN WE PUT MORE RATIOANLLY THO, [in red] A. Lovejoy, Great Chain of Being, II & IV (TSK-MIT, Box 1, Folder 21).

The notes clearly show that it is Lovejoy that clears the fog for Kuhn and is most probably the source of Kuhn's lecture notes on the subject. The lecture notes on the topic clearly point to the role of Lovejoy in Kuhn's understanding of the this material. We also come face to face with Plato's Ideas and the notion of the ineffable in Lovejoy's *The Great Chain of Being*[86] in the chapters Kuhn recommends to his students. In chapter II:

> Thus Plato's philosophic testament is nothing other than a reaffirmation of the doctrine of Ideas in its most unqualified and mystical form […] His deepest and "most serious" conviction, Plato declares is, "by reason of the weakness inherent in language," incapable of adequate expression in words […] "there is a certain true argument" which both leads towards it and makes clear why, in itself, it must remain ineffable. What that argument shows is that the true objects of rational knowledge, the only genuine realities, are the immutable essences of things […] (Lovejoy 2001/1936, 33-34).

And in Chapter IV, we encounter an elaboration on Cusanus:

> The Cardinal Nicholas Cusanus, one of the subtlest though hardly one of the clearest philosophical minds of the later Middle Ages, had transferred to the physical universe the paradoxical figure which theologians had sometimes employed to express the doctrine of the "immensity" of God [...] The mind of Cusanus, however, is less concerned with astronomical questions than with a species of mystical theology (111-113).

The above citation referring to Cusanus as advocating "mystical theology" may have been the source of Kuhn's lecture note and his conviction that "THE man's a Christian Mystic". It also appears that for Lovejoy, neither Cusanus, nor Bruno are really interested in the workings of the physical universe. In fact, in the same book chapter, Giordano Bruno suffers in the hands of Lovejoy, the same fate as Cusanus:

> Bruno is, in short, precisely in those features of his teaching in which he seems most the herald and champion of a modern conception of the universe, most completely the continuer of a certain strain in Platonistic metaphysics and in medieval theology (117).

In other words, Lovejoy appears to associate, in a very clear way, modern thinking with the last sentence in the citation above. Therefore, bringing back Cassirer into the picture, I

[86] In Lovejoy's book, Cassirer is mentioned twice in the chapter notes, and in connection with Leibniz, namely under *Leibniz: Hauptschriften zur Grundlegung der Philosophie*, II (1906), 26 and 556-559. Also, we can confidently assume that Lovejoy as a philosopher and scholar was familiar with Cassirer's other writings as well.

suppose, we could conclude without much difficulty that the difference between Lovejoy and Cassirer's thought on the origin of modern thinking is only a matter of depiction. Cassirer writes:

> Cusanus' Deutung steht im Zeichen des metaphysischen *Begriffe*; Leonardos Grundanschauung ist in seiner Auffassung der künstlerischen *Gestalt* zentriert; Galileis Kosmologie und Dynamik hat ihren Mittelpunt im Gedanken des Gesetzes, den er zuerst in wahrhafter Exaktheit konzipiert (Cassirer 1937 Erster Teil, 124).

In other words, where Cassirer speaks of 'Exaktheit' and 'measurement', Lovejoy, points to Platonism as a whole. The point of contention between the two, however, is the emphasis on the "Kantian" picture. Here, we can see very clearly how Cassirer organizes his understanding of Cusanus, Leonardo, and Galileo into Kantian terminology:

> Nikolaus Cusanus gibt die erste metaphysische Deutung des Universums; er bezeichnet und bestimmt seinen reinen Begriff. Leonardo da Vinci ergreift das Universum in einer einheitlichen und allumfassenden Intuition, die ebensowohl das Werk des Gedankens, wie des künstlerischen Schauens und Bildens ist. Galilei aber wird zum ersten wissenschaftlichen Theoretiker des Universums; er will die elementaren Kräfte aufdecken, die seinen Aufbau beherrschen und er will zeigen, wie sie in diesem Aufbau zusammenwirken (122).

Galileo's type of questioning, Cassirer writes, aims to unveil the characteristic 'conception of truth' (*Wahrheitsbegriff)* and specific 'truth requirement'*(Wahrheitsforderung*) (125). Here, Cassirer's 'applied' historiography elegantly comes to the fore by telling us that we have to approach Galileo not in only one area of his activity, but through concentric circles of his diverse interests:

> Wir müssen vielmehr, gleichsam in konzentrischen Kreisen, vom Zentrum seiner intellektuellen Tätigkeit zu ihren immer weiteren und umfassenderen Auswirkungen vordringen (125).

All along, Koyré's reservations and criticism of Cassirer is turning "Plato into Kant" (Koyré 1978, 223):

> Thus for him [Cassirer], Galileo's 'Platonism' is expressed in his giving priority to function […] and law over being and substance. According to this interpretation Galileo inverted the Scholastic proposition *operatio sequitur esse* (223).

Cusanus' intellectual contribution is a case in point that clearly crystallizes both points of proximity of thought, and points of contention not only between Cassirer and Koyre, but it also brings in Lovejoy and his direct points of contact with Kuhn into the picture. I have tried to show that despite similarities and differences – both in content and interpretation of the ideas advanced by the tripod Cassirer, Koyre, and Lovejoy – Kuhn's lecture notes are closest to Lovejoy.

Dimitry Gawronsky in his 1949 book on Cassirer wrote:

> He [Cassirer] was the first to introduce into the history of philosophy such names as Kepler, Galileo, Huygens, Newton, and Euler by giving a detailed analysis of their philosophical conception, scientific methods and achievements, and by providing their fundamental importance for the theory of knowledge (Gawronsky 1949, 15).

In this section, I have demonstrated that Kuhn may have encountered Cassirer's thought indirectly through Lovejoy and Koyré, particularly on pivotal topics that shaped Kuhn's teaching—as reflected in his lecture notes when discussing Galileo, Cusanus, Leonardo, and medieval thought. As both a contemporary and a key intellectual interlocutor, Cassirer was an influential figure for both Koyré and Lovejoy. Cassirer was among the first to explore and publish on what later emerged as the new historiography, which ultimately became central to Kuhn's philosophy of science.

