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Philipp Frank: Philosophy of Science, Pragmatism, and Social Engagement

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(Monograph)

by

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Abstract

Philipp Frank—physicist, philosopher, and early member of the Vienna Circle—is often neglected in retrospective accounts of twentieth century philosophy of science, despite renewed interest in the work of the Vienna Circle. In this thesis, I argue that this neglect is unwarranted. Appealing to a variety of philosophical and historical sources, I trace the development of Frank's philosophical thought and, in so doing highlight the roles played by history, sociology, values, and pragmatism in his philosophy of science. Turning to contemporary literature, I then argue that Frank's work should be understood as an early instance of what is now called "socially engaged philosophy of science." This understanding is explored through a careful consideration of his work on education, where previous work on history, sociology, values, and pragmatism is applied to an important, real-world problem. This socially engaged reading of Frank extends beyond pragmatic issues of theory application, because as I show, Frank used sociology to argue for the meaningfulness of metaphysical claims. However, Frank's account of meaning may seem to be problematic since it heavily relies on Percy Bridgman's operationalism. So, I outline the problems associated with Bridgman's account of operationalism and show that Frank's view does not fall prey to the same criticisms. After these objections are addressed, Frank's work is contextualized in the broader debate about value-free science, where I argue that Frank did not endorse the value-free ideal. As a result of these findings, we will not only have a clearer picture of Frank's philosophical contributions, but also a better understanding of how the philosophy of science can better engage important social issues.

Keywords

Philipp Frank, Logical Empiricism, Vienna Circle, Philosophy of Science, Pragmatism, Science and Values, History of the Philosophy of Science, Socially Engaged Philosophy of Science, Operationalism.

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1 Chapter One

1.1 Introduction

Recent research has revealed a picture of logical empiricism that is as multi-faceted as it is fascinating, of talented men and women troubled about the social and political issues of their time as well as issues in science. For some of them this meant undertaking a radical philosophical program to address all these concerns in a holistic system where these seemingly disparate components of human thought are intimately related. Apart from the mercurial and brilliant Otto Neurath, this multiplicity of intellectual concerns and remarkable breadth of human interests is perhaps best exemplified in the logical empiricist movement by Neurath's friend and colleague, Philipp Frank.

Frank was a physicist and member of the Vienna Circle. He studied mathematics and physics at the universities of Vienna and Göttingen under Ludwig Boltzmann, Felix Klein, and David Hilbert[1]. In 1912 Frank obtained a position at the German University of Prague, as "the successor to Einstein" (Carnap 1962, p. 32[2]). Later, in 1931, Frank helped to bring about Carnap's appointment to the position of Professor of Natural Philosophy at the same institution, "despite the strong opposition of the adherents of traditional philosophy" (Frank 1949a, p. 45). In 1938 Frank traveled to the United States, giving lectures at over 20 universities. As a result of the growing scourge of Nazism across

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[1] Dr. phil. in 1906 (22), Thesis title: On the criteria for the stability of the movement of a material point and its relation with the principle of the smallest effect (Stadler 1997, p. 631).

[2] See also Frank 1949a, p. 31.

Europe, Frank was unable to return to Czechoslovakia and instead accepted a limited-duties appointment at Harvard University. Like the other members of the Vienna Circle, Frank was trained as a scientist but was also deeply interested in philosophy. However, Frank's interest in philosophy is often at variance with what we have come to expect from logical empiricists.

The distinctive nature of Frank's philosophy of science was recognized during his lifetime. Rudolf Carnap described Frank as "much interested in the sociology of scientific activity" and "wary of any proposed thesis that seemed to him overly radical, or of any point of view that seemed too formalistic" (Carnap 1962, p. 32). Ernest Nagel emphasized Frank's strong interest in the "social import of science," and identified an important goal of Frank's philosophy of science: "to clarify the nature of scientific ideas and to dispel the misconceptions that so frequently envelop them" (Nagel 1965, p. xxiv). Hilda von Mises provided an elegant discussion of Frank's epistemological position:

> Frank's aim is to conceive science and philosophy as *one* coherent system of thought; not as a finished existing system which we merely have to discover but rather as one 'in statu nascendi,' subject to continuous modification. Of no scientific proposition, of no basic statement or axiom, do we know what will change with experience in the future. Certainty can be found only in tautological statements–which do not tell us anything about the world of experience (von Mises 1965, pp. xxi-xxii italics original).

She also stressed how Frank used traditional aspects of logical empiricist thought – emphasizing "clear thinking and unceasing logical criticism of language" – to challenge "intolerance, fanaticism, and injustice" (von Mises 1965, p. xxii).

We do not have to look far to find evidence of Frank's distinctive approach to logical empiricism. At the conference entitled "The Epistemology of the Exact Sciences," which Frank organized in Prague in 1929, he emphasized, "that the modern science is incompatible with the traditional systems of philosophy" (Frank 1949a, p. 41). In contrast to those traditional systems, Frank promoted a broad view[3] of the philosophy of science:

> I now put the greatest emphasis on presenting physics, and science in general, as part of our general pattern of thinking and acting. I presented it on one hand as a logical system that has to be checked by physical experiments and on the other hand as one of the means of expressing man's attitude towards the world, the small world of society and politics and the large world that is our astronomical universe (Frank 1949a, p. 51).

Indeed, as we will see throughout this thesis, Frank's philosophy of science often blended together traditional forms of logical empiricism with his interest in sociology, politics, and "man's attitude towards the world" (*ibid.*). While it is uncontroversial that Frank did indeed advance a unique blend of logical empiricist philosophy of science, little work has been done evaluating Frank's philosophy as a whole.

The aim of this thesis is to develop a fuller account of Frank's work, by showing it to be a systematic philosophical program that enriched logical empiricism through the careful consideration of values, history, sociology and pragmatism. In developing Frank's work, this thesis will make two main contributions. The first is historical. In the present chapter, I will endorse a more expansive view of logical empiricism, one that I argue was pursued by Frank from the outset. Furthermore, I will also address how Frank's pursuit of topics such as values, history, sociology and pragmatism affected his relationship to logical empiricism. Motivated by Frank's broad approach, contemporary scholars (e.g., Richardson and Hardcastle 2003) often ask: Was Frank a logical empiricist, or a pragmatist? And, if Frank was a pragmatist, then is his mature philosophy of science

[3] I use this particular phrase because it is how Robert S. Cohen and Marx Wartofsky introduced Frank's work in their preface to the 1965 edition of the *Boston Studies in the Philosophy of Science*.

tantamount to a rejection of logical empiricism? In exploring these questions, I will show that Frank's interest in pragmatism does not imply a rejection of logical empiricism, but rather an *extension* of it. Specifically, I argue that as early as 1907, Frank pursued a unique vision of logical empiricism, one that uses pragmatism to emphasize the social value of science.

The second contribution will consist in systematizing Frank's philosophical work, especially his socially engaged philosophy of science, account of meaning, and use of pragmatism. The bulk of this thesis will concern the second contribution. Throughout this thesis I closely attend to how the seemingly disparate aspects of Frank's thought are in fact connected. The first chapter provides an intellectual history of Frank's work, highlighting the philosophical and historical influences that shaped Frank's thought. I argue that Frank's interest in sociology, history and values were central to his philosophical thought. So, by tracing the influence of several formative experiences in Frank's life, we will see that social engagement was by no means an afterthought for Frank.

However, this line of argument is not without controversy. Indeed, Sarah Richardson (2009 a/b) argued that attributing such a position to any logical empiricist is wrongheaded and, perhaps, harmful to the cause of political philosophy of science. Given that social engagement is central to my reading of Frank, I take up Richardson's arguments in the second chapter. Following a presentation of Richardson's arguments and the literature that she responded to, I argue that it is both helpful and accurate to understand Frank's philosophical work as a precursor to contemporary work on the issue of socially engaged philosophy of science. This approach is then demonstrated in two ways. First, we will see that Frank’s work contributes to contemporary work in the field of socially engaged philosophy of science. By appealing Frank’s work on education, we can add to Kitcher’s account of tutoring in well-ordered science. Second, we will see that Frank’s view of science does indeed encourage discussions of value. This second conclusion stands in direct contrast to Richardson’s arguments.

Once we accept that Frank’s work ought to be viewed as a form of socially engaged philosophy of science, we will then move on to understand how these beliefs figured into the specifics of Frank’s philosophy of science. So, in chapter 3, we turn to the issue of verificationism. Verificationism holds that the meaning of theoretical terms is determined by their method of verification. Due to its emphasis on methods, verificationism is usually thought to prohibit any substantial role for values or politics in the determination of the meaning of scientific statements. Consequently, many scholars who attribute some form of social engagement to Frank’s work also distance his view from this problematic philosophical doctrine. But, as I will argue, these readings are not fully consistent with Frank’s work. In assessing Frank’s relationship to verificationism, we will see that he not only adapted this doctrine but also that his adaptations allowed for value-laden discourse to play important roles in scientific thought.

Many of Frank’s arguments presented in chapter 3 rely on Percy Bridgman’s operationalism. Bridgman was a close friend and collaborator of Frank’s, so the fact that Frank read and relied upon Bridgman’s work is to be expected. However, Bridgman’s own account of operationalism is relies on methodological solipsism and is radically individualistic, which may seem to contradict Frank’s social account of science. But, as we will see in the fourth chapter, Frank does not hold to all aspects of Bridgman’s operationalism. By closely examining both Bridgman's and Frank's accounts, we will see that Frank's understanding of operationalism can indeed be reconciled with his commitment to socially engaged philosophy of science. Additionally, we will see that viewing Frank's work through the lens of socially engaged philosophy of science even helps to explain the peculiar arguments Frank made at the end of his career[4].

By understanding Frank's account of meaning, we will be able to assess how his discussions of history, sociology and values each figure into a larger philosophical project. So, in the fifth and final chapter of this thesis I argue that Frank used these distinctive elements of his philosophy to challenge the value-free ideal. When considering the social effects of scientific research, one important contemporary issue has been the value free ideal (or the idea that social and political values should not influence science). Indeed, logical empiricism is often remembered as a major perpetrator of this ideal.

So, by arguing for the systemization of Frank's work, I show how social and political concerns guide his thought throughout his career. For example, in the present chapter I argue that Frank's use of pragmatism and his interest in social concerns evolved together and that any accurate discussion of Frank's philosophy of science must appreciate how these two aspects of his thought are related. By treating Frank's varied interests as if they were distinct philosophical pursuits we are led to incorrectly describe Frank's complex vision and to mistakenly assess the potential that his work might have for future generations of philosophers. Further, I emphasize that by overlooking the

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[4] See Frank's 1962 discussion of Carnap published in the Schilpp volume.

interconnections within Frank's philosophy, contemporary scholars sometimes draw incorrect conclusions about the aim and value of Frank's arguments.

The current chapter will trace the development of Frank's thought and will focus on the development of his account of the interrelationship of science, values, history, sociology and pragmatism. Frank's emphasis on these issues is noteworthy given how little attention most other logical empiricists paid to them (with the notable exception of Charles Morris). This chapter will proceed by first introducing logical empiricism, by contrasting it with the Received View of scientific theories and distinguishing among logical empiricism, logical positivism, and the Vienna Circle. The moniker, "Received View," as it is used here, refers to the form that logical empiricism took in the 1950's and maintained through its demise in the 1970's. The reader should note that the Received View is not coextensive with the philosophy of the Vienna Circle. So, the Received View refers to the set of beliefs held by many North American philosophers of science and not just the members of the Vienna Circle.

Putnam coined the term in 1960, at the International Congress for Logic, Methodology, and Philosophy of Science in Stanford, California (Putnam 1962, p. 240)[5]. There he presented the paper "What Theories Are Not," in which he argued that the then standard distinction between observation terms/statements and theoretical terms/statements was unfounded. This distinction – crucial to the logical analysis of language, as endorsed by the logical empiricists – never recovered (Suppe 1977). And, as debates about science evolved into debates about how to account for scientific practice

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[5] Putnam's paper was published in 1962, in the proceedings of the conference.

using first-order classical logic[6], critics of logical empiricism began to see the Received View as an obstacle to progress within the philosophy of science. As a result, the term "Received View" connotes the growing need of logical empiricism (in its North American form) to justify its own methodology, which in turn precipitated a growing interest in the justification of formal methods. Thus, the Received View represents the late, embattled form of logical empiricism as it was understood in North America.

Due to the fact that the Received View was so closely associated with logic and formal methods, critics of the Received View tended to emphasize the importance of topics such as history, sociology, and the role of values in science. Noting that Frank too emphasized these topics, contemporary scholars sometimes question Frank's commitment to logical empiricism. Some scholars even go so far as to question whether or not Frank was really a logical empiricist. For example, Richardson and Hardcastle (2003) explain that a thorough consideration of Frank's philosophy of science should cause us to ask the following questions:

> …Are these the activities of a man who had become alienated from professionalized logical empiricist philosophy of science and who, in light of these activities, was rendered marginal in the philosophy of science? Or are these the actions of a leading logical empiricist, actions that give lie to any claim that logical empiricism was disconnected from history and sociology of science and from the larger social and cultural contexts of science and its philosophy?
>
> These questions are important because, among other things, Frank, his courses in philosophy of sciences (which he taught in the Physics Department at

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6 More sophisticated accounts of logical empiricism employed higher-order logics in order to address problems faced by the received view of scientific theories. The Carnap-Ramsey sentence is a notable example. Despite the initial success of the Carnap-Ramsey sentence in resolving some of the problems faced by correspondence rules (discussed further below), it too failed to adequately define theoretical terms. The present discussion of the Received View is a very general and hence incomplete account of the form that logical empiricism took in North America. For a more complete account of Carnap's use of higher-order logic see Carnap 1956. For an introduction to the issue see either Hempel 1965, pp. 185-222 or Andreas 2013. I will continue to introduce the Received View discussing only first-order classical logic.

> Harvard), and his books were well known to James B. Conant and, at least through Conant, to Thomas Kuhn. He was also well known to George Sarton, a colleague of Percy Bridgman, and a key mentor of Gerald Holton, Marx Wartofsky, and Robert S. Cohen. Frank was, that is to say, the member of the Vienna Circle who worked most closely with physicists, historians of science, and sociologists of science in the American context. It is enormously important to figure out, therefore, whether his relations with those groups were part of a *turning from* logical empiricism or a part of a *commitment to* logical empiricism. The issue is central to understanding the place of logical empiricism not merely in the history of philosophy of science and not merely in the history of philosophy but in the history of our culture's twentieth century attempts to understand science as a human activity (Richardson and Hardcastle 2003, p. xix).

On one hand, Richardson and Hardcastle require us to ask if we have correctly understood logical empiricism. Perhaps Frank's interests in sociology, history, and ethics were an important part of logical empiricism from the beginning, which for whatever reason, the discipline wrongly ignored. On this reading, Frank's interests indicate that philosophers of science have misjudged our own history. On the other hand, this challenge requires us to consider the philosophy of science broadly. Perhaps we ought not conceive of our discipline as one that should be centered around epistemological questions, as proponents and critics of the Received View would have it. On this broader reading, we should not simply ask how science produces knowledge, but also what the production of that knowledge means in the context of society.

In the following section we will explore the former question. In part this is because Frank always considered himself to be a logical empiricist (Frank 1949a), which was confirmed by Gerald Holton and Robert Cohen in 2014 interviews[7]. To answer the latter question, I will provide an intellectual biography of Frank, covering his time in the

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[7] Interviews were conducted with Gerald Holton and Robert S. Cohen in Cambridge and Watertown Massachusetts in February of 2014. Both Holton and Cohen knew Frank and each provided helpful guidance during the early stages of this project. These interviews will be cited in the text as follows: (Holton 2014) and (Cohen 2014).

first Vienna Circle (introduced below) through his time at Harvard University, ending my discussion in 1947. I argue that Frank's social, historical, and value-laden concerns should be thought of as representing an alternative form of logical empiricism and I show that Frank began developing this form of logical empiricism as early as 1907. But, in order to properly explicate these problems we must first understand key terms, such as "logical empiricism," "logical positivism," and "Vienna Circle."

1.2 Understanding Logical Empiricism

The primary aim of this chapter is to better understand Frank's legacy by developing a philosophical history of his life and work. In order to do this, we must also understand how Frank fits into the history of logical empiricism and what his work might add to that history. In this section I briefly explain the standard account of logical empiricism and then show how that account is more complex than usually thought. I then highlight the various ways in which Frank's work differs from the Received View, and argue that Frank offered a broader, more socially conscious version of logical empiricism.

The intellectual program of the logical empiricists brought together the radical empiricism of Mach and new work in logic and foundations of mathematics in order to develop a philosophical system that was capable of reconstructing the language of science – including theoretical and empirical statements, rules by which they could be connected, and formal systems that were sufficiently sophisticated to express these kinds of complex statements. In other words, the language of science could be developed as a purely logical language coupled with an empirical interpretation. Using this system, theoretical statements that could not be connected to empirical statements were said to be unscientific.

The upshot of the approach is that it distinguished between genuine and pseudo-philosophical concepts. Inspired by Wittgenstein, some members of the Vienna Circle argued that many “problems” in contemporary philosophy were instead the by-product of oddities particular to natural language. These “problems” were shown to be illusory through a rigorous analysis of language, a process by which the empirical, logical, and mathematical elements of language could be isolated and definitively shown to be clear and true, or unclear and false. Thus, careful attention to these aspects of language would definitively show why propositions were or were not scientific. On this analysis, two kinds of statements have cognitive significance – *a priori* analytic and *a posteriori* synthetic propositions. Statements of the former kind are defined as tautologies; the latter are verifiable, empirical claims.

This program, however, failed to achieve its aim and was ultimately discarded following devastating critiques. It is well known, for example, that logical empiricists were unable to account for scientific explanations using first-order classical logic in, for example, the deductive-nomological model. And the analytic-synthetic distinction that grounded much of Carnap’s work was widely thought to be untenable after Quine’s *Two Dogmas*; so the language of science could not be reworked into clearly distinct classes of analytic and empirical statements (without invoking metaphysics). Further, Putnam argued that the observational/theoretical distinction was unfounded, for observational terms could not be used to interpret theoretical terms, thus calling into question whether or not the logical analysis of language was indeed empirically grounded (1962). Despite these failures, logical empiricism categorized many other philosophical systems as pseudoscientific, meaningless or nonsensical, which alienated the adherents of many other philosophical communities.

While the above description remains accurate, a resurgent interest in logical empiricism has added depth to our understanding. As part of this emerging scholarship, differences among logical empiricists have been more closely attended to. Philosophical differences are evidenced by the fact that members and associates of the Vienna Circle could not even agree on how best to refer to the group. Neurath coined the term “Vienna Circle,” because “he thought that this name would be reminiscent of the Viennese Waltz, the Vienna woods, and other things on the pleasant side of life” (Frank 1949a, p. 38). Reichenbach, who coined the term “logical empiricism,” did so in order to better distinguish his views and those of his Berlin colleagues, from those of the Vienna Circle, which Reichenbach (among others, to be sure) called “logical positivism” (Uebel 2013). At the same time, Frank and Schlick introduced the phrase “The Scientific World Conception” in 1929 (*ibid.*). And as Frank noted, there were several members[8] of the Vienna Circle who disliked the idea of referencing either “philosophy” or “positivism” in the group’s name (Frank 1949a, p. 38). Additionally, Ayer’s 1936 introduction of the movement to the English-speaking world further complicated matters. Ayer was not a member of either the Berlin or Vienna circles but nevertheless came to be a central figure of the movement in the minds of most English-speakers.

Secondary analyses confirm that there was no thoroughgoing agreement about how best to refer to the group, as Thomas Uebel helpfully summarizes:

The conclusion is that there is no systematic unity of usage of the terms “logical

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[8] Frank does not specify to whom he is referring to here.

> positivism" and "logical empiricism" even among the participants and early critics of the tradition in question. While nobody objected to the term "logical empiricism," opinions were decidedly divided over the term "logical positivism." Since the dispute concerns precisely the adequacy of this term for naming the philosophies of the Vienna Circle in a contrastive way, there is, accordingly, no agreement precisely where it matters (Uebel 2013, p. 85).

Even if we were to restrict our focus to Frank's work, we would not be able to clearly and meaningfully distinguish between the terms "logical empiricism" and "logical positivism" (Uebel 2013, p. 84). So, in order to be clear, throughout this thesis I will refer to the movement in general as "logical empiricism," and will refer to the group around Schlick as the "Vienna Circle." "Logical positivism," will be avoided as much as possible.

By way of general introduction, "the Vienna Circle" refers to the philosophers who were invited to attend a reading group at the University of Vienna in the early 1920s by Moritz Schlick shortly after his arrival at the University of Vienna. In his history of the Vienna Circle, Friedrich Stadler divides this series of meetings into two phases: the "non-public" and the "public" (1997). The non-public phase began in 1922 when Hans Hahn, a long-time friend and colleague of Frank, successfully lobbied to have Schlick appointed to the chair of *Naturphilosophie* at the University of Vienna. The Vienna Circle began reading Wittgenstein shortly afterwards. By 1926, Carnap joined the faculty at the University of Vienna and the reading group. Around Schlick, the group had nineteen "core" members, who regularly attended meetings and contributed to the discussions[9]. In 1929, the group went public. This period was marked by the publication

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[9] They were: Gustav Bergmann, Rudolf Carnap, Herbert Feigl, Philipp Frank, Kurt Gödel, Hans Hahn, Olga Hahn-Neurath, Bela Juhos, Felix Kaufmann, Viktor Kraft, Karl Menger, Richard von Mises, Otto Neurath, Rose Rand, Josef Sthächler, Moritz Schlick, Olga Taussky-Todd, Friedrich Waismann, and Edgar Zilsel. See Stadler 1997 or 2007 for complete lists of the so-called "periphery." Notable members of the

of a "Manifesto" outlining the future direction of the Vienna Circle, the establishment of the journal, *Erkenntnis*, the formation of the Ernst Mach Society (the official version of the Vienna Circle), and inauguration of the book series edited by Frank and Schlick (Stadler 1997, 2007; Uebel 2013).

The Vienna Circle is sometimes defined by appeal to a common philosophy: a preference for empiricism, the new logical methods of Frege, Russell, and Whitehead, an explicit interest in Einstein's work, and scientific methodology. These interests were coupled with rejections of absolutism and *a priori* intuition. However, the specific opinions of members diverged greatly. For example, as Stadler summarizes:

> Something that is widely ignored in many histories of the Vienna Circle is that neither the idea of a physicalist unified science nor the verification principle was universally accepted for significant lengths of time. The Circle's pluralism of views and perspectives increased over time. For instance, the influence exerted by Wittgenstein decreased at the beginning of the 1930's on Hahn, Neurath, and Frank, but not Schlick and Waismann (Stadler 2007, p. 25).

Not only did the movement change over time, but so too did the philosophical perspectives of its adherents. Frank was no exception. In 1947, nearly a decade after his arrival at Harvard, Frank began to consider "the actual meaning of the metaphysical interpretations of science" (Frank 1949a, p. 51) by developing an account of logical empiricism that could empirically account for metaphysical beliefs through their effects on human behavior. (This aspect of Frank's philosophy will be discussed in sections 3.6 and 3.7.) But, as I argue in chapter 3, Frank did this without abandoning the basic tenets

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"periphery" included: A.J. Ayer, Josef Frank, C.G. Hempel, W.V.O. Quine, Frank P. Ramsey, Hans Reichenbach, and Alfred Tarski (Stadler 2007, p. 15).

of logical empiricism. This change constitutes only one of many that Frank underwent throughout his intellectual development.

Political differences also existed within the Circle. This was most famously exemplified by Schlick's rejection of the Manifesto due to its explicitly political overtones (Stadler 1997; Uebel 2013). Neurath, as chief author of the Manifesto of the Vienna Circle, laid out his characteristically political vision in the now-famous closing paragraph:

> Thus, the scientific world-conception is close to the life of the present. Certainly it is threatened with hard struggles and hostility. Nevertheless there are many who do not despair but, in view of the present sociological situation, look forward with hope to the course of events to come. Of course not every single adherent of the scientific world-conception will be a fighter. Some, glad of solitude, will lead a withdrawn existence on the icy slopes of logic; some may even disdain mingling with the masses and regret the 'trivialized' form that these matters inevitably take on spreading. However, their achievements too will take a place among the historic developments. We witness the spirit of the scientific world-conception penetrating in growing measure the future prospects of personal and public life, in education, upbringing, architecture, and the shaping of economic and social life according to rational principles.
>
> *The scientific world-conception serves life, and life receives it* (Neurath, Carnap, and Hahn 1929, in Stadler 1997, p. 339).

The thoughts expressed here contrast sharply with the common understanding of Received View, or the technical line of thought for which members of the Vienna Circle became famous in North America. Furthermore, remarks like this reveal another facet of the movement. Influenced by his training as a social scientist, Neurath was a perpetual voice of dissent and change, both philosophically and socially.

While Neurath was politically active, many members of the Vienna Circle did not share his radical leftist politics. For this reason we should be cautions when attempting to ascribe particular social views to the Vienna Circle as a whole. In addition, the political orientation of the members changed over time. After 1918, Reichenbach and Carnap "no longer considered theory and practice to be inevitably linked" and no longer seemed to believe that their work needed to be politically potent (Stadler 1997, p. 56). At the same time, the Viennese members of the movement – Neurath, Zilsel and others – never seemed to abandon the idea that the interplay of theory and practice was important.

Following Schlick's assassination, additional fractures appeared. For example, it was made clear by Friedrich Waismann, Schlick's student and close associate, that Schlick's views were not to be mistaken for Neurath's. The differences among the group were not limited to Schlick's rejection of Neurath's radical Marxism (Stadler 1997, p. 872). In the group's first major public debate – the protocol sentence debate – philosophical tensions came to a head regarding the correct understanding of the object of the language of science as either phenomenological or physical (Carnap 1932/1987, 1934; Neurath 1932/1983). There would later be more disagreement when Neurath objected to Carnap's emerging interest in the semantics of scientific theories (Reisch 2005) and the direction that logical empiricism would take in North America (Frank 1949a; Reisch 2005). So, it should not be surprising that by the 1950's the already loosely defined philosophical projects pursued by members of the Vienna Circle were becoming increasingly disjointed:

> The great impact of the movement upon the contemporary philosophical scene was due to the cooperation between men who were principally interested in logical analysis and men whose eyes were directed towards the way in which it handles experiential material and sets up far flung speculative hypotheses. Other members of that group directed their attention to the role which these scientific theories have played in the history of mankind. All these trends together have received in the U.S.A. the label "Logical Positivism," or Neurath, who liked to emphasize the positively integrating character of the movement, preferred later the name "Unity of Science" as designation of the goal [sic.].
>
> During the war and after Neurath's death the contact between the members

> was disrupted or at least, greatly weakened. Each participant in the movement devoted himself to the problems in which he was mainly interested: some produced excellent papers on formalized languages, on semantics, on the degree of confirmation, on induction, on the role of abstract entities in our language, while other ones investigated the problems arising from actual science as an individual and social enterprise. The lack of sufficient cooperation can account for a situation in which the specialists in different fields who actually worked on different places of a common enterprise could hardly understand one another and, to be frank, even often misunderstood one another (Frank 1956a, p. 15-16).

As Frank's remarks indicate, the disruption in the collective work of the logical empiricists was made worse by the fact that they may not have understood each other.

So, returning to Richardson and Hardcastle's challenge, we should recall that they suggest two readings. The first emphasizes historical revisions with respect to how logical empiricism is understood and the second encourages a "broad" view of the philosophy of science. In the quote above, Frank also describes a similarly broad project: one that involves attending "to the role which these scientific theories have played in the history of mankind" (*ibid*.). As Uebel's insights and my discussion show, the former approach is misguided since there was never much agreement within the Vienna Circle about what logical analysis can and should accomplish. The broader view is vague, but perhaps also more promising.

This broad reading is not just suggested by Richardson and Hardcastle, but also by philosophers who knew Frank. In their remembrance of Frank, Cohen and Wartofsky (1965, p. vii) note that Frank influenced their work in the Boston Colloquium for the Philosophy of Science by encouraging a "broad view of the discipline." In a similar vein, Carnap adds to that account:

> Frank recognized more clearly, I think, than most other philosophers and scientists that it is of greatest importance that those who work in theoretical fields be aware of the role of their work in the wider context of life, of the whole of society and culture...And further he regarded it as important that those working in

> theoretical fields, even in fields which seem quite remote from practical life, like theoretical physics or analytic philosophy, should be aware of their responsibility for the development of humanity (Carnap 1965, p. xi-xii).

These sentiments are at odds with many well-known depictions of the Received View, and even with what was claimed by other members of the Vienna Circle. The technical work that was characteristic of the Received View and of well-known members of the Vienna Circle, such as Carnap himself, is not conducive to broadly applicable philosophy of science. However, this so-called "broader approach" was not unique to Frank and harkens back to Neurath's passage from the Manifesto, where it seems that logical pursuits were relegated to "the icy slopes." Deemphasizing technical philosophy of science, both Frank and Neurath advocated instead for a socially conscious approach to philosophy.

Given their shared interest in a broader approach, it is perhaps telling that Frank and Neurath also turned to pragmatism as a means of pursuing this approach[10]. And, as Hempel noted, this project did not suffer from the same criticisms as the Received View:

> When people these days talk about logical positivism or the Vienna Circle and say that the idea is *passé,* this is just wrong. This overlooks the fact that there were two quite different schools of logical empiricism, namely that of Carnap and Schlick and so on and the quite different one of Otto Neurath, who advocates a completely pragmatic conception of the philosophy of science...And this form of empiricism is in no way affected by any of the fundamental objections against logical positivism. It is flourishing and thriving. Well, now it is important that we look at Neurath and then say that it is his ideas that despite their somewhat

[10] It is important to acknowledge that Carnap did leave room for pragmatic concerns. However, these concerns were introduced as concerns that are external to a given linguistic framework. So, on Carnap's account, the pragmatic concerns may be invoked only to assist in the choice of linguistic framework. On the accounts of both Neurath and Frank pragmatism plays a far more significant role. In this chapter I will explain the development of Frank's account of pragmatism and in Chapter 3 I will explain how pragmatism figures into Frank's account of meaning. For a detailed account of Neurath's view see Cartwright et al. 1996.

> primitive form give us hints to how the philosophy of science has to be developed further. This is all wonderfully tenable, and all the traditional criticism is meaningless with respect to this form of empiricism. And we may look into the future with confidence and gather under the banner of pragmatic empiricism that may yet live long, flourish and thrive. (Hempel, quoted in Wolters 2003)

Even though Hempel does not name Frank in the quote about, Frank's significant interest in pragmatism places him squarely in the pragmatic camp.

At Harvard, Frank often argued that pragmatism, operationalism, and logical empiricism were representatives of the scientific conception of the world – the cornerstone of progressive science. In his unpublished manuscript, *Science, Facts, and Values*[11], Frank devoted chapters to exploring the interrelationship between American Pragmatism and logical empiricism, all the while arguing that an alliance would be necessary to overcome reactionary, fascist metaphysics. In *Relativity, a Richer Truth* (1950), Frank uses a pragmatic epistemology to better understand science in the context of democratic states, and earlier Frank had credited Einstein with showing him the importance of pragmatism (Frank 1949a, p. 10). There is also evidence that, following Neurath's death, Frank took up aspects of his project. Neurathian phrases, such as the "humanization of science," receive significant attention by Frank during the Harvard period (Frank 1949a, SFV). The fact that Frank was trained as a physicist and Neurath was trained as a sociologist may account for differences in their approach (despite the broad agreement on many questions). Insofar as Hempel's remarks suggest a return to

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[11] Since Frank did not publish *Science, Facts, and Values*, the manuscript draft was never cleaned up. Additionally, we cannot be sure if Frank ever revised the document. George Reisch not only made the manuscript available on his website, but also provided helpful editorial remarks throughout the manuscript in which he noted issues that arose during the transcription of the document.

pragmatic logical empiricism, Frank's intellectual legacy deserves more attention than it has so far received.

As a careful study of the development and evolution of Frank's philosophy will show, the motivations for Frank's unique approach to logical empiricism were never far from broader concerns about the role of science in society and how the scientific attitude might be used to serve life. This implies, in turn, an underappreciated richness within logical empiricism. Scholars have long known that many logical empiricists had deeply held social and political beliefs. Yet, many philosophers are surprised when these beliefs are mentioned since the logical empiricists often had hostile relationships with more obviously political systems of belief. Accordingly, examining this aspect of logical empiricism through Frank's work should contribute to further reconsiderations of logical empiricism and to a broader, more pragmatic approach (that is inspired by science conceived of as a human activity) to contemporary thought on the interrelation of science, values, and society. In the next section we will examine the origins of Frank's broad approach.

1.3 Philipp Frank and the Crisis of Philosophy

Since very little attention has been paid to Frank in the literature, it is important to understand the development of his particular views. To this end, I highlight how and why Frank developed his interest in socially engaged philosophy of science and pragmatism. But, in order to tell this story, we must first understand the intellectual context in which Frank worked. This context, according to Frank, was marked by a period of intense intellectual discord brought about by the unwillingness of philosophers to accept Einstein's theory of relativity. But, during this same time period, Frank was already exposed to many of the ideas that eventually formed the cornerstone of logical empiricism.

Beginning in 1907, Frank met regularly with the other members of what has become known as the “first Vienna Circle.” This Circle also included Hans Hahn and Otto Neurath and was an informal, philosophical discussion circle in Vienna that met before the First World War (Uebel 2003; Frank 1949a, p. 1). While members of this group had anti-metaphysical leanings, something that would later characterize the circle around Schlick, their interests appear to have been significantly more expansive. Frank describes the following scene:

> I used to associate with a group of students who assembled every Thursday night in one of the Viennese coffee houses. We stayed until midnight and even later, discussing the problems of science and philosophy. Our interest was spread widely over many fields, but we returned again and again to our central problem: How can we avoid the traditional ambiguity and obscurity of philosophy? How can we bring about the closest possible *rapprochement* between philosophy and science? By “science” we did not mean “natural science” only, but we included always social studies and the humanities (Frank 1949a, p. 1 Italics original).