6

Friedman's Claim

6.1 Friedman's Narrative in *Dynamics of Reason*

I have already addressed many aspects of the genesis of *The Structure of Scientific Revolutions* and the inherent historicism reflected in the sources Kuhn draws upon. We have also examined, in particular, the philosophers who, according to Kuhn himself, influenced his 'historical thinking.' I believe I have demonstrated that Kuhn's ideas in *Structure* indeed resonate with historicist traditions, both of continental and American origin. Before turning to a critical engagement with Michael Friedman's account in *Dynamics of Reason* (2001), however, it is necessary first to briefly examine the intellectual background of Kuhn's predecessors.

In *Human Understanding*, Stephen Toulmin elegantly traces concerns and questions posed by a Kuhn predecessor R.G. Collingwood twenty years before the publication of *The Structure of Scientific Revolutions*, namely, 'On what occasions, and by what processes is one set of fundamental concepts or constellation of absolute presuppositions, displaced by another?' (Toulmin 1972, 98). In reframing the question, Toulmin writes:

> […] we must set out to show how a single set of factors and considerations, interacting in different ways, can be used to explain both why our 'forms of thought and perception' – concepts, standards of rational judgment, *a priori* principles and the rest – vary rapidly in some cases, situations, and circumstances, and also how, in other cases, situations, and circumstances, they can remain effectively unchanged" (98).

According to Toulmin, the distinction in question is between historical phases of two contrasted kinds, which are referred to as 'normal' and 'revolutionary' expounded most persuasively by T. S. Kuhn in his 1962 book (98). It is worth mentioning that Toulmin emphasizes close parallels between Kuhn's position and Collingwood's "that a glossary can be established for translating between them" (99). The background to the question Kuhn eventually asked in *Structure*, was populated by other sociologists, philosophers, and historians that had attempted to address in the 1950s answers to similar questions, namely, N. R. Hanson's *Patterns of Discovery* (1957), Toulmin's own Mahlon Powell Lectures for 1960 at Indiana University (published in *Foresight and Understanding*), and still others including Thomas Merton, Bernard Barber, and Joseph Ben-David (99).

Now, Friedman points to later Kuhn admitting in 1993 that his:

> […] structured lexicon [Kuhn's late version of "paradigm"] resembles Kant's a priori when the latter is taken in its second, relativized sense. Both are constitutive of *possible experience* of the world, but neither dictates what that experience must be (Friedman 2001, 43).

Friedman adds that Kuhn towards the end of his career:

> […] fully acknowledged the point that his own philosophical conception is closely akin to the relativized view of the a priori earlier articulated by Reichenbach and Carnap (42).

Friedman's "own ambition is to supplement Kuhnian historiography of science with a parallel, and interrelated appeal to the concurrent history of scientific philosophy" (44). Friedman's 1999 Kant Lectures at Stanford University and the subsequent publication of the lectures in *Dynamics of Reason* (2001) is such an attempt at exploring the following important point:

> What I find unfortunately lacking in Kuhn is precisely such a parallel historical treatment of philosophy. Indeed, in Kuhn's book philosophy is treated quite *ahistorically*, and in an entirely polemical manner, as what everyone else supposedly thought before Kuhn himself arrived at the scene (20).

Dynamics of Reason as its subtitle outlines is primarily on Kant, Kuhn, and a post-Kuhnian strategy based on the notion of 'scientific philosophy'; this can easily be demonstrated by reviewing the number of references in the book both on Kant and on Kuhn.
Specifically, Friedman's narrative unravels as follows: It depicts 'scientific philosophy' as a reaction to Kantian thought and its subsequent transition of the movement "into neo-Kantianism on the one side, and what then became logical positivism, on the other" (21):

> Never, not even within this relatively circumscribed world of post-Kantian German philosophy, is anything like a stable consensus on a common paradigm attained. We see only a constantly shifting dialectic of thought, now fragmenting and dividing, now momentarily coalescing, between similarly shifting philosophical positions and school (21).

In other words, Friedman tells us that Scientific thinkers of the nineteenth century such as Helmholtz and Poincare found Kant's necessary conditions of possibility of the new paradigm for mathematical physics created by Newton, too restrictive, struggled to articulate new types of scientific philosophy, which eventually led to Einstein's discoveries, and a resulting paradigm to the philosophy of logical positivism (21). Friedman's point is that it is exactly in periods of transition of a dominant normal science to a new scientific paradigm that the sciences have reverted to a *philosophical mode* in order for the reorientation to make sense. Therefore, according to Friedman this is what needs to be added to Kuhn's account (22). In this context, Thomas Oberdan's take on Friedman's thesis puts it more succinctly:

> only an understanding of the relativized synthetic apriori, like that which figured in Reichenbach's interpretation of Relativity Theory, can provide an account of the constitutive apriori requisite for a truly philosophical understanding of science *writ large* (Oberdan 2009, 194).

In other words, this constitutes the underlying rationale of Friedman's narrative in *Dynamics of Reason* and explains why he seeks to align Kuhn with it. Yet for Kuhn himself, at least as early as November 10, 1953, the intellectual situation appears to have been markedly different.

In his lecture notes for this day, Kuhn points (TSK-MIT Box 1, Folder 21) to "A REAL PUZZLE" in examining Copernicus' system. Kuhn writes: "In any case he [Copernicus] comes up with the basic idea of letting all the planets go around the sun. […] The overall system is comparable to Ptolemy's in complexity (THOUGH APPARENTLY NOT QUITE SO BAD), and it gives no better results using the available date .[…] because this isn't a usable astronomical system FOR ASTRONOMERS."

And here in the notes, we come to a point that I have been after to put against Friedman's "shifting philosophical positions and school" mentioned in the citation above, or what I interpreted as reverting to a philosophical mode. In what appear as coming to terms with his "puzzle", Kuhn (in contrast to Friedman) continues: "And this [erasing epicycles] certainly must be involved in the conversion [87] process […]. Kuhn then continues with the point that I wish to emphasize:

[87] Here Kuhn means 'converting others,' the "community" of astronomers, if you will.

> And it's by and large astronomers who initially take up the system (AT LEAST ITS MATH) and preserve it long enough for Kepler's astrono[my] and Newton's phys. to be joined with it.