The interests of the members of the first Vienna Circle were different from what we might have expected on more traditional readings. In his remembrance, Frank was careful to acknowledge that there was always an “inclination towards empiricism on one hand and long and clear-cut chains of logical conclusions on the other” (Frank 1949a, p. 1). But the interests of the first Vienna Circle extended beyond empiricism and logic, and included religion, the general humanities, and social and political theory (*ibid.*). In the spirit of this intellectually open attitude, Neurath enrolled in the divinity school at the University of Vienna for one year to better understand Catholic philosophy and won an award for his paper on moral theology[12]. Frank describes the discussion circle as one without a common set of political or religious beliefs, but with a converging attitude towards the creation of a new form of thought regarding how to go about answering philosophical questions.

It was also during this period that the members of the group first read Duhem, Poincaré and Abel Rey (Frank 1949a; Haller 1991). In Frank's descriptions of these early meetings, he emphasizes the influence of Abel Rey, a French conventionalist. Despite their desire to bring about a "rapprochement" between science and the humanities, the first Vienna Circle remained skeptical of philosophy and specifically of philosophical interpretations of science. Haller (1991) describes Frank's early motivations as primarily a defense of scientific practice. In line with this reading, Frank feared that "a great many people believed, or at least wanted to believe, that the time had come to return to the medieval ideas…" (Frank 1949a, p. 3)[13], thus turning away from scientific practice.

In this section I will argue that the problems presented by Rey—insofar as they undermined scientific practice—helped to shape some of the unique aspects of Frank's thought. More specifically, Rey's influence led to Frank's emerging interest in the emancipatory role of science. Frank and other members of the first Vienna Circle held that science was progressive (that its descriptions of the world were becoming ever more accurate and that it would serve human life if allowed to flourish), but also recognized that if Rey was correct they could not account for this aspect of scientific thought. The

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[12] Frank indicates in *Modern Science and its Philosophy,* that Catholicism and Catholic philosophy would have been taken to be politically reactionary in the first decade of the 1900's, and particularly in the social setting in which members of the first Vienna Circle lived and worked (pp. 1-2).

[13] This is the origin of Frank's concerns regarding "school philosophy," which he brands as a form of idealistic metaphysics. This is discussed in more detail below.

problems introduced by Rey would go on to concern Frank throughout his life. I will begin by explaining the nature of the crisis introduced by Rey.

Rey argued that the then contemporary philosophies of science – mechanistic materialism and neo-Kantianism – could make no sense of the progressive nature of science. Mechanistic materialism held that rigid laws that were more or less directly discoverable governed the world. This view stood in opposition to the Hegelianism that dominated German-language universities in the 1850s and to neo-Kantian interpretations of science that would eventually come to dominate German-language universities in the 1900s. Suppe summarizes the general objectives of mechanistic philosophy:

> Thus there is no place for *a priori* elements in natural science or empirical knowledge. Observation of the world is immediate in the sense that no *a priori* or conceptual mediation is involved in obtaining observational knowledge; observation in accordance with the procedures of natural science is sufficient to yield knowledge of the world's mechanistic nature (Suppe 1977, p. 8).

On this view, important forms of human understanding such as formal systems and formal notions of truth would seem not only to be useless but possibly antiscientific. As a result, this view also precludes an active role for the mind in the act of theorizing. On a mechanistic materialist picture of scientific theories, such as the one described above, the scientific method can yield immediate and objective knowledge of laws without any recourse to philosophical speculation (*ibid*.). But, this view does not adequately appreciate the fact that there are usually a variety of ways of presenting natural laws (e.g., Newton's mathematical account of gravity could be given by using analytic algebra, geometric proofs, or calculus). Since mechanistic materialists did not address epistemic problems (e.g., should we prefer one account over another? If so, why?), they were unable to account for the conceptual revolution brought about by the introductions of new forms of mathematics in the early 20th century, as well as the corresponding advances in science.

As explained by Rey, Einstein's work also produced problems for neo-Kantians. The neo-Kantian interpretation held that Euclidian geometry is a precondition of our perception of space. This view necessitated that Euclidian geometry, a pure form of our intuition of space, formed the basis of our perception of space, as laid out by Kant in the "Transcendental Aesthetic" of the *Critique of Pure Reason*. As a result, many neo-Kantians rejected Einstein's theory of relativity because it called this basic tenet of Kant's philosophy into question[14]. As Rey explained, neo-Kantian philosophy also failed to explain the progress of science insofar as it would not accept advances in science if those advances required a rejection of Kant's philosophy.

Machian, neo-positivist schools of thought – of which Göttingen was a stronghold – rejected mechanistic materialism and were quick to accept Einstein's early results (Suppe 1977). Mach's positivism initially required a rejection of any *a priori* elements in scientific theories. This sort of thinking resulted in problems similar to those faced by the mechanistic materialists, and the position was softened to include a modified form of Poincaré's conventionalism, wherein mathematical and logical terms were regarded as commonly agreed upon definitions. The rejection of other *a priori* elements of scientific thought, such as absolute conceptions of space and time, primed Machians for their early acceptance of Einstein's theory of special relativity, even though Mach never accepted Einstein's theory of relativity.

As many Machians did, Frank often looked to Poincaré's conventionalism,

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[14] Cassirer was a notable exception. See Suppe 1977, pp. 9-11 for a brief discussion of Cassirer and Neo-Kantianism in German-language universities in the early 20th century.

ultimately believing that (some) scientific laws had the character of definitions, and were chosen because of their usefulness (thus distinguishing them from universal claims). On Frank's account of this view, scientific laws were nothing more than "a practical tool that can be used for a description of the physical world" (Frank 1949a, p. 21)[15]. However, Mach's radical version of empiricism was not without problems of its own, for his commitment to observability precluded the acceptance of scientific theories such as quantum mechanics.

So, during the time of the first Vienna Circle[16], the philosophy of science was in a state of disarray. None of the dominant views could adequately account for the progress of science, and they often led their followers to reject new scientific theories rather than to reject their philosophical assumptions. It was this tension between philosophy and science that motivated Frank and the other members of the first Vienna Circle to develop the "closest possible *rapprochement* between science and philosophy," so that philosophy would no longer stand in the way of science. But, Rey called the feasibility of this goal into question by challenging the notion that philosophy *could* make sense of scientific knowledge. This in turn prompted the first Vienna Circle began to explore how philosophy could account for the progress of science. As we will see, Rey's criticism not only presented epistemological problems for the first Vienna Circle, but also that those epistemological problems raised serious social concerns: If we can not make sense of scientific knowledge then why should we it expect it to benefit society?

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[15] Frank's discussion of Poincaré from 1949 lacks subtlety. Indeed, Poincaré's view is far more complex than either Frank's or Rey's descriptions imply. For a detailed account of Poincaré's conventionalism see Bland 2011.

[16] Between 1908 and 1912 (Frank 1949a, p. 3).

According to Frank, there are two components of Rey's criticism: the philosophy of science must account for the epistemology of science and it must also preserve some account of how science aids in human progress. With regard to the tension between science and philosophy, Frank notes the breakdown of two characteristic beliefs of nineteenth-century science during its last decades:

> 1.) the belief that "all phenomena in nature can be reduced to the laws of mechanics," and
> 2.) "the belief that science will eventually reveal the 'truth' about the universe" (Frank 1949a, p. 4).

On Frank's account, a resolution of this crisis would not require returning to a metaphysical notion of truth. Moreover, Frank did not think that the crisis could be avoided if we could somehow show that the laws of nature were mechanistic. Instead he advocated for a view of scientific theories that better approximated the methods of science. The "rapid transformation of scientific thought" needed to be met with an equally rapid transformation of philosophy, or "by rapidly changing methods in the scientist's approach to philosophy and in the philosopher's approach to science" (Frank 1949a, p. 5).

This aim is related to the second overall goal, that science should improve the lives of people. This is because Frank worried that if epistemic points 1 and 2 fail, then people would begin to question the epistemic value of science. And, if science does not correctly explain the world, then we should look to another source of knowledge for answers, and base our philosophy of science upon other, non-scientific systems of thought. Therein lies Frank's concern:

> Remembering our old arguments in the Vienna coffeehouse around 1907 about Abel Rey, Ernst Mach, and Henri Poincaré, I devoted some work to applying the newly developed "scientific world conception" to overcome the new crisis. I tried to show that there is not the slightest reason to see in twentieth-century theory an

> argument for an idealistic or spiritualistic world conception, and that this opinion only arises from a lack of scientific formulation of the new physical theories. This lack has its source in the poor training of physicists in philosophy, which makes them faithful believers in the metaphysical creeds imbibed in their early youth "from a nurse or schoolmaster[17]" (Frank 1949a, p. 46).

As this passage indicates, Frank worries that the crisis in physics and the philosophy of science could lead to a resurgence of antiscientific worldviews. These worldviews are not merely wrong but also dangerous and politically reactionary because they are frequently accompanied by problematic social and political beliefs[18].

The reader should note that the time of the first Vienna Circle was characterized by radical, divisive, and sometimes violent conflict. Romizi sums up the situation this way:

> In 1907, at the time when the so-called first Vienna Circle started its meetings, universal male suffrage was introduced in Austria: This meant the domination of the political scene by mass parties...these mass parties (not only Social-Democrats but also the conservative Christian-Socials and the German Nationalists) constituted *Weltanschauungsparteien*; that is, they were political parties fighting not only in order to increase their power and to foster the interests of their electoral supporters but also in order to establish a specific worldview (Romizi 2012, p. 21).

During the first Vienna Circle, misinterpretations of scientific theories were frequently associated with the world views described above, and these misinterpretations had social effects that would have been well known to its members. During this time, it would have

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[17] The quoted phrase in this passage is from A.N. Whitehead's *The Principle of Relativity* (1922). The reader should note that since Frank attributes this phrase to Whitehead, it may be the case that the phrase "school philosophy" did not originate in Frank's work, as is commonly thought (see Mormann 2009). This issue will be discussed at length in Chapter 5.

[18] Frank explicates these risks as follows: "Their real source is the urge to find support for a metaphysical creed that, for some reason, one cherishes. And this reason is, as we have already hinted, the fitness of this metaphysical creed to bolster up some political or religious creed that one believes to be indispensable for the well being of mankind. This sociological aspect has for many years been familiar to me from the discussions of our old Viennese group [the First Vienna Circle] … ." (Frank, 1949a, pp. 46-7)

been natural to think that dispelling pseudo-scientific myths would have been of benefit to society as a whole. Despite the fact that the first Vienna Circle "had no particular common predilection for a certain political or religious creed," their views had political overtones that would likely have been appreciated by their contemporaries (Frank 1949, p. 1). And, both Cohen and Holton separately confirm that Frank's sympathies did ultimately lie with the Social Democrats (Cohen 2014; Holton 2014), which indicates that Frank's worldview would have been directly at odds with those of the mass political parties described by Romizi. Frank does not say if he thinks that one aspect of Rey's challenge (i.e., either the epistemic or the social) is of primary importance or if they are both equally important. However, Frank's reflections from 1949 certainly indicate that the social concern was important by 1907.

The task of reforming the philosophy of science as laid out by Rey, and as understood and pursued by the first Vienna Circle, can be said to require several key elements. The first was that members would need to develop their own approach to the philosophy of science, an approach that would be free from the tendency of metaphysical views to reject scientific theories. Given that Frank studied at Göttingen, where Mach's views were popular and where Einstein's work was quickly accepted, a revival of Ernst Mach's philosophy would have been a natural and convenient choice. Frank's exposure to Poincaré's conventionalism aided in the updating of Mach's view, and ensured that a revitalization of Machian philosophy was at least tenable[19]. Lastly, such a view could

[19] Frank, along with other members of the first Vienna Circle, worried that Mach's philosophy of science did not do justice to contemporary work in math and logic (Frank 1949a, p. 7). Specifically, they "felt that considering the principles of science as nothing but abbreviated descriptions of sense observations [sense data] did not account fully for the fact that the principles of science contain clear-cut mathematical relations

contribute to emancipatory science by highlighting how metaphysical interpretations of science might be used to unjustifiably support political worldviews.

1.4 Conventionalism and Causality

In the first decade of the twentieth century it was not yet clear to Frank that his contemporaries could interpret his scientific views as political. During his time in Vienna, Frank seemed to understand the different schools of thought as merely intellectual alternatives to each other. Indeed it would not be until Vladimir Lenin cited Frank, along with Poincaré and Einstein, that Frank began to thoroughly integrate his thoughts on science with those about society and politics. This integration was not something that he arrived at on his own. Following an early publication (1907), Frank received two influential responses, one from Einstein and another from Lenin. In order to properly explain how those responses affected Frank, it is necessary to first discuss Frank's very early work on causality. That will be the topic of this section.

Frank's early philosophical thought was initially developed in response to two main figures in the philosophy of science: Kant and Poincaré[20]. Foundational ideas in the neo-Kantian school, especially Kant's identification of Euclidean geometry as a pure form of intuition, were beginning to crumble. Like other members of the first Vienna Circle, Frank did not trust *a priori* reasoning. In particular, he thought that Kant's account of Euclidean geometry would prevent acceptance of the innovative work that was

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among a small number of concepts, whereas every description of observations contains a great number of vague connections among a great number of vague concepts" (*ibid*.).

[20] For a more detailed discussion of the intellectual climate in Vienna at this time see Suppe, *The Structure of Scientific Theories,* the first section of the forward. See also *Wittgenstein's Vienna* (Janik and Toulmin 1996).

then being done on non-Euclidean geometries. Poincaré seemed to provide a solution. Rather than seek a new foundation on which to reconstruct science, Frank rejected foundationalism altogether. He instead argued for a conventionalist[21] stance towards basic concepts in scientific thought (such as time, space, causality, etc.). Such concepts may be useful for scientific thought and may even have their roots in the structure of our perception. However, that understanding does not show that these concepts are *a priori* features of the world. So, Frank reasoned, they are just intellectually helpful, and socially agreed upon ways of describing the world.

As Rudolf Haller (1991) argues, conventionalism, as understood by members of the first Vienna Circle, emphasized the need to use and justify their use of both theoretical and observational content. On this reading, theories are the means by which one may come to understand experience but do not uniquely determine that experience. The use of a particular theory is, rather, a negotiated choice through which one might identify facts and organize them in a manner that is consistent with our experiences and with some larger picture of the world (Haller 1991, p. 99).

Furthermore, Frank was deeply influenced by Mach's criticisms of Newton; so the necessity ascribed to Newtonian mechanics in the *Critique of Pure Reason* was also deeply unsatisfying to Frank. The failure of Kant's philosophical system characterized exactly what Frank took to be the problem with philosophy: philosophical prejudices were blinding scientists and philosophers to the facts. Newtonian mechanics was failing, and work on non-Euclidean geometry was producing many important and innovative

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[21] By "conventionalist," I mean that Frank thought that scientific terminology is not about the world, *per se.* Rather, the terms are established by appeal to theoretical principles.

results that philosophers were failing to recognize.

At this time, Frank was deeply influenced by Poincaré's conventionalism. It seemed to Frank that many of the problems of traditional philosophical systems were products of false certainty. This false certainty usually manifested itself in the form of absolute and foundational systems of beliefs. Conventionalism was initially an attractive alternative for Frank because it denied *a priori* certainty to physical laws and their underlying philosophical concepts.

Of Kant's work, Frank said, "Kant's primary aim was to answer the question of how the human mind can make statements about facts of the external world with an absolute certainty even if these assertions are not the result of experience about this world" (Frank 1949a, p. 9). So while Kant's formulation of the *a priori* forms of understanding had to be corrected, Frank appreciated that resorting to any alternative account that postulates a direct connection between the world and us could only explain human understanding by appeal to some other aspect of human understanding. For this reason, Frank agreed with Nietzsche "that Kant's explanation is merely equivalent to saying that man can do it [describe human understanding without referencing empirical content] 'by virtue of a virtue'" (*ibid.*). His initial attempt to resolve this tension is an application of Poincaré's conventionalism to the law of causality.

In a 1907 paper, Frank argues that questions of the correct understanding and application of the law of causality are "only question[s] of terminology" (Frank 1907/1949). By way of responding to Kant, the definitional character of the law means that it cannot be true *a priori,* but that it is instead "only a product of human imagination" (Frank 1907/1949, p. 57). Frank begins his discussion of the law of causality by challenging Hans Driesch's arguments[22] that the law of causality is true *a priori*, and as a result is not subject to empirical confirmation. Frank is careful to emphasize that his own account does not advocate for the empirical confirmation of the law of causality. Rather, Frank argues that Driesch draws the wrong conclusion: the law of causality cannot be true *a priori* for it is only ever an approximate description of the world.

On Frank's account, the approximate nature of the law of causality can be understood if we attend to how the law is used in scientific practice:

> It is important to understand that the law [of causality] can be applied only to the whole universe and not to a part of it. This, however, makes it impossible to test the law empirically. In the first place, one can never know the state of the whole universe, and in the second place, it is in general not certain whether it is possible for a state *A* of the universe to ever return. If no state *A* could ever be repeated, the law would be meaningless theoretically, since it refers only to reoccurring states.
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> Fortunately, it is not the exact law of causality itself which finds application in science, but a formulation of it that asserts only something approximate. This says that if, in a finite region of space, the state *A* is at one time followed by state *B* and at another time by state *C*, we can make the region sufficiently large by adding to it its environment that the state *C* becomes as close to the state *B* as we please.
>
> In other words, in finite systems the law of causality is the more nearly valid the larger the system. In the application of the law to a finite system, the answer to the question whether the system is large enough depends on the degree of accuracy required for the occurrence of the predicted effect (Frank 1907/1949, pp. 54-55).

In his discussion, Frank indicates that we are free to describe physical systems however we want, and to adjust our descriptions of physical systems so that they accurately predict some effect. But because these descriptions of a physical system are freely chosen and adjusted by us, then they do not have a unique and stable empirical interpretation. Therefore, we can only ever argue that the law is approximately true and not that the law

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[22] Here, Frank is citing Driesch's *Naturbegriffe und Naturuteile* ("Concepts and Principles of Natural Science," 1904). Driesch was a German scientist and philosopher of biology.

is true *a priori.*

Granting that his interlocutors would readily acknowledge the impossibility of describing the entire universe, Frank argues for the possibility of a restricted, approximate account of causality. (See the last two paragraphs in the quote above). When approximating the law of causality, we may restrict the system so that all of the relevant variables can be described. From these descriptions, we can say that state *B* follows from state *A.* So, in this way, the law of causality has been approximated. But, as Frank urges, we must not confuse our approximation of the law of causality with the "real" law.

> [The] law of causality as applied to finite systems is not the real one [law], but only a substitute for it. The real law itself is only an ideal which the law for finite systems approaches as a limit as these systems are made larger and larger (Frank 1907/1949, p. 55).

Due to the approximate nature of the law of causality, Frank argues that any application of the law of causality requires us to make practical decisions and to employ definitions (e.g., what counts as a state?) in order to present a workable form of the law. As a result, approximate forms of the law of causality rest not upon empirical interpretations, but upon definitions. And for this reason, Frank concludes that the law of causality is a convention.

1.5 Responses by Einstein and Lenin

In this section, I will explain how Frank's 1907 arguments about causality failed to account for pragmatic and social considerations. Einstein's and Lenin's responses forced Frank to seriously consider the roles played by pragmatism and social concerns in his work. The evolution of these ideas is important, because they mark the beginning of Frank's broader approach to logical empiricism. Frank's work is sometimes regarded as pragmatist form of logical empiricism (Uebel 2003, 2011; Reisch 2005) and scholars also look to Frank for early examples of socially engaged philosophy of science (Howard 2009; Douglas 2009, p. 50; Mormann 2009). Since this thesis will examine these aspects of Frank's work, understanding Frank's reaction to these formative criticisms is essential.

The first response that Frank received was from Einstein. (This was Frank's first personal contact with Einstein.) Recall that Frank gave the following account of the law of causality: given a certain amount of time, if state *A* of a system is followed by state *B*, then whenever *A* occurs, *B* will follow (Frank 1907/1949). Approximations of this general form of the law of causality are used in order to show *A* may be defined by appeal to a specific set of variables such that *A* will necessarily bring about *B.* Approximations of this kind require limits to the number of variables that are used to define *A.* Otherwise the law would have to describe all the variables within a system, which is an untenable requirement since we cannot describe the whole universe. But, because we must define *A* and the law of causality rests on our account of *A*, then the law itself rests on these definitions. And, on Frank's 1907 account, all empirical definitions are admissible. So we are free to define *A* however we please, in principle. Therefore, the law of causality does not have the status of a law of nature, because it can always be redefined and modified as desired.

Frank's reasoning, as outlined above, was generally acceptable to Einstein, though Einstein argued that Frank's paper showed only that there was a conventional element in the law of causality and not that the law itself was a convention. Einstein argued that in one sense, Frank was right: no one can ever prove that there was a violation of the law of causality in nature. This is a result of the freedom that we all have to specify and define philosophical concepts (such as causality) as needed. However, Einstein argues that if a violation of the law of causality were found, making *post hoc* modifications (e.g., including a large number of definitions, or a very high degree of specificity of what may be counted as *A*) would eventually produce a description that is confusing and difficult to use (Frank 1949a, p. 10). In other words, Einstein argued that Frank did not sufficiently appreciate the importance of simplicity in his paper. As Einstein pointed out, Frank's 1907 account ignores the fact that language must be understandable and useful. While in principle many definitions can be added to Frank's conventionalist account of the law of causality, in practice adding too many definitions will make the law become difficult to understand and even more difficult to use. An overly complicated account of causality will, in practice, cause scientists to reject it.

Consider the following example that is meant to explain Einstein's point: when I drop a ball, it is easy to correctly judge that my letting go of the ball is what caused it to fall.[23] In principle, on Frank's 1907 account, I could describe the system by appealing to the anatomy of my hand, my process of decision making, how easy the ball is to grip, etc. And all of that information, no matter how complex, would be scientifically admissible. However, if all we needed to know was why the ball fell, then it is impractical to include irrelevant details. So, in one sense, Frank is right, I could include incredibly detailed descriptions in my causal story about the falling ball and that description would still be scientific. But the inclusion of too much detail of the hand-ball system would likely make my account clumsy, and far more complicated than it needs to be.

This reconstruction of Einstein's point shows that there is a gap between what we

[23] This is my own example. For an expanded discussion of Frank's view, see chapters 2, 3, and 4 of *Relativity, a Richer Truth*.

can, in principle, include in a particular account of causality and the kinds of causal accounts that might be helpful in practice. The letter from Einstein led Frank to realize that Poincaré's conventionalism needed qualifications, that conventional choices had pragmatic constraints. As Frank says, "[o]ne has to distinguish between what is logically possible and what is helpful in empirical science. In other words, logic needs a drop of pragmatic oil" (Frank 1949a, p. 11)[24].

Einstein's role in Frank's life would extend far beyond the exchange discussed above. Shortly after this exchange, Einstein recommended Frank for the chair he vacated at the German University of Prague. Einstein began his discussion of Frank's work by stating "[the] great amount of able scientific work that this merely 28-year-old man has already produced is something to be admired…[Frank's work] combines a rare mastery of the mathematical tools with a good grasp of the problems of physics" (Einstein, May 1912, republished in Beck (trans.) & Howard (consultant) 1994/2014 v. 5, p. 302-3, doc. 400)[25]. Einstein paid particular attention to Frank's scientific contributions, singling out the following achievements: using Lorentz's electron theory to derive Minkowski's equations for the electrodynamics of moving bodies (1908); clear discussions of space-time transformations in classical mechanics (1908) and relativity theory (1909), and showing that only three kinds of transformations may be used to form a one parameter, linear, homogeneous group. Citing Frank's paper on causality, Einstein said that the paper (among others) showed Frank's "versatility" and his "ability to grapple with problems of general knowledge" (*ibid.*).

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[24] The reader should note that Poincaré allowed for pragmatic considerations in his account of conventionalism.

[25] Available online at: http://einsteinpapers.press.princeton.edu/vol5-trans/322

The second response received by Frank came from Lenin and was not brought to Frank's attention until sometime in the early 1920's (Frank 1949a, p. 11). Before introducing Lenin's criticism, it is pertinent to mention the last sentence in Frank's paper on causality: "With the question of world conception in the ethical-religious sense, all this has nothing whatsoever to do" (Frank 1907/1949, p. 60). This shows that at this early point, Frank did not see his work as directly relevant to *Weltanschauungsparteien*, despite the fact that his empiricism would have been unpopular among many academics in Vienna precisely because it undermined the political status quo that was grounded on Catholic theology (Stadler 1995). However, this presumed irrelevance to *Weltanshauungsparteien* is exactly what Lenin's criticism called into question.

In *Materialism and Empirio-Criticism* Lenin spent hundreds of pages attacking Kantian, Machian, and idealistic philosophies. He devoted only a few pages to a critique of Frank's work. But, in those pages, Lenin accused Frank of defending a Kantian, and hence an idealistic, worldview. In particular, Lenin openly took Frank's use of Poincaré's conventional account of causality as a means of defending Kant. And inasmuch as Frank was taken to defend Kant, he was accused by Lenin of attacking materialism, a foundational component of Lenin's dialectical materialism.

Lenin introduces Frank via Poincaré, whom Lenin describes as an idealist. Essentially, Lenin's understanding of Poincaré's argument is that science constitutes a system of symbolic representations (Lenin 1908). That system of representations is the product of the human mind. So, on Lenin's account of Poincaré, the laws of nature are constituted not in nature itself, but in our mental representation of nature. This conventional creation of symbolic laws implicitly denies the existence of an objective world, or any kind of concrete reality external to us. And since that denial is essential to Poincaré's understanding of how we are to understand the nature of scientific laws, Lenin argues, Poincaré's conventionalism implicitly denies the existence of objective knowledge of the world. Recall that in Frank's paper on causality, he denies that the law is either empirical or *a priori* (Frank, 1907, p. 57). And, because Lenin takes Frank to agree with Poincaré that there are conventional elements within this law, Lenin relegates Frank's work to the status of idealism along with both Kant and Poincaré. Lenin goes on to say that:

> For the essence of this point of view does not necessarily lie in the repetition of Kant's formulations, but in the recognition of the fundamental idea *common* to both Hume and Kant, *viz*., the denial of objective law in nature and the deduction of particular "conditions of experience," particular principles, postulates and propositions *from the subject*, from human consciousness, and not from nature. Engels was right when he said that it is not important to which of the numerous schools of materialism or idealism a particular philosopher belongs, but rather whether he takes nature, the external world, matter in motion, or spirit, reason, consciousness, etc., as primary (Lenin 1908).

According to Lenin the fundamental problem shared by Kant and Hume is that they both offer philosophical systems that are divorced from material reality. In the case of Kant, the world itself is not primary since we only have access through our facilities of perception. And, in the case of Hume, our knowledge is based upon experience of the world and not the world itself.

Lenin was correct to notice Kant's (limited) influence on Frank's view[26]. While Frank did not adopt Kant's view, he did carefully consider it and acknowledge that our observations are mediated through our understanding. Whereas this mediation was

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[26] The space left for the human subject that Lenin refers to is the idea that our mind plays some role in how we experience the world, which Frank explicitly granted in his 1907 paper.

governed by our *a priori* intuitions of space and time for Kant, Frank understood this mediation as a form of definition.

> Whereas experimental science describes the properties of bodies as given by our senses, and the changes in these properties, the task of theoretical science is to provide bodies with fictitious properties the chief purpose of which is to insure the validity of the law of causality. Theoretical science is not research but a sort of remodeling of nature; it is work of the imagination. From this it is clear whence the so-called pure – that is, a priori – science, the possibility of which led Kant to write his *Critique of Pure Reason*, derives its conviction of being right. The principles of pure science, of which the foremost is the law of causality, are certain because they are only disguised definitions (Frank 1949a, p. 57).

As previously noted by Haller (1991), Frank's main concern is with the practice of science. The passage quoted above is an analysis of competing accounts of scientific practice where Frank dismisses Kantian understandings of scientific practice because they mistakenly understand the role of theoretical science. Such analyses purport to describe reality when in fact they "only [give] directions for portraying nature" (*ibid.*).

There is no evidence that Frank would have understood his initial conventionalist reasoning about the law of causality as a commitment to some form of neo-Kantianism. Recall that in attempts to save the law of causality, one is always free to assign state variables as needed. So perhaps it is not surprising that Lenin took that freedom to imply a distinction between the mental activity of science and the reality or phenomena under investigation. But Frank's position relies on no such distinction. Instead Frank's account of the definitional character of conventions results from imperfect attempts to understand the world. Descriptions of the world do not stand apart from the world. For Kant, on the other hand, causality is a relation and is identified as one of the twelve categories. The categories are pure concepts of the understanding and concern *a priori* objects of intuition. Thus causality is an *a priori* object of intuition, and is not, for Kant, a product of the imagination (Allison 1983/2004; Kant B160-1)[27]. Frank's emphasis on imagination at the end of the quote above underscores his divergence from Kant.

The reader might have noticed that all mention of politics has been conspicuously absent from my discussion of Frank's and Lenin's arguments. However, Lenin took Frank's supposed idealism to be a direct assault on materialist philosophy, despite Frank's apolitical intentions. Frank learned an important and formative lesson from Lenin: even seemingly benign philosophical arguments can be taken up in the interest of social and political causes. At this point in Frank's intellectual development, he certainly would have known that political and religious thought relied heavily on philosophical systems. However, that science and scientific philosophy would be taken to challenge these positions surprised Frank (Holton 2014). So following Lenin's criticism, Frank was left with an acute sense of how philosophical arguments that are meant to be politically neutral are not always interpreted that way. This sparked an interest that would stay with Frank for the rest of his life. It would become a defining concern during his Harvard Period.

1.6 Frank in the Nonpublic and Public Phases of the Vienna Circle

Before moving on to the Harvard period, it is necessary to position Frank's thought during what Stadler calls the "nonpublic" and "public phases" of the Vienna Circle. These periods refer to the times between 1924-1928, and 1929-1936, respectively (Stadler 1997, p. 178). Just before this time, Frank was primarily concerned with

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[27] This is a citation from the metaphysical deduction, in which Kant argues "perception stands under the categories" (Allison 1983/2004, p. 193).

foundational work in the development of a logic of science, in addition to the work described above. At the same time, Hans Hahn returned to Vienna and worked to get Schlick appointed to the chair of the philosophy department at the University of Vienna (Stadler 1997, p. 53; Frank 1949a, p. 32)[28]. Following Schlick's appointment, while Frank was working in Prague, the Vienna Circle began meeting and developing the early forms of logical empiricism. Following this series of events, the Vienna Circle can be said to be in its nonpublic phase (Stadler 1997, p. 53 & 631). Following the establishment of the Ernst Mach Society (*Verein Ernst Mach)* in 1928 and the publication of the Manifesto of the Vienna Circle in 1929, the group moved into the so-called public phase. At this time the journal *Erkenntnis* was founded, and the Circle began its largest public outreach program – the unity of science movement. During this period, in 1931, the philosophy department in Prague hired Carnap[29], at Frank's insistence (Carnap 1963; Frank 1949a p. 45). Later, the first meeting of International Congress for the Unity of Science was held in Prague in 1934 (Frank 1949a, p. 49). This phase lasted until the mass emigration from Europe began, and the National Socialist government of Austria formally dissolved the Ernst Mach society (Stadler 1997).

It is already standard to classify Neurath as a member of the pragmatist wing of the Vienna Circle (Cartwright *et al*. 1996; Reisch 2005). The scant scholarship on Frank has begun to move in a similar direction[30]. This section will trace Frank's growing interest in pragmatism and his explicit and persistent alignment of the work of the Vienna

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[28] In stating that Hans Hahn was the first to pursue a careful study of Wittgenstein's work and that he advocated for Schlick's appointment, Frank argued that Hans Hahn was the true founder of the Vienna Circle.

[29] Carnap had been hired at the University of Vienna in 1928, at the behest of Schlick and Hahn.

[30] See especially Holton 1993, 2003; Reisch 2005, pp. 208-233; Richardson & Hardcastle 2003; Uebel 2003a, 2011.

Circle with pragmatism. The primary object of this section is to highlight Frank's particular account of pragmatism. I will do so by arguing that Abel Rey continued to influence Frank's thought and that this influence led to Frank's interest in social and political issues within the context of his philosophy of science. Frank's pragmatism is marked by its emphasis on behavior, and that emphasis eventually became the means by which Frank addressed the problems introduced by Rey.

Recall that Frank was inspired to think along broadly pragmatist lines long before even the nonpublic phase of the Vienna Circle – as early as 1908, following Einstein's criticism of his conventionalist account of causality. There is evidence that by 1928 Frank's interest in pragmatism was substantially developed and that he saw it as an important if not constitutive element of logical empiricist thought. Shortly after the publication of Carnap's *Aufbau* Frank wrote to Carnap, asserting that the *Aufbau* was rooted in American Pragmatism, as he recalls in the passage below:

> When I read this book it reminded me strongly of William James's pragmatic requirement, that the meaning of any statement is given by its "cash value," that is, by what it means as a direction for human behavior. I wrote immediately to Carnap, "What you advocate is pragmatism." This was as astonishing to him as it had been to me. We noticed that our group, which had 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, p. 33).

As Frank explains, this connection was evidence of what he perceived as a thoroughgoing intellectual relationship and an early sign that the unity of science movement would fulfill its international ambitions. In reading the *Aufbau*, Frank first saw the early results of the new work in logic by Frege, Russell, and Wittgenstein applied to the concerns of the Vienna Circle. However, as can be gleaned from the passage quoted above, the core of Frank's understanding of a pragmatic analysis required that human behavior be directed towards some end. Thus Frank interpreted "cash value," a notoriously unclear phrase, as indicating some effect on behavior. As Thomas Mormann argues, it is possible to read Frank's pragmatic description of the *Aufbau* as an attempt to align his philosophical project with that of Carnap (Mormann 2009). Mormann also acknowledges that Frank made a more subtle point: that meaning is connected to action. Mormann, however, does not duly acknowledge Rey's influence.

There is an outstanding issue presented by Rey that occupied the attention of the first Vienna Circle: The philosophy of science should account for the cognitive significance of scientific theories; there must be some account that demonstrates how scientific statements are meaningful. Consider again the crisis in philosophy that was described in section 1.3 above. Due to the failure of Mechanistic Materialism, and neo-Kantianism to a lesser extent, Rey argued that science had been reduced to a series of technically useful formulae that provide "no cognition of the phenomena to which this system or these recipes are applied" (Rey 1907, p. 16 quoted in Frank 1949a). This problem is epistemological: philosophy cannot explain why science improves our understanding of the world. While that concern poses a significant philosophical challenge, it was not the only problem described by Rey that motivated Frank and the other members of the first Vienna Circle.

Rey's influence is also what led Frank to begin to consider the social value of science. When considering the development of Frank's thought, scholars such as Mormann typically emphasize Frank's pragmatism as the means by which he developed his interests in the social and political elements of science. On the contrary, I argue that Rey's influence was a more significant factor in Frank's developing interest in pragmatism and social concerns. This is because Frank regarded the epistemological problem presented by Rey as deeply connected to a social problem:

> If these sciences, which historically have played an emancipatory role, were tarnished by a crisis that leaves them no other value than that of recipes which are technically useful and that deprives them of any significance in the cognition of nature, a complete upset in the art of logic and history of ideas must result. Physics loses all of its educational value; the positive spirit which it represents is a misleading and dangerous spirit. Reason, rational method, and experimental method must be considered in good faith as having no cognitive value. All these methods are, then, procedures of action, not means of cognition. They can be developed in order to obtain certain practical results, but we must be well aware that they have no value except in their restricted domain. The cognition of the real must be sought or given by other means. We must guard against the dangerous illusion of rationalism and scientism (Rey 1907, p. 18 quoted in Frank 1949a, p. 5).

So, the epistemic problem described by Rey also undermines the traditional emancipatory role of science. This is because if science is only instrumentally useful, then it cannot be said to add to our understanding. And if it cannot add to our understanding, then science cannot justifiably be used to help change the world along some ethical or socially conscious lines. Frank and Rey's shared interest in preserving the educational value of science also indicates Frank's continued interest in emancipatory science.

Despite the many social issues associated with science during his lifetime (e.g., the creation of the atom bomb, un-ethical human experimentation, etc.), Frank focused heavily on the educational value of science, as did Rey[31]. And, furthermore, the educational value of science is not restricted to the methods of science, but to something akin to a marriage of science and the humanities:

> There is a widespread belief that the rising contempt for tolerance and peace is somehow related to the rising influence of scientific thought and the declining influence of ethics, religion and art as guides of human actions. This

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[31] Frank's account of education is discussed in detail in Chapter 2.

> contention is, of course, debatable. There is hardly a doubt that the causes of war can be traced back quite frequently to religious or quasi-religious creeds and very rarely to the doctrines of science. The humanities, including religion and ethics, have been for centuries the basis of education and the result has been, conservatively speaking, no decline in the ferocity of men. The scientists have never had a chance to shape the minds of several generations. Therefore, it would be more just to attribute the failure of our institutions to educate a peace-loving generation to the failure of ethical and religious leaders than to impute the responsibility to the scientists….
>
> …According to my opinion, this [bridging the gap between science and the humanities] can be done only by starting from *the human values which are intrinsic in science itself*. The instruction in science must emphasize these values and convince the science students that *interest in humanities is the natural result of a thorough interest in science* (Frank 1946/1949, pp. 260-261, italics original).

In the quote above, Frank is clearly responding to the then commonplace belief that science harms (or, perhaps, does not benefit) the causes of tolerance and peace. But, in addition to that point, Frank also highlights the importance of the values within science by emphasizing that scientific values in particular can play some positive role in the development of better social conditions.

To understand why Frank thought it valuable to support "an unprejudiced outlook on life" (Frank 1949a, p. 37) in the service of emancipatory philosophy, it is helpful to attend to "school philosophy". Frank's longstanding concern with so-called "school philosophy" is motivated by his pragmatic analysis of scientific theories. Richard von Mises, longtime friend and collaborator of Frank, summarizes Frank's account of school philosophy as: "an abbreviation for the entirety of doctrines usually taught in the philosophy courses of, for instance, a contemporary university…these theories are in great part in mutual contradiction, much more so than the valid results of positive science" (von Mises 1956, p. 27). However, school philosophy was not just philosophically problematic. It was understood by the Viennese academic establishment to support fascist or totalitarian doctrines:

> Despite what several valuable historical-philosophical descriptions may appear to imply, the empiricist, anti-metaphysical and language oriented 'typical Austrian philosophy' did not dominate the philosophical life of the university of Vienna. For at the same time, adherents of such different currents as e.g., German idealism (in particular, neo-Kantianism, Herbartianism), of natural law scholasticism, of Christian world-view philosophy (*Weltanschauungsphiolsophie*), of neo-romantic Universalism, were active there [at the University of Vienna]. Nearly all of these currents viewed philosophy as the speculative 'queen of the sciences.'...It is a fact in the sociology of science that all of these currents which moved outside the field of neo-positivism...were academically constituted as the traditional metaphysical school philosophy – as Edgar Zilsel noted at the time. The social function of this scholastic philosophy, deeply anchored in the bourgeoisie, and endlessly creative of new disciplines, was to stabilize the established system and legitimize authority... (Stadler 1995, p. 44).

Insofar as school philosophy sought to buttress the established powers in Austria, academics who explicitly challenged the fundamental philosophical assumptions that were common to all forms of school philosophy were viewed as not just anti-metaphysical, but also unpatriotic, anti-establishment and ultimately dangerous (Stadler 1995).

As a result of the social context in which Frank worked, it would have been natural to connect school philosophy with the actions that were predicated upon it. So it is unsurprising that throughout Frank's work, he is concerned with philosophical systems that "seek to direct human conduct." On Frank's account, this happens when meaningless, unscientific content is transformed into ethical commands:

> Of greater scientific interest is the logical structure of these philosophic misinterpretations. The process of thought leading to them consists of two steps. First, physical propositions that are really statements about observable processes are regarded as statements about a real, metaphysical world. Such statements are meaningless from the standpoint of science, since they can be neither confirmed nor contradicted by any observation. The first step is therefore the transition to a meaningless metaphysical proposition. In the second step this proposition, by means of a rather small change in wording, goes over into a proposition which again has a meaning, but is no longer in the realm of physics; it now expresses a

> wish that people should behave in a certain way. This proposition is then no longer metaphysical, but has become a principle of morality, of ethics, or of some other system of conduct (Frank 1936/1949, pp. 160-161).

School philosophy is the means by which meaningless, unscientific content can be regarded as real, and can therefore serve to direct behavior (Frank 1936/1949, p. 163). In this light, Frank's behavioral reading of James' pragmatism is not just an idiosyncratic aspect of Frank's philosophy, but is rather an attempt to incorporate social concerns into his philosophy of science. Pragmatism became the means by which Frank could solve Rey's problem because it provided Frank with conceptual tools that allowed him to understand school philosophy in terms of the behaviors with which it is associated.

Rey's concern, as described by Frank, is that science might be regarded as merely technically useful and thus unrelated to why particular statements are meaningful. Recall, if "physics loses all of its educational value" then the spirit of science can become "a misleading and dangerous spirit" (Rey 1907, p. 18 quoted in Frank 1949a, p. 5). School philosophy is one example of a "misleading and dangerous spirit" because it relies on misinterpretations of science in order to direct behavior. In order to address the dangers of school philosophy, we will have to understand it. To do this philosophers of science (if they wish to challenge school philosophy) must seriously consider science as a form of human conduct where that conduct is usually directed towards some goal. Here is where Frank's pragmatism becomes especially important[32]. Frank describes pragmatism as the

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[32] Furthermore, given Frank's behavioral account of pragmatism, it is not surprising that he saw the attack on school philosophy as a common goal for both logical empiricists and American Pragmatists: "A direct attack against the truth concept of the school philosophy was made by the American psychologist William James in his book *Pragmatism....* According to James, the truth of a system of principles – a physical theory, for instance – does not consist in its being a faithful copy of reality, but rather in its allowing us to change our experience in accordance with our wishes. According to this view, which essentially agrees

"final criterion for the decision of general problems" for it guides our judgment of the "adequacy of a solution within the framework of science as a human activity" (Frank 1956a, p. 18). Thus, on Frank's account, we may use pragmatism to better understand the social goals of school philosophers and we can then better understand how their social goals are served by their misinterpretations of science.

The social and political tensions that characterized life in Vienna during the public phase of the Vienna Circle and the subsequent exodus of Vienna Circle members led Frank to appreciate that metaphysical claims can have a direct effect on politics, and hence people's lives and actions. So, as a result of Frank's interest in the emancipatory component of Rey's challenge, he became interested in how pragmatism might be used in order to address social concerns. But to help make sense of this turn in Frank's thought, we must also understand the social and historical contexts in which he was working.

1.7 Frank in Vienna and Prague

During the non-public and public phases of the Vienna Circle, the political situation in Vienna was tense and marked by large-scale conflicts and social change. In the early interwar period, Vienna was the center of many reformist movements. Many of these were organized under the larger organization called "The Free Union of Cultural Associations" (Stadler 1997, p. 180). While the Vienna Circle and Ernst Mach Society

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with that of Mach, but rejects even more bluntly the concept of school philosophy, every solution of a problem is the construction of a procedure that can be of use to us in in the ordering and mastering of our experiences" (Frank 1930/1949, p. 101). The origin of Frank's use of the term "school philosophy" and perhaps also Frank's concern with it comes from Henri Bergson in his French introduction to James' *Pragmatism* (Frank 1930/1949, p. 101).

were more scientifically inclined and competent than the other groups, they fit in well with these larger cultural movements:

> What all the late Enlightenment currents had in common was a basic humanitarian-cosmopolitan perspective, an uncompromising orientation toward progress and reason, and the advocacy of social and cultural reform. In addition, the groups involved worked, both theoretically and in practice, on forming an anti-metaphysical worldview and shaping an ethical foundation for everyday life through non-revolutionary strategies that corresponded well to an ethos fundamentally radical-bourgeois in nature (Stadler 1997, p. 180).

Many members of the Vienna Circle participated in activities that fell under the purview of the Free Union. Frank was in Prague during much of this time but did participate in some components of the socialist-cultural movements: the Vienna Movement for Adult Education (1918-1934) and the "*Austromarxists*" group (Stadler 1997, p. 585).

The general political orientation of the Vienna Circle through its association with the Free Union is described by Stadler (*ibid.*) as having three essential components:

1. Human beings may determine their own lives;
2. Humanity has the capacity for determining their own circumstances, and;
3. Every liberating action presumes knowledge of the world that is best obtained with the scientific methods (Stadler *ibid.*).

Even though there was popular support for the work of the Vienna Circle and for the groups with which its members associated, the conservative elements in Austria worked to marginalize their gains. After the initial success of Red Vienna, anti-Semitism, National Socialism, and Catholic-nationalism gained popular strength[33] (Beniston 2006; Romizi 2012). Vienna's cultural conflict came to a head in 1934 when the parliamentary organization of the Social Democrats violently resisted the ruling government, headed by

[33] See Romizi 2013, Fleck 1996, and Uebel 2003b. Helmut Grubner's *Red Vienna* is noteworthy for its detail, and Eva Blau has a series of publications describing Neurath's and Joseph Frank's work during this time, see her 1999 book for a detailed discussion.

Dollfuss. Following the defeat of the Social Democrats, their organizations as well as those associated with communist organizations were banned (Fleck 1996). This included the Ernst Mach Society and hence the Vienna Circle.

At the same time, a political rift formed in the Vienna Circle. When the Ernst Mach Society was dissolved, Schlick wrote to officials of the Dollfuss regime twice, arguing not only that the Society was apolitical but also that it stood in solidarity with the regime (Stadler 1997, p. 347). This action was criticized by both Carnap from Prague and Neurath from Moscow (Fleck 1996; Stadler *ibid.*). There is no evidence to suggest what Frank's view was regarding this specific matter, though one assumes that his sympathies lay with Carnap and Neurath[34]. The events leading up to this point were so dramatic that Neurath did not return to Vienna but instead joined Frank in Prague (Fleck 1996, p. 83). Thus began Neurath's long exile. Just a few years after this, in 1936, Schlick was assassinated by a former student and apparent Nazi-sympathizer (Stadler 1997, p. 905). Tensions were again heightened following the murder of Schlick.[35] A member of the department of philosophy at the University of Vienna blamed Schlick for his own murder:

> ...Dr. Nelböck cannot be regarded as a born psychopath, but that he only turned into one, according to certain signs, under the radically destructive philosophy which Prof. Schlick had been teaching at the university since 1922; that is to say that the bullet was not guided by the logic of some lunatic looking for a victim, but rather by the logic of a soul deprived of its meaning of life...I know several cases myself where young students have lost all faith in God, the world, and

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[34] At that time Frank's political associations suggest a position closer to that of Carnap, Hahn, Neurath, von Mises, and Zizel than to Schlick. In particular, Schlick participated in many neoliberal activities, specifically with Miles von Mises and Friedrich Hayek. He was never active in the left-wing circles in which Frank sometimes participated.

[35] This letter was written by Dr. Austriacus, a pseudonym for Johnn Sauter, "*Privatdozent* and *extraordinary professor*" in the school of Law and Political Science at the University of Vienna.

> humanity under the influence of Schlick's philosophy (Sauter 1936 quoted from Stadler 1997, p. 871).

Sauter went on to call for the end of the Circle, citing "the challenge" it posed to the "Christian state." In this way, Schlick was not the sole target of the author's ire. In the same essay, Frank and Neurath were indicted as philosophical accomplices:

> A close associate of Schlick, Professor Frank from Prague, therefore commented quite frankly, two years ago, that the "anti-metaphysical movement" in Europe was represented mainly by Schlick; the Vienna Circle was the "combat patrol of anti-metaphysical studies"...the board comprised well-known leading freemasons as well as Otto Neurath, communist minister in Munich during the soviet period and a close friend and collaborator of Schlick[36] (Sauter 1936 quoted from Stadler 1997 p. 872).

In the same letter Sauter revealed that his sympathies were strongly aligned with Nazism: "Hopefully, the terrible murder in the Viennese university will serve to bring about a truly satisfactory solution of the Jewish question" (Stadler 1995, 1997 p. 876)**.** We must remember that the author's opinions were not unique in Vienna. In an interview, Gerald Holton emphasized the fervor that was whipped up during the the *Anschluss*:

> Vienna greeted Hitler in such a way, that even, even the Austrians as a whole...there are some amazing stories of when the soldiers came into Austria in March '38 they were told that the drivers in these open vehicles with the troops had to put goggles on because their eyes would be damaged by the flower bunches that were thrown at the soldiers as they entered (Holton 2014).

Similar sentiments began to appear in Prague. As R. Fürth, remembered in 1965, two of Frank's close colleagues were eventually taken to concentration camps. In the same remembrance Fürth recalled that Frank

> strongly resisted any attempts of Nazi sympathizers amongst staff and students at the University to apply any radical doctrines to the admission of students or the

[36] Naturally, Waismann was quick to reply, making clear that Schlick and Neurath we never close.

> appointment of staff…[By 1938, Frank], who seemed to understand better than most of us what was going on, had become very pessimistic about our future prospects (Fürth 1965 p. xvi).

The political situation that precipitated both Schlick's murder and Frank's struggles with his colleagues placed the members of the Vienna Circle and their intellectual sympathizers in a precarious position[37]. Following the rapid and tragic degradation of social and political life in continental Europe, a massive exodus of intellectuals began (Stadler 1995). By 1938 the exodus of intellectuals, Austrian Jews, and leftists of various stripes, including most members of the Vienna Circle, grew into a torrent. Viktor Kraft, who remained, was banned from his profession, and following the conclusion of the war was able to obtain only short-term positions. Further, in post-war Austria, philosophy departments were dominated by either Christian-worldview philosophy or systematic idealistic philosophy. And it is important to note – though this detail is often omitted in European histories– no one who emigrated was ever invited back[38](*ibid.*).

This history is important to remember when reading Frank's philosophical work. It is easy to read that work and upon seeing his frequent discussions of sociology, history, and pragmatism, to be somewhat surprised. These sorts of interests are not what we have

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[37] "In the year 1936…Professor Schlick was assassinated near his lecture hall in the University of Vienna by a student. At the court trial the attorney for the defendant pleaded extenuating circumstances because the student was indignant about Schlick's 'vicious philosophy.' Everyone who knew Schlick had been full of admiration for his noble, humane and restrained personality. The political implications of the expression 'vicious philosophy' were obvious. The student received a ten-year prison term. When, however, two years later, the Nazi troops occupied Vienna and arrested a great many people, Schlick's murderer was released from prison" (Frank 1949a, p. 49).

[38] Those were: Gustav Bergmann, Carnap (from Prague), Feigl, Frank (from Prague), Gödel, Felix Kaufmann, Karl Menger, Richard von Mises (from Istanbul), Marcel Natkin, Neurath, Rose Rand, Josef Schächler, Waismann, Zilsel. For obvious reasons, they might not have wanted to return. However, it is clear that the political establishment in Austria did not encourage their return.

come to expect from the logical empiricists! However, understanding this historical context is essential for understanding the work of Frank and the other logical empiricists, and is especially important if we wish to contextualize their critiques of metaphysics. Don Howard has already described the importance of this context in an impressive fashion:

> A proper appreciation of the Vienna Circle's opposition to metaphysics would place political concerns at center stage and would present the philosophical critique as but a means whereby to pursue the political critique. We forgot that Carnap, like his colleague Neurath, was a socialist. We forgot that the Austrian Social Democratic Party, which Neurath served as Director of Vienna's Social and Economic Muesum, defined itself in part by its promotion of a curious mix of Marx and Mach (yes, Ernst Mach) known as Austro-Marxism. We forget that Austro-Marxism, in particular, but also Marxism more generally had long targeted metaphysics as one of the chief intellectual tools for the defense of bourgeois class-interest, faulting specifically the non-dialectical character of metaphysical character of reifications such as "Volkgeist" (Neurath, Hahn, and Carnap 1929) (Howard, unpublished).

All of this was deeply formative for Frank, and would help to contribute to the dramatic turn he made in 1947 during the Harvard Period, when he began to examine the possible meaning of metaphysical statements (Frank 1949a, p. 51). At this time, Frank was deeply concerned with the role played by philosophical systems in the apparent legitimation of authority in central Europe. To better combat those systems, Frank decided that it was important to understand them empirically. Following Frank's immigration to the United States, his work would be deeply motivated by these final experiences in Europe. Not only would the background to these events appear in many of his writings from 1938 on, only three of his forty-plus papers and books from this period were on physics (Stadler 1997, p. 632). The rest are either philosophical, social, biographical, or organizational. The intellectual focus of his work had changed. His targets would be Thomism, Dialectical Materialism, and Nazism[39]. In response to these views, he would offer a synthesis of American Pragmatism and logical empiricism that was meant to undermine school philosophy. Eschewing technical work, Frank approached these questions from the perspective of the Scientific Conception of the World[40]. In some ways Frank was beginning to position himself as a public intellectual (Nemeth 2003), but it would also seem that he felt a social obligation to continue the work of the Vienna Circle. The Harvard Period begins with this social and intellectual turn.

1.8 Conclusion

In this chapter, I have attempted to outline the broad developments in Frank's thought, particularly those that led to Frank's social and political turn in the Harvard Period. After receiving responses from Einstein and Lenin (the pragmatic and the political, described in section 1.5), Frank slowly began to explore pragmatism and social concerns. Furthermore, Frank's developing interest in pragmatism was shaped by his time in the first Vienna Circle, and in particular, by the crisis in philosophy as described by Abel Rey. Rey's continuing influence caused Frank to seriously consider the so-called emancipatory potential for science, which in turn helped to shape Frank's behavioral account of pragmatism.

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[39] "At stake, Frank argued, was the fate of the world. Ideologies, combinations of philosophical with political creeds, underpinned both the right wing with its organismic metaphysics and the left wing with its dialectical materialism. Prominent 'cardinals of the church' espoused their Thomism (so Frank continued), while political leaders including Lenin plunged his followers into dialectical materialism. Only the student with logico-empirical analysis in one hand and socio-psychological analysis in the other could navigate these waters…" (Galison 1998, p. 49).

[40] This is the description of the Vienna Circle as laid out in the *Manifesto*, which describes the movement not only as a philosophical project but also as a social project. For a more complete discussion see Romizi 2012.

As shown in the philosophical biography above, some of the most distinctive characteristics of Frank's thought (such as his interest in an alliance between American pragmatism and his socially and politically conscious approach) were not new in the Harvard Period and had been evolving since 1907, at least. Frank's colleagues in the first Vienna Circle shared his broader orientation, which thrived even within parts of the Vienna Circle proper during the public phase. Frank was not alone in his turn to pragmatism and sociology, for at least Neurath shared these interests.

As we can tell from Frank's discussions of Rey and his emphasis on elements of pragmatism in Carnap's *Aufbau*, Frank's vision of logical empiricism was always markedly socially engaged. That is, the central questions motivating Frank's logical empiricist analysis focused on the progressive potential of logical empiricism. This may also explain why, when Frank chose to re-publish some of his early papers, he did not select the technical work that Einstein had praised in his recommendation letter. Instead, Frank chose papers that focused on the historical legacy of logical empiricism (such as his readings of Mach as an enlightenment philosopher),[41] his papers explaining the historical role of school philosophy,[42] and papers on misinterpretations of scientific theories[43]. Further, eight of the papers anthologized in *Modern Science and its Philosophy* (1949a) were previously anthologized in *Between Physics and Philosophy* (1941)[44], which suggests continuing attention by Frank to these topics rather than a

[41] See Frank 1917/1949 and 1938/1949.
[42] See Frank 1930/1949, 1934/1949, and 1939/1949.
[43] See Frank 1936/1949, 1941/1949, 1944/1949, and 1947b/1949.
[44] While the collections of essays in these books are nearly identical, the introductions do vary in important ways. The main difference is summarized by Uebel as follows: "My concern here has been to render explicit and plausible what I take to be the agenda enlivening Frank's histories [given in the introductions]: Behind the remarkable stress on the role of the first Vienna Circle—indeed, its elevation to the status of

temporary emphasis on social issues. In the next chapter, I will focus on what Frank's view can contribute to a socially engaged, socially responsible philosophy of science.

founding fathers—lay the desire to reaffirm the pragmatic-historical approach of this early group and its existential and social engagement at a time when the pressures of the academicization of logical empiricism pointed at the opposite direction" (Uebel 2003a, p. 164).

2 Chapter Two

2.1 Introduction

Until fairly recently, the Vienna Circle was not often remembered for its social engagement. In general, socially engaged philosophers of science seek to apply knowledge from their discipline to social and political issues or problems. Usually, that knowledge has to do with the status of theories, evidence, and explanations. The "Manifesto for the Joint Caucus of Socially Engaged Philosophers and Historians of Science" (2012)[45] asserts that members' actions will apply knowledge from the history and philosophy of science to "matters of social welfare." The Joint Caucus Manifesto highlights the following discussions as particularly important to members' future work as socially engaged philosophers: "science funding, research ethics, race and gender in science, risk assessment, climate science, the status of embryos, genetically modified foods or organisms, and other scientific and technological matters involved in public policy debates." In each instance, socially engaged philosophy of science is marked by the application of the conceptual tools developed by philosophers of science (i.e., theories of evidence, confirmation, and truth) to practical problems of broad public concern. But even in this short document, the founders of the Joint Caucus looked back to the Manifesto of the Vienna Circle for inspiration, pointing to the "progressive" vision of science described in that earlier Manifesto.

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[45] The Joint Caucus is affiliated with both the Philosophy of Science Association and the History of Science Society. This organization was founded in 2012 in order to encourage philosophers of science to use their expertise to address issues of social welfare, broadly construed.

Contemporary scholars working in the history of philosophy of science have argued that the Manifesto of the Vienna Circle not only represents a vision of progressive science, but that it also constitutes a political philosophy of science. The most notable example of this is Thomas Uebel's Left Vienna Circle thesis. Uebel argues that the philosophies of Carnap, Frank, Neurath, and Hahn were motived by their leftist politics (Uebel 2005, p. 756). This thesis is not just another way of identifying the left wing of the Vienna Circle, for Uebel also claims that the politics of each of these members somehow contributed to their philosophical work:

> Importantly, the term 'left (wing of the) Vienna Circle' was (and is) prominently, but not primarily, suggested by their shared political beliefs, but was (and is) also meant to indicate their shared philosophical doctrines and approaches to philosophical problems as evidenced by the discussions of the Circle. Against the background of agreement on the basic tenets of neo-positivism, it was members of the left wing who in the Circle argued for the rejection of Wittgenstein's strictures on meaningfulness and against metalinguistic discourse and it was also these members who came to reject all foundationalist ploys in philosophy (Uebel 2005, p. 756).

Uebel does acknowledge that the specific views of individual members varied and that at times the differences were significant. However, Uebel's 2005 version of the Left Vienna Circle thesis requires that the reader accept the following claims: that non-cognitivism did not prevent the Left Vienna Circle's political engagement, that members' attitude towards philosophy and science made philosophy a tool for clear thinking (as opposed to a tool for the justification of reactionary politics), and that these beliefs were deeply enmeshed in the leftist politics of Red Vienna (Uebel 2005, p. 758).

This view is not without problems, however. The most vigorous challenge comes from Sarah Richardson (2009a/b), who argues that Uebel's appeal to the progressive politics of the members of the Vienna Circle is not sufficient to establish that their philosophy of science was political. Furthermore, she contends that the philosophy of the Vienna Circle lacks a robust conception of science and values, which at best results in an inordinately weak account of the relationship between science and politics. And, if there are no resources to address social and political issues—a necessity for a political philosophy of science—within the philosophical system of the Vienna Circle, then we cannot argue that the Manifesto of the Vienna Circle should inform contemporary political philosophy of science.

In Uebel's (2010) response to Richardson, he again appeals to the history of Red Vienna to reinforce his argument, and is careful to highlight errors and over-simplifications in Richardson's account. He then concludes by suggesting what Richardson should have asked: "...whether the [Left Vienna Circle's] political philosophy of science is 'political enough' for contemporary purposes – remains an interesting one. In closing I simply note that this question at least in part turns on whether non-cognitivism about values can be sustained or not" (Uebel 2010, p. 219). Without question, Uebel's first remark is vague; it is not at all obvious what constitutes a political philosophy of science. Does political philosophy of science serve the aims of a political party? Or, does it contain its own political theory? Uebel suggests the latter, insofar as he contends that any evaluation of the Left Vienna Circle thesis requires coming to terms with whether or not the ethical non-cognitivism of the Left Vienna Circle could co-exist with some form of political philosophy of science. But that clarification does not provide an answer to the larger question: Did the Left Vienna Circle offer a political philosophy of science? And if so, what was it and will it be of any use to contemporary philosophers of science?

While thought-provoking, Uebel's suggested course of action – attending to meta-ethical issues within political philosophy – may not be helpful. First of all, even though meta-ethicists are still debating ethical non-cognitivism, it is unlikely that they will come to a clear consensus on the issue that will be of value for philosophers of science. Furthermore, the topics that philosophers of science are well trained in are not, usually, co-extensive with those that concern political philosophers. And lastly, not all members of the Vienna Circle, including the Left Vienna Circle, agreed on the particular form (if any) that ethical non-cognitivism should take within their philosophy of science. But, with respect to these issues, Philipp Frank is an interesting figure. On the one hand, he often wrote about political topics. And, on the other, he did not develop a political philosophy of science, if we assume that doing so requires arguing for a clear position within political philosophy.

So instead of following Uebel's prescribed course of action, I suggest that we turn towards the set of questions introduced by the Joint Caucus: How might philosophers of science contribute to social and political issues affecting science such as, policy debates, questions relating to the ethics of research, and issues relating to the funding of or reporting on scientific research? In other words, we should address the social and political issues that emerge within the context of scientific theorizing and practice. Not only do these issues avoid (likely) irresolvable meta-ethical debates, but they also depend on issues that philosophers of science can helpfully discuss. For example, philosophers of science often wade into debates about the quality of evidence and risk assessment, specifically in cases that affect the public, such as climate change. Frank's philosophy of science does indeed provide one early example of socially engaged philosophy of science, and it avoids the problems currently plaguing Uebel's account.

Given that socially engaged philosophy of science is likely to provide a more fruitful avenue of investigation, I will position Frank's work within the context of this emerging field of interest. Frank's philosophy of science did not include political theory, even though he often contextualized his work by referencing political topics. To demonstrate the value of understanding Frank's contributions as socially engaged rather than political, we will more closely examine the Left Vienna Circle debates, where I will highlight the varied ways in which each interlocutor defines "political philosophy of science." These definitional differences result in a muddled and unhelpful discussion. A careful study of Frank's contributions suggests a more interesting question and one that is in line with socially engaged philosophy of science: How do we become informed political actors?

Frank's answer to this question focuses on education. By avoiding topics in traditional philosophy of education, Frank's educational proposals offer a helpful way of understanding the complex issues that arise in popular discussions of scientific theories. Insofar as socially engaged philosophy of science must apply some theory in the philosophy of science to a real-world problem, Frank's interest in education proves to be an early example of socially engaged philosophy of science. At Harvard Frank contributed to James B. Conant's education proposals by envisioning them as a foundation that would enable students to become citizens and to more fully participate in democracies. Conant was the driving force behind Harvard's General Education Program, which was designed to introduce students to topics of study beyond their chosen scholastic field. Furthermore, these educational proposals serve to enrich Phillip

Kitcher's intriguing account of what he calls tutoring in well-ordered science, as we shall see. And even though Frank does not develop anything like a political philosophy, he nevertheless succeeds in including values within his scientific theorizing. I conclude that Frank's particular proposals help to preserve the possibility of scientific communication and suggest how ideologically motivated rejections of scientific theories can be addressed in the classroom.

2.2 The Left Vienna Circle Thesis

As we saw in Chapter 1, Frank was keen to apply his philosophy to the world and eventually came to believe that it was essential to do so. Frank's time at Harvard marked an important shift in his work as he increasingly advocated for greater attention to social and political topics. In order to understand the relevance of Frank's philosophy with respect to socially engaged philosophy of science, it is useful to attend to some current discussions of the Left Vienna Circle (hereafter LVC). I will survey the recent debate between Sarah Richardson and Thomas Uebel and will also explore the implications of Donata Romizi's contribution to the literature. Throughout this discussion we shall see that each discussant employs a different sense of "political" and also that the politics of the Vienna Circle were firmly engrained within Viennese life. While each argument offers important insights, I ultimately conclude that "social" (as opposed to "political") engagement is a more useful and less problematic way of understanding the nature of Frank's contributions.

The label "Left Vienna Circle" (LVC) was originally coined by Thomas Uebel (2005) but even before Uebel coined this term historians of the philosophy of science were already working to recapture and analyze the political engagement of the Vienna

Circle[46]. LVC scholars are those who not only emphasize the historical and political contexts of the Vienna Circle but also discuss how the philosophy of the LVC might influence present-day work situated at the intersection of science and values. According to Richardson (2009a, p. 14), LVC scholars include Thomas Uebel, Alan Richardson, George Reisch, Ron Giere, and Don Howard. To that list I would add Donata Romizi. Four years after Uebel's introduction of the LVC thesis, Sarah Richardson[47] argued against it by specifically citing the role played by ethical non-cognitivism and value-neutrality (or, the idea that the claims of science should be neutral with respect to questions of ethics or values) in logical empiricism (Uebel 2005; Richardson 2009a/b). In essence, Richardson's criticisms force us to ask the following question: Did the members of the LVC offer a political philosophy of science, or were they simply philosophers of science who were also political?

The gist of Richardson's argument is as follows. The philosophy of the LVC must include a political thesis for it to be regarded as a proper form of political philosophy of science. But no such thesis was embraced by the LVC because the members endorsed some form of value-neutrality[48] in science or they held to the thesis of ethical non-cognitivism, which would seem to preclude the development of a powerful political

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[46] See especially Howard (2003), Alan Richardson (2002, 2003, 2004), Richardson and Hardcastle (2003), and Uebel (1998, 2003, and 2005).

[47] Throughout this section I discuss Sarah Richardson's work and not that of Alan Richardson.

[48] Uebel helpfully distinguishes between value-neutrality and value freedom. The former "asserts the scientist's strict neutrality vis-à-vis political matters" and the latter "asserts the illegitimacy of unconditioned value statements in science" (Uebel 2010, p. 219). Contra Richardson, Uebel argues that the value freedom internal to Neurath's economic proposals does not imply that Neurath's proposals were value-neutral, for they were indeed informed by his Marxism. Notwithstanding this important point, I will continue to use Richardson's language of neutralism for the sake of clarity. The issue of value freedom will be addressed in Chapter 5.

philosophy of science (Richardson 2009a, p. 16). As a result, recent scholarship on the LVC "stops short of demonstrating that the LVC's logical empiricism, invited, encouraged, or provided the tools for the social and political dimensions of science" (Richardson 2009a, p. 15). Richardson then goes on to show how both Carnap and Neurath fell short of these standards in different ways. She argues that Carnap's "politics were personally important, but they were peripheral to his philosophy" (Richardson 2009a, p. 23) and that Neurath "balked at explicitly theorizing his politics" (Richardson 2009a, p. 24).