So, beyond the point about the 'Math' that may gets preserved, Kuhn also offers a "pragmatic" explanation:

> And it seems like that they were more impressed by the aesthetic than by practical advantages of this system. SEEMS TO FIT TOGETHER BETTER .[…] CALL THIS NEATNESS.

Thus, I have made two key points on this matter: first, regarding the preservation of what might be called "constitutive" elements (which aligns with Friedman's view), and second, the significance of the aesthetic dimension. However, contrary to Friedman's interpretation, there is no indication of a philosophical debate in Kuhn's account. In other words, according to Kuhn—and in contrast to Friedman—the "conversion process" ultimately comes down to "a matter of taste" (see 4.4). In short, Friedman appears to be attempting to revise an aspect of Kuhn's narrative that is, in fact, central to it. Viewed from this perspective, Friedman's account falls short precisely because it overlooks the very *essence* of Kuhn's explanation (see also 4.2).

Returning to the points with which we opened this chapter, it is important to highlight that Friedman's narrative rests on the assumption that Kuhn arrived at his conclusions largely by following German historicist accounts of scientific development, but that he then, perhaps carelessly, left his ideas open to charges of relativism. Friedman thus proposes to "supplement" Kuhn's work in order to reorient the field toward a post-Kuhnian philosophy of science. However, the extent to which this assumption holds true has, in part, been the focus of this dissertation.

In connection with the Kantian question, let us now return to Oberdan. Reichenbach had argued in *Relativity Theory and A priori Knowledge* (1920) that there is a distinction between 'axioms of connection', expressing scientific claims, and 'axioms of coordination', which are implicit in the architecture of any scientific theory. The latter:

> […]function in Reichenbach's epistemology in much the same way that the synthetic a priori figured in Kant's: they serve to explicate the insight that the object of knowledge is constructed from conceptual elements which are not given in experience (Oberdan 2009, 195).

Reichenbach's conclusion, Oberdan tells us, is that "[…] since the a priori presuppositions of distinct scientific theories may differ, so coordinative principles are relative to the specific theory that presupposes them" (196). Here again, Friedman's critique of Kuhn is that an appeal to 'scientific philosophy' *throughout the stages in the development of novel scientific theories*, including new and revolutionary concepts, seems the rational stance to adopt. Friedman's strategy on how to supplement Kuhn has four lines of argumentation:

First, he draws a distinction between Meyerson and Cassirer respectively in their "ontological rather than a mathematical interpretation to the question of theoretical convergence over time" (Friedman 2008, 246). He attributes to Cassirer "a continuously evolving sequence of abstract mathematical structures" (246) while associating Kuhn with "the Meyersonian viewpoint, that there is rational continuity over time only if there is also substantial identity" (246).

Second, he disagrees both with Cassirer and the Marburg neo-Kantians respectively that "the general theory of relativity continues to exemplify the fundamental Kantian insight that the unity of nature as such can only be due to our understanding" (247) and the Marburg neo-Kantians for their tendency to minimize the Kantian faculty of pure intuition or pure sensibility on behalf of the faculty of pure understanding (247).

Third, according to Friedman, the necessary mathematical development, and the necessary physical development, still need an additional set of parallel developments in scientific philosophy to tie together the former and the latter (250). Einstein was only able to arrive at the results he did by:

> […] delicately situating himself within earlier philosophical debate on the empirical and conceptual foundations of geometry between Helmholtz and Poincare. If this is correct, however, we need a more far-reaching revision of Kantian transcendental philosophy than Cassirer has suggested in this case. It is by no means true, in particular, that Einstein's general theory of relativity can be incorporated within transcendental philosophy 'without difficulty,' since this philosophy, in Kant's original form, is unavoidably committed to the *a priori* necessary validity of both Euclidean geometry and the fundamental principle of Newtonian mechanics (251).

Fourth, Friedman then suggests that "the only way forward is to *relativize* the Kantian *a priori* to a given scientific theory in a given historical context (following Reichenbach in 1920) and, as a consequence, to historicize the notion of transcendental philosophy itself"

(251). Finally, he concludes by agreeing with the central idea of the Marburg School that it is possible to continue the tradition of transcendental philosophy of science even at the times of radical revisions. It is required, again, according to Friedman's thesis, that transcendental philosophy itself become relativized and historicized (251). This effort, Friedman claims, leads to revolving "the Kuhnian problem of understanding the rationality of revolutionary transitions involving essentially discontinuous or incommensurable scientific paradigms or conceptual frameworks" (252). Reichenbach in 1949, however, argued vehemently against Kant's apriorism in physics of relativity. Milič Čapek remarks:

> This view of Kant that pure experience is completely structureless and that every relation, including the relation of temporal sequence, is added to the 'sense-material' by a spontaneous fiat of reason, was more or less retained by Reichbach even when he later resolutely turned against Kant (Capek 1958, 86-87).

Reichbach wrote:

> Plato's attempt to solve the problems of geometry by a theory of ideas, Kant's attempt to account for the nature of space and time by a "*reine Anschauung*" and by a transcendental philosophy, these represent answers to the very questions to which Einstein's theory has given a different answer at a later time (Reichenbach 1949, 290).

And:

> […] philosophers began to give in and depart from Kant's apriorism. Let us hope that this development will continue and eventually include even those philosophers who in our day still defend an apriorist philosophy against the attacks of the mathematical physics (300).

"What has happened in Einstein's theory" Reichenbach writes "is a proof that knowledge within the framework of Kantian principles is not possible." And that "For a Kantian" this means a "breakdown of knowledge" (309).

> It is a fortunate fact that the scientist was not a Kantian and, instead of abandoning his attempts of constructing knowledge, looked for ways of changing the so-called *a priori* principles. Through his ability of dealing with space-time relations essentially different from the traditional frame of knowledge, Einstein has shown the way to a philosophy superior to the philosophy of the *synthetic a priori* (309).