Richardson's second paper (2009b) makes a meta-philosophical point: when contemporary scholars suggest that the LVC should inspire our current attempts to develop a political philosophy of science, they ignore the fact that feminist philosophy of science has already done so. Furthermore, they do not even countenance the idea that feminist philosophy of science might serve as inspiration for our renewed interest in political questions. So, turning her attention to the role played by the LVC in disciplinary histories, Richardson goes on to argue that the scholars who advance the LVC thesis "mobilize disciplinary history to position themselves as arbitrators of the properly philosophical and the properly analytical" (Richardson 2009b, p. 170). This is because they argue that the LVC prescribes "the form of political engagement most suited to philosophy of science" and that the prescribed model is "framed in strong disciplinary terms" (*ibid.*).

On Richardson's reading, the LVC literature serves only to reinforce a tradition that systematically excludes approaches to the philosophy of science that do not conform to either the prescribed form of political engagement or to the established disciplinary boundaries. As a result, newer philosophical approaches, including feminist philosophy of science, are overlooked and, worse, made subordinate to the purportedly "more analytic" and "more scientific" approach of the LVC.

> Thus the assertions that ties to early logical empiricism will ground a more "middle ground," "properly philosophical," "sophisticated," or "clear," political philosophy of science than developed so far. As a result, and perhaps unwittingly, LVC scholars' new disciplinary narrative does not advance a vision of philosophy of science that invites and engages insights of feminist philosophy of science but reiterates and reinscribes the view of feminist work as marginal, illegible, and unrigorous held by much of mainstream philosophy of science (Richardson 2009b, p. 172).

Indicating how the LVC literature might be developed, Richardson argues that LVC scholars will need to engage in a serious discussion of "oppression, society, and ideology," and how "emancipatory knowledge projects in the academy" are structured (*ibid*.). And, in so doing, they must also attend to those specific issues in the contexts of "ethics, epistemology, and methodology" (*ibid.*) – topics that were largely out of bounds to most of the LVC.

In response to Richardson's argument, Uebel (2010) argued that Richardson missed the point of his original (2005) LVC thesis. However, Uebel addresses only the arguments that appear in the first of Richardson's two essays. He challenges Richardson by criticizing her use of secondary sources (2010, p. 215) and argues for the importance of the context of Red Vienna to an evaluation of the LVC. Leaning on that background, Uebel then questions what we might mean by "political philosophy of science."

> For Richardson, this value minimalism [of the LVC] is incompatible with political philosophies of science generally and therefore [incompatible with the LVC political philosophy of science]. "We may acknowledge the political resonance of their neutralism" – her term for the LVC abstinence from unconditional value statements, presumably – "while still seeing that their philosophy of science itself simply does not offer critical conceptual tools for, nor prescribe, the philosophical treatment of the question of social values in science" ([Richardson] 2009a, p. 23).

> Now readers are not given an explanation of what a philosophical treatment of these questions involves, but one may not go too far wrong to suspect it to encompass assertions of unconditional value statements and normative prescriptions based thereon (Uebel 2010, p. 216).

In other words, according to Uebel, Richardson wants a political philosophy of science to include some forms of strong value statements (strong enough to address the intersectional issues of power and oppression head-on). However, Uebel is not quite right when he says that Richardson's arguments require "assertions of unconditional value statements." In her second argument she advances a pluralistic position that recommends that LVC scholars more carefully consider other forms of political philosophy of science. For example, on Richardson's argument, it would be perfectly acceptable to take up postmodern forms of feminist analysis that eschew singular and simple normative positions.

Richardson's argument against value-neutrality is more restricted than Uebel suggests, because we need only to adopt some account of the role of values in science and acknowledge that others have successfully done so already[49]. But even if we acknowledge this weaker form of Richardson's position, it seems that neither Uebel nor the members of the LVC offer such a position. Indeed, Uebel is careful to point to his much more restricted claim from 2005 that the LVC only "recognized the influence of extratheoretical values within science and allowed for their pursuit by engaged scientists" (Uebel 2005, p. 760; 2010, p. 217). As a result, Uebel has shown us that his standard for "political enough" is significantly lower than Richardson's.

Like Uebel, Romizi primarily addresses the historical part of Richardson's paper.

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[49] Sections 3 and 4 of Richardson 2009b explicitly address this point.

But, unlike Uebel, she raises the standards for political involvement. From the outset, Romizi clarifies the question that her paper will answer: "whether we can ascribe to the Vienna Circle a 'politically engaged philosophy of science'?" (Romizi 2012, p. 2)[50]. It is important to note a shift in Romizi's argument. She does not argue simply that the LVC advanced a politically engaged philosophy of science, but rather that the Vienna Circle as a whole did. In brief, her point is that in the context of Red Vienna (under the umbrella of the Freethinkers, and in response to the Christian Socialists and German-Nationalists), the Viennese liberals allied with radical leftists. As a result, the Vienna Circle itself was unified against conservative elements in Vienna. Within this alliance the Vienna Circle, as the Ernst Mach Society, operated as a "unitary," or unified, political actor (Romizi 2012, p. 19)[51]. Given that the scope of Romizi's argument includes the LVC, we may move from a discussion of one "circle" to the other without loss, despite the change of context.

Seemingly cognizant of Richardson's demand for a robust ethical theory, Romizi states that we cannot measure the political engagement of the Vienna Circle accurately if we apply only contemporary standards of political theorizing and engagement. Romizi

[50] "Politically engaged philosophy of science" and "political philosophy of science" are slightly different terms, and both are distinct from socially engaged philosophy of science. As I argue below, the use of these distinct terms caused each interlocutor to talk past each other.

[51] The political actions taken by members of the Vienna Circle were often responses to politically charged "defamations" of science (Romizi 2012, p. 21). So actions such as teaching science, publishing about science, emphasizing the social value of science above that of, say, theology would have all had explicitly political overtones for members of the Vienna Circle. Indeed, as Romizi describes, "the Ernst Mach Society …was in fact one of the societies founded on the initiative of the Freethinkers," which was a well-known political organization that included members from both the liberal bourgeoisie and from the social-democrats (Romizi 2012, p. 26). It was through this strong association that the members of the Vienna Circle were the most politically active for they advocated for equal treatment of members of all religious faiths, non-denominational education, the possibility of divorce for separated Catholics, reform of the criminal law, full equality for women, and for the right of women to access abortion (Romizi 2012, pp. 27-28).

defines the contemporary meaning of a politically engaged philosophy of science as "a value laden, normative, critical analysis of science" (2012, p. 3). On Romizi's account, the political engagement of the Vienna Circle is not to be found in its philosophy of science *per se*, but rather in the attitude taken by members of the Vienna Circle towards science. In the contexts of post-World War I Austria and later of Red Vienna, the idea of a society governed by policies that originated from empirical study, as opposed to theology, was very controversial. So, the political engagement of the Vienna Circle is exemplified by their advocacy and use of the scientific attitude, or the scientific world conception (this is described below). This attitude guided the philosophical praxis (or the interrelationship of theoretical and practical knowledge that guides actions) of the members of the Vienna Circle. Thus, Romizi's argument requires us to conceive of the philosophy of science as a set of theoretical beliefs and also as a set of actions based on those philosophical beliefs. In this way, the political aspect of the Vienna Circle is to be found in the theoretically informed actions of its members (Romizi 2012, p. 6).

The Scientific World Conception (SWC) guides the theoretically informed actions of the Vienna Circle, on Romizi's account. She argues that the SWC captures the "basic attitude" of the members of the Vienna Circle, and it should not be understood simply as a theoretical position. Indeed, the Manifesto itself seems to highlight the attitudinal aspect of the Vienna Circle: "the scientific world conception is characterized not so much by a thesis of its own, but by its basic attitude, its points of view and direction of research" (Carnap 1929, p. 305-6, quoted from Romizi 2012, p. 13)[52]. She then defines four basic

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[52] Presumably aware of the fact that Schlick did not approve the Manifesto, Romizi is careful to point out particular ways in which Schlick nevertheless endorsed the SWC (*ibid*).

tenets of the SWC:

i. The deep appreciation of science and the attribution of a "distinguished" epistemological status to scientific knowledge;
ii. The requirement of conceptual and linguistic clarity;
iii. The exclusive reliance on empirical evidence and on logically sound arguments as means for acquiring or recognizing genuine knowledge;
iv. Antimetaphysics, as the wish to avoid vague concepts and uncontrolled statements (Romizi 2012, p. 12).

As the so-called basic attitude of the Vienna Circle, the SWC is not argued for but is used as a basis from which arguments can begin. In the above formulation it is not obvious that the SWC requires us to do anything with our theoretical (i.e., philosophical) beliefs. So, in this version of the SWC the active aspect of the philosophy of science is not made explicit. If the philosophy of science is to serve as a guide to praxis for members of the Vienna Circle in their political actions, then the SWC has to be understood as recommending those actions. So, Romizi reworks the SWC to reflect the active component of the philosophy of science:

i*. Do attach value to scientific knowledge; rely on it!
ii*. Whatever you want to state, state it as clearly as possible! Make yourself intelligible to others.
iii*. Do believe in a statement or in a theory only on the basis of empirical evidence or logically sound arguments! Do not make/accept ontological commitments that cannot directly or indirectly rely on empirical evidence!
iv*. Do not believe statements or theories that escape intersubjective control! Do not believe in any alleged extrascientific knowledge! (Romizi 2012, pp. 14-15).

Using her modification of the SWC, Romizi's argument proceeds by showing how the SWC was used to guide the political actions of the Vienna Circle (as previously described in my discussion of the public phase of the Vienna Circle in Chapter 1).

Romizi justifies her normative account of the SWC by appealing to the history of Vienna, showing how members of the Vienna Circle used the SWC to navigate a political atmosphere that was defined by party politics[53]. Clearly tenets i*-iv* informed the philosophy of the Vienna Circle. But, what is less clear is how these tenets informed their political praxis. As Romizi (2012) explains in great detail, the politics of Vienna during the time of the Vienna Circle was defined by debates about the nature of knowledge (e.g., does knowledge come from God or from experience?) that in turn permeated discussions of education, religious tolerance, law, and social policy. With regard to these issues, Vienna Circle members aligned themselves with the Freethinkers intellectually and, guided by the SWC, actively participated in social reforms in Red Vienna (Romizi, p. 27-28). The political engagement of the Vienna Circle, on Romizi's account, did not result in a robust ethical theory of the kind sought by Richardson. However, it did result in a historical-contextual form of politically engaged philosophy of science.

Despite Romizi's powerful arguments in favor of this historical-contextual account, her argument does not share Richardson's standard for political engagement. This is because i*-iv* are essentially attitudes towards epistemological questions. Even when we grant Romizi's normative reading of the SWC, her normative epistemological account does not seem to support any kind of normative ethical or political view. Subsequently, it is perfectly possible to hold the epistemological tenets i*-iv* without also holding the particular ethical or political views that members of the Vienna Circle

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[53] As described in Chapter 1, Viennese politics was dominated by political parties that were defined by their preferred social polices and also by their worldviews (this is the *Weltanschauugsparteien*). With regard to this political tendency, Richardson was concerned about the highly circumscribed nature of personal choice under the Social Democrats in Red Vienna (2009a, p. 15). But, as Romizi argues, the party-political character of early twentieth-century Viennese politics was well established at least as early as the 1907, during the first Vienna Circle (Romizi 2012, p. 21). As a result, it is a mistake to see the social democratic policies in Vienna as a precursor to the left-totalitarian policies that often characterize other communist governments.

held. The outreach done by members of the Vienna Circle does seem to constitute a genuine diffusion of the attitude of the SWC in a political context. However, if we abstract the SWC from the context of Red Vienna, it becomes unclear how we might apply it to social and political issues, and what sort of reforms we should strive to achieve, because nowhere in the SWC are such reforms outlined. In other words, Romizi's normative reading of the SWC does not result in a political philosophy of science that can be adapted to our contemporary needs.

In arguing against Richardson's claims, both Uebel and Romizi take a historical approach. That is, these counter-arguments require us to interpret the work of the Vienna Circle within the context of Red Vienna. These historical arguments are important because the history of Austria is often overlooked in North American educations. And given that most LVC scholars and Richardson are from North America, stressing how Austrian history shaped the Vienna Circle constitutes an important contribution. So, by highlighting what we are likely to overlook, both Uebel and Romizi help us to contextualize and hence to better understand the political realities within which the work of the Vienna Circle and the LVC evolved. However, simply arguing against Richardson's account of history risks missing her point; for at her most intellectually compelling, Richardson emphasizes the scope of feminist thought as well as the social and political achievements of feminism:

> Over the last several decades, feminist philosophers have allied with scientists, historians and sociologists of science, and activists to bring the tools of philosophy to bear on understanding – and changing – the complex and troubled relationship between science and gender (Richardson 2009b, p. 172).

Her point is that feminist philosophers did not just seek to understand the world, but they also changed it. And they were able to do that by forming intellectual and real-world alliances. As Romizi's paper carefully illustrates, collaborative work of this sort was done by the Vienna Circle in Vienna. But, we must also acknowledge the deeply contextual nature of Romizi's arguments because what was political in Red Vienna may not be political today, as Richardson suggests.

In making her arguments, Richardson highlights an important tension regarding how we ought to conceive of practicing the philosophy of science (its praxis, so to speak). Richardson is correct when she points out that feminist philosophy has been and continues to be overlooked as a form of philosophical praxis. We should acknowledge the contributions that feminists have made and should also be careful not to overemphasize the value of recapturing the politically engaged legacy of the Vienna Circle or LVC. Implicitly or explicitly assuming that the Vienna Circle or LVC has a privileged claim on political philosophy of science risks minimizing – if not outright neglecting – the achievements of other traditions that may have been excluded as insufficiently "scientific" within some interpretations of logical empiricism, particularly in its North American form.

Neither Uebel (2010) nor Romizi (2012) addresses the second half of Richardson's argument. It will therefore be of value to respond to some of the issues she raises. First, there is no reason to think that the projects of feminism and of LVC scholarship are mutually opposed, so long as we are careful not to minimize or marginalize any one intellectual tradition. Indeed, on one reading of Richardson's paper, she encourages historians of philosophy of science to use some of the work done by feminists in order to better achieve their political ambitions. Further, LVC scholarship uncovers a disciplinary tendency to marginalize projects that are "too concerned" with extrascientific factors (e.g., values, politics, gender, etc.). Such instances of what is now called "gate-keeping" behavior seem to be part of an implicitly conservative tendency in the philosophy of science, which feminists have correctly pointed out for decades. Rather than minimize the very real problems encountered by feminists, LVC scholarship could support its claims by highlighting yet another form of gate-keeping behavior and by calling attention to what has been lost from our shared tradition as a result.

But if we turn our attention away from the disciplinary concerns discussed by Richardson in the second half of her argument, we should notice another equally important issue: What does it mean to be a political philosopher of science in the present day? This is a related worry—about how we should define political or socially engaged philosophy of science—that is not explicitly addressed by Richardson, Uebel, or Romizi, even though some notion of "political philosophy of science" or "politically engaged philosophy of science" plays a significant role in each argument. This point is of central importance here because, for Richardson's argument to succeed, there needs to be a shared sense of "political enough." Uebel (2010) intentionally leaves this question unresolved when he suggests that the controversy turns on our understanding of ethical non-cognitivism (discussed in 2.1 above). Since nothing further is said to define what should count as "political" within the context of this discussion, Richardson and Uebel seem to be talking past one another. Furthermore, the question of what constitutes "political enough" is not simple. We want the right kind of politics (liberatory and anti-oppressive, as Richardson specified) and the right kind of integration of values into scientific methodology.

This consideration is important because the inclusion of political and value-laden beliefs into scientific methodology has sometimes been helpful and at other times misguided. For example, Lysenkoism is often cited as an example of an improper and catastrophic use of values in science (Harding 1989, p. 24). However, values may also play important (even constitutive) roles in scientific theorizing, as best exemplified by action research[54] in the social sciences (Reason & Bradbury 2001). As a result, we may be inclined to ask, "what value system is right?" and, "how can we apply that system in science?" But, in answering these questions, we would then be doing traditional and applied ethics, respectively. These are important questions but they are not the ones that should concern us here. This is because those answers will not address the epistemological and methodological issues that are particular to the philosophy of science, especially as members of the Vienna Circle pursued it. For these reasons, I believe it will be helpful to avoid wrestling with the question of what constitutes "political enough" for present purposes.

So if we want to introduce values into the philosophy of science in a way that does not cross over into outright ethical theorizing but is still integrated into the philosophy of science, then we will have to think differently about how we regard values in science, particularly in the case of the LVC's philosophy of science. This is, in effect,

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[54] Action research is a method used in the social sciences. Reason and Bradbury define action research as "a participatory, democratic process concerned with developing practical knowing in the pursuit of worthwhile human purposes, grounded in a participatory worldview which we believe is emerging at this historical moment. It seeks to bring together action and reflection, theory and practice, in participation with others, in the pursuit of practical solutions to issues of pressing concern to people, and more generally the flourishing of individual persons and their communities" (p. 2). Action research aims to challenge the power dynamics that emerge between study participants and researchers and encourages researchers to conduct their research in a manner that directly challenges the value systems that perpetuate institutional inequality.

what Frank does throughout his Harvard period, when he advocates for practical reforms to education. Recall that Frank regards science as an activity. As a result, many of the value-laden issues that he is concerned with have an active component. As we will see in the following sections, Frank is not concerned with the prospect of authority or the possibility of misinterpretations of scientific theories. Rather, he is concerned with actual abuses of authority and how they lead to misinterpretations of scientific theories (Frank 1956b). He does not address these problems with a theory of values or by developing an ethical theory. Instead Frank addresses them with a practical institutional proposal that attempts to address the origins of the practical problems: If we want science to function in a democratic context, then we must develop citizens who are capable of understanding, discussing, and democratically deciding upon scientific issues. His proposal is not ethically neutral, but neither is it an ethical theory in the traditional sense. Frank's proposal is a practical instructional proposal that takes ethical problems seriously. In this case, the ethical problem has to do with how students, future democratic actors, might avoid politically driven misinterpretations of scientific theories. Actual change requires attention to what we do and how we organize ourselves.

2.3 The Harvard Period in Context

In October of 1938 Frank came to the United States for a lecture tour, and was forced to remain in Cambridge, Massachusetts after Czechoslovakia fell to the Nazis (Frank 1949a). In the U.S, Frank quickly resumed his work with Carnap (who had arrived just a little earlier), Hempel, Reichenbach, Ernest Nagel, and with American collaborators such as Charles Morris, Percy Bridgman, and W.V.O. Quine, among others (Holton 2006). By 1939, following a lecture tour of about 20 universities, Frank had begun teaching in Harvard's Department of Physics and Mathematics (Frank 1949a)[55]. He remained at Harvard for the duration of his career (from 1939-1953).

Prior to discussing Frank's work on education, it will be helpful to understand the new context that Frank entered into when he first came to America. During this period, Frank's work centered on two major pursuits: the development of the Unity of Science Institute and a fuller realization of his ambition to bring the philosophy of science into close contact with social/political concerns[56]. These pursuits were interrelated. Frank's relationship to James B. Conant, president of Harvard at that time, was essential to the development of this project[57]. After Frank settled at Harvard, with an office in Jefferson Physical Laboratory, he became increasingly concerned that students of science were not intellectually equipped to wrestle with the philosophical and political aspects of their discipline (Frank 1947a/1949). Likewise, Frank noted that students concentrating primarily on topics in the humanities lacked the ability to think critically about contemporary research in pure science (Frank 1946/1949). In the early days of the atomic age and of the Cold War, the need for improved understanding was urgent.

Conant, like Frank, was deeply concerned with the divides that were beginning to emerge among all fields of academic work. For this reason, the two were important allies

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[55] Despite Frank's relationship with Conant, he never received a full appointment. This allowed Frank to pursue his many intellectual activities but resulted in a financially unstable life. See Holton 2006.

[56] Given Frank's talent as a physicist and long-time friendship with Einstein, it might be surprising to some readers that Frank did not devote more attention to this line of research during his time at Harvard. Laszlo Tisza implicitly noted this in his remembrance of Frank, wherein he said that the period between 1904 and 1930 formed the core of Frank's achievements in physics, which indicates that Frank did not devote much time to work in physics during the Harvard Period. (This information came from a printed remembrance that Holton put together and circulated in 1966, shortly after Frank's death. Holton provided me with a hard copy in 2014.)

[57] For example, see two of Frank's writings on general education: "The Place of the Philosophy of Science in the Curriculum of the Physics Student" (1947/1949), and "Science Teaching and the Humanities" (1946/1949). Both papers can be found in *Modern Science and Its Philosophy.*

and often took friendly approaches to each other's work. At this time Conant was beginning to implement Harvard's General Education Program[58]. He envisioned an interdisciplinary set of undergraduate requirements that would prepare students to engage in deliberative, democratic debates about important scientific issues. While Conant's vision has undergone changes over the years, it has influenced many of North America's systems of higher education. Frank, always sensitive to the potential for collaboration, organized the Fifth International Congress of the Unity of Science at Harvard. Conant was invited to give the opening remarks and spoke explicitly of the relationship between the unity of science movement and the General Education Program (Holton 2006).

In addition to forging an intellectual alliance with Conant, Frank began again the process of organizing a diverse group of scholars, who would meet regularly as a formal discussion group. (As in the Vienna Circle under Schlick, invitations were generally required.) It was from this discussion group that the Unity of Science Institute emerged (Holton 2006). The aims of the group were as follows:

> The purposes for which the corporation is formed are to encourage the integration of knowledge by scientific methods, to conduct research in the psychological and sociological backgrounds of science, to compile bibliographies and publish abstracts and other forms of literature with respect to the integration of scientific literature, to support the international movement for the unity of science, and to serve as a center for the continuation of publications of the unity of science movement (Holton 2006).

"Integration" is the methodological component of the unity of science movement. Integration itself has two components on Frank's view: theoretical and applied. The theoretical component consists in a logical empiricist and pragmatic understanding of

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[58] This program is sometimes called the "Program in General Education."

science, as discussed in Chapter 1. Based on Frank's autobiographical remarks, written during this time, we can say that the theoretical component of integration required the following elements: conventional interpretations of scientific definitions, an explicit role for pragmatic considerations (such as language choice and simplicity) and an application of the logical analysis of language to assess the meaning of scientific statements.

The applied component of the unity of science movement was fundamentally interdisciplinary and sought to organize scientists from a variety of backgrounds (including sociology, linguistics, physics, biology, psychology, and cybernetics) in order to better understand the shared principles of scientific research and to communicate those results to the scientific community, students, and the general public (Holton 1993). As noted above, this involved hosting meetings (formally recognized first as the Interscientific Discussion Group, and later as the Institute for the Unity of Science), preparing bibliographies, and publicizing the results of the discussions. This process could be extended beyond the realm of physical sciences to include the social sciences, and even the humanities, as Frank's *rapprochement* claim suggests (Frank 1949a, p. 1). At Harvard, Frank's work at the Institute for the Unity of Science and within the Interscientific Discussion Group was carried out in good company, for it included Percy Bridgman, Edwin C. Kemble, and Harlow Shapley (all of whom worked to secure an appointment for Frank), Quine, three Nobel laureates, and many other eminent scholars, all working towards the goal of improved understanding among scientists (Holton 2006, p. 303).

The Interscientific Discussion Group received funds from the Rockefeller Foundation in 1947, which provided the discussion group the means to transform itself into a fully fledged Institute within the American Academy of Arts and Sciences (Reisch 2005, p. 345). The funding allowed the group to pay more attention to its "proselytizing functions," including public talks, and increased attention to publication efforts (such as support for the monographs of the Encyclopedia of the Unity of Science) (Holton 2006, p. 303). As Holton (1993) emphasizes, this group was not only focused on epistemological issues in science; its members always sought to contextualize scientific work by understanding science as an activity that could play a progressive role in society. For this reason the group incorporated discussions of science and values, using whatever lessons could be learned from sociology, history, and psychology (*ibid.*)[59].

If we return to the role played by Conant, who was then trying to implement a Program of General Education at Harvard, we can see a clear connection between his work and Frank's growing interest in education. By 1945, Conant's reforms, championed by Frank and other members of the Institute, resulted in new course requirements and textbooks, in which the history and conceptual background to a particular field would be emphasized in all major fields of study (*ibid.*). But even this was not done as general education for its own sake. Rather, as the title of Conant's proposal indicates, general education would serve a larger social role; the proposal is called *General Education in a Free Society.* This suggests that informed students are vital to free and democratic societies.

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59 The best example of this comes from Frank's *The Validation of Scientific Theories,* which anthologized the papers presented at the annual meeting of the American Association for the Advancement of Science in 1953. The Institute for the Unity of Science hosted this meeting, and the volume included papers by C. West Churchman and Richard Rudner. Both scholars worked in the social sciences and strongly advocated for the inclusion of values in science. Their work, along with Frank's contribution, is discussed at length in Chapter 5.

The themes of Conant's General Education Program were expanded upon by Frank in his 1950 "Introductory Remarks" to the American Academy of Arts and Sciences' *Proceedings*. In these remarks Frank outlined the goals of the Institute and his own research:

> ...we have to consider science as a human enterprise by which man tries to adapt himself into the external world. Then a "pragmatic" criterion means, exactly speaking, the introduction of psychological and sociological considerations into every science, even into physics and chemistry. It seems, therefore, that the sociology of science, the consideration of science as a human enterprise, has to be connected in a very tight way with everyday considerations which one might call logical or semantical (Frank 1951 quoted in Holton 2006).

The considerations that Frank calls "logical or semantical" refer to work done in logical analysis, where he notes that progress had been made in clarifying scientific methodology. Using Carnap's distinction between internal and external questions, Frank highlights the role of linguistic choice and thereby focuses his discussion on the role of external questions. Carnap defines external questions as "questions concerning the existence or reality *of the system of entities as a whole*" (Carnap 1947/1984 p. 242). "Do numbers exist?" is an external question because in attempting to ask about correctly applying the predicate "existence" to numbers, the question instead asks something about systems in which predicates are defined. Pragmatics enters when we question whether or not it is useful to accept the system as a whole, though the decision is not "of a cognitive nature" (*ibid.*). The possible pragmatic considerations that Carnap cites are "efficiency, fruitfulness, and simplicity" (*ibid.*).

In the Harvard period, Frank worked to show that the philosophy of the Vienna Circle could be used in the service of concrete social aims. By emphasizing the value of social and psychological considerations to logical empiricism, Frank insisted that logical empiricists expand their research acumen to include these topics. Additionally, Frank provided the institutional mechanisms (by organizing the Institute for the Unity of Science) and developed the social network (by developing a relationship with Conant) to facilitate this research. It was in this context that Frank's socially engaged philosophy of science began to grow.

2.4 Frank, Education, and Socially Engaged Philosophy of Science

In this section, I will argue that Frank's philosophy aimed to address social and political problems pragmatically. I will show that Frank was especially concerned with misinterpretations of facts and argue that this concern set him apart from his contemporaries. I discuss how this concern informed Frank's work on education during the Harvard period. My contention is that his work on education was intended to mobilize the philosophy of the Vienna Circle to develop students' capacities as political, democratic actors via Conant's General Education Program.

Frank's educational proposals were guided not by an interest in the philosophy of education, but rather by an interest in how well formal education transforms students into informed democratic citizens. In particular, Frank was explicitly concerned with the tendency to blame science and science education for any perceived degradation of students' morality. On Frank's account this tendency was not new; it could be traced back to Plato:

> In his book, *The Laws,* Plato discusses the attitude which an ideal government should take towards its science. Briefly speaking, he regards science not so much as a search for truth [but] as an instrument for the education of "good citizens." He deplores, like many educators and politicians of our own age, the bad influence of science on religion and political education (Frank 1951, p. 16).

On Frank's account, the disparagement of science on the part of religious and political leaders results from their interest not in the "'truth' of a scientific doctrine, but in it's 'goodness,' as we may call it, meaning its usefulness for the education of 'good citizens'" (Frank 1951, p. 17). In other words, whereas scientists seek truth, religious and political leaders seek good citizens. And even though religious and political leaders cannot change the facts, they can require "the abandonment or acceptance of scientific theories" (Frank 1951, p. 18).

If Frank is right, and religious and political leaders attempt to direct the public's acceptance or rejection of scientific theories, then we should ask an additional question: Why is it that religious and political leaders are able to do this in the first place? Since authorities cannot change facts, then how is it possible for any authority to require us to accept or to reject these facts? To better understand Frank's response to this question, we should recall Lenin's response to Frank's 1907 paper on causality. Lenin's comments prompted Frank to appreciate that no matter how politically neutral one's philosophy might seem to be, it may not be perceived as neutral by others, especially to political actors. These comments led Frank to posit that even if a scientist, say, interprets certain work as politically neutral, a political actor might interpret that same work as politically meaningful. As a result, on Frank's account, facts can always be though of as politically significant.

In order to address the question raised by Lenin's critique – how can we be required to reject facts? – Frank repudiates a clean distinction between facts and values. For example, in *Relativity a Richer Truth,* Frank argues that the "*jobs of finding facts and of interpreting facts are indivisible,*" (Frank 1950, p. 71, italics original). For example, Frank recalls the introduction of "faith-healing" into the medical establishment by the

Nazis (1950, p. 75). That introduction was justified by a vague reference to "uncertainty," which attempted to refer to Heisenberg's uncertainty principle. As Frank points out, the Nazis used the concept of uncertainty as a means of supporting their vicious attacks on "liberalism and rationalism" (*ibid*.). On Frank's analysis, values played some role in the misleading interpretation of the term "uncertainty" and supported previously held political beliefs. (The potential for a positive role of values in Frank's thought will be explored below.) In this way, the social and political beliefs of the Nazis infected their understanding and interpretation of facts. Most importantly, Frank tasks philosophers and scientists with challenging such systems of belief. This claim is presented in the context of Frank's argument that scientists and science educators must attend to the inherent ambiguity of language in order to guard against potential misinterpretations.

Misinterpretations and illicit use of scientific work were deeply consequential in Frank's view. He frequently discussed these misinterpretations, especially with respect to the Catholic Church, Nazis, and the Soviet Union[60]. Furthermore, Frank argued that each the misinterpretations characteristic of each group were rooted in their value systems, and specifically in their accounts of absolute values. He saw the use of apparently absolute values as a thinly veiled attempt to consolidate political power against would-be dissenters, as marshaling an undemocratic sentiment to "direct human conduct" along desirable lines. By absolute values, Frank meant universal ethical claims. Frank argued that these sorts of claims are never absolute in practice, using a specific example:

> I can express this advice in two different ways: I can say that the principle "Thou shalt not kill" must be upheld under all circumstances. Then we must define the

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[60] See Frank 1950, 1957, and SFV for extended discussions of each topic.

> circumstances under which the word "killing" is to be applied. We may say, for instance, that the execution of a criminal is not meant by "killing."...But under the same circumstances I can give a very different formulation to the same advice….If a criminal is executed under a legal death sentence of a recognized court, the rule "Thou shalt not kill" can be disregarded. (Frank 1950, p. 98).

In this passage, Frank means to show that interpretations based upon practical considerations often confound the application of absolute ethical principles. In the example above, what it is to kill someone is subject to interpretation, and varying interpretations of a single state of affairs undermine the potential for any ethical claim to be held without question. In such cases what we mean is underdetermined: Does "to kill" (in a given context) refer only to unjustified killing, or does it refer to all instances where one person ends the life of another? Furthermore, what motivates our preferences for one account over another? While linguistic choices of this kind can be explained and justified, they cannot be assumed. And it is precisely this sort of uncritical assumption that Frank wished to challenge[61]. In the Harvard period, Frank actively sought to reposition a logical empiricist analysis of language as one that is essential for reflective democratic discourse[62].

Frank's pragmatic account positions logical empiricism as a "broader" tool for analysis. Frank seemed to know that outside the context of Vienna, the spirit of the

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[61] Frank does not address discussions regarding the status of observations such as that between Galileo and Cardinal Bellarmine, where they addressed the question: Does the Earth move? Nor does Frank address the debate between Tycho Brache and Kepler, where the dependence of observations on theories is again highlighted (see Hanson 1958). In part, this is because Frank is not very concerned with how old, incorrect scientific theories affect our observations. Rather Frank's concern is broader, insofar as he seeks to explore how those theories are partly justified by a particular social and political system.

[62] See Frank 1950, sections 19, 20, and 22 for his full account of why this is so.

original program of the Vienna Circle could be lost[63]. At Harvard, he sought to use logical empiricism to "influence the real world" (Frank to Neurath, December 10, 1943; quoted in Uebel 2003). Seeming to allude to his colleagues' pursuit of a purely logical approach and to admonish them for it, Frank wrote:

> Scientists and scientifically minded people have often been inclined to say that these "nonscientific" influences upon the acceptance of scientific theories are something which "should not" happen; but since they do happen, it is necessary to understand their status within a logical analysis of science. We have learned by a great many examples that the general principles of science are not unambiguously determined by the observed facts. If we add requirements of simplicity and agreement with common sense, the determination becomes narrower, but it does not become unique. We can still require their fitness to support desirable moral and political doctrines. All of these requirements together enter into the determination of a scientific theory (Frank 1957, p. 355)

In the quote above, Frank does not say that we should choose a scientific theory because it happens to be in line with our social or political beliefs. Rather, Frank adds to the familiar account of theoretical underdetermination by arguing that we can – and in some instances do – choose a theory based upon non-epistemic value considerations. For example, Frank cites the "fight against Einstein's theory of relativity in Soviet Russia" (*ibid.*), where Einstein's theory was rejected by authorities because it was inconsistent with dialectical materialism. In the context of theory rejection, a contemporary example would be the rejection of evolutionary theory by religious conservatives, for in this case evolution is thought to undermine the dignity of man in the eyes of God, a cornerstone of certain versions of Christian ethics.