In other words, Einstein's theory of space and time emerges from a radical empiricism that accepts only sense perception and the analytic principles of logic as legitimate sources of knowledge (310). Accordingly, the "scientific philosophy"—championed by Friedman—would have, for Reichenbach, amounted to little more than an afterthought—one essentially limited to deducing the implications of a scientific theory and situating it within the broader history of philosophy (311). Moreover, the article's concluding sentence is both sterile and vague, asserting only that "many have contributed to the philosophy of Einstein's theory, but there is only one Einstein" (311). Yet even here, the point is not about Einstein's synthesis of and engagement with various scientific philosophies, as Friedman has repeatedly argued. In short, Reichenbach's 1949 work no longer appears to endorse any particular relativized form of the a priori.[88]

6.2 Friedman's "global" Approach to Kuhn's Later neo-Kantian Affinities Too Narrow to Account for the Genesis of *Structure*

Despite the context outlined above, Friedman has maintained that Kuhn's philosophy possesses a neo-Kantian core—primarily evidenced through early Reichbach—and has highlighted neo-Kantian affinities in the philosophers Kuhn mentioned, both in *Structure* and in subsequent writings. Friedman reminds us of Cassirer's work:

> *Das Erkenntnisproblem* exerted a decisive influence on early 20th -century history of science, especially in its more philosophically oriented guises. It was especially important for such writers as Emile Meyerson, Léon Brunschvicg, Hélène Metzger, Anneliese Maier, and Alexander Koyré […] (Friedman 2008, 243).

Kuhn late in his career (1984) wrote:

> The concept of historical reconstruction that underlies [this book] has from the start been fundamental to both my historical and my philosophical work. It is by no means original: I owe it primarily to Alexander Koyré; its ultimate sources lie in Neo-Kantian philosophy (243).

[88] The point made by Friedman of Schlick's influence on Reichenbach aside (see Friedman 1999: "Geometry, Convention, and the Relativized A Priori: Reichenbach, Schlick, and Carnap" in *Reconsidering Logical Positivism*), the fact remains that the later Reichbach is vehemently and in principle against any notion of a Kantian pure reason in the empirical fact of science and scientific theorizing.

Elsewhere, Friedman states that all the figures on Kuhn's list, in one way or another, are taking inspiration from, and reacting to, Cassirer's seminal work on the history of modern science and philosophy, *Das Erkenntnisproblem* (Friedman 2003, 30).

> Meyerson (1930) – beginning in the second, 1912, edition – often refers to *Das Erkenntnisproblem*, as does Brunschvicg. Metzger, in turn, was substantially influenced by Meyerson and also by Brunschvicg […] Indeed, virtually all the historians who initiated the study of modern science in the twentieth century, including especially Dijksterhuis and Burtt […] drew significant inspiration from Cassirer's work (39).

Note 40 of Friedman (2003) cites F. Floris Cohen's acknowledgement of Cassirer's influence, who nonetheless contends that "only Burtt, Dijksterhuis, and Koyré were to elaborate such views [on the mathematization of nature] into detailed examinations of the birth of modern science" (39). Friedman's invocation of Kuhn's later engagement with Kantian philosophy is, in effect, overly broad yet fundamentally too narrow to account for the genesis of *Structure*. In the following discussion, I will qualify this position further.

6.3 A Novel Construct

In archival materials, Kuhn can be seen engaging in an ongoing process of intellectual assimilation. He not only grappled with established scientific facts but also actively sought connections, inspiration, interpretations, and validation for contemporary ideas. In forging these links, Kuhn's interconnected intellectual *construct* was influenced not only by the Vienna Circle but also by a broad cast of other figures.

Gazit and Beaney have highlighted aspects that I suspect may have been of relevance to Kuhn – albeit the *later* Kuhn:

> The centrality of neo-Kantian anti-psychologism also persisted in the work of Popper's follower, Imre Lakatos. […] the history of science, he wrote, is "a history of events which are selected and interpreted in a normative way" ([1971] 1978, 121). According to Lakatos, rational reconstruction follows the rules of a specific normative methodology that the reconstructor applies, implying that there can be *many different* rational reconstructions, and not just one, as Popper assumed (Yael Gazit and Michael Beaney, forthcoming in 2025).

Therefore, when scholars point to traces of neo-Kantian affinities in Kuhn's work, they are, for the most part, referring to his later thought—particularly as he reassessed and reconciled his earlier ideas. Both Kuhn and his contemporaries criticized the logical empiricist tradition for its inherent inadequacies. A newspaper article in *The Washington Post* (Kuhn had kept clippings of the article) published on July 30, 1982, titled "Thinking Big: Thomas Kuhn and the Nature of Scientific Inquiry" by James Lardner:

> And the "Structure" was not the only book to suggest that scientists are humans too, with the full complement of human values and motives. Kuhn himself cites Norwood Hanson, Michael Polanyi, Paul Feyerabend, Stephen Toulmin, Imre Lakatos and Mary Hesse as rough contemporaries whose writings "look harder at actual scientific practice and point out the inadequacies of the logical empiricist tradition" (TSK-MIT).[89]

Kuhn explored influential figures such as Wittgenstein, Polanyi, Piaget, Koyré, and Fleck, and even ventured into analogies with biological evolution as a model for scientific progress, while identifying the very "inadequacies" in question. Notably, others, including Norwood Hanson, reached similar conclusions. Matthew Lund writes:

> During his years at Cambridge, Hanson became fully immersed in history of science and contemporary physics. Through Butterfield and Hall, Hanson came to study Duhem, Mach, Whewell, and Koyré—all seminal influences on Hanson's conception of the history of science. […] (Lund 2010, 55).

Hanson had also found logical positivism 'inadequate' because of its lack of historical contextualization:

> Hanson found the epistemologies of science of his day to be inadequate. The logical positivist conception was so committed to a pet foundationalist epistemological theory that it falsified the essential facts of psychology and the history of ideas (48).

Seen particularly from a neo-Kantian perspective – as advocated by Friedman – philosophy has a myriad of tasks. Thomas Mormann writes:

> The principle of the primacy of function over structure is just another formulation of the basic principle of the "transcendental method" characteristic for Marburg neo-Kantianism in general. According to it, philosophy does not operate in empty space but had to rely on the historically established facts of science, language, ethics, art, religion, and myth that provided it with its proper content. The task of philosophy is to "justify" these

[89] TSK-MIT, Box 1, Folder 1 "Biographical."

> symbolic productions of the human spirit by elucidating their basic assumptions and principles thereby understanding and making proper use of them (Mormann 2012, 163).