While Frank does not preclude the use of his social and political account of

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[63] For example, at the end of Frank's *Introductory Remarks* (1956a), he highlights the fractures that existed among the remaining members of the Vienna Circle. While Frank was indeed hopeful that the original spirit of the Vienna Circle could eventually be recaptured, he also indicated that doing so would be challenging.

underdetermination within the context of theory acceptance, he most often applies it to theory rejection. Consequently, he often seeks to identify and understand the role of "nonscientific influences" (*ibid.*) on the rejection of scientific theories. On Frank's account, no philosophical reflection or system is sufficient to ensure a unique, neutral interpretation of a scientific theory. The specific issue of problematic "moral and political doctrines," (*ibid.*) requires knowledge of the historical roots and sociological motivations of these doctrines. But, even though such knowledge will be helpful, the question of interpretive underdetermination cannot be avoided.

For these reasons, philosophers of science ought to identify the questions and tasks necessary to understand and address the relationship between theory choice and interpretation. Philosophers of science should work to better understand how social and political factors affect theory acceptance or rejection. This additional aspect of Frank's work suggests that philosophers should not attend only to problems defined as important by the discipline. Rather they should use the tools and human resources available to them to engage with social problems. Through publicly accessible works such as *Relativity a Richer Truth*, Frank showed that such an analysis was possible, even if it was unpopular among the intelligentsia[64].

In addition, a socially engaged philosophy of science ought to reshape how philosophy and science are taught to students. In Frank's view, the goal of this kind of analysis was to ensure that students would benefit from the work of the unity of science movement. Unified science, when presented in the educational context would start "from *the human values which are intrinsic in science itself*" and should then "emphasize these

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[64] See Toulmin 1951, Ushenko 1951, Putnam 1958, and Gotesky 1950 for critical receptions of this work.

values and convince the science students that *interest in the humanities is the natural result of a thorough interest in science*" (Frank 1946/1949, p. 261). If students are not provided with the intellectual tools necessary to understand the relationship between science and values, Frank cautions that "large sections of the public will become grist for the mills of some organized propaganda groups" (*ibid*., p. 264).

In his discussions of education, Frank is not simply cautioning against the misuse of science or the potential threat that anti-science might pose for science and society. Instead he is cautioning against the idea that the appropriate role of scientists is to "stick to the facts only" (*ibid.*, p. 264):

> Thus, the physical sciences provide very good examples from which students can learn that the expression "sticking to the facts only" is frequently used as a pretext for avoiding all logical analysis, and therefore for favoring all kinds of obsolete prejudices. What one should reasonably mean by sticking to the facts only is to make only statements that can be checked by experience, that is, by observable facts. This habit is certainly of great use in debunking empty slogans and bigotry in politics or religion.
>
> As "sticking to the facts" is the slogan of traditional physics teaching, "ignoring the facts" is a slogan cultivated in the traditional teaching of mathematics. Both of these slogans are logically legitimate within a restricted domain of thought. However, on occasion, the students have to learn the limitations of these slogans; otherwise, the meaning of the most important laws of nature cannot be made clear to them, and the very goal of general education on the basis of science would be frustrated (Frank 1946/1949, pp. 264-265).

Frank's agenda here is not strictly pedagogical. He is not seeking to better understand education or systems of education. His proposals extend further because they attempt to use the tool set of the philosophy of science in order to show students the limits of disciplinary knowledge.

Additionally, Frank aligns his educational proposals with Conant's General Education Program at Harvard, in which he regularly taught a course on the history of science (Holton 2006, p. 275). The General Education Program requirements were described in Harvard's *Crimson* as follows:

> All students would take the same lower-level Humanities course, the same Social Sciences course, and one of two courses in the Natural Sciences. In addition, they would take three more courses outside of their department of concentration, two of them outside of the area (Hum, Nat Sci, or Soc Sci) in which that department fell (Bevard 1964).

In one respect, the General Education Program may look like a limited form of contemporary distribution requirements. However, in *General Education in a Free Society,* the Harvard Committee reported that a general education was intended to provide students with "tools on which any civilization depends" (p. 36). The General Education Program was where Conant "hoped to provide citizens with the tools to follow debates to the degree required by well functioning democracy" (Biddle 2011, p. 554).

While Frank continued to be motivated by the devastation in central Europe, Conant, who had worked on the Manhattan Project, was increasingly concerned about a society that was newly empowered to destroy itself and deeply ignorant of the technology it now possessed. Like Frank, Conant understood the essential role of values in scientific research:

> There is a fairly common fallacy that if you are dealing with scientific and technical matters, judgment of values rarely, if ever, enters in. Facts speak for themselves in science, we are often told. Anyone who is familiar with the course of scientific research and development knows this is nonsense. What is true is that the area of debate is fairly definitely circumscribed. (Conant 1952, p. 113; quoted in Biddle 2011, p. 556).

On these grounds the General Education Program challenged the apparently dominant theory of value-neutrality. Even though Conant did not spell out exactly what was meant by his theory of value-neutrality, Biddle argues that Conant's epistemological holism and his attendant interest in the underdetermination of scientific theories implies that he was particularly interested in the role played by values in the *acceptance* of scientific theories. As Biddle points out, Conant described the entire process of scientific inquiry as "shot through with values" (*ibid.*). And as president of Harvard, Conant proposed an institutional mechanism that would serve as an arena in which scientific decisions could be transparently debated. Debaters would argue for and against a particular proposal in a system of "quasi-judicial review" (Biddle 2011, p. 560). As Biddle emphasizes, the debaters (who need not be scientists) would not be "disinterested evaluators" (*ibid.*). Instead the value-laden aspect of scientific work, and particularly of theory acceptance and application, would be institutionalized as a necessary part of decision-making.

Frank's educational proposals share Conant's aspirations for a democratic model of scientific decision-making. In fact, the role of philosophy of science in the physics curriculum is to mold students into "a critically minded type of scientist" (Frank 1947a/1949, p. 230). Frank's proposals complement those of Conant; for if Conant's institutionalization of scientific debate is to succeed, the debaters must be both competent and critical. In Frank's 1947 paper, he emphasizes the importance of historical knowledge and of scientific methodology. Historical knowledge is important, because without it:

> The student does not learn the way in which a powerful organization can oppose a doctrine established by science, and how this opposition can muster the support of reasonable and bright persons like the father of British empirical philosophy, Francis Bacon, who denounced the Copernican system as violating our common sense (Frank 1947a/1949, p. 232).

The scientific method, on Frank's presentation, requires that students understand "the exact relation between mathematical proof and experimental confirmation" (Frank 1947a/1949, p. 234). Students need to be able to understand in detail how we organize information and how we confirm it so that they avoid thinking that a particular law is trivial or the result of some kind of miracle (Frank 1947a/1949, p. 236).

> They [instructors] will dodge as much as possible the new general laws that have replaced the Newtonian science. The student will get little of Heisenberg's principle of indeterminacy or Bohr's principle of complementarity...And if these points are touched, the same thing will happen as in the ordinary treatment of relativity. The student will be left with the impression that the new physics is somehow obscure and even "irrational." This perfunctory presentation of the new principles is, in some respects, even worse than the practice of restricting the teaching precisely to the formulas that describe actual observations. For the feeling that science has to be satisfied with an obscure theory makes the student an easy victim of quacks who exploit modern science in their endeavor to prove that we have to surrender our reason to a kind of blind instinct. This means in practice that we have to surrender our good judgment to the control of people who are bold enough to pretend that they are in possession of that intuitive instinct (Frank 1947a/1949, p. 240).

Science teachers must attend to the scientific method, in detail. For if they do not, then the pattern of reasoning particular to science, a pattern that promotes understanding, will be lost on students. And should this happen, the basis for Conant's institutionalized debates would disappear.

In 1956 Frank again points towards "bitter complaints that the students of science, and even the students of engineering, are educated in such a way that their thinking is directed exclusively towards progress in their own field" and that many students remained "completely ignorant concerning the wider problem of what their work means in this human life as a whole" (Frank 1956b, p. 17). In this paper, Frank specifically cites the reception of "nuclear energy" and the fact that scientific work in that field was thought by the general public to show a "lack of humane interests in the personalities of the scientists" (*ibid*). Here he cited Conant as an ally:

> The movement towards "humanization of the sciences" gained momentum in the American universities when J. B. Conant, President of Harvard University, published in 1947 his book "On Understanding Science". He made very strongly the point that a student [cannot] really understand science without grasping, to a certain degree, the historical and philosophical background of science. In a great many American universities lecture courses under the name "General Education Science" were introduced, in which science was taught not from the purely technical viewpoint, but as a human enterprise which is closely connected with other human enterprises. But when these courses were to be introduced it turned out soon that the science teacher with the usual training is not really able to teach "general education science" to the students. One had to introduce courses which gave the teacher of "General Education Science" the type of knowledge which he needs to know, besides a thorough knowledge in his special field. This additional knowledge the future science teacher can get in courses on the "History and the Philosophy of Science", and occasionally in "Sociology of Science". Recently the courses on the Philosophy of Science which I am giving at Harvard University are partly also given in such a way that they can serve this educational purpose (*ibid*.)[65].

This shows that from at least 1947, Frank envisioned a new institutional role for the Unity of Science movement. Frank's account complements Conant's by emphasizing education as the primary means by which students can engage in scientific discussions at the higher levels of policy and governance. In this way, we can see more clearly Frank's vision for the unity of science. The unity of science movement was not limited to epistemic goals of seeking and describing the way in which all sciences share in a common language and methodology. In order to realize its epistemic vision, the movement would also have to develop a critically engaged body of scientists and scientific laypeople who would be essential to democratizing scientific practice.

2.5 Future Directions

A final question that should be addressed here is whether or not Frank's work can

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[65] Frank makes a nearly identical point in 1950, in the beginning sections of *Relativity, a Richer Truth.*

contribute to socially engaged philosophy of science today. To develop a clear standard for what constitutes socially engaged philosophy of science, we can again look to the *Manifesto for the Joint Caucus of Socially Engaged Philosophers and Historians of Science* (2012). The authors define socially engaged philosophy of science as philosophical work that "promotes a widespread understanding of science's relations to society and social welfare," including engagement at many levels (the public, education, policy, and governing bodies).

Fehr and Plaisance (2010) use the broad definition above, and emphasize that socially relevant philosophy of science is not characterized by a unique approach to philosophical methodology (e.g., "standard conceptual analysis" may be used but is not required). Instead they offer a diverse program that "engages socially relevant scientific topics in ways that allow for the assessment or advancement of traditional topics in philosophy of science (e.g., prediction or explanation)" or that may also "[focus] on socially relevant science to develop new approaches to and positions within philosophy of science" (Fehr & Plaisance 2010, p. 303). Consistent with the Joint Caucus statement, the approach laid out by Fehr and Plaisance seeks engagement with stakeholders in a variety of settings and promotion of work outside the academy.

As in Frank's work, none of this requires the endorsement of a specific political view. Rather, socially engaged philosophy of science seeks to use the specific tools of the philosophy of science (e.g., theories of evidence, confirmation, scientific laws, etc.) in order to address social questions. Frank's proposals may add to this goal. Like Conant, Frank envisaged a broad goal for the philosophy of science because it "linked" science and the humanities. That educational goal would then enable the public to better understand the complex issues emerging from scientific research. In this section I will argue that Frank's educational-institutional proposals may be adapted to address contemporary concerns. Specifically, I will discuss Philip Kitcher's concept of ideal tutoring and its role in well-ordered science, arguing that Frank's educational proposals can be used to develop this concept.

In *Science, Truth, and Democracy* (2001) Kitcher develops the idea of well-ordered science, a democratic ideal that can be used in order to develop locations of institutionalized, deliberative debate regarding the trajectory of scientific research. Early on in his discussion of well-ordered science, Kitcher distances his proposal from vulgar democracy and "rule by the ignorant" (Kitcher 2001, p. 117). Thus Kitcher's account of a well-ordered science requires an enlightened, educated, and pluralist set of deliberators.

> For perfectly well-ordered science we require that there be institutions governing the practice of inquiry within the society that invariably lead to investigations that coincide in three respects with the judgments of ideal deliberators, representative of the distribution of viewpoints in the society. First, at the stage of agenda-setting, the assignment of resources to projects is exactly the one that would be chosen though the process of ideal deliberation I have described. Second, in the pursuit of the investigations, the strategies adopted are those which are maximally efficient among the set that accords with the moral constraints the ideal deliberators would collectively choose. Third, in the translation of results of inquiry into applications, the policy followed is just the one that would be recommended by ideal deliberators who underwent the process described (Kitcher 2001, pp. 122-3).

Well-ordered science is a regulative ideal in that it may never come to fruition, but it can serve as a useful tool by which we may guide our thinking and actions. Indeed, for this proposal to succeed without descending into elitism, rigorous "tutoring" must occupy a central role in well-ordered science. As Kitcher explains, *enlightened democracy* "supposes decisions are made by a group that receives tutoring from scientific experts and accepts input from all perspectives that are relatively widespread in the society"

(Kitcher 2001, p. 133). As Kitcher indicates, developing such an institution, in the absence of ideal deliberators, is an important challenge for this proposal (2001, p. 123).

With respect to that challenge, Frank's educational proposals may indeed be of some use because his philosophy of science was designed to directly address the social and political issues that affect the acceptance of scientific theories. Frank's view makes the empowerment of citizens as democratic actors its central goal (evidenced by Frank's alignment of his proposals with Conant's general education program). In order to make Kitcher's conception of ideal deliberators work in practice, we must envision a system that develops such deliberators. In other words, without a complementary educational proposal, Kitcher's ideal might prove to be impractical. Frank's work on education is particularly well suited to meet these challenges. In his lifetime, the rejection of Einstein's theory of relativity by some, uncertainty about atomic technology, dialectical materialist analyses of scientific information in the Soviet Union, and Nazi race theory were troubling examples of the need for better scientific education. Today the rejection of climate change and vaccines, the evaluation of GMO technologies, and the lies told by pharmaceutical companies (among many other phenomena) highlight a continued problem with how scientific theories are interpreted. So Frank's concern with the role of extra-scientific factors seems not too far removed from contemporary concerns.

On Frank's account, philosophy alone is not capable of resolving these issues. In his educational proposals, he aimed not to tell students what to think, but instead to provide them with tools for analysis so that they could understand the critical issues associated with problems of interpretation. The improvement of students' analytical skills would empower them to be truly autonomous democratic participants because (ideally) they would be free from the influence of problematic misinterpretations of scientific theories. Frank's particular account of science and values, characterized above as his pragmatic approach, aimed to facilitate this process:

> In the recent writings of prominent logicians like Quine and Carnap, there is one result which has emerged with increasing force; the final criterion for the decision of general problems, like the problem of meaning or the problem of reality, is in every case the pragmatic criterion, the adequacy of a solution within the framework of science as a human activity. Moreover, we have to remember that reference to a pragmatic criterion, may it be called "adequacy," or "agreement with common sense," "practical fitness," or otherwise, means always reference to the result of research in the social sciences (Frank 1956b, p. 18).

Understanding how work in the philosophy of science should proceed is inextricably tied up with social science. This is because social science helps us to understand why people act as they do.

As we saw earlier in this chapter, Frank was concerned that undergraduate education at Harvard was limited and that students lacked foundational knowledge in their own fields. Because of this, students also lacked an understanding of the interconnections among disciplines and would thus be unlikely to successfully navigate the complex debates that they would encounter outside the university. Frank did not regard general education as a panacea for these problems, but he did think that it was a necessary part of the solution. Given Frank's worries about those who seek to direct human conduct (Frank 1950, 1951), his pedagogical concerns are relevant in democratic contexts, where value-laden discourse is often used in the hope of directing individual conduct. From these arguments, I believe that we can abstract some general principles that would be of use in promoting well-ordered science.

Frank's proposal is rooted in the belief that simply understanding one's own discipline cannot enable students to develop a clear picture of scientific thought and how it promotes understanding and "critical analysis" (Frank 1946/1949, p. 265). Frank argues that there are two sources of this problem:

> We have shown that our traditional way of teaching science is responsible for two deficiencies in the education of our students. First, not all instructors in science are up to the role that science, particularly physics, has to play in our general cultural life. Second, the twentieth-century (relativity and quantum theory) are still an obscure domain outside the well-illuminated field of common-sense physics. Uneasiness has not been removed by our usual teaching of physics (Frank 1947a/1949, p. 241)

In other words, the first problem has to do with the quality of scientific instruction, and the second has to do with clarifying scientific discourse. In discussing his conception of tutoring in well-ordered science, Kitcher argues that tutoring must strive to "engender common understanding" so that representatives in well-ordered science are at least acquainted with the needs of citizens and scientific lay people (Kitcher 2001, p. 125). But in order to achieve Kitcher's aim, we must ensure that the representatives in well-ordered science are able to communicate with citizens and scientific lay people. As a first step, science education must be able to help students to distinguish between ideological positions and accurate information that will be necessary to develop a well-informed position on scientific issues.

In order to address these problems within the context of education, Frank provides four recommendations that I will summarize below:

1. Pedagogical: Students must be familiar with the foundations of "geometry and mechanics," because if they are not, then "the consequences of this carelessness will leave imprints on the minds of the student that will make it very hard to train him later as a teacher of modern physics" (*ibid.*) That is, teachers must be trained so that they can explain scientific methodology from the bottom up, as a means of addressing gaps in understanding where misinterpretations are likely to occur. In so doing, the instructor should strive to develop a coherent presentation of the tradition of the philosophy of science while at the same time remaining open-

minded (Frank 1947a/1949, p. 250).

2. Logical/formal: "…we have to insist, above all, that the student learn to analyze the statements of his science in such a way that it becomes obvious what is a statement of observation and what is a logical conclusion" (Frank 1947a/1949, p. 242). This is important because the student must be able to distinguish tautological claims from claims about the world, and they must also understand that not every claim made in science may be appropriately interpreted as a claim about the world.
3. Observational/semantic: By distinguishing observational from logical claims, students should be taught how to understand what parts of a given scientific theory include empirical claims, as well as how those claims are related to the purely logical or mathematical claims. In part, this includes understanding the experimental context, and how different parts of scientific theories are used in those contexts such that the theory itself may be tested. Lastly, this helps to provide students with the understanding necessary to begin to distinguish between appropriate and illicit uses of scientific theories. (Frank 1947a/1949, p. 243-248).
4. Pragmatic: This analysis requires teaching students how "extra-scientific reasons" can "be responsible for the predilection in favor of or the aversion to some symbols" (Frank 1947a/1949, p. 248). This recommendation encourages science teachers to use work done in the history and sociology of science to explain why and how misinterpretations of science occurred. This is particularly important, for it requires instruction in ideology and its effect on science in both the contemporary and the historical field (Frank 1947a/1949, p. 258).

Even though I have divided these recommendations into four discrete categories, we need not view them as independent goals. Frank's holistic view of scientific theories would stand in conflict with such a view, and it would undermine his arguments for the important role played by values in interpreting facts. In this section, I will explain Frank's account of values insofar as it relates to his educational proposals. In section 2.7, I will argue that Frank held a subjectivist account of the role of values in science (which is as close as he comes to specifying a positive role for values). As we can see, requirements 2 and 3 fit into a more or less standard picture of the logical empiricist version of the philosophy of science. We should therefore turn our attention to 1 and 4. These are especially important because they correspond to the two issues outlined above (viz., the possibility of communication and the ability to distinguish ideological positions from accurate interpretations of scientific claims). These are issues that must be addressed in order to amend and strengthen the role of tutoring in Kitcher's well-ordered science.

To better understand what Frank means when he describes his ideal form of science education in 1 above, it will be helpful to examine his thoughts regarding Ernst Mach. Indeed, in the essay just discussed, Frank cites Mach as an example of an eminently commendable science teacher (Frank 1947a/1949, p. 250). In a 1930 paper, Frank presents his account of the "chief lines of Ernst Mach's philosophy" and of "the essence of Mach's doctrine" (Frank 1930/1949, p. 80-81). In so doing, Frank positions Mach's anti-metaphysical position as an attempt to clarify scientific discourse. This is because Mach did not characterize his position as philosophical, but rather as a "standpoint which does not have to be abandoned immediately when we look over into the field of another science" (Mach 1866/1914 quoted in Frank 1930/1949, p. 81). Mach's standpoint is taken in order to support better and clearer interactions between scientific disciplines, and in this way Frank regards it as a precursor to unified science. Unified science is not possible without this standpoint, for non-metaphysical claims are not an end in themselves, but rather the means by which a shared understanding of scientific claims can emerge (Frank 1930/1949, p. 83). Frank's position is similar to Neurath's arguments in favor of intersubjectivity in the protocol sentence debate; we should all strive to be understood. (This is similar to Romizi's ii*, above.) The positions of empiricism and physicalism afford the best ways to avoid obscurantist metaphysical language and hence the best ways by which we can make ourselves clear.

The "pragmatic" recommendation in 4 above helps to clarify the role of ideology. Frank almost always discusses ideology in the context of theory acceptance and rejection

(Frank 1946/1949, 1951, 1956b, 1957). In each of the papers just cited, Frank's arguments are similar. In essence, he points out that the acceptance of scientific theories is not always guided by "scientific" criteria and that social values such as simplicity[66] and agreement with common sense may affect why a particular theory is accepted or rejected. Particularly in the case of theory rejection, authorities might judge a theory as counterproductive to the creation of "good citizens" (Frank 1951, p. 17).

> For the scientist, the first requirement is that the principles of science should be "true." This means, for the scientist, that these principles should be confirmed by sense observations and logical conclusions. But the authorities who are responsible for the good conduct of citizens require, in addition to "scientific truth," that the principles of science should be useful, or at least not harmful in the education of "good citizens." These authorities may be the state, the church, the school, or the opinion of the "man in the street." Their chief interest is not in the "truth" of a scientific doctrine but in its "goodness," as we may call it, meaning its usefulness for the education of "good citizens" (Frank 1951, p. 17)

In this way, the criteria for the evaluation of scientific theories vary. In particular, the criteria dramatically change as we shift from the scientific to the authoritarian context[67].

Given that ideological evaluations employ different standards from scientific evaluations, it is not surprising that Frank thinks "it is not sufficient to approach these turning points of human thought by logico-empirical analysis only" (Frank 1946/1949, p. 280). The "turning points" that Frank refers to are monumental shifts in scientific thinking, such as the Copernican Revolution and the introduction of Einstein's theories of

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66 Frank seems to regard simplicity as a non-epistemic value insofar as he argues that what we regard as "simple" is determined by our social background. See the last chapter of Frank 1957 for his full account of simplicity.

67 In the case of authority, Frank always addresses illegitimate uses of non-scientific values. This is because Frank is concerned with the way that authorities seek to standardize interpretations of scientific theories. However, Frank does leave room for legitimate uses of non-scientific values, which I will discuss in section 2.6.

relativity. In the just-quoted 1946 paper, Frank insists on the importance of teaching students that it is not always obvious which theory is true. So, in teaching students about the so-called "turning" points, we must emphasize the importance of the distinct social and scientific backgrounds within which both Copernicus and Einstein worked. And, in so doing, we must use these historical disagreements as pedagogical examples. Explanations of error in these examples can be used to help students develop a clear understanding of the complex process of theory acceptance.

The student must be taught the history and sociology of scientific thought along with its political aspects.. This is because, as Frank argues, only a careful examination of these issues will allow the student to understand and guard against future transgressions.

> Every satisfactory instruction in the philosophy of science has to discuss these choices of symbols on the basis of logical and historical analysis. The influence of political and religious trends on the choice of these symbols should by no means be minimized, as is often done in the presentation of the philosophy of science. On the other hand, if "metaphysical integrations of science" are discussed, particular attention should be given to those integrations that have played a role as a basis of ideologies. For this reason, doctrines like Thomism or dialectical materialism should be carefully and correctly presented to the student and more time should be devoted to them than is devoted to some sophisticated systems that have played only a small role in human life and human actions (Frank 1946/1949, p. 283).

These historical, social, and political roots of ideological positions should be an important topic of discussion in educational systems. This helps students to understand the true nature of ideological conflicts, which itself will be necessary if we seek to democratize (and hence to politicize) science. Rather than exploring the origins of these patterns of beliefs, we tend to disparage and dismiss them, which will not prepare students who may need to make careful and informed arguments against such problematic misinterpretations of scientific theories.

2.6 An Account of Values in Science?

Frank's pragmatic and pedagogical recommendations might supplement contemporary accounts of science and education, such as Kitcher's account of tutoring in well-ordered science. However, these two recommendations stop short of supplying us with a positive account of values in science. That is, Frank's arguments thus far seem only to address problems that are created by ideological misinterpretations of science where problematic values ought to be excluded from science. As a result, we have not seen any argument by Frank that suggests that the inclusion of values into science might be beneficial. This point is particularly troubling in light of Richardson's arguments against the neutralism that dominated most LVC philosophies.

Recall one of Richardson's most compelling arguments: that LVC scholarship "stops short of demonstrating that the LVC's logical empiricism, invited, encouraged, or provided the tools for the social and political dimensions of science" (Richardson 2009a, p. 15). As I argued above, Richardson's criterion of "political enough," by which she judged the tools of LVC philosophy of science, is unhelpful. Rather than the vague notion of "political enough," it might be helpful instead to consider Frank and other LVC philosophers in light of the emerging scholarship on socially engaged philosophy of science. That line of argument alone, however, is also inadequate. This is because Richardson also cites the value-neutrality thesis, which is often attributed to members of the LVC. So, if Frank offers only a sophisticated account of neutralism, then Richardson's arguments are corroborated: LVC accounts identify members of the Vienna Circle who were personally political but do not show that they developed or embraced a genuinely political philosophy of science. Contra Richardson, in this section, I will argue that Frank did indeed acknowledge the importance of values and invite value-laden discussions into the philosophy of science. While this not a political philosophy of science, it does invite and encourage value-laden discussions in a manner that Richardson does not acknowledge (where the values in question are not just epistemic).

It is true that the majority of Frank's discussions of values in science seem to imply that values ought to be excluded from science. However, such a characterization is overly simplistic. This is because Frank's negative discussions of values in science are usually aimed at the role of *absolute* values. For example, throughout *Relativity a Richer Truth* (1950), Frank mounts many arguments against absolute values but none against the role of values *per se*. Distinguishing between a complete rejection of values and a rejection of absolute values (only) is important: in the latter can only a subset of values is rejected. As I will show, Frank's position requires a self-aware, subjective account of scientific truth. Extrapolating from this, I argue that Frank does indeed invite value-laden approaches to scientific thought. And, on his argument, value-laden stances are critically important if we wish to avoid the calcification of beliefs into problematic forms of absolute values. Additionally, I argue that this position influences Frank's account of the role of truth in science that underwrites his educational proposals above.

In order to understand Frank's account of values in science, we have to look to an early work, in which he discusses Ernst Mach's philosophy of science. This paper, "The Importance for Our Times of Ernst Mach's Philosophy of Science" (1917/1949), dates back to the period after the first Vienna Circle and before the non-public phase of the Vienna Circle, when Frank "was very much interested in the criticism launched by

Nietzsche against Kant's idealistic philosophy" (Frank 1949a, p. 9)[68]. In this paper, Frank argues that Mach's criticism of mechanistic materialism should be considered more broadly, because the ultimate goal of Mach's position was to "debunk" auxiliary concepts (Frank 1949a, p. 18). Frank defines auxiliary concepts as "statements about observations that can be expressed in a more convenient and practical way" (*ibid.*)[69]. These are statements of science that are simplified in order to make them easier to understand for the scientific layperson. However, such simplifications can be misleading because they imply the existence of an underlying metaphysics when no such metaphysics is implied by the science itself. For example, if we say that our perceptions are mediated by our mind, this may be taken to imply the existence of Kantian noumena, even though such an assertion extends far beyond the scope of the initial statement. On Frank's analysis therefore, auxiliary concepts constitute an important part of scientific communication, but may also result in misleading interpretations of scientific theories.

Auxiliary concepts are necessary in scientific communication, on Frank's account, because they allow us to express complex ideas in a simple manner. Despite their practical value, however, they may also serve to create a sense of false certainty.

> Every period of physics has its auxiliary concepts, and every succeeding period misuses them. Hence, in every period a new enlightenment is required in order to abolish this misuse. When Sir Isaac Newton and his contemporaries made the concepts of absolute space and time the basis of mechanics they were able to represent a large domain of physics properly and without contradictions. It does not follow, however, that these concepts form a basis of mechanics that is

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[68] This paper has already been identified as one of Frank's most important discussions of Enlightenment philosophy, science, and education. See Uebel 2004 for an example (especially pp. 56-57). In this section I will not address the question whether Frank's reading of Mach is consistent with Mach's view. This is because I am trying to establish that *Frank's own position* invited discussions of value.

[69] These concepts therefore seem to be an early form of Frank's account of "common sense" philosophy.

> satisfactory from the standpoint of the theory of knowledge…And when Einstein joined Mach and in his general theory of relativity really erected an edifice of mechanics in which space and time properly speaking no longer occurred, but only the coincidence of phenomena, the elimination demanded by Mach of the auxiliary concepts of space and time–useful only in a limited domain–was completed[70] (Frank 1917/1949, pp. 73-74).

In his exposition of auxiliary concepts above, Frank takes issue with the use of metaphysical concepts. But in that description, Frank does not dismiss auxiliary concepts simply because they are metaphysical. Rather, Frank's target is the false certainty that accompanies auxiliary concepts. This is demonstrated when Frank emphasizes the role of Enlightenment thinking, which for him was defined by the process of dismissing misleading auxiliary concepts. Indeed, this sort of thinking serves to abolish metaphysical concepts in order to allow for science to develop and progress. In other words, the problem with auxiliary concepts is not simply that they are metaphysical but that our fidelity to them impedes the rejection of inadequate scientific theories.

On Frank's reading, the ability of Mach's philosophy to debunk auxiliary concepts is central: "Mach had no special bias against…mechanistic terminology," but rather his aim was to "debunk all types of auxiliary concepts insofar as they pretended to describe ontological realities or metaphysical entities" (Frank 1949a, p. 18.). It is important to recognize that Frank did not think that auxiliary concepts had to be debunked because they were meant to describe ontological or metaphysical entities. Frank's rejection of auxiliary concepts is not grounded on the simple belief that

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[70] This reading of Einstein's theory of relativity is contentious among contemporary philosophers of physics. It is unclear whether Einstein encouraged Frank to view his theory in this way. Notwithstanding any possible inaccuracies in Frank's exposition of Einstein's position, we should focus here on Frank's account of theory change, whereby scientific progress is understood as a repeated cycle of rejections of scientific theories.

metaphysics is not science. Instead, Frank was skeptical of the absolute notions of truth that tacitly underwrite these systems.

> As enthusiastic students of contemporary science our group [the first Vienna Circle] rejected Kant's doctrine that the forms of experience provided by the human mind were unchangeable. We looked for some way to construe these forms as subject to an evolution that would be in accord with the evolution of science (Frank 1949a, p. 8).

In this way, Frank's rejection of metaphysics was informed by his dynamic (or evolutionary) view of science. Furthermore, in the quote above, Frank argues that forms of experience evolve with science. So as our way of understanding the world changes, so to do our ways of experiencing the world. (Recall that following the protocol sentence debate, both Neurath and Carnap embraced some form of anti-foundationalism, by which they held that all statements of science were revisable in principle[71].) Throughout the remainder of this section we will closely examine the origin of Frank's account of auxiliary concepts and how those concepts contribute to his general account of values.

At the end of Frank's 1917 essay, he argues that the misuse of auxiliary concepts by the rationalists of the Enlightenment led to a return to something like scholastic theology (Frank 1917/1949, p. 74). On Frank's reading, the genesis of this problem of resurgent scholasticism can be found in the reification of the term "materialism" during the late Enlightenment (*ibid.*). At that time, materialism's particular view of matter was assumed to be a universal truth: "The fact that matter, too, was only an auxiliary concept was forgotten and people began to regard it as the essence of the world" (*ibid.*).

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[71] See Uebel 1996 and Neurath 1934 for a fuller account of anti-foundationalism within the Vienna Circle. Readers should note that Uebel's attribution of anti-foundationalism to Schlick has been challenged (see Oberdan 1998), while it is now widely accepted that Neurath did indeed offer a strong anti-foundationalist thesis (see Cartwright et al. 1996, Reisch 2005).

Consequently, defenders of science who wish to take up the project of the Enlightenment must avoid committing problematic reifications. To capture this Enlightenment attitude Frank develops "the critical point of view" (*ibid.*).

In trying to understand this critical point of view – how we protect ourselves against the tendency to reify scientific concepts – Frank turns to Nietzsche. And in so doing, he pays particular attention to Nietzsche's account of perspective and subjectivity.

> That things have a quality in themselves, quite apart from any interpretation and subjectivity, is an idle hypothesis: it would presuppose that to interpret and to be a subject are not essential, that a thing detached from all relations is still a thing (Nietzsche, *The Will to Power*, No. 289 quoted in Frank 1917/1949, p. 77).

Frank's later discussions echo this interest in Nietzsche's account of subjectivity. Recall that in *Relativity a Richer Truth,* Frank argues that the "*jobs of finding facts and of interpreting facts are indivisible,*" (Frank 1950, p. 71, italics original). Interpretation is an essential part of Frank's account of how we identify what counts as a fact. And when we examine this a bit more carefully, it will be clear that Frank regards interpretation subjectively.

Subjectivity is an important part of Frank's dynamic account of science. In part this is because we cannot appeal to absolute values. Given these restrictions, our only recourse in addressing the problems raised by auxiliary concepts is to acknowledge the subjective nature of our position as observers and, perhaps also, as scientists. As a result, we have to promote ways of critiquing our subjective position. This is the self-aware and critical perspective that Frank seems to borrow from Nietzsche. Adopting the critical perspective does not preclude acknowledging values and the roles that they might play. Rather, on this view we may invoke values, provided that we resist their reification:

It is not to be denied that the philosophy of the enlightenment possesses a tragic

> feature. It destroys the old systems of concepts, but while it is constructing a new system, it is also already laying the foundations for new misuse. For there is no theory without auxiliary concepts, and every such concept is necessarily misused in the course of time. The progress of science takes place in eternal circles. The creative forces must of necessity create perishable buds. They are destroyed in the human consciousness by forces which are themselves marked for destruction. And yet, it is this restless spirit of the enlightenment that keeps science from petrifying into a new scholasticism. If physics is to become a church, Mach cries out, I would rather not be called a physicist. And with a paradoxical turn[72], Nietzsche comes out in defense of the cause of the enlightenment against the self-satisfied possessor of an enduring truth (Frank 1917/1949, p. 78).

There is no "view from nowhere," and there is no ideal position from which we can judge science. Consequently, objectivity itself can become a problematic value. So it seems that Frank's analysis results in the following conclusion: we can find agreement based on the phenomena, but we can never speak simply about *what* we observe, for we must also address *how* we observe it. On Frank's account, our observations of the world are always coupled with our interpretations of those observations. Insofar as Frank regards interpretation as an unavoidable aspect of observation, values may enter into how we observe the world.

On this dynamic picture of science, we cannot escape the question of value. Why and how values influence our views must be part of an ongoing critical conversation. Additionally, adopting a political or value-laden stance would be admissible on this account. For example, a feminist perspective showing how androcentric bias has functioned as an unstated and problematic feature of scientific thought would not be incommensurate with this view of science. Indeed because in this example a feminist perspective is critically exposing auxiliary concepts (such as the idea that the male body

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72 "Paradoxical" because Nietzsche was otherwise staunchly critical of the Enlightenment.

is the exemplar of a “normal” or “standard” body), it would be encouraged on Frank’s account. However, this view need not result in uncritical relativism, since Frank requires only a limited form of critical subjectivism and only to the extent that it avoids reification. Value-laden stances may be adopted in order to analyze how concepts are being used in scientific theorizing, but those stances may not then be regarded as universally true. This is why it is too simple to argue that values are forbidden. Instead, we should appreciate that Frank’s position is open to the provisional adoption of values, but is dismissive of values that are uncritical or absolute.

Frank stops short of providing us with any guidance regarding what sorts of values we should prefer. However, the critical attitude introduced in 1917 appears again in 1946, in one of Frank’s discussions of education:

> The average textbook of physics tells us very little about the evolution of the principles of this science, except some dates of anniversaries. Very often these books speak about ancient and medieval science in a derogatory way; they claim not to understand why for ages people were not able to discover such a simple law as the law of inertia… They even block the understanding of this and similar principles. For it is clear that a principle which intelligent men have not found through centuries cannot be as obvious as the statement presented by these books as the law of inertia. This complacent attitude imperils even the understanding of the evolution of thought and helps to spread the spirit of intolerance and bigotry among the students, while an attitude of logico-historic analysis would contribute toward good will between people of different backgrounds and different creeds. (Frank 1946/1949, p. 279).

Here we can again see that Frank positions the critical attitude as an important feature of the dynamic (or evolutionary) character of scientific theories. In the same essay, Frank stresses the role of intellectual solidarity by quoting (but not citing) Neurath: “We must never forget that metaphysics divides people and science unites them” (Frank 1946/1949 p. 276). This provides additional insight with regard to how Frank understood the critical attitude; a critical education can foster tolerance.

Importantly, in 1946 Frank also seems to leave room for some other forms of analysis: "it is not sufficient to approach these turning points of human thought by logico-empirical analysis only, for the human mind is not strong enough to carry out such a complex analysis" (Frank 1946/1949, p. 280). Here again, Frank argues in favor of an evolutionary account whereby students learn the various roles played by values, which are not presented as wholly negative[73]. Indeed nowhere in Frank's works are values in science dismissed out of hand. Rather, as we have seen in this section, Frank presents a compelling and sophisticated invitation to consider scientific thought from a variety of perspectives.

Let us now return to Sarah Richardson's criticism that the LVC "stops short of demonstrating that the LVC's logical empiricism, invited, encouraged, or provided the tools for the social and political dimensions of science" (Richardson 2009a, p. 15). As we have seen, Frank's 1917 paper does invite, encourage, and (to a lesser extent) provide tools for the examination of values in science. And, with regard to the question of socially engaged philosophy of science, Frank's version of LVC philosophy of science not only acknowledged that interpretations of science are value-laden but also argued that it is important for science educators to address these issues. As Frank cautions, by ignoring the social and political aspects of scientific theorizing, we will also ignore the problematic misinterpretations of science that inhibit progress.

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[73] See section 2.5, above, regarding Frank's (1946/1949, p. 283) argument that students must be taught about other systems of thought, particularly philosophical systems that compete with scientific theories as accurate sources of facts about the world.

2.7 Conclusion

In this chapter we sought to understand Frank as developing an early form of socially engaged philosophy of science. After surveying the LVC literature, we can see that the standard by which we judge "political enough" is unclear. Richardson's most powerful arguments against the LVC thesis are not sufficiently addressed by either Romizi or Uebel. Furthermore, Richardson's argument and Uebel's reply result in an impractical conclusion: that philosophers of science will have to resolve meta-ethical controversies in order to judge whether or not the LVC is indeed "political enough." By rejecting the standard of "political enough" in favor of "socially engaged," we may avoid more confusion.

In arguing that Frank's philosophy of science was politically engaged, I have focused on his work in education, because that body of work explicitly brought logical empiricist thought to bear on social and political issues. Frank's proposal envisioned science education as an important component of 20th century democracies. I have argued that his work on education engaged with social and political issues through its application in Conant's General Education Program. In particular, Frank sought to prepare students to become informed political actors by teaching them how science can be misinterpreted in the service of undesirable political aims. Looking forward, I then argued that Frank's work contributes to Kitcher's account of tutoring in well-ordered science. Specifically, Frank recommended ways to preserve the possibility of scientific communication and also showed how ideologically motivated rejections of scientific theories can be addressed in the classroom. Lastly, I returned to the LVC debate and addressed Richardson's claim that the members of the LVC did not invite or encourage the inclusion of values within their scientific theorizing. Using Frank's 1917 discussion of

Ernst Mach's philosophy of science, I showed that he argued for a critical attitude by which value-laden stances may be adopted. Whereas Frank did not develop an ethical theory, he did encourage recognition of the role of values in science and science education.

3 Chapter Three

3.1 Introduction

When trying to understand and evaluate historical work that challenged accepted wisdom in ways that now seem prescient, it is tempting to imagine the author as a lone visionary. But challenging accepted wisdom does not require working outside a tradition (although that is possible). This lesson is particularly important to keep in mind when evaluating Philipp Frank's contributions to logical empiricism. Frank's focus on pragmatism and the sociology of science may appear to be a major break with logical empiricist thought, but such a story would be far too simple. Although this narrative has been used recently to position Frank in the literature, I shall argue that it is seriously incomplete and potentially misleading.

In this chapter, I present Frank's account of meaning in the Harvard period (1938-1966), including his accounts of verificationism, and the sociology of science. In so doing, I assess Frank's relationship to verificationism. Reisch (2005) and Uebel (2011) have appealed to the fact that Frank rarely employed formal methods (i.e., math and logic) in order to argue that he distanced himself from logical empiricism (Reisch) or from the problematic doctrine of verificationism (Uebel). Admittedly, a move away from verificationism would be a boon to Frank's philosophical project. However, and for different reasons, I argue that both Reisch and Uebel overstate the extent of Frank's move away from traditional forms of logical empiricism and from verificationism (3.2). Indeed, there are many examples of Frank using verificationist thought in his mature work. For example, Frank approvingly discusses Carnap's use of verificationism in 1957:

“Certainly the main principle of empiricism, or even logical empiricism as Carnap understood it, is the principle of verifiability of confirmability” (Frank 1957, p. 335). So, to better understand Frank’s relationship to verificationism, we will examine his account of meaning throughout this chapter.

I will also argue that pragmatism and the sociology of science are constitutive aspects of Frank’s account of meaning, despite his continued adherence to a modified form of verificationism. Two important aspects of Frank’s account of verificationism will be assessed in section 3.3: The distinction between observational and theoretical terms or vocabularies and the use of correspondence rules. As we will see, Frank’s mature philosophy of science preserves a modified form of both of these aspects of verificationism. This aspect of Frank’s thought is easy to overlook, because Frank rarely uses any form of logic in his philosophical discussions. I argue that this is because Frank appropriates aspects of Hilbert’s philosophy and is specifically influenced by Hilbert’s account of geometry. Even though the philosophical projects of Hilbert and Frank are quite different, by surveying contemporary accounts, we will see that Hilbert’s influence on Frank’s understanding of geometry is consistent with Frank’s larger philosophical project (3.4). And, by closely considering Frank’s 1957 discussion of geometry, we will see how he preserves the two aspects of verificationism identified above (3.5).

The most unconventional aspects of Frank’s philosophy of science–his use of pragmatism and sociology–dominate Frank’s thought in the Harvard period. During this time, Frank expanded logical empiricist thought into new territory by seeking to understand how metaphysical statements could be made meaningful. In section 3.6, the development of Frank’s interest in sociology will be explored. Here we will pay particular attention to why Frank became interested in the pragmatic effects of metaphysical systems of belief by examining his analysis of scholastic thought. Lastly, in section 3.7, we will examine two related accounts of how social factors may be interpreted within scientific theories. I use the term “social factors” to identify these aspects of Frank’s thought. There are two social factors, one negative and one positive. The negative social factor helps us to identify and eliminate ideological biases. The second positive form allows for metaphysical claims to be rendered meaningful because it uses social information to interpret those claims. Although these interests are unique to Frank’s work, at no point did they undermine his commitment to verificationism.

3.2 Contemporary Scholarship and Resisting the “Neurath Narrative”

Problematic interpretations of Frank’s work argue (or suggest) that his views were merely a continuation of Neurath’s program. But given that Frank sought to understand how metaphysical claims could be made meaningful and that Neurath took a far less tolerant approach, it is not helpful to understand Frank’s scholarship during the Harvard period in that manner. It must be admitted that it is difficult to write about Frank because of the dearth of secondary literature devoted to a careful study of his work. So discussing Frank’s work in the context of the work of his better-known contemporaries, such as Neurath, can be a very helpful way to begin. Frank, to a certain extent, licenses this tendency; there is much overlap between Neurath’s work and that of Frank. For example, in *Science, Facts, and Values*, Frank devotes an entire chapter to the humanization of science (Frank SFV, Pt. I, Ch. 2), and “humanization” is commonly associated with Neurath’s work (Hartman 2005; Neurath 1945/1973). It is also difficult to disassociate Frank’s interest in the sociology of science from Neurath, the only sociologist in the

Vienna Circle. Finally, of course, there can be no doubt about Frank's deep personal loyalty to Neurath. Nonetheless I will avoid structuring my analysis of Frank's thought by appeal to works of closely associated philosophers because doing so can be misleading.

In this section I will discuss two accounts of Frank's work that are particularly important with respect to understanding his account of meaning. Those are from George Reisch (2005) and Thomas Uebel (2011). Reisch's account is important to address because in it Reisch argues that Frank sided with Neurath in a consequential debate over the role of semantics in scientific theorizing. In addition to the fact that Reisch's account is incorrect, his conclusion also suggests that Frank rejected basic tenets of logical empiricist thought in North America. That concern is given a more complete expression in Uebel's paper, which argues that Frank de-emphasized verificationism in favor of a new theory of meaning. So, following my discussion of Reisch, I will address Uebel's version of Frank's account of meaning and will explicate a traditional form of verificationism. This discussion will be picked up again at the end of this chapter, where I will show that Uebel misjudges Frank's account of meaning by downplaying his interest in formalism and by incorrectly understanding his account of pragmatism.

Reisch's social history of the Vienna Circle provides one of the more extensive discussions of Frank's life and work in the Harvard Period. It is important reading for anyone working in the history and philosophy of science, especially for scholars who are interested in the early 20th century. However, with regard to Frank's account of meaning, I believe that Reisch misjudges the influence that Neurath's philosophy exerted over Frank. Specifically, I argue that Frank's later account of meaning is quite different from that proposed by Neurath. Most notably, Frank did not reject Carnap’s semantic turn (i.e., Carnap’s adoption of Tarskian semantics) as Neurath did (Reisch 2005, p. 209). Reisch identifies three key objections that Neurath leveled against Carnap’s semantics: “one philosophical, one scientific, and one political and cultural” (Reisch 2005, p. 193).

Neurath’s philosophical objection was that Carnap’s semantic turn resulted in a dubious trade-off: “clearness and exactness” came at the expense of pluralism (Neurath 1943, quoted in Reisch 2005, p. 193). On Neurath’s account, Carnap’s use of semantics attempted to posit a direct connection between statements and the world. In part, this was a result of the fact that Carnap did not acknowledge the existence of the speaker, as Neurath required during the protocol sentence debate (Reisch 2005, p. 194). Carnap’s adoption of Tarskian semantics resulted in statements of the following sort: “The snow is white” is true iff the snow is white. Neurath’s protocols had the following form: Otto's protocol at 3:17 o'clock: [Otto's speech-thinking at 3:16 was: (at 3:15 o'clock there was a table in the room perceived by Otto)] (Neurath 1932/1983).

A Neurathian protocol statement is a report of an observation at a particular time and may be revised as needed. Thus the revisability of protocols is tied to the fact that they are speaker-dependent. If they are not dependent on a particular speaker, then we cannot revise them. Neurath thought that Carnap’s semantics did not allow for the revision of protocols and that his account therefore could not be pluralistic. This is because Neurath’s account of pluralism allows for new protocol statements to replace old ones. So, thinking that Carnap’s growing interest in semantics precluded any kind of pluralism, Neurath claimed that absolutism and hence metaphysical approaches to the analysis of scientific statements were once again on the rise (*ibid.*). This argument, however, resulted from an incorrect reading of Carnap.

Reisch does acknowledge some shortcomings of Neurath's arguments but is not explicit about the extent of Carnap's pluralism. For even in his early attempts to define truth, Carnap rejected all metaphysical notions of truth. Carnap's conception of truth is language-dependent, and thus pluralistic insofar as any language is acceptable in principle as long as its terms and rules are clearly specified. And if Neurath's philosophical objection to Carnap's semantic turn, rooted in the former's concern for pluralism, is unsuccessful, then so too are his other critiques that rest on the same accusation of absolutism. In other words, Neurath's attempted defense of empiricism – that absolutism is anti-empiricist – as well as his politically motivated concerns (that semantics implies a kind of totalitarian thought)[74] missed the mark in criticizing Carnap.

Nonetheless, Neurath's argument did reveal a lingering concern about the direction of logical empiricism, an entirely different kind of criticism. It is here that I believe Frank and Neurath found common cause, though for different reasons. Indeed, both Gerald Holton and Robert S. Cohen confirmed that (in their estimation) there were no significant conflicts between the philosophical projects of Frank and Carnap (Holton 2014; Cohen 2014). While this is a vague claim, it is useful as a heuristic, especially since Cohen was in frequent contact with both Carnap and Frank (Cohen 2014). Thus, the following claim made by Reisch seems to be generally correct:

> Frank also shared Neurath's worries about the intellectual direction that logical empiricism was taking. Not unlike what Neurath said about Carnap's semantics, Frank said he detected the beginnings of a new scholasticism and abstractness that

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[74] These concerns are discussed in Reisch 2005 (pp. 195-201).

> could prevent the movement from growing and gaining influence after the [second world] war. Many logical empiricists were moving "more and more into pure formalism which means almost into a new scholasticism"– a scholasticism that was scarcely able to "influence the real world" (Reisch 2005, p. 209 with quotes from Frank 1942).

Shared concerns do emerge in a meta-philosophical context, when Frank said that he wanted to "make science teaching really an element in the progressive evolution of the mind" (Frank 1943, quoted in Reisch 2005 p. 210). Scientific education was central to Neurath's philosophical work following his tenure at the Museum for Society and Economy and during his development of Isotype (Fleck 1996; Neurath 1931/1973). However, it is too hasty to suggest, as Reisch does above, that Frank shared Neurath's critique of Carnap's semantics. Such a suggestion is even more suspect given Frank's interest in semantic analyses of theories, which I will argue for in subsequent sections.

It is true that both Frank and Neurath were concerned about their colleagues' tendency to view problems in the philosophy of science through the lens of formal approaches to the philosophy of science. However, Frank did not share Neurath's particular concerns regarding semantics, which suggests that the roots of Neurath's and Frank's concerns were in fact quite different. Consider again that Neurath accuses Carnap of an "absolutistic and unempiricist" account of truth in response to Carnap's growing interest in the semantics of scientific theories (Reisch 2005, p. 199). To understand this issue, we must appreciate that even after Neurath's spat with Carnap and his correspondence with Frank, the latter gave semantics a central role in the philosophy of science:

> This logico-empirical analysis, or "semantic analysis," is a general method of "making our ideas clear." It is an efficient method for communicating our thoughts and for influencing people. This analysis is the chief subject that we have to teach science students, in order to fill all the gaps left by traditional science teaching (Frank 1947a/1949, p. 245).

While this quote does not explicitly mention Carnap's work, it does indicate that Frank did not accept Neurath's arguments against semantics. Thus, it is unlikely that Reisch's analysis aligns Neurath and Frank in the correct way. Neurathian ideas can be found in Frank's work (or, perhaps, Frank's ideas are present in Neurath), but we must be careful not to allow the apparent alliance between Neurath and Frank to be overstated. In fact, in an introduction to the Institute for the Unity of Science (of which he was head), Frank summarizes the tensions among logical empiricists as follows:

> The lack of sufficient cooperation can account for a situation in which the specialists in different fields who actually worked on different places of a common enterprise could hardly understand one another and, to be frank, even often misunderstood one another. The logicians developed artificial languages which seemed to the representatives of empirical science a return to a Platonian [sic.] doctrine of ideas. On the other hand, the views of the scientists seemed to the logicians to contain flagrant contradictions, or at least to lack amazingly in logical subtlety. The movement seemed to fizzle out into divergent subgroups, each of which held different views which were apparently in clear contrast to the original program of the Vienna Circle and Logical Empiricism (Frank 1956/1958, p. 16).

As already pointed out in Chapter 1, the above discussion concluded on a hopeful note because Frank thought that the newly formed Institute could recapture the collaborative spirit that characterized the first Vienna Circle (*ibid.*). This suggests that Reisch's arguments do not reflect the subtlety of Frank's thought. And, as a result, they do not help us to accurately understand Frank's contributions.

Thomas Uebel's 2011 paper on Frank's late anti-metaphysics (i.e., post-1947) is, in general, an excellent account of Frank's philosophical work post-1947. Uebel helpfully traces Frank's reasoning to his time in the First Vienna Circle, and is careful not to conflate Frank's work with that of Neurath. However, despite the value of Uebel's contribution, he overstates the extent to which Frank's philosophy moved beyond verificationism, specifically in his account of metaphysics. According to Uebel, Frank's account of metaphysics did not rely upon "a faulty meaning criterion" (Uebel 2011, p. 63), namely the formal account of the verification criterion of meaningfulness. Instead, based on Frank's discussions of metaphysics, Uebel postulates that Frank offered a reformed account of meaning, one that is built around the idea of "refutable theories" (Uebel 2011, p. 60)[75]. As we will see, Uebel argues that Frank developed a *distinct* account of meaning in 1947 to identify the problematic nature of metaphysical beliefs. Contrary to this position, I will argue that Frank's account of meaning, as it relates to metaphysics, is much more closely related to verificationism than Uebel's analysis suggests.

On Uebel's account, Frank adds to the logico-linguistic analyses typically offered by logical empiricists in this way: the meaning of metaphysical statements is now based upon the idea of "common sense" philosophy (explained below). Uebel's discussion proceeds by offering two interrelated arguments. The first argument is that Frank found the traditional verificationist account to be inadequate. The second argument given by Uebel is that Frank supplanted the flawed and inadequate verificationist theory of meaning with a new account.

Uebel begins his first argument by saying that Frank "questioned whether this [direct and indirect verification] amounted to a general criterion of empirical significance

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[75] Note that Uebel is careful to distance Frank's position from Popper's. While Popperian falsification provided a general account of refutation, by which we could demarcate science from non-science, Frank's account proceeds by an analysis of how metaphysics is used in specific instances of theory acceptance or rejection (see Uebel 2011 pp. 58-61).

as such, deriving from a definition and relieving us of the need to consider the specifics of individual cases" (Uebel 2011, p. 59). Note that direct verification occurs when a theoretical term is directly defined by an observational term and indirect verification occurs when a theoretical term is defined by appeal to another theoretical term (or perhaps several) that is (or are) subject to direct verification. So, according to Uebel, Frank specifically thought that formal methods were unable to account for the empirical significance of scientific statements[76].

Since the formal account of the verification criterion of meaning is inadequate, Uebel argues that Frank gave an alternative (but complementary) theory of meaning that is "exemplar based, not formal" (Uebel 2011, p. 64). Emphasizing the continued importance of Mach to Frank's thought, Uebel then specifies Frank's revised criterion of meaning: "Where neither confirmation or refutation is possible, science is not concerned (Mach 1883/1960, p. 587)" (Uebel 2011, p. 61). Accordingly, on Uebel's reading Frank takes his cue from scientific practice, whereby "concrete practices of claim acceptance in different disciplines" form the new standard of meaning (Uebel 2011, p. 61)[77]. On this reading of Frank, metaphysics is distinguished from empirical content because

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[76] "[Frank] began to question whether empirical significance could really be reduced to such a criterion as being able to yield, given suitable auxiliary hypotheses, an observation sentence. The generality of this criterion was of a purely formal-syntactical nature and abstracted from any specific content. Frank's objection thus questioned whether such a formal criterion of empirical significance was adequate to its tasks" (Uebel 2011, p. 60).

[77] These comments were made in Frank's discussion of theory acceptance. When discussing theory acceptance, Frank often discusses it descriptively and does not provide a normative account of theory acceptance (as present-day philosophers commonly do). It is uncontroversial that pragmatics play an important role in Frank's descriptive account of theory choice insofar as his account helps us to understand why a particular group either accepted or rejected some scientific theory. Though related, Frank's account of theory choice is distinct from his account of meaning.

metaphysics is an attempt to interpret the principles of science in the language of an older system of science, where that language has since become enmeshed in everyday life. It is because of this "commonsensification" of science that concepts are rendered as absolute and thus lose their hypothetical character (Uebel 2011, p. 62)[78]. Thus, on Uebel's account, metaphysics is meaningless because metaphysical claims cannot be treated hypothetically. And, furthermore, their non-hypothetical status has nothing to do with either logical or formal accounts of meaning. So, Frank's revised theory of meaning can be summarized as follows:

1. A distinction between observational and theoretical terms is preserved, but empirical significance is not tied to a definition of a theoretical term. Formal methods are de-emphasized, because they inadequately account for the empirical significance of scientific statements.
2. There is a preference for an individualized, exemplar based method whereby a specific account of empirical significance would be provided for a given phenomenon with reference to concrete examples from scientific practice. Meaningfulness is then understood by appeal to examples from scientific practice where "commonsensification" occurs.
3. Metaphysics would be rendered meaningless by virtue of its resistance to empirical testing; metaphysics depends on absolute notions of truth.

Uebel acknowledges that in Frank's later works, he seems to appeal to the verificationist criterion of meaning:

> It cannot be denied that in some writings Frank did little to foreground the pragmatic nature of his criterion of empirical significance. Consider his "Metaphysical Interpretations of Science": "The case for the 'meaninglessness' of metaphysical statements, according to accepted scientific criteria of truth, can be made easily. We mean by a 'science' (e.g., physics) a system of general principles from which, by chains of logical conclusions, statements about observable facts can be derived. These facts are then checked by observation and experiment" (1950b, 61). It appears as if he opened his conception to the very problems that felled the formalist definition for separating the scientifically meaningful from the

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[78] This process is similar to that described in section 2.6.

> scientifically meaningless. (What is to prevent our sentence about the absolute from being joined to that "system of general principles"?) But he added: "Occasionally the term 'metaphysics' has been used in such a broad sense that every statement about quantities which are not directly observable has been called 'metaphysical,' even if it can be checked by experiment or observation. *However we shall include such principles in science and reserve the name 'metaphysics' for statements and interpretations which do not add to a scientific theory anything which would improve the agreement with observed fact*" (Uebel 2011, p. 63 emphasis added).

Uebel tries to dodge the problems brought about by the fact that Frank's writings endorse the verification criterion of meaning, or something very close to it. In particular, Uebel tries to dodge this problem by emphasizing that Frank defines metaphysics by stating that it does not "add to agreement with observed fact," Since metaphysics adds nothing new, Uebel argues, "Frank's criterion makes essential reference to an existing science: it is exemplar based, not formal" (Uebel 2011, p. 64). Despite the many valuable contributions made by Uebel, based upon his argument, it is far from clear that Frank actually did endorse a new account of meaning based upon his idea of common sense. Without question, the role of common sense was important in Frank's thought, but it is less clear that Frank's discussions of common sense were part of his theory of meaning. So, the current disagreement centers on how Frank's remarks (quoted in Uebel above) should be interpreted: Are the remarks above a rejection of the verificationist criterion of meaning?

By way of comparison, it will be helpful to contrast Uebel's version of Frank's account of meaning, with a traditional account of verificationism. Carnap, in "The Methodological Character of Theoretical Concepts" (1956), offers the most sophisticated account. In it, Carnap identifies two formal languages, L_T and L_O. L_T and L_O each contains its own vocabulary (of theoretical and observational terms, respectively). Correspondence rules connect the vocabularies of L_T and L_O, and either directly or indirectly (i.e., by referencing additional theoretical terms) provide the vocabulary of L_T with an empirical interpretation, hence giving it empirical significance[79]. So, when it is said that a term is made meaningful by its method of verification, "the method" refers to the mode of interpretation as laid out in the correspondence rule. The verificationist criterion of meaning can then be summarized as follows:

A. A distinction is made between observational and theoretical languages and vocabularies. Each language is explicated formally, usually using first-order classical logic.
B. Correspondence rules are then used to connect the vocabularies of the observational and theoretical languages.
C. On this account, pragmatic concerns are meta-theoretical, with respect to the theoretical and observational languages.

In the following section, we will see that the first and second of Uebel's claims are incorrect because formal reasoning plays an important role in Frank's post-1947 thought. However, I also argue that Frank falls short of A and B on Carnap's account because even though Frank seems to endorse verificationism he does not employ a traditional logical analysis of language, which suggests the need for further refinement.

3.3 Frank and Verification

Frank rarely participated in the technical work that served to define the verificationist criterion of meaning, and for this reason, Uebel's argument that Frank rejected verificationism has significant appeal. Indeed, Frank's apparent aversion to formalism motivates the first and second statements that summarize Uebel's account. The first part of Uebel's position holds that Frank rejected the possibility that formal definitions of theoretical terms could provide a generalized account of empirical

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[79] Correspondence rules were usually expressed as conditional or bi-conditional statements. The notion was plagued by serious challenges in the 1950's.

significance. Recall that this is why Uebel argues that Frank's general theory avoided formalism in favor of an account that connected empirical significance with the possibility of refutation. The second part refers to the preference for an exemplar-based method, which frees Frank's account from verificationism and its problematic reliance on correspondence rules. In this section, I will refine 1 and 2 above by analyzing a 1957 discussion of meaning provided by Frank.

Uebel is correct when he describes Frank's particular account of why metaphysics is problematic; metaphysics incorrectly characterizes statements as commonsensical, or as obviously true and not subject to refutation[80]. However, the question remains: Did Frank's interest in refutation preclude a continued commitment to the value of verificationism, either in whole or in part? Uebel summarized his position as follows:

> If this is correct, then we should find two points made by Frank: first, that the formalist strategy was unlikely to deliver and, second, how an alternative strategy might work. The first element was addressed initially by Frank only obliquely. He noted: "If we applied the name 'metaphysics' to a system of statements the 'truth' of which is judged according to the experimental criterion of meaning, there would be *no distinction between science and metaphysics*" (1947/1949, 295; emphasis added). Taking "experimental criterion of meaning" to mean something like direct testability by observation, Frank's point was hardly new and, moreover, widely taken to be met by the development of a suitable notion of indirect testability. Frank himself, too, was happy to continue to invoke the notion of indirect testability—but not as definitive of empirical significance (Uebel 2011, p. 61).

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80 Frank provides two complementary accounts of metaphysics in the paper on which Uebel is basing his argument: "I am using the term metaphysics here with a positive and precise meaning: direct interpretation of the basic principles of science in terms of common sense or everyday experience. I think that it is not sufficient to characterize metaphysics as 'meaningless.' There are a lot of meaningless statements that are not at all metaphysical" (Frank 1947b/1949, p. 290) and; "In metaphysics a statement or system of statements is regarded as "true" if our common sense understands the validity of the principles without having to draw long chains of logical conclusions from these principles and without checking some of these conclusions against our observations (Frank 1947b/1949, p. 296).

Here Uebel argues that the "experimental criterion of meaning" fails to distinguish science from metaphysics because it relies on a direct notion of testability. Uebel states that Frank's conception of empirical significance did not rely on indirect notions of testability. As I argue below, this claim does not accord with Frank's own arguments. Additionally, Uebel's first point is irrelevant with respect to the question of meaning, because we may separate metaphysics from science without invoking our theory of meaning. This is to say that our account of demarcation need not be dependent on our account of meaning.

As indicated above, Uebel's argument requires that Frank did not account for meaning (i.e., empirical significance) by appeal to indirect testability. This claim, however, is contradicted by the very text that Uebel's argument relies upon (Frank 1947b/1949):

> According to this new conception, it is true that physical theories are the product of free imagination, if we take the word 'free' with a grain of salt. But it must not be concluded that these theories are products of metaphysics. *For these theories are subjected to the operational or experimental criterion of meaning, though in a more indirect and complex way.* The criterion of truth remains ultimately with the checking of sense observations, as the older "positivists" claimed. *But we know now that this checking is a more complex process than was believed by men like Comte and Mach to be* (Frank 1947b/1949, p. 259, emphasis added).

Not only does Frank seem to endorse the idea of indirect testability in the quote above, but also he explicitly distances his account from that of Mach. Recall that Mach's account of direct testability formed the backbone of Uebel's account of Frank's theory of meaning. While this does not prove that Frank was a verificationist, it does suggest important flaws in Uebel's version.

Further problems arise in Uebel's argument when he conflates Frank's discussions of demarcation (i.e., distinguishing between metaphysics and science) and

Frank's development of a new theory of meaning. Specifically, on Uebel's account it is not clear how Frank would explain the meaning of scientific statements, especially in cases where the issue of demarcation does not arise. Uebel emphasizes Frank's requirement that statements are checked against observation, which suggests an exemplar-based theory of meaning. But, Uebel does not duly appreciate that these very statements *are logical conclusions*, typically required in verificationist accounts (as we will explore, in detail, below). For this reason, Uebel's resulting account is not a sufficient theory of meaning. And, apart from highlighting the conspicuous absence of formalism in Frank's work, Uebel does not provide any evidence that Frank intended to distance himself from verificationism. To better assess Frank's relationship to verificationism, it will be helpful to explore how he used formal methods post-1947. This will be the topic of the remainder of this section.

In *Philosophy of Science: The Link Between Science and Philosophy* (1957), Frank presents an analysis of scientific theories that relies on a logical empiricist understanding and analysis of science. In so doing, his discussion employs a number of features of verificationism. Consider, for example, Frank's discussion of the definition of mass and the derivation of physical laws using Newton's second law of motion. Frank begins by presenting Newton's second law, $\boldsymbol{a} = \boldsymbol{f}/m$. From this, if we consider two massive bodies, m_1 & m_2, we can then derive $\boldsymbol{f} = m_2\boldsymbol{a_2} = m_1\boldsymbol{a_1}$, independently of the mass of the body or its rate of acceleration. The ratio of accelerations, $\boldsymbol{a_1}/\boldsymbol{a_2} = m_2/m_1$, can similarly be derived (Frank 1957, p. 112-3). Frank then goes on to explain how this information can be translated into observation language:

> ... to apply [this formula] to observable phenomena, we must give operational definitions of the terms. The operational meaning of "mass" is now found in the

> ratio of accelerations. If *m* is defined in this way, an operational definition of "force" can be given by the equation $\boldsymbol{f} = m \times \boldsymbol{a}$. It is a unique definition because $m \times \boldsymbol{a}$ depends only upon the external circumstances of a body, and is independent of its mass *m*. In this sense, the formula $m \times \boldsymbol{a} = \boldsymbol{f}$ is certainly only a "definition" of "force" and not a physical law that could be checked by experience. However, if we substitute for ***f*** the simple law … [$s=\frac{1}{2}gt^2$, where the force ***f*** is assumed to be constant over a short distance the relation of] $m \times \boldsymbol{a} = \boldsymbol{f}$, where ***a*** means the acceleration with respect to an inertial system, is a law about physical facts and no longer a mere definition of force (Frank 1957, p. 113).