Mormann mentions that Cassirer had said that:

> [L]ogical empiricism was deeply "unKantian" in that it put foundational "structure" at center stage, neglecting the role of "function" (162)

Ironically, the shortcomings of the logical positivists' program did not lead directly to a revival of strictly neo-Kantian conceptions in philosophy—at least not for Kuhn before *Structure*. Instead, these shortcomings served to foreground *interconnected* historicist scientific methods.

6.4 A "Gap" or "Interconnectedness"?

It is not trivial that Frank, in his letter to Cassirer, was carefully placing the logical constitution of Cassirer's work close to that of his own and those of Carnap's and Schlick's. Recall that for Carnap, "the only task of philosophy of science proper was the purely theoretical analysis of the formal structure of language of science" (157).

According to John Micheal Krois, "Genau zu der Zeit, als der Wiener Kreis als Gruppe zu existieren aufhörte, befaßt sich Cassirer mit ihm eingehend" (Krois 2000, 116). Krois draws Cassirer's ideas even close to Quine's by way of the Vienna Circle:

> Das Ganzheitsdenken in der Tradition des Wiener Kreises setzte sich in Quines Behauptung fort, daß wir immer von der ganzen Sprache ausgehen, wenn wir von der „Verifikation" einen Satzes reden. Für Cassirer stand auch fest, daß die kleinste Einheit mit empirischer Bedeutung nicht geringer ist als die „ganze Wissenschaft". Cassirer's Auffassung von einer Pluralität von symbolischen Formen oder (in Anlehnung an Humboldt) von der Pluralität und Relativität von Sprachen war ähnlich konzipiert. (113)

Cassirer vehemently disagreed with the Carnapian project:

> Der Psychologe, der seine Versuchspersonen als blosse Sprechmaschinen (Garmmophone) behandeln u. ihre „Äußerungen" in diesem Sinne „protokollieren" würde, könnte daher niemals zu irgend einem Resultat kommen… (115) Z.T hängt freilich diese Problematik damit zusammen, daß Carnap u. der Wiener Kreis die Basis des Problems viel zu schmal ansetzen […]. (114)

In “Two Dogmas of Empiricism “, Quine had argued that our knowledge forms a “web of belief”. The editors write in an introduction:

> For Quine, no piece of knowledge was sacred or beyond potential revision. His work in the 1950s partially anticipates and lays the groundwork for Thomas Kuhn’s *Structure of Scientific Revolutions* (1962) […]. That scientific theories are simply description of observed phenomena rather than deep description of “reality”—is a clear extension of Quine’s web of beliefs (Quine 2008, 44).

Quine himself wrote in 1999:

> Observation sentences, already theoretic to varying degrees, are learned outright and helter-skelter by direct holophrastic conditioning. Further ones are synthesized along the way from bits of those at hand. They vie with one another in a surging equilibrium of evidential claims. Such is the web of belief (263).

This, “vision of science” Quine says, “is a step from Karl Popper toward Thomas Kuhn” (263).

Viewing these connections as forming a *spectrum* is one way to interpret the foregoing discussion. Positioning Kuhn both philosophically and historically within such a spectrum involves acknowledging the influence of numerous thinkers, each contributing to varying degrees of agreement and divergence. As Alan Richardson observes, “Carnap’s version of scientific philosophy descends from the Kantian and neo-Kantian tradition” (Richardson 2008, 298). In a footnote on the same page, Richardson further highlights affinities not only with Kantian ideas but also, in an extended sense, with American pragmatism.

> Pragmatism was named after the pragmatic as it appeared in Kant’s philosophy, as was stressed by Dewey (1931, chapter 1), following Peirce. Peirce wished to stress the distinction between the transcendental elements of Kant’s account of freedom (the practical) and the empirical elements (the pragmatic) and to endorse only the latter (298).

Friedrich Stadler has pointed out that “In spite of the predominant narrative of a strictly anti-Kantian Logical Empiricism, in particular of the Vienna Circle […] one can note that its representatives examined permanently Kant’s theoretical [as demarcation] and practical philosophy as well as variants of neo-Kantianism” (Stadler 2015, 789). Stadler encouraging

"comparative research", concludes that "This would also put into perspective the prevailing dichotomy between analytic and continental philosophy" (790).

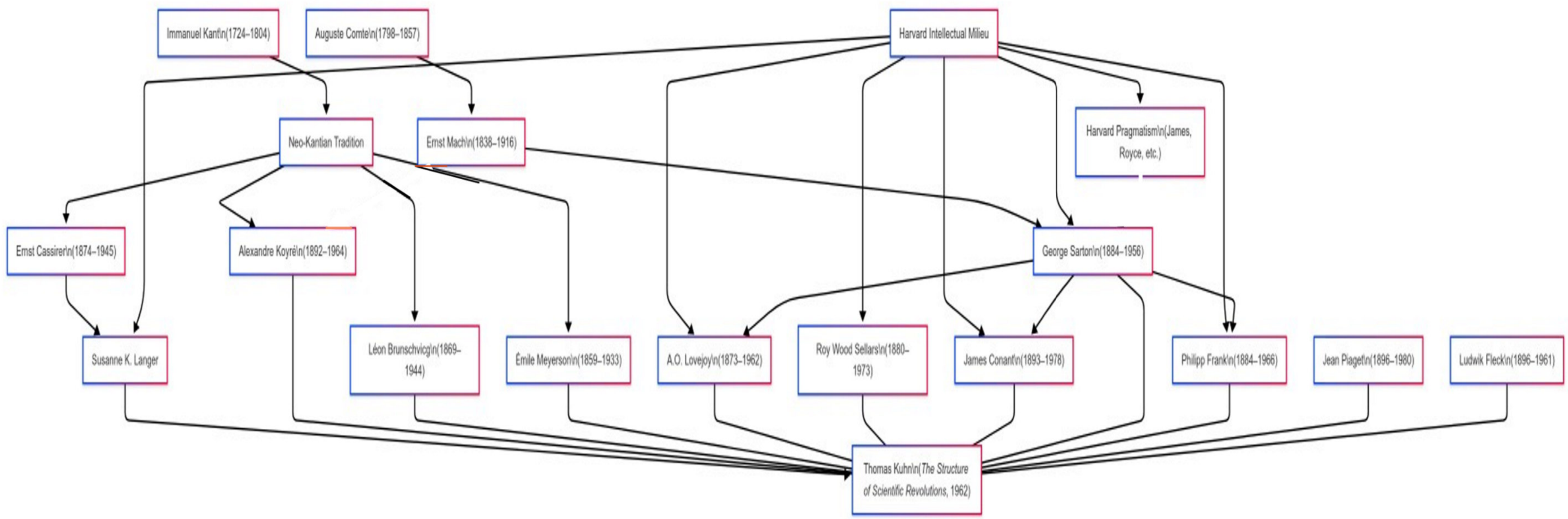
On the final page of *The Structure of Scientific Revolutions* (1962), Kuhn closes with a strikingly 'transcendental' question: "What must nature, including man, be like in order that science be possible at all?" (Kuhn 1970, 173), followed immediately by, "Why should consensus endure across one paradigm change after another?" (173). These questions strongly echo Richardson's analysis of Carnap, namely, that Carnap's commitment lies not simply with empiricism but with "scientific philosophy," and that "the relation of Carnap's philosophy to Kant's […] is rather a matter of the relations between properly scientific questions and questions of other sorts" (Richardson 2008, 298).