In the above, the "external circumstances" of the body are the inertial state of the system, or the location of the body in absolute space, if the law is reconstructed according to a Newtonian framework. Whatever suffices to determine ***a*** from these basic considerations, it is clear that statements that can be checked against experience can also be derived from axioms of the system in question. In this way, Frank's discussion relies on a separation of observational and theoretical terms, which must be connected via operational definitions[81]. From these considerations, Frank gives the following conditional statement: "If Newton's law, $m \times \boldsymbol{a} = \boldsymbol{f}$, is valid relative to [arbitrary system] (*S*) and $\boldsymbol{f} = 0$, it follows that the motion relative to (*S*) is rectilinear" (Frank 1957, p. 113). This purely logical conclusion forms the basis of empirical significance on Frank's account: "We can check by physical measurements whether or not a motion relative to a concrete physical system (*S*) is rectilinear and uniform" (Frank 1957, p. 114). In other words, this is confirmed when, given some (*S*) defined relative to an inertial system and $\boldsymbol{f} = 0$, the motion of a body is indeed rectilinear and uniform.

In this system, mass is constant and is thus independent of velocity. As Frank explains, the status of the above claim and the conditional from which it was derived

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[81] These rules were developed by Percy Bridgman and function much like correspondence rules. See Suppe 1977 for a full account of the relation between operational definitions and correspondence rules. Detailed discussion can be found below and in Chapter 4.

depend on empirical results. Thus, when applied to the motion of high-speed particles, *m* can no longer be treated as constant. For example, "[i]f electrostatic forces are acting in the direction of the actual velocity, high speed particles (i.e., with speeds comparable to the speed of light) obtain accelerations which are noticeably smaller than the acceleration of slow speed particles in the same electrostatic fields" (Frank 1957, p. 115). If the rates of acceleration vary, then the ratio of masses to acceleration given in the operational definition above does not hold because the definition of mass in this system is not constant, and produces a contradiction. That is, ***f/a*** does not have a "physical interpretation" at high speeds. For this reason it is impossible to obtain an empirical interpretation of *m* for electrostatic forces. The conditional above is logically true, and may still be empirically true if applied to movement significantly smaller than the speed of light. That the speed of light is nowhere accounted for in the formal system offered restricts the range of empirical interpretations and, thus, any claim of the law's universal validity.

Here, the reader should notice that in Frank's discussion, empirical significance depends on the ability of a formalized, operational account to be empirically tested. But the testing procedure itself depends on how theoretical terms have been defined; this is why Frank was at pains to define the ratio of masses to accelerations in the example above. From this discussion Frank concludes that mass is not constant and the constancy of mass "can only be introduced into mechanics if the ratio a_2/a_1 turns out to be, according to our experience, independent of the velocities of the bodies concerned" (*ibid.*).

While Uebel was correct to point out that the role of common sense was important to Frank, particularly when Frank discusses the rejection of scientific theories, he was wrong to treat common sense as an important part of Frank's theory of meaning. Following his discussion of Newtonian force, Frank also discusses the role of common sense:

> From Newton's definition it seemed self-evident that the "mass" of a body is constant and cannot depend on its velocity; it seemed obvious from the common-sense image that is invoked by the expression "quantity of matter." If we buy a quantity of meat or of canvas, it seems obvious that this "quantity" is something intrinsic in the meat or canvas, and cannot depend on its velocity (*ibid*.).

As explicated above, the assumption that mass is independent of velocity seems obvious based on our day-to-day experience, even though such an assumption is incorrect. We should note that in this discussion of common sense, Frank makes no mention of the meanings of terms. Rather, he is only highlighting a specific mistake: understanding Newton's laws as having "universal validity" (Frank 1957, p. 115). "This would mean that Newton's equations as a 'formal system' ($m\boldsymbol{a} = \boldsymbol{f}$, where m is a constant) could be applied to all motions that have occurred and will occur in the universe" (*ibid.*). In this example, meaning is given in the formal account described above. And, according to Frank, if we do not carefully attend to this account of meaning then we risk mistaking falsities for common sense. Therefore, despite the importance of common sense within Frank's philosophy of science, it does not function as his account of meaning post-1947.

In the example provided above, Frank's analysis employs several important aspects of verificationist thought. Frank distinguishes formal results from single statements that may be empirically tested. Definitional work and interpretive results are mutually dependent, linking empirical and theoretical terms, as required by all forms of verificationism. Hypothetical statements can be derived and tested only when an empirical interpretation is possible, which is another typical feature of verificationist accounts. Further, it is precisely when an empirical interpretation is not possible – when *definitions* of theoretical terms such as "mass" resist *the possibility* of empirical testing – that Frank argues that the system has failed. This failure is a direct result of the logical inconsistencies produced when the law is interpreted empirically and is not based upon whether or not "mass" is treated as an article of common sense. If we understand scientific terms such as mass by appeal to common sense, then that is indeed a problem. That problem, however, is not a problem of meaning on Frank's account.

Nevertheless, Frank's analysis was clearly distinct from that of the Received View because his work in no way relied on first-order classical logic or on non-classical formulations that closely resemble first-order classical logic[82]. The conditional statements characteristic of Bridgman's operationalism do make an appearance, but without the problematic strictures of either a strictly logical analysis or certain aspects of Bridgman's specific view. The topic of Bridgman's operationalism and its influence on Frank will be addressed in Chapter 4.

With these conclusions in mind, we can now revise Uebel's account. Recall that the first requirement of Uebel's account was summarized as follows:

1. A distinction between observational and theoretical terms is preserved, but empirical significance is not tied to a definition of a theoretical term. Formal methods are de-emphasized, because they inadequately account for the empirical significance of scientific statements.
2. There is a preference for an individualized, exemplar based method whereby a specific account of empirical significance would be provided for a given phenomenon with reference to concrete examples from scientific practice. The

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[82] An example of this can be found in Carnap's *The Logical Syntax of Language,* in "language I." This language is intuitionistic, but includes an axiom similar to the law of the excluded middle, where the disjunction is not universally quantified.

> meaningfulness of metaphysics is then understood by appeal to examples from scientific practice where "commonsensification" occurs.

I suggest the following reformulation of the first part of Uebel's account:

1*. Formal methods may be used, but they need not be given in first order classical logic. Observational and theoretical terms will be distinguished, and theoretical terms will be defined operationally by reference to one or more theoretical terms.
2*. Operational rules will be used to connect theoretical and observational terms. Reference can be made to concrete examples from scientific practice.

So Frank may not have been a verificationist in the traditional sense; he did not use the methods of logical analysis. But, importantly, he did not condemn those methods or reject that philosophical project. The contours of Frank's account seem to be much closer to verificationism than Uebel acknowledges. Indeed Frank not only gives a formal account of meaning, but he also distinguishes between observational and theoretical terms and indicates that they should be connected by operational rules. Since so much of Uebel's discussion hangs on Frank's relationship to logical analysis, it is important to try to understand why logical analysis did not feature prominently in his work.

3.4 Hilbert and Frank in Contemporary Scholarship

In attempting to understand Frank's account and use of mathematics in place of first-order classical logic, we must first clarify how Frank understood formal systems. It is here that the influence of Hilbert is significant, for Hilbert's axiomatization of Euclidean geometry convinced Frank that mathematics, geometry in this case, was a sufficient way of analyzing scientific theories. In other words, the analysis of scientific theories does not require the use of formal systems and, in the context of Frank's philosophy, logical systems are not required for the analysis of scientific theories. In this section I will present and evaluate two arguments about Hilbert's influence on Frank. The dissimilarities between Frank and Hilbert that are presented in these two arguments do not undermine the claim that Hilbert's influence on Frank was significant.

The influence of Hilbert on Frank's work stands out because, "[it] never occurred to the minds of the logical empiricists that Hilbert might take geometry as a 'natural science'…" (Majer 2002, p. 216). Yet, in his 1957 book, that is exactly what Frank does in the chapter "Geometry: An example of a science" (Frank 1957, pp. 48-89). Additionally, Majer argues that the logical empiricists failed to understand Hilbert's "tireless efforts to extend the axiomatic method beyond the domain of geometry to the totality of physical sciences" (*ibid.*). Majer's arguments do not include any consideration of Frank's work, and given Frank's present status in the discipline and the literature, such an oversight is understandable. However, a careful consideration of this part of Frank's work will show that Majer's claims are slightly overstated in his case.

As previously mentioned, in reading Frank's work, one will rarely encounter the method of logical analysis, despite the fact that Frank shared the verificationist epistemology of his colleagues. And to be sure, those beliefs ruled out the acceptance of many of Hilbert's fundamental claims. For example, Frank was concerned with Hilbert's (perceived) silence on the status of spatial intuition as either empirical or *a priori* (Frank 1957, p. 85). It is likely that Frank harbored lingering concerns about the reemergence of Kantian philosophy; he thought Hilbert's agnosticism on the empirical status of spatial intuition presented the possibility of metaphysical interpretations of mathematics.

Frank's stance on the concept of spatial intuition, as outlined above, leaves open the possibility for spatial intuition to be given some kind of empirical interpretation. He does not elaborate on this possibility, but given that Frank does indeed regard geometry as an example of natural science, Majer's suggestion that it never occurred to the logical empiricists to regard geometry as a natural science may be overstated (if only slightly). Turning to the early work of Frank, specifically his 1906 dissertation and *The Law of Causality and its Limits*, Michael Stöltzner points out that Frank may have deliberately avoided addressing critical elements of Hilbert's program – the *Tieferlegung* (deepening the foundations) in particular. "Deepening the foundations" is defined by Stöltzner as "an epistemological reduction availing itself of the unity of mathematical knowledge" (Stöltzner 2002, p. 246). In arguing for his conclusion, Stöltzner suggests that Frank would have prescribed a "rigid boundary between mathematics and physics" (*ibid.*). He argues that Frank's rejection of this aspect of Hilbert's program was the result of Frank's supposed commitment to logicism. However, because Stöltzner's arguments focus only on Frank's work in the 1930's, they may not accurately describe Frank's work during the Harvard Period.

Stöltzner argues that the *Differential- und Integralgleichungen der Mechanik und Physik* (hereafter, the *Frank-Mises*), Frank's co-edited classic textbook on physics, provides one example of his employment of a rigid distinction between mathematics and physics. Indeed, the work was divided into two volumes, one dealing with topics in applied mathematics and the other with topics in theoretical physics (Siegmund-Schultz 2007). But even there, the distinction is not as clear as we might wish it to be. For, as Reinhard Siegmund-Schultz argues, Frank advocated for a "middle ground," where the connection between mathematics and physics "would be the main area treated in the second physical part…" (Siegmund-Schultz 2007, p. 42). This was done despite worries that this volume was too "'mathematically abstract' and not 'physically intuitive'," (*ibid.*). If we follow Siegmund-Schultz's reading of the *Frank-Mises*, Frank may not have held as rigidly to the separation between mathematical and physical concepts as Stöltzner suggests. Stöltzner's argument relies on the claim that Frank adhered to some form of logicism, which requires a sharp distinction between the tautologous statements of logic/math and synthetic empirical claims. While Stöltzner is correct that Frank was careful to distinguish between logical/mathematical and empirical interpretations of geometry (Frank 1957, pp. 69-72), Frank does not discuss logicism at all in his 1957 presentation of geometry as an example of a science. Additionally, Stöltzner argues that Frank would have been skeptical of Hilbert's claims that "a particular axiom-system revealed a pre-established harmony," arguing that Frank would have rejected this as a form of mysticism (2002, p. 256).

It is important to note that these considerations do not undermine the totality of Stöltzner's claim. Rather, it softens the blow, for as Stöltzner points out, the concept of "deepening the foundations" is illustrated in Hilbert's paper "Axiomatic Thought" in at least eight different ways, half of which Stöltzner hypothesizes would have been acceptable to Frank[83]. While Frank did not accept some elements of Hilbert's project, he treated geometry as a kind of science, which set him apart from his peers. And he allowed geometry to perform a role usually reserved for first-order classical logic, explicitly defining terms. In this, Frank did not give up on the basic principles of logical empiricism; he instead formulated those principles differently.

3.5 Frank's Use of Hilbert

In attempting to understand Frank's account and use of mathematics in place of

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[83] For example, Stöltzner claims that Hilbert "treats deepening as a kind of scientific interpretation of mathematics, which gives rise to an empirical verification of basic geometrical concepts" (Stöltzner 2002, p. 258). Frank (1957, pp.69-70) explicitly rejects this idea.

first-order classical logic, we must clarify how Frank understood formal systems. It is here that the influence of Hilbert is significant. For Hilbert's axiomatization of Euclidean geometry convinced Frank that mathematics–geometry in this case–is suitably structured for the analysis of scientific theories. In other words, the analysis of scientific theories does not require the use of formal systems beyond what is already available. In this section, I argue that Frank was influenced by Hilbert's account of geometry and regarded mathematics as the characteristic structure of scientific theories, provided that it could be physically interpreted. Throughout this discussion, we will see that Frank's verificationism is consistent with his use of Hilbert's axiomatization of geometry.

Frank understood the basic point of Hilbert's *Foundations of Geometry* to be:

> ... that geometry should be developed in such a way that one could go from the axioms to the theorems without depending on what the concepts in the axioms meant. If we formulate the properties of straight lines in the axioms, and use in the deductions only these properties of straight lines, we do not need to know what a straight line is (Frank 1957, p. 76).

For example, with regard to the common presentation of Euclidean geometry in textbooks, Frank notes that the property of congruence is not usually defined logically. Rather, the property is defined in that context by the physical process of mapping two rigid, congruent triangles and showing that they have identical shape and size (Frank 1957, p. 70). That is, triangle A and triangle A´ are said to be congruent if it is possible to render A and A´ coincident. Given that coincidence is a physical process, this example tells us about the world but not about the pure notion of congruence. Thus, geometrical systems may be treated as purely logical systems if geometrical concepts such as coincidence (which is a physical interpretation) are eliminated.

To demonstrate this, assume that *A, B,* and *C,* are angles on a triangle and let "≡" denote "congruent to." So, *A´, B´,* and *C´* are angles on a congruent triangle. Frank gives one of Hilbert's axioms of congruence as follows: "If *AB* ≡ *A′B′* and *AC* ≡ *A′C′*, and angle *CAB* ≡ angle *C′A′B′,* then angle *ACB* ≡ angle *A′C′B′* " (Frank 1957, p. 77).

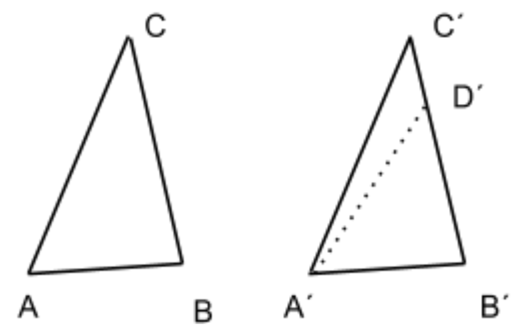

Figure 1 Diagram of Triangles

Frank proceeds to give a proof of the first theorem of congruence: If *AB* ≡ *A′B′* and *AC* ≡ *A′C′*, and angle *CAB* ≡ angle *C′A′B′* then *BC* ≡ line *B′C′*. Overall, the proof is conditional; we will assume the antecedent and derive the consequent. Frank shows that if two lines on a triangle and the angle they form are congruent, then the third line will also be congruent. The antecedent of the theorem is already given: *AB* ≡ *A′B′* and *AC* ≡ *A′C′* and angle *CAB* ≡ angle *C′A′B′*. Assume now that *BC* is not ≡ to *B′C′*. Then, we may assume the existence of a point *D′* on the same line as *C′B′* such that *B′D′* ≡ *BC*. This is because we have assumed that *BC* is not congruent to *B′C′*, which implies that there must be another point *D′* on the same line as *B′C′* that is congruent to *BC*. From this, it follows that *AB* ≡ *A′B′, BC* ≡ *B′D′*, and *AC*≡ *A′D′*. We already know that if there are two congruent sides of a triangle then if follows that their corresponding angle will also be congruent. Applying that information (which is given in the axiom above), we can deduce the following: angle *CAB* ≡ angle *D′A′B′,* angle *ACB* ≡ angle *A′D′B′*. But, we also know that the last conjunct in the antecedent is: angle *CAB* ≡ angle *C′A′B′*. Because angle *CAB* is congruent to both *C′A′B′* and *D′A′B′*, it follows that *B′C′* ≡ *B′D′*.

Therefore $B'C' \equiv BC$[84] (Frank 1957, p. 78)[85].

So by Hilbert's axiom, the idea of congruence can be explicated without reference to empirical content. In this manner, uninterpreted syntactical theories or systems can play the role ascribed to first-order classical logic in traditional accounts of verificationism. They may, as in the case above, stipulate axioms from which particular definitions can be derived. These definitions can be checked for logical consistency, which in turn preserves an account of what Carnap frequently refers to as "analytic truth in formal systems." So, this suggests that Frank's account is indeed much closer to traditional forms of verificationism than it is to Uebel's 2011 account presented above.

Frank's understanding and use of axiomatic methods also fulfills the verificationist requirement that theoretical terms be distinguished from observational terms. This is because Frank's account of congruence, as described above strove to show that congruence functioned as a theoretical term. Additionally, Frank thought that his account was important insofar as it showed that the so-called "mapping process" (or the physical interpretation) is importantly different from the theoretical definition The uninterpreted theoretical terms (i.e., congruence) can be checked for consistency without reference to empirical content. And theoretical terms such as congruence can be rendered physically meaningful only when operational definitions are introduced because those definitions are used to connect theoretical understandings of congruence to a particular physical instance of congruence.

On Frank's account the operational definitions play both connective and semantic

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[84] Transitivity is also an axiom of congruence in Hilbert's system.

[85] This was simplified from Frank's original description of the proof. The diagram was adapted from the same text.

roles. That is, they provide rules to develop an interpretation of a formal system where the rules stipulate testing procedures and admissible empirical content. With regard to the task of this section – trying to understand the way in which Frank may be considered a verificationist, his analysis of theories shows that he was able to maintain a distinction between theoretical and observable content.

The separation of theoretical terms and the logico-mathematical aspects of theories from empirical content is also endorsed by Frank in his intellectual autobiography. (It is important to mention this in order to show that the analysis of congruence offered in 1957 (and discussed above) was not unique to Frank's 1957 view but was instead a key aspect of how Frank understood scientific theories.) In his intellectual autobiography in *Modern Science and its Philosophy* (1949a), Frank explains the importance of preserving this distinction: "The traditional presentation of physical theories frequently consists of a system of statements in which descriptions of observations are mixed with mathematical considerations in such a way that sometimes one cannot distinguish clearly which is which" (Frank 1949a, p. 12). Frank goes on to emphasize that it is crucial that the axioms be understood as "relations between signs," and that, for this reason, this kind of system is "occasionally referred to as a structural system" (Frank 1949a, p. 12). He specifically credits Hilbert's work with providing the idea that "axioms were conventions about the use of geometric terms," stressing that a proper logical analysis must acknowledge this distinction (Frank 1949a, p. 13). To briefly return to the motivation for demarcating between observational and mathematical considerations, analyses such as those offered by Frank are important because they are the means for identifying and removing Platonic or metaphysical elements from systems in the foundations of mathematics. According to Frank, this contribution was essential to the development of logical empiricism.

Despite Hilbert's important contribution, the issue of interpreting formal systems persisted. It is important to note that on Frank's understanding of Hilbert, the role of physical interpretations was not settled. Recall that Hilbert "refused to discuss 'whether our spatial intuition is *a priori* [seeing with the eyes of the mind] or empirical'..." As a result, the interpretation of the "formal system of axioms with the properties of physical bodies was still only described by vague and ambiguous terms like 'spatial intuition.'" (Frank 1957, p. 85). As was shown in Chapter 1, Frank's earliest answer to the question of meaning relied on Poincaré's conventionalism. Frank highlighted the importance of Poincaré in the development of logical empiricism, claiming that he "was a kind of Kant freed from the remnants of medieval scholasticism and anointed with the oil of modern science" (Frank 1949a, p. 8). Einstein convinced Frank that conventionalism would need to be supplemented with pragmatism (Frank 1949a, p. 10) and, in the Harvard period, Frank saw his work "as an attempt to integrate Mach, Poincaré, and Einstein" with regard to the question of supplying meaning to formal systems[86] (Frank 1949a, p. 44). As Einstein pointed out to Frank, conventionalism must be restricted by pragmatic considerations because some conventions are not useful in the context of scientific practice (Frank 1949a, p. 10-11). As Frank's mature philosophy of science developed, his interest in pragmatism became significantly informed by Percy Bridgman's operationalism.

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[86] Frank (1949a, p. 44) goes on to note that Reichenbach "[pointed] out that what is needed is a bridge between the symbolic system of axioms and the protocols of the laboratory."

In the Harvard period, Frank primarily used Bridgman's operationalism to discuss formal languages and theoretical terms:

> Bridgman stressed the point that any term, *e.g.,* "straight line," which occurs in axiomatic geometry must be coordinated with a technical procedure for manufacturing the object described by this term. Every procedure of this kind can be described in terms of our everyday language, hence the name "operational definition." The core of the definition is the reduction to "physical operations." (Frank 1957, p. 87).

The procedure mentioned by Frank requires "a description of physical operations" (Frank 1949a, p. 44) that one might perform in the laboratory in order to measure or use some phenomenon. Frank's use of Bridgman's operationalism requires only that a measuring device provide the physical interpretation (Frank 1957, p. 80; SFV Part II, Ch. VI, Section 6, pg. 7). However, Frank's particular use of Bridgman varies from Bridgman's account of operationalism significantly. (This will be discussed in Chapter 4). Frank argues that operational processes are reliable insofar as they can be taught, used, and repeated by other scientists (Frank 1957, p. 141).

In *Science, Facts, and Values* Frank classifies Bridgman's work as pragmatic, explicitly linking his view to those of Peirce and Neurath (SFV, pt. II, ch. VI). The pragmatic component of Bridgman's philosophy emphasized by Frank is that operationalism provided a program of action for the scientist. "His work entailed carrying out a program of action, the question that concerned him was how this system of planned action was connected with the system called a physical theory" (SFV pt. II, ch. VI, pg. 7). This program might involve oral, written, or experimental work. Consider the act of defining a triangle. It can be demonstrated that a given triangle meets the definition of "triangle" in a Euclidean framework.[87] Frank, however, stops short of saying that the operational definition of a triangle confirms Euclid's theory; instead he asserts only that a particular physical interpretation is confirmed. Thus, "we say that Euclidean geometry is 'true' in the sense that it has a certain application for us" (Frank 1957, p. 80). This discussion glosses over the fact that Bridgman failed to distinguish between the semantics and pragmatics of scientific theories. In the former case, operationalism can be used to interpret the truth of a formal system and in the latter case, it can be a semiotic interpretation (an interpretation of the signs, a topic researched by Charles Morris and commonly identified as one facet of the pragmatics of scientific theories). Grünbaum (1954) argues that operationalism can be usefully applied to the latter topics, but not the former. This discussion will be addressed in Chapter 4, where I will argue that Frank does not mistake semantic concerns for pragmatic concerns.

According to Frank, theoretical terms are distinct from empirical terms, and each plays an important role in how we understand the meaning of scientific theories. Moreover, these two sets of terms are connected by correspondence rules. This account is undeniably similar to a traditional account of logical empiricism. The only notable difference in Frank's work is that he never makes explicit use of formal logic in his arguments:

Thus, experiment can never confirm a system of geometrical axioms, but only a

[87] Frank acknowledges that Bridgman's operationalism, even when construed pragmatically, does not work well for "theories of high generality." As a result of Bridgman's particular account of operationalism, the concept of length is tied to a particular measurement. So, every type of measurement would result in a unique concept of length. As a result, it would be difficult to classify these accounts and incorporate them into a single theory of meaning. Frank suggests that a behaviorist theory, or even a sociological analysis would help to clarify the problem and cites Bridgman as agreeing. However, Chang (2009) challenges the idea that Bridgman would have agreed. These differences will also be explored in Chapter 4. Sociology and behavior will be discussed in more detail below. For Frank's full account, see (SFV Part II, Ch. VI, Section 6).

> "geometry" *plus* a physical interpretation of it. The problem is always this: If the enlarged system, consisting of the axioms of geometry plus their physical interpretation, fails to check with experiment, we can drop either one or the other part of the system. We have repeatedly stressed the point that the formalized system of geometry does not tell us anything about the world of physical experiments, but consists of "arbitrary" definitions ... . If there were nothing in the physical world which fulfilled the axioms of geometry (*e.g.*, no rigid bodies), that system of conventions would be of no practical interest because it could not be applied to anything. Nevertheless, because of its if-then character, geometry would remain true. Thus, we can say that such logical structures as geometry are true by themselves, independent of what happens in the world and independent of the meaning of their terms. The meaning of their terms is irrelevant. (Frank 1957, p. 81).

Physical interpretation, on Frank's account, is thus necessary in order to apply laws in physics, but not to decide the internal validity of the system (which is a purely logical question). And in this sense, Frank's view appears to be rather commonplace for his time; he is clearly endorsing a philosophical account of meaning that is very close to verificationism, despite the fact that Frank's particular exegesis of verificationism seems to have been influenced by Hilbert's axiomatization of geometry.

Frank even quotes Einstein in order to advance key elements of verificationist thought:

> The progress attained by axiomatic geometry consists in the clear separation of the logical from the factual and intuitive content. According to axiomatic geometry, only the logical-formal is the object of mathematics; but not the intuitive content that is connected with the logical-formal ... The statements about physical objects are obtained by coordinating with the empty concepts of axiomatic geometry observable objects of physical reality. In particular: solid bodies behave according to theorems of three-dimensional Euclidean geometry. (Einstein, *Geometry and Experience,* 1921, quoted in Frank 1957, p. 87)[88].

In the chapter "Geometry: An example of a science" in his *Philosophy of Science: The Link Between Science and Philosophy* (1957)*,* Frank sought to bring logical empiricism

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[88] *Lecture before the Prussian Academy of Sciences, January 27, 1921*

to bear on the analysis of scientific theories. In so doing, he more or less maintained a verificationist criterion of meaning. That is, he preserved the separation of theoretical and observational vocabulary, of formal and empirical notions of truth (a version of the analytic/synthetic distinction), the importance of testability, and the assurance of the role of testability through his use of correspondence rules. Thus, with regard to the issue of verificationism, Frank's view did not differ substantially from that of his colleagues. Rather, the apparent differences seem to be limited to the fact that Frank did not provide a general logical framework by which theories could be analyzed, which, as I have argued, seems to have been motivated by the influence of Hilbert.

Frank drew on Hilbert to develop a structuralist-formalist understanding of scientific theories. The structures used by Frank in this analysis were uninterpreted mathematical systems. Semantic content, which is used to render a scientific theory testable, is introduced only by a physical interpretation of the theory. This is to say that the semantic element of Frank's theory is derived from the language-relative truth of systems, which are tested only after an interpretation is developed. Frank's analysis of geometry (an example of his general approach to analysis) functionally treats geometry as a model by adding semantic content to logical-mathematical systems. And in this way, we are compelled to conclude that Frank was indeed a verificationist. While it may be disappointing to draw such a conclusion, I argue that Frank applied his verificationism to a unique set of problems and that this is where his contribution lies.

3.6 The Development of Frank Conception of Social Factors and the Influence of Scholastic Thought

Throughout Frank's writing on the formal elements of his thought, some of which has been discussed above, there is a lingering worry about the influence of scholastic worldviews. Indeed scholasticism provides one influence by which scientific theories might be problematically misinterpreted, as discussed in section 3.3. And one of the problematic features of scholastic truth (involving claims that are deemed universally true because they can be derived from first principles), which Frank calls "philosophical truth," is mentioned by Uebel 2011 as a motivation for Frank's supposed dismissal of verificationism. But, now that we know that Frank did not fully reject verificationism, we should try to understand how his concern with scholastic thought figured into his philosophy of science.

In this section, I argue that Frank's concern with scholastic thought motivated his development an account whereby (what I call) "social factors" can figure into a traditional logical empiricist account of meaning. There are two kinds of social factors that play a role in Frank's mature account of meaning. The first is negative and allows him to identify and eliminate ideological biases. The second kind of social factor is positive, and allows Frank to explain how metaphysical claims are meaningful because it accounts for how political and social beliefs affect our actions. By associating actions and beliefs, Frank argues that we can empirically understand the effects of metaphysical beliefs.

Frank's form of analysis stands in contrast to a scholastic, or Thomistic, understanding of science and philosophy. Scholastic philosophy sought to develop a stable conceptual framework that did not simply ensure that a particular description of facts might hold true given a particular interpretation, but that this description was necessarily true. Frank appeals to the Thomistic distinction between two criteria for believing that a statement is true:

> One reason for believing a statement is that we can derive results from it which can be checked by observation; in other words, we believe in a statement because of its consequences. For example, we believe in Newton's laws because we can calculate from them the motions of the celestial bodies. The second reason for belief – and medieval philosophy considered this to be the highest one – is that we can believe a statement because it can be derived logically from intelligible principles (Frank 1957, p. 16).

The first criterion of scholastic thought, which Frank calls "the scientific criterion" (*ibid.*), requires only that the theory should account for physical phenomena. This criterion alone is far too permissive, for it only requires "that these principles may be right, but it does not follow that they must be right" (Frank 1957, p. 16).

In a 1944 essay[89], Frank addressed the dual notion of "philosophical" or scholastic truth that is characteristic of Thomistic philosophy. In both essays, the first notion of truth is developed from Osiander's account of empirical adequacy. When Copernicus published his account of the heliocentric solar system, Osiander added a preface explaining that the reader need not believe in the truth of Copernicus' system. Instead, Osiander argued that the reader need only believe that the system is empirically adequate, or that it explains the phenomena.

> The hypotheses of this book are not necessarily true or even probable. Only one thing matters. They must lead by computation to results that are in agreement with the observed phenomena (Osiander [1543], 'Ad lectorem de hypothesibus huius operis,' Osiander's unsigned preface to *The Revolutions of the Celestial Bodies*, quoted in Frank 1944/1949, p. 223)

Hence the issue of truth was separated from issues having to do with empirical adequacy, scientific fecundity or the explanatory power of the heliocentric system. If the models are regarded as scientific principles only, then Frank is right to conclude, "our observations

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[89] This earlier essay of Frank's was used because it is consistent with the arguments given by Frank in 1957 and because I believe that his discussions in 1944 add depth to his later discussions, helping us to better understand Frank's argument.

cannot decide between two principles" (Frank 1957, p. 16). This intellectual permissiveness would allow the Catholic Church to continue to endorse the truth of the geocentric system while also allowing for the technical application of Copernicus' system. However, that same permissiveness will not help us to uniquely confirm[90] the theory. As we can see from the description above, the scientific criterion does not select only one theory. Frank (1957, p. 16) explains that this is so because the scientific criterion does not give us any guidance with regard to what hypothesis we ought to prefer. Many different hypotheses may be confirmed by the same set of observations.

The second aspect of a Thomistic conception of truth requires appeal to first principles, which are regarded by its adherents as the unshakeable foundations of belief. By appeal to these principles, one theory can be uniquely selected for acceptance as true. Necessary truth can be ensured by deduction from these first principles, which are immediately understandable (i.e., "intelligible") and are taken to be self-evident. On Frank's analysis, this requires that the principles be expressible in ordinary, uncomplicated language. However, why a particular principle is regarded as intelligible will vary. For example, it may fit within a religious view that is already held, or it may seem to explain a large body of empirical evidence. In any case, the essential element of these kinds of principles is that they are stated in ordinary and simple language. On Frank's analysis, such principles are rooted in school philosophy (discussed in Chapter 1). In the context of Thomistic thought, these intelligible principles are part of an "organismic" view, one that pictures everything acting as an organism would – i.e.,

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[90] In this section (from the 1957 book), Frank uses both "truth" and "confirmation," but says that the "correct way of speaking is to say that experiment 'confirms' a certain hypothesis" (Frank 1957, p. 16).

intelligibly.

> It was believed that everything had a certain nature, and acted according to this nature, which was meant for a certain purpose – the nature of a bird was to fly, of a frog to jump, of a doctor to cure (optimistically speaking), of a stone to fall down, of smoke to go up, of celestial bodies to move in permanent circular motion. Everything acted according to its nature. In a general way, without details, one could derive from this statement how a stone would behave, etc. (Frank 1957, p. 23).

Frank (1957) goes on to ask why it is that people persist with outdated descriptions of the world. He eventually suggests that scholastic philosophers thought these (outdated) principles were more intelligible and possessed greater dignity. These principles originate from "vague analogies with everyday experience," analogies that seem plausible (Frank 1957, p. 41). In other words, these analogies satisfy our common-sense assumptions. So, when a scientific theory contradicts our common-sense assumptions, we are inclined to reject it.

But, what is the nature of this rejection? Scientific systems may be rejected because of a desire to preserve consistency within our set of philosophical beliefs. This desire to preserve consistency is pragmatic, for it figures into what we do with our beliefs (e.g., whether we accept theory x or not) and not how we form those beliefs. For this reason, it is important to understand the pragmatic effect of intelligible principles.

> In a way, these "intelligible" principles are more practical in their effects than the physical principles. The technical effects of science are more indirect than a blunt command to someone as to what he must do. Thus the most general principles are also practical but on a different level – in a way, they are more practical. Bluntly speaking, science proper provides us with the technical means by which we can produce weapons to defeat the enemy, but the philosophic interpretation of science can direct man in such a way that he makes actual use of the weapons (Frank 1957, p. 19).

The introduction of social factors is Frank's attempt to address the pragmatic problems introduced by metaphysical systems of thought (in this case, those that reference intelligible principles). While Frank does not think that the philosophical system (Thomism) on which the principles he is considering are based is tenable, he seems to be primarily concerned with the social effects of these systems of thought. Thus Frank seeks to analyze Thomism and other troubling variants of school philosophy by understanding them through the application of logical empiricist thought. Frank achieves this goal by empirically understanding how these systems of belief seek to direct human conduct. So, in addition to explaining why he thinks Thomistic philosophy persisted, Frank also seeks to understand its social role and effects. He asks: "On what grounds is such a statement accepted? What is its practical function? Such statements have just as practical results as the scientific ones; they have a direct effect on human behavior" (Frank 1957, p. 37). In the next section we will examine why Frank regards "effects on human behavior" as a practical result.