Looking back to section 1.1 of this dissertation, I raised the question: could the point of departure for *Structure* be found in the analytic-continental divide—the notorious notion of the so called "gap" which C. P. Snow presented as the "two cultures"[90]? Put differently: Did *Structure* ultimately resolve the philosophical dichotomy that so profoundly shaped Kuhn's generation? In light of the foregoing narrative, this too, was indeed an aspect of the historicism that was eventually resolved in *Structure*. Kuhn's intellectual environment at Harvard should be seen less as a series of discontinuous influences and more as an *interconnected* construct—a continental "Bridge" if you will. Frank, Quine, the Vienna Circle and even the notion of "transcendence"—of Harvard critical realists (see 2.7.2)—were integral elements of Kuhn's intellectual world.

In summary, while Friedman's interpretation of Kuhn's neo-Kantian affinities is not entirely mistaken, it remains overly narrow in scope. His emphasis on Kuhn's later engagement with Kantian philosophy does not fully capture the broader intellectual context or the specific developmental dynamics that ultimately gave rise to *Structure*.

[90] Kuhn owned a copy of C.P. Snow's *Two Cultures and the Scientific Revolution* (1960) (TSK-MIT_Inventory).

THE CONTINENTAL "BRIDGE"



This diagram is intended as a conceptual overview rather than an exhaustive account of all the nuanced lines of influence.
Image: MNB on Mermaid Editor

7

7.1 Conclusion

The Origins of *Structure* and the Historicist Thought

When *The Structure of Scientific Revolutions* burst onto the scene in 1962, its radical claims about paradigms, scientific revolutions, and incommensurability struck many readers as having come out of nowhere. In fact, a close look at Thomas S. Kuhn's undergraduate syllabi, lecture notes, examination questions, and correspondence reveals that the seeds of his most famous ideas were already germinating in Harvard's own early-twentieth-century philosophical tug-of-war. This dissertation has shown that the genesis of *Structure* developed along lines of historicist scientific thinking—though not necessarily through Kantian or neo-Kantian pathways. My investigation reveals how Kuhn's historicist approach emerged from Harvard's unique intellectual environment, which synthesized European and American philosophical traditions in distinctive ways.

In Part I ("The Student Years"), I established that Kuhn's education at Harvard unfolded within what can be called a philosophical "melting pot." While acknowledging the influence of Harvard's Kantian-inspired pragmatism and its later logical developments (through figures such as Sheffer, Russell, Whitehead, and Langer), I identified a more direct lineage of Machian historicist thinking, transmitted through George Sarton to James Conant and ultimately to Kuhn. This connection is supported by early reviewers of *Structure*, particularly Marie Boas Hall, who explicitly noted traces of Comte and Sarton in Kuhn's work (see 2.6). The Machian influence was further reinforced by Philipp Frank's "Unity of Science" program at Harvard.

Throughout this analysis, I maintained Harvard pragmatism as a crucial background framework. More significantly, I demonstrated how Alexandre Koyré's historicism, and A.O. Lovejoy's rigorous approach to intellectual history—including his persistent anti-Russellian battles against neo-realism (see 2.7)—may have shaped Kuhn's thought. My research shows that Lovejoy's *The Great Chain of Being* influenced not only Kuhn's historical methods (as Kuhn himself acknowledged), but also his broader philosophical outlook, as evidenced, among other sources, in Kuhn's correspondence with Lawrence S. Kubie.

The relationship to American critical realism proves particularly revealing. While Matthias Neuber has suggested neo-Kantian origins for this movement (see 2.7), I have argued that the American critical realists had diverged significantly from Kantian foundations—both chronologically and conceptually—to be accurately described as such, at least within the narrative I have outlined. Roy Wood Sellars' definitive statement that "The whole position is a frank physical realism very different from Kant's construction" (1929, 451) strongly supports this view (see 2.7). Nevertheless, Kant's profound influence ensured that neo-Kantian traces persisted in later philosophical movements—even as many thinkers moved beyond Kant.

In short, I have argued that *Structure* owes its genesis not only to European historiography—some of which was propagated by prominent neo-Kantians such as Cassirer[91]—but also to a creative union with various strands of American philosophical thought. Recall that Sellars' remarks on the 'interpretive function' notably echoed Kuhn's own insights (see 2.7).

In Part II ("The Teaching Years and the Development of *Structure*"), I traced how Kuhn integrated additional influences, including Piaget's developmental psychology and Fleck's concept of "textbook science" (see 4.1–4.3), along with the historicist approaches of Meyerson, Brunschvicg, and Koyré. These thinkers offered Kuhn alternative models of the history of science, which he first began to develop in his Lowell Lectures (1951).

Cassirer's neo-Kantianism certainly contributed to Harvard's intellectual atmosphere, though often indirectly—particularly through debates with Koyré on Platonism and Lovejoy's references to Cassirer in his work (see important footnote below). While Cassirer represented a vital continental European strand and actively shaped philosophical discourse through these exchanges, non-Kantian figures such as Koyré ultimately proved decisive for Kuhn's intellectual development.

During the genesis of *Structure*, Kuhn's intellectual position reflected a complex heritage that extended beyond the figures he explicitly acknowledged (see 4.2). This broader

[91] **As mentioned in 5.6 in a footnote, my latest archival findings show that Kuhn owned *Language and Myth* (1946) [*Sprache und Mythos* 1925] (translated by Susanne K. Langer), *Philosophy of the Enlightenment* [*Philosophie der Aufklärung* 1932] (1955), and *An Essay on Man: An Introduction to a Philosophy of Human Culture* (1948/1944) (TSK-MIT_Inventory). Therefore, Kuhn early in his career, pre-*Struture*, was indeed familiar with both Cassirer's early and later works.**

constellation of influences challenges Michael Friedman's approach, which relies heavily on identifying a "cast of characters" with a Kantian background while neglecting the more eclectic, historically grounded context of Kuhn's development. In short, it strains Friedman's post-Kuhnian construct: a philosophy of science framed primarily through Kantian critique, using Kuhn's philosophy—and *Structure* itself—as a springboard. I have demonstrated that this approach is too narrow and cannot adequately capture the true genesis of Kuhn's ideas. As shown, the intellectual formation of *Structure* cannot be understood solely by appealing to Kuhn's later philosophical commitments.

While scholars such as Michael Friedman (2000) have focused almost exclusively on the mature, "late Kuhn," this dissertation has argued that overlooking Kuhn's Harvard background is problematic. I contend that *Structure* grew directly out of the so-called "Harvard Hub"—the milieu shaped by William James's pragmatism and the subsequent split between two realist camps. On one side stood *critical realism*, the representational, historiographic account of knowledge championed by Arthur Lovejoy; on the other, *neo-realism*, the logical-atomist, direct-perception school associated with Bertrand Russell.

In contrast to Friedman's critique—which seeks to extend Kuhn's philosophy of science along Kantian lines—I have emphasized the role of historical awareness and historicism (whether Kantian in origin or otherwise) that characterized Harvard's vibrant intellectual milieu. The evidence reveals that a current of scientific-historicist thinking was already well established in the early twentieth century. The thinkers who inspired Kuhn's historical method—his intellectual "Honor List," so to speak—were key participants in this broader historicist, historiographical movement. It is this era, I have argued, that finds elegant and culminating expression in Kuhn's distinctive philosophy of science in *The Structure of Scientific Revolutions*.

My Interview with Paul Hoyningen-Huene

10 Questions: Q &A with Paul Hoyningen-Huene (July 23, 2024)

1. In a review of your book *Reconstructing Scientific Revolution* (Brit. J. Phil. Sci. 45, 1994, 923-926) Stathis Psillos writes:

   "The point that Hoyningen-Huene presses in the first half of the book is that Kuhn's underlying philosophy is relativized neo-Kantianism [Michael Friedman's thesis at least since *Dynamics of Reason* (2001)]. It is neo-Kantianism because Kuhn sharply distinguishes between the *world-in-itself*, which is epistemically inaccessible to cognizers and the *phenomenal world* [Kuhn would call this the "given" world] which is constituted by the cognizers' paradigm and is within their epistemic reach [Kuhn would call this 'belief']. But Kuhn's philosophy is relativized neo-Kantianism, because, unlike Kant, Kuhn posits *a plurality of phenomenal worlds* …" [I would associate this with theory-ladenness].

   Now, I have a comment and a question: My comment is that the thesis advanced in your book precedes that of Friedman's by almost a decade. So, you were perhaps the first philosopher who associated neo-Kantianism with Kuhn's thinking, which is remarkable.

   This is not quite correct. Gerd Buchdahl has seen the parallels between Kant and Kuhn already in 1969. See footnote 19 on p. 33 of my *Reconstructing Scientific Revolutions*.

   My question then is: What was Kuhn's stance on you advocating such a thesis?

   He liked it and expressed himself to be a Kantian of sorts; see the footnote mentioned above. Alexander Bird thinks that I pushed Kuhn somewhat in this direction because he was not so philosophically sophisticated at the time when he met me, see Bird's "Kuhn's wrong turning".

2. And since we are on the subject of neo-Kantianism, did he ever mention that he knew or had read Cassirer? (Although there is a cursory mention of Cassirer in the *Essential Tension*).

   No, I am not aware that he ever mentioned Cassirer.

3. Did Kuhn ever speak of Harvard philosophy, or his undergraduate teachers (H. M. Sheffer, D. C. Williams, or others)?

   I am not aware of it, but may have forgotten.

4. What about Harvard pragmatism? Did he address Harvard pragmatism as having any influence on him?

   Again, I am not aware of it.

5. My own research of early Kuhn (pre-*Structure*) connects Kuhn with Critical Realism espoused among others by Arthur O. Lovejoy. Critical Realism, according to M. Neuber, has its roots in neo-Kantianism of Riehl, Külpe, and the early Schlick. Now, besides Kuhn's well-known and documented admiration of Lovejoy's *The Great Chain of Being* (1936), did Kuhn ever mention Lovejoy's *Revolt Against Dualism* (1930) (in which Lovejoy dedicates two chapters to criticizing Russell), or were there any references to Lovejoy in your interactions with him?

   I don't remember any.

6. Kuhn's lecture notes of the early '50s reveal his critique of Russell. He criticizes Russell's philosophy of *The Scientific Outlook* (1931) – as it related to characteristics of scientific method – calling it "inflexible" and one that "does not allow for enough variation" (In fact, Kuhn developed an examination question in three parts on his critique of Russell for his students allowing them 60 minutes to respond. Kuhn's critique revolves around theory-ladenness, underdetermination, and fruitfulness. As I understand it, theory-ladenness for Kuhn implicitly implies that theory choice is to some extent arbitrary).

   It is not implicit, Kuhn mentions the element of arbitrariness three times in *Structure* (1970) on pp. 4-5.

   Now, I have hypothesized in my dissertation that Kuhn's critique of logical positivism in the *Structure* (that the logical positivists do not allow for incommensurability in theory change) may have its roots in Kuhn's critique of Russell almost a decade earlier. What are your thoughts on this?

   Unfortunately, I don't have any, I simply don't know.

7. I also want to ask you about Fleck. As early as 1949, Kuhn was not only familiar with Fleck's work his notion of "textbook science" (as archival material show), but he also praised him in both the *Structure* and elsewhere. Yet, he never mentioned that the idea of "textbook science" was Fleck's except, if I am right, it is first in Kuhn's forward (1976) to the translation of Fleck's book *Genesis and Development of a Scientific Fact* (1979) that he admits that the idea may have originated from Fleck. I hope I am not exaggerating, and I am certainly not trying to question Kuhn's integrity. But I wonder why Kuhn had to "guard" this notion almost as a secret? What has been your thoughts over the years on Fleck and specifically on Fleck's "textbook Science" and why Kuhn may have been slow in attributing this notion to him?

   I am not really aware that Kuhn did not correctly attribute to Fleck. However, I never seriously investigated this question because I was much less interested in the history of Kuhn's theory than in a reconstruction that collected the best of the theory.

8. Kuhn sometimes presented his ideas somewhat vaguely. In your opinion, was he concerned with over-exposing himself, or did he find outright commitment to certain thoughts as too restrictive? The following might be a clue to Kuhn's

vagueness, but I wanted to hear from you if you ever experienced such bouts of vagueness when discussing certain topics with him.

In many cases, he was vague because this was his temporary state of clarity. His philosophical instinct told him that this must be so-and-so, but at the time he was unable to explicate further. Therefore, incommensurability developed over the decades.

Sometime in early '50s he wrote:

[one must] not give a pattern which would be applicable to any conceptual scheme, because I don't think there is one. *Any attempt to categorize a repeated development almost invariably ends up being too restrictive* [italics my emphasis] It [generalizations] is almost sure to be fatal. The only way this can be avoided is by making the generalization as broad as Conant's or as narrow and modest as Priestley.

This could be the prudence of the historian of the 20th century, who mistrusts historical generalizations.

9. Another question and a puzzle for me has been Kuhn's relationship with George Sarton at Harvard. In the 'Athens interview' at the end of Kuhn's career, Kuhn goes very harshly on Sarton; he calls Sarton "a Whig historian". However, Kuhn's lecture notes show that Kuhn used Sarton's materials for introduction to certain historical periods. So, it is surprising that he would later in life be so critical towards him.

   Not necessarily because the materials themselves that a Whiggish historian collects may be quite useful.

   Is there an untold story behind this? In other words, are you aware of any interactions that Kuhn spoke of or related to involving Sarton? Why was he so critical of him?

   For Kuhn, Sarton was the prototype of a Whiggish historian. The Whiggish image of science was the main enemy in *Structure*, as Kuhn explains in its Introduction. Kuhn must have realized the difference between Whiggish and non-whiggish historiography in all its details between 1947 and 1961. I am not aware of any other untold story.

10. My final question is about what may be ahead for philosophy of science. What would a post-Kuhnian philosophy of science look like? I would like to connect this last question to my first question to you, namely, what are the chances that a post-Kuhnian philosophy of science be along Kantian or neo-Kantian thought?

    Well, it looks like it does look today! And because everything in philosophy is and probably will remain controversial, also the role of neo-Kantian elements will remain controversial in philosophy of science. They are now probably more explicitly present than, say, in the 1940s and 1950s.

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Selected Archival Sources

Selected from Thomas S. Kuhn archival materials at MIT, Distinctive Collections Cambridge, Massachusetts, USA

The archival materials below were collected by the author of this dissertation Mark Nader Basafa during a stay at MIT in June of 2023 and are made available online on Valep https://valep.vc.univie.ac.at/ for reviewers of this dissertation. This research was made possible by a doctoral student travel grant from the University of Vienna's philosophy department. Special thanks to MIT Distinctive Collections and its dedicated staff and for the permission to use these materials for my research purposes. Finally, I would like to express my gratitude to Mag, Dr. Christian Damböck, Privatdozent and my dissertation advisor at Institute Vienna Circle who wholeheartedly supported me throughout this endeavor.

Box 1

TSK-MIT, Box 1, Folder 1 "Biographical".
TSK-Box 1, Folder 3 (Undergraduate Papers 1940-46).

TSK-MIT, Box 1, Folder 6 labeled 'INCOMPLETE MEMOS & IDEAS' [1949 is the year on this folder in handwritten, but there are book loans from the library that are from 1950. There are also some undated writings, which I suspect could go back as early as 1946].

TSK-MIT Box 1, Folder 11
TSK-MIT, Box 1, Folder 14
TSK-MIT, Box 1, Folder 17
TSK-MIT, Box 1, Folder 16_Natual Science 4, 1952-56.
TSK-MIT, Box 1, Folder 21
TSK-MIT, Box 1, Folder 22
TSK-MIT, Box 1, Folder 23
TSK-MIT, Box 1, Folder 25
TSK-MIT, Box 1, Folder 30

Box 4

TSK-MIT, Box 4, Folder 2 (MONOGRAPH: Chapter 1 – 1st Draft).

Box 5

TSK-MIT, Box 5, Folder 67_Family Resemblances [Letter to L. Kubie].
TSK-MIT, Box 5, Folder 84.

Box 25

TSK-MIT, box 25, folder 21 (Alexander Koyré the History of Science, 1970).

Box 28

TSK-MIT, Box 28, Folder [Athens]

(Includes unpublished off-the-record material. This is the original unedited copy of the so-called Athens Interview, which was copy-edited by Kuhn himself. It is otherwise, for the most part, identical with the published interview with Kuhn by Aristides Baltas, Kostas Gavroglu, and Vassiliki Kindi titled "A Discussion with Thomas S. Kuhn" in *The Road Since Structure* published in 2000).

Book Inventory: Boxes 1-25 (out of a total of 40 boxes)

TSK-MIT_Inventory
(As of April 15, 2025. These were books in Thomas Kuhn's possession that were given to MIT as a gift).

Selected from Karl Popper Collection at University of Klagenfurt, Austria

Box 39

Karl Popper-Sammlung der Universität Klagenfurt (unpublished materials), KPA-Box 39, Folder 6.
Karl Popper-Sammlung der Universität Klagenfurt (unpublished materials), KPA-Box 39, Folder 7.