3.7 The Social Factors

The role and importance of human behavior frequently figures into Frank's analysis of scientific theories. As I argued in Chapter 1, this interest evolved slowly throughout Frank's life, and only became a constitutive aspect of his work in the Harvard period. Indeed, Frank says that 1947 was when he began investigating the meaning of metaphysical statements (Frank 1949a, p. 51). He frequently attended the Conference on Science, Philosophy and Religion during the Harvard period, and his attention to social factors seems to have been developed in light of this experience (*ibid*.). This is because throughout his time attending the conference Frank witnessed many rejections of scientific theories and many arguments against the intrinsic value of science (Frank

1950). In *Science, Facts, and Values,* which was drafted post-1947[91], he wrote:

> The salient point is that metaphysical propositions about the physical universe are actually meaningful propositions about human behavior or, in other terms, propositions of sociology... The point is that, according to the opinions of the Vienna Circle, metaphysical propositions about the physical world are meaningless within the system of physical concepts, but have meaning within the wider "universe of discourse" that embraces physical and sociological concepts. (Frank SFV, Part II Chapter 5 section 4 p. 8)

Human behavior can be treated as empirical content that Frank uses to operationalize metaphysical statements because these statements have an observable effect on human behavior. Since human behavior is analyzed in the science of sociology, it is possible to understand the sociological effect of metaphysical beliefs in a manner that is consistent with logical empiricist thought. However, the grounds on which metaphysical content is operationalized may vary. In this section, I will argue that Frank's discussion of social factors (a phrase that is mine rather than his) requires that philosophers understand metaphysical statements as statements about human behavior. While Frank did not use the term "social factors," these factors nonetheless play important roles in his thought post-1947. I use this term in order to highlight the fact that sociology played a prominent role in Frank's mature philosophy of science.

The social factors are the means by which Frank understands metaphysical statements as meaningful and the means by which he analyzes interpretations of scientific theories. To better understand how the social factors operate in Frank's thought, we shall examine *Science, Facts, and Values*, Part I, Chapter V. I will use Frank's work in Chapter V to show that there are two types of social factors, one negative and one positive. Negative social factors result in rejections of scientific theories that clash with cherished

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[91] George Reisch indicates that the manuscript was written between 1956-1963.

metaphysical beliefs, while positive social factors allow us accept certain metaphysical beliefs as meaningful if they connect to actions in an appropriate manner. This way of understanding the meaning of metaphysical statements occurs at the level of interpretation, when what counts as a "fact" is determined. If the "fact" is an instance of philosophical truth (in the sense described above), then we will appeal to negative social factors. These result in an instance of theory rejection, and rely on a philosophical notion of truth whereby consistency with previously held beliefs is prized. In this case, the motivation for rejecting a particular scientific theory lies in the realm of the pragmatics of scientific theories (since it is motivated by extra-scientific reasons and is not a legitimate account of meaning that accords with verificationism). Frank's account of positive social factors allow us to operationalize social concepts and to treat certain metaphysical beliefs as meaningful, if they affect the actions of an individual or group. These factors have to do with meaningfulness, insofar as actions are treated as observable quantities that may be scientifically verified. We will begin with the negative social factors.

Frank's analysis of negative social factors begins with a discussion of Marxist accounts of ideology. In the Marxist tradition, ideology is structured by class distinctions, where the class interests of the bourgeoisie effectively bias members of that class. This bias limits their ability to understand the world as it is, and on Marx's account, this is why members of the bourgeoisie cannot have class-consciousness. Their view of the world is shaped by their need to serve their class interests and hence is never a reliable source of facts. On this reading, ideology is a distortion of truth. Furthermore, Frank aligns this account with one task of the pragmatic conception of truth:

> In this respect the Marxist [doctrine] runs parallel to the pragmatic theory of truth. If we ask for the "operational meaning" of a statement which is formulated in

> very abstract terms, we challenge the author of the statement to "show cause" why we should not regard it as meaningless. The method of "operational definition" is explained by examples taken from geometry, Newtonian Mechanics, relativistic and subatomic mechanics; it is presented, in a general way, in the chapter "science of science." In the working of that method, we can often note a "debunking" process. In some way, the theory of relativity "debunks" the use of concepts like "mass of a body" or even "length of a body" in the sense in which they had been used traditional physics. We have learned that we can keep on using these old concepts although this is not practical from a purely scientific point of view. We have learned that the use of the old concepts allows in some cases for a metaphysical interpretation that is believed to be useful for supporting a desirable way of life. In such cases, the discourse in which the old concepts are used usually plays the role of an "ideological" discourse in the Marxist sense. (SFV, Part I, Ch. 5, section II, p. 2).[92]

One important feature of science, on Frank's account, is that it "debunks" old concepts in favor of better ones. (An example of this is given in section 3.3 above, when Frank discusses mass as a ratio of accelerations.) Conversely, ideologically motivated theories neither employ operational definitions nor debunk previously held beliefs (as a result of their commitment to philosophical truth). Rather, in the service of "old concepts," ideological theories serve to direct human conduct. Such direction results in the rejection of scientific theories and also in ideologically motivated actions.

On Frank's account, social factors can function by biasing us, though he denies that social conditioning can play any role in justifying a scientific theory. "[Social] factors certainly determine the life situation of the author, but they cannot influence the validity of a pronounced statement" (SFV, pt. I, ch. V, p. 4). This argument is not unique to *Science, Facts, and Values*; in a 1956 paper, Frank also argues that ideology does not affect the justification of scientific theories:

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[92] Frank may be referring to the chapter the "Science of Science" from the book *Philosophy of Science: The Link Between Science and Philosophy.* This chapter was discussed in the preceding section, 3.6.

> Summing up, we have to say that every condition of indecision in the general principles of science will bring about interference of authorities. They regard it as their duty to support the teaching and the advances of such doctrines as have a healthy influence on morals and religion. The search for technical results is to a high degree free, but the philosophical interpretations of scientific theories have been always under the strong influence or even pressure of "authorities" (Frank 1956, p. 338).

So, negative social factors are associated with "interference" by an ideologically motivated group (e.g., "authorities") that promotes adherence to some form of philosophical truth. A philosophical interpretation, such as a scholastic interpretation, does not change the meaning of the theory on Frank's account. Rather, it only affects whether or not the theory is accepted or rejected. Therefore, negative social factors function only in the context of the pragmatics of scientific theories.

While the Marxist account of ideology serves only a negative function, Frank also discusses a positive account, or how social factors can contribute to how a theory is understood. Recall that his account of scientific theories requires a formal structure and a semantic interpretation of that structure. The semantic interpretation is connected to the formal structure of a theory and is therefore the means by which a scientific theory gains empirical significance. Taking into account positive social factors allows us to understand human behavior as an empirical category within the context of sociology.

Recall that positive social factors allow us to operationalize social concepts and to treat certain metaphysical beliefs as meaningful insofar as they affect the actions of an individual or group. Since sociological observations furnish theoretical statements with an empirical interpretation, positive social factors make metaphysical claims meaningful "within the wider 'universe of discourse' that embraces physical and sociological concepts" (Frank SFV, Part II Chapter 5 section 4 p. 8).

Frank provides the following explanation of how the positive social factors might be applied:

> "State" and "nation" will not be treated as natural wholes but will be described by combining recurrent traits of human life and human history as consisting of larger agglomerations like "state," "nation," "national spirit," "loyalty to his faith," which will reappear at any point in human history. The adherents of radical liberalism have been inclined to build up human history by using elementary traits of behavior, like conditioned reflexes, memory, etc. By using this description, we can be fairly certain that the basic concepts (e.g., conditioned reflex) will be relevant at every point of human history ... (SFV, pt. I, ch. V, p. 6).

Effectively, what Frank means is that the basic concepts measured by or in a sociological theory will characterize how subsequent events will be understood. In this case, the term "conditioned reflex" derives from liberalism. So, if "conditioned reflex" is measured at the outset of a theoretical analysis and new social conditions are subsequently observed, the meaning of those conditions is related to liberalism. Therefore, since "conditioned reflex" is strongly associated with liberalism, we should not be surprised to find that future observations operationalized by a measuring these conditioned reflexes continue to be related to liberalism. Thus, the "basic concepts" of liberalism "will be relevant at every point of human history." Further, if "conditioned reflex" derives from a larger normative theory, such as "radical liberalism," then the normative concepts of liberalism have in part characterized the unit of measurement of the theory (if Frank is right that "conditioned reflex" is indeed characteristic of liberal thought).

Unfortunately, Frank wrote *Science, Facts, and Values* at a time when the methods of sociology were still underdeveloped. And there is no discussion of social science methods or methodologies (the theoretical underpinnings of different research strategies, which include methods) in any of his work. For this reason, it will be helpful to apply his analysis to a contemporary example. We will now consider the difference between utility metrics and the capability metrics that have been proposed by Amartya

Sen and Martha Nussbaum.

A classical utility metric measures an outcome relative to an experimental intervention or some unit of time where that outcome is understood as an indicator of the health of a population. These measures identify a particular variable that is thought to indicate how healthy one population is relative to another population (or relative to a predetermined account which is used to define "healthy"). For example, the survey, "Quality Adjusted Life Years" (QALY) operationalizes[93] health as "a unit that combines both quantity (length) of life and health-related quality of life into a single measure of health gain" (Simon *et al.* 2013). However, this utility score does not take into account other fundamental features of human well-being, such as whether or not women have the same opportunities as men, if wealth is equitably distributed, and how "happy" people are. The diversity of opinion about what constitutes well-being in different regions, for different people, and within the context of different cultures cannot be captured if the only thing we measure is how long people live.

Utility metrics can lack the cultural flexibility and sensitivity that is necessary to understand the *quality* of life experienced by people around the world. And indeed when such measures are used, we impose a particular view of quality of life. The capabilities approach is one method of understanding well-being based on how a variety of social factors affect people's ability choose and pursue a life that they deem worthwhile. To avoid an overly narrow measurement, Nussbaum identifies ten essential capabilities: life; bodily health; bodily integrity; emotions; affiliation; control over one's environment;

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93 "Operationalize" is the term used by in the paper.

practical reason; other species; play; and, senses, imagination and thought (Nussbaum 2003). The items in this list are considered as capabilities because they are the areas in which a person should be able to freely function, or undertake an activity that they deem to be valuable. To reflect this broader view of well-being, Simon *et al.* (2013) developed an alternative survey based on Nussbaum's 10 essential capabilities. Their research has shown that studies that rely on QALY data, all things being equal, will not disagree with a capacities analysis on the "facts," on descriptive measures such as mortality and morbidity rates; nor will they disagree on the fundamentals of statistical analysis and other background assumptions often made in the social sciences. Rather, the disagreement lies in the social assumptions that we use to analyze human experiences.

When performing such an analysis we designate certain behaviors or actions as important. For example, QALY data operationalize longevity as the basic concept for human well-being. However, it is entirely possible that while someone lived a long life, he or she did not live a good life. The capabilities approach strives to understand this aspect of life, or the quality of life relative to a person's ability to achieve a life that they find worth living (Nussbaum & Sen 1993). Simon *et al.* (2013) and Anand *et al.* (2009) have both operationalized Nussbaum's criteria in order to measure how various health-related interventions improve not just longevity, but also emotional and social well-being. To use either theory in the design of a study requires commitment to a particular view of well-being that in turn constitutes a particular description of facts. These varying theoretical accounts also help us to understand the kinds of interventions that will promote positive outcomes relative to one's social and political commitments.

Theoretical and philosophical differences in scientific work, such as in the instance discussed above, are not differences in the practice of science, but represent the multiplicity of philosophical systems that underwrite the kinds of scientific work that we choose to engage in. The use of a particular philosophical system will guide, and may even structure, what observations are deemed relevant. Quantitative tools, such as the QALY surveys described above, allow us to operationalize different social beliefs. Our preferences for one or another measurement indicate views that extend beyond scientific practice, and reflect political or social beliefs. So long as these beliefs do not lose their hypothetical character by becoming instances of philosophical truth, they are empirically admissible.

Frank's positive social factors make the role of social beliefs explicit. If we are trying to understand how some forms of loan, say, improve the well-being of individuals, and if well-being is understood in terms of mortality rate or GDP growth, then other facts, such as local women's ability to access financial resources, may not be reflected. The fact that an intervention had some measurable effect on well-being cannot be separated from the initial assumptions regarding the definition of well-being formulated in the study in question. In this way, "there is no theory without experience," nor "are there facts without theory" (Frank SFV, I, Ch. V, section 8, p. 10). Facts themselves are determined by our theories. In this case, whatever facts we identify are a result of philosophical theories, which can include social and political assumptions. As with physical theories, metaphysical statements are understood relative to the set of actions they may cause.

On Frank's systematic account, there can then be two modes of understanding metaphysical statements. The first, inspired by the Marxist account of ideology, occurs in the pragmatic context and results in the rejection of scientific theories in favor of so-called "philosophical truth." The second (positive) mode identifies what the belief in metaphysical systems tells us about the people who hold those beliefs. Following Dewey, Frank argues "metaphysical propositions are not propositions about the objective reality of the physical universe, but propositions about human aspirations" (SFV, part II, Ch. VI, sect. 6, p. 8)[94]; he later claims that these are "propositions about human behavior" or "propositions of sociology" (*ibid*.). These propositions, as propositions of sociology, are therefore empirically significant in the context of social science. In the next chapter, we will evaluate Frank's use of pragmatism and operationalism.

Now let us return once more to one of Uebel's main points. Recall that Uebel defined Frank's account of meaning by virtue of its ability to demarcate metaphysics from science. This was summarized as follows:

1. A distinction between observational and theoretical terms is preserved, but empirical significance is not tied to a definition of a theoretical term. Formal methods are de-emphasized, because they inadequately account for the empirical significance of scientific statements.
2. There is a preference for an individualized, exemplar based method whereby a specific account of empirical significance would be provided for a given phenomenon with reference to concrete examples from scientific practice. The meaningfulness of metaphysics is then understood by appeal to examples from scientific practice where "commonsensification" occurs.
3. Metaphysics would be rendered meaningless by virtue of its resistance to empirical testing; metaphysics depends on absolute notions of truth.

As we saw in previous sections, this resulted in a conflation of two issues, meaning and demarcation. Furthermore, 1 and 2 proved to be inadequate accounts of Frank's conception of meaning. As a result, Frank was perhaps more of a verificationist than Uebel initially judged. However, even though the social factors do not contradict the

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[94] Frank says that this is similar to Carnap's idea of "attitudes towards life."

traditional account of verification, they do extend it. Verificationism is summarized below:

A. A distinction is made between observational and theoretical languages and vocabularies. Each language is explicated formally, usually using first order classical logic.
B. Correspondence rules are then used to connect the vocabularies of the observational and theoretical languages.
C. On this account, pragmatic concerns are meta-theoretical, with respect to the theoretical and observational languages.

C clearly covers negative social factors. But, since Frank attempted to operationalize metaphysical beliefs in terms of positive social factors, there is no obvious way to categorize positive social factors in the system outlined above. So, I will add to the recommendations that were made in 3.3 (additions are italicized):

1*. Formal methods may be used, but they need not be given in first order classical logic. Observational and theoretical terms will be distinguished, and theoretical terms will be defined operationally by reference to one or more theoretical terms. *Observation statements can make reference to either physical or sociological phenomena. Social phenomena can be used to render metaphysical beliefs meaningful.*
2*. Operational rules will be used to connect theoretical and observational terms. Reference can be made to concrete examples from scientific practice.
3*. *In the context of the pragmatics of scientific theories, metaphysical beliefs motivate rejections of scientific theories. Pragmatic concerns are meta-theoretical, with respect to the theoretical and observational languages.*

Frank's account of meaning certainly adds to verificationism, for his contemporaries simply did not address many of the issues that most interested him. Uebel (2011) provided many useful insights into Frank's work, but as we have seen, Frank's project can best be thought of as an extension of verificationism rather than a rejection of that embattled doctrine.

3.8 Conclusion

In this chapter we saw that Frank offered a unique account of verificationism. In line with the logical empiricist tradition, Frank distinguished between the logical and mathematical structure of scientific theories and the physical interpretations by which those theories were judged. Frank also adhered to tradition by relying on operational definitions to connect the logico-mathematical structures to the empirical interpretations of those structures. However, Frank's particular use of Hilbert and his long-standing interest in the sociology of science sets him apart from the tradition.

Unlike his contemporaries, Frank seems to have been willing to take Hilbert's work on geometry as an example of science. Frank was not a formalist, and Stöltzner was correct to caution against reading Frank in that way. However, Stöltzner's conclusion that Frank adhered to some form of logicism is overstated, especially with regard to Frank's work during the Harvard period. As we saw, Hilbert exerted more influence on Frank's work than did any logicist. And Frank's treatment of geometry is unquestionably influenced by Hilbert. This influence serves to distance Frank somewhat from the method of logical analysis (though he stops short of rejecting it) and instead locates Frank's preferred mode of formal analysis within the formal methods already used by scientists.

Frank also stood apart from his colleagues in trying to understand the meaning of metaphysical statements. This was done consistently within a logical empiricist account of meaning because Frank took metaphysical statements to be forms of speech that could be studied as empirical phenomena by virtue of their ability to affect our actions. The social factors that I identified in Frank's work, both positive and negative, represent unique applications of logical empiricist thought. Through attention to social factors, a variety of social and political issues relevant for science can be identified and discussed.

4 Chapter Four

4.1 Introduction

Percy Bridgman is described by Peter Galison as a "high-pressure experimentalist… inventor of operationalism and one of the strongest American prewar boosters of the Unity of Science movement (having also served on the Inter-Scientific Discussion Group steering committee)" (Galison 1998, p. 61). A sign of his importance to both Frank and other logical empiricists is that Bridgman gave the inaugural lecture, "Analysis of Fundamentals in Thermodynamics," to the Inter-Scientific Discussion Group on October 31st, 1940 (Hardcastle 2003, pp. 172-173). In addition to his work with the Inter-Scientific Discussion Group, Bridgman also won a Nobel Prize in physics, for his work in high-pressure physics (Holton 2002). Gerald Holton, one of Bridgman's students, provides an excellent description of Bridgman in the lab, from which we can begin to understand the origins of Bridgman's philosophical thought:

> I believe that Bridgman could not be fully understood unless one watched him day after day, as he was doing his lab work, coming in early, usually on his bicycle, practically before every body else except the shop crew. He would change immediately into his well-used lab coat and work on new equipment at the lathe, making much of his apparatus by himself, and race back and forth between the experiment and measuring instruments during his daily runs. Except for his part-time helper in the shop, and an assistant who, also on a part-time basis, helped him with readings, he preferred to be alone in his fairly narrow and cramped surroundings exuding energy and seriousness and accomplishing, in a few hours, a typical run that might take me, a student working near-by, a day or more (Holton 2002, p. 340).

This emphasis on Bridgman's solitude is not merely a description of his personality, but in addition, Holton is subtly gesturing to an important part of Bridgman's thought. The idea is picked up again in another of Holton's descriptions: "Bridgman's struggle was always with himself, with his own understanding, with his attempts to think situations through to his own satisfaction" (Holton 2002, p. 341). Appealing to Mach, Bridgman wished to understand science in the light of "performable action" (*ibid.*). And, in his philosophical account of these actions – operationalism – Bridgman emphasized the value of personal, individual knowledge, "so much so that he was accused of solipsism, to which he scarcely objected" (*ibid.*).

In the context of late forms of logical empiricism, operationalism was generally understood as an alternative presentation of correspondence rules. For example, in Suppe's classic account, operationalism was described as "nothing other than a special version of the requirement that correspondence rules be explicitly defined" (1977, p. 19). Explicit definitions of correspondence rules were given in first-order logic and were intended to connect empirical terms with the theoretical terms found in rationally reconstructed scientific theories. In this context, operational definitions would be descriptions of the experimental or testing procedures used to measure some phenomenon. We will return to the operationalization of length in the following sections; for now let us continue to discuss operationalism in the context of the Received View.

Seemingly simple issues increasingly vexed the project of developing and defending correspondence rules. The Received View account of operational definitions was not immune to these criticisms, insofar as operational definitions were taken to be forms of correspondence rules. For example, consider the problems regarding how dispositional properties like "fragile" could be explicitly defined (Suppe 1977, p. 21)[95].

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[95] For additional discussions of correspondence rules see Carnap 1936 and for an additional account of operationalism see Hempel 1965, pp. 123-133. Carnap addressed criticisms of correspondence rules, and of

(If the glass is struck with a certain force, then it will break. But what if it is never struck?) Should this issue remain unresolved (as it did), then explicitly stating how simple properties have cognitive significance would seem to be impossible. Various attempts to resolve this issue met with serious problems[96]. And so operationalism was relegated to the status of a promising, but ultimately unsuccessful theory of correspondence rules.

As a result, Frank's heavy reliance on operationalism, as discussed in Chapter 3, should be troubling. For if operationalism failed and Frank frequently used it, then it would seem that Frank's view must face the same fate. Furthermore, Bridgman's individualism seems to contradict the socially engaged picture of Frank's work that I argued for at length in Chapter 2. Such conclusions would, however, be hasty. There are few discussions of operationalism in the contemporary literature, and even fewer that discuss operationalism in the context of Frank's thought. Holton is the notable exception; he provides some very helpful historical discussions (1992, 1993, 1995, & 2006). But it remains to be seen whether Frank's reliance on operationalism undermines his overall approach. I argue that even though Frank frequently refers to Bridgman's work on operationalism in the Harvard period, he seems not to have embraced some of the more problematic aspects of Bridgman's view.

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dispositional terms in particular, late into his career. But, despite Carnap's tenacity, these accounts became increasingly complex, and were far removed from scientific practice. See Carnap 1956 for an example of how he used reduction sentences in order to address the problem of dispositional properties. See Suppe 1977 for an explanation of why these attempts failed to adequately describe scientific practice.

[96] See Chapter 2 of Frank 1954. This edited volume includes papers by Margenau, Bergmann, Hempel, Lindsay, Bridgman, Seeger, and Grünbaum.

In the present chapter we will see that Frank's account of operationalism does not succumb to the same flaws as did Bridgman's. Operationalism will first be discussed broadly and will then be evaluated, paying particular attention to issues that might undermine Frank's work. Specifically, those are Bridgman's individualism, his private account of scientific practice, and methodological solipsism. Despite the many challenges posed to Frank's view by operationalism, we will find that Frank did not embrace these troubling aspects of Bridgman's view. Bridgman's treatment of meaning is perhaps the most serious flaw in his account, and we will use Grünbaum's (1954) argument against operationalism to examine it[97]. In this paper, Grünbaum maintains that operationalism fails to adequately distinguish between the pragmatics and semantics of scientific theories. Again, we will see that Frank's view did not succumb to the problems posed by Bridgman's account; I argue that Frank does not fail to distinguish between the semantics and pragmatics of scientific theories. Given that Frank successfully avoids the historical problems faced by operationalism, we will turn our attention to contemporary applications. Specifically, we will examine the pragmatic problems introduced by Hasok Chang's *Grammar of Scientific Practice* (2011). Using Frank's work on education, I suggest one way in which he can add to Chang's project. Lastly, we will examine Frank's account of "active positivism" and will see that Frank's pragmatism allowed him to address a host of issues usually ignored by logical empiricists.

4.2 Bridgman and Operationalism

Bridgman's operationalism is featured heavily in many of Frank's later works,

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[97] This paper was chosen because I have previously criticized accounts of Frank's work that mistakenly conflate his pragmatic and his sematic views. So, it is important, in the context of my presentation of Frank, to show that his use of Bridgman does not result in the same mistakes.

including *Modern Science and its Philosophy* (1949), *Relativity, a Richer Truth* (1950), *Philosophy of Science: The Link Between Science and its Philosophy* (1957), and *Science, Facts, and Values*. Of Bridgman's many important contributions, Frank was chiefly interested in applying Bridgman's theory of operationalism to the analysis of scientific theories. As we saw in Chapter 3, operationalism also played an important role in Frank's theory of meaning. However, as Chang (2009a) explains, the development of a systematic theory of meaning was not Bridgman's chief aim, though he flirted with the idea. We can see from Bridgman's discussion of the concept of length that he regarded the meaning of a concept as synonymous with the operations that determine its applicability:

> We may illustrate by considering the concept of length: what do we mean by the length of an object? We evidently know what we mean by length if we can tell what the length of any and every object is, and for the physicist nothing more is required. To find the length of an object, we have to perform certain physical operations. The concept of length is therefore fixed when the operations by which length is measured are fixed: that is, the concept of length involves as much as and nothing more than the set of operations by which length is determined. In general, we mean by any concept nothing more than a set of operations; the concept is synonymous with the corresponding set of operations. (Bridgman 1927, p. 5)

The application of Bridgman's operationalism to questions of meaning is straightforward; we understand length when, for example, we have defined an operation to determine (i.e., measure) the length of any object. Since meaning is restricted to just those sets of actions, Bridgman's operationalism does not posit any underlying metaphysics, which is one reason why Frank was drawn to it[98]. Another motivation for Frank's attraction was that operationalism was similar to the coordinative definitions used by logical empiricists.

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[98] For an example of Bridgman's opposition to metaphysics see "Meaningless Questions," pp. 28-31 in Bridgman 1927.

Remember, connecting beliefs with actions is an essential part of Frank's analysis of metaphysical statements; this is how Frank construed metaphysics as meaningful, in terms of empirical claims about human behavior.

Bridgman's operationalism results in definitions of concepts that depend on the measuring devices used. In Bridgman's discussion of length, he said:

> In *principle,* the operations by which length is measured should be *uniquely* specified. If we have more than one set of operations, we have more than one concept, and strictly there should be a separate name to correspond to each different set of operations (Bridgman 1927, p. 11, italics original).

On this account, the definitions of concepts include the set of actions required to produce the measurement, the devices used, and reference to the background assumptions relied upon. Bridgman gives the following account emphasizing the thoroughgoing connections between operational processes and the associated concepts: "[If] the operational definition of a concept is accepted, for experience is described in terms of concepts, and since our concepts are constructed in terms of operations, all of our knowledge must be inescapably relative to the operations selected" (Bridgman 1927, p. 25). So, if we use a meter stick to measure a stationary object and later use a more advanced technique to measure objects moving at a high velocity (Bridgman suggests cathode rays or reference to the stars), then we have employed different operations and hence different concepts of length[99]. So, Bridgman reasons, "Einstein's operations were different from our operations above, *his 'length' does not mean the same as our 'length.'*" (Bridgman 1927, p. 12)

[99] See Suppe 1977, pg. 20 for a concise summary of this problem presented in the context of developing cognitively significant correspondence rules.

italics original[100].

Given that Bridgman often defined operations as physical processes, his account of mental operations, such as those involved in mathematics, is not obvious. However, his view of mathematical operations remains very close to his account of physical operations. Bridgman regards mathematics as a tool through which operations can be specified, thus producing concepts:

> Now we shall repeatedly see that it is the most difficult thing in the world to invent concepts that exactly correspond to what we know about nature, and we apparently never achieve success. Mathematics is no exception; we doubtless come closer to the ideal here than anywhere else, but we have seen that even arithmetic does not completely reproduce the physical situation.
>
> Mathematics appears to fail to correspond exactly to the physical situation in at least two respects. In the first place, there is the matter of errors of measurement in the range of ordinary experience. Now, mathematics can deal with this situation, although somewhat clumsily, and only approximately, by specifically supplementing its equations about the limit of error, or replacing equations by inequalities—in short, the sort of thing done in every discussion of the propagation of error of measurement. In the second place, and much more important, mathematics does not recognize that as the physical range increases, the fundamental concepts become hazy, and eventually entirely cease to have their meaning (Bridgman 1927, pp. 62-63).

So, mathematics fails to correspond to reality when it either errs in the context of "ordinary experience" or when it fails to account for very large physical spaces or expanses of time, where the concepts connected to ordinary experience break down. As a result, Bridgman recommends a "development of mathematics by which the equations could be made to cease to have meaning outside the range of numerical magnitude in

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[100] Note that Bridgman holds to this distinction (e.g., measurements of objects at high and low velocities) even when they result in similar results. Indeed, this is the reason that Bridgman is willing to use the same name to denote different concepts, on his account: "The practical justification for retaining the same name is that within our present experimental limits a numerical difference between the results of the two sorts of operations has not been detected" (Bridgman 1927, p. 16). The "two sorts" of operations that Bridgman refers to here are measurements of triangles at small and large scales. See Bridgman 1927, pp. 14-16 for a full account.

which the physical concepts themselves have meaning" (Bridgman 1927, p. 63). In other words, because mathematics is not properly constrained, its applications do not always have meaning. Accordingly, Bridgman is skeptical about the parts of mathematics that cannot be physically interpreted even though they may still be used in science.

Later, perhaps to account for the fact that his 1927 account seems to undermine the validity of mathematics, Bridgman attempted to account for the mathematical process itself by appeal to what he called "pencil and paper" operations, where we can know that some mathematical argument is true when we have proved it. In his reply to critics, Bridgman said that his view will not "place the slightest restrictions on the theoretical physicist to explore the consequences of any free mental construction that he is ingenious enough to make" (Bridgman 1954, p. 79). So while it may be clear that Bridgman's view of mathematics is problematic, he did strive to account for the problems faced by operationalism.

Now that we have outlined Bridgman's view, we can see that Frank's reliance on it is somewhat problematic. Indeed, Bridgman's view of mathematics seems to require a level of empiricism that even the logical empiricists did not embrace. (They usually accounted for logical and mathematical truth by characterizing it as a kind of system-dependent *a priori* truth.) Further, Frank's interest in the social aspects of theory acceptance, which requires treating some social and psychological information as both public and empirically verifiable, is at odds with Bridgman's account. These initial observations, indicating a tension between Bridgman's project and Frank's use of it, introduce two important questions: To what extent was Frank actually using Bridgman's version of operationalism? And what was the intended goal?

4.3 Evaluating Operationalism

Despite the fact that Frank relied heavily on Bridgman's operationalism, he did not attempt to resolve its well-known problems. We can be sure that Frank was aware of these problems because he published a number of criticisms of the view in *The Validation of Scientific Theories* (1954), as well as a response by Bridgman. In this discussion I will try to avoid rehearsing the standard arguments against operationalism. Rather, I would like to evaluate the aspects of operationalism that are inconsistent with Frank's analysis of scientific theories. I will focus on Bridgman's private, individualist account of science and the associated "solipsism" (1936, pp. 12-15; 1940). At the end of this section, I will argue that Frank does not hold either of these views. First, however, I will discuss some of the similarities between Frank and Bridgman on operationalism.

The first major similarity between Frank and Bridgman lies in their mutual admiration of Einstein (Frank 1949a, p. 10; SFV, II, Ch. 7, p. 6; Bridgman 1927, 1936). In particular, both men thought that insufficient attention had been paid to experimental facts before Einstein developed the theory of relativity. This insufficient attention resulted in the early, incorrect rejections of Einstein's theory of relativity. Bridgman begins *The Logic of Modern Physics* (1927) by emphasizing the importance of experimental knowledge over *a priori* reasoning:

> Implied in this recognition of the new possibility of experience beyond our present range, is the recognition that no element of a physical situation, no matter how apparently irrelevant or trivial, may be dismissed as without effect on the final result until proved to be without effect by actual experiment.
>
> The attitude of the physicist must therefore be one of pure empiricism. He recognizes no *a priori* principles which determine or limit the possibilities of new experience. Experience is determined only by experience (Bridgman 1927, p.

3)[101].

Frank similarly rejects the role of *a priori* reasoning in experimental science (Frank 1930, 1949a). As we saw in Chapter 1, Frank regarded the adherence to *a priori* philosophy as responsible for the all-too-common, unfounded rejections of Einstein's theory of relativity. Additionally, both Frank and Bridgman held to some version of relativized epistemology. Bridgman argues that our beliefs are relativized to operations. Frank, borrowing from Bridgman, argues that operationalism's relativistic character may allow for us to better understand the variety of meanings associated with social language, such as "freedom, democracy, and religion" (Frank 1950, p. 26). By identifying and attending to the multiplicity of interpretations, we may better understand how different philosophical positions affect our actions. (As we will see below, these accounts of relativism are not as thoroughgoing as Frank (1950) assumed; for Bridgman locates relativism in operations while Frank locates it in interpretation.) In a more general way, both Frank and Bridgman thought that the use of operationalism would help us to distinguish between meaningful and meaningless questions (Bridgman 1927, pp. 28-31; 1936, p. 12; Frank 1949a, p. 44; 1950, p. 32). And, as Gerald Holton explains, Frank and Bridgman embarked on a fruitful collaborative relationship as soon as Frank arrived at Harvard (Holton 1993, pp. 33-36).

Despite the similarities between Frank and Bridgman's view, there were important differences between the two. On September 4th, 1939, Bridgman presented a paper entitled *Science: Public or Private?* at the Fifth International Congress for the

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[101] Bridgman makes a similar remark in *The Nature of Physical Theory* (1936, p. 9).

Unity of Science held in Cambridge, Massachusetts. In this paper, Bridgman made his position clear: "My point of view is that science is essentially private" (Bridgman 1940, p. 36). This view, he noted, was contrary to the position held by most logical empiricists, "that it [science] must be public" (*ibid.*).

In this paper, Bridgman begins his argument by addressing the importance of agreement in the scientific community. He argues "if ten of my normal neighbors independently assure me that the sum of a column of figures is 137, it is highly probable that I will accept 137 as the 'correct' result, without making the addition myself" (Bridgman 1940, p. 39). Reflecting on the example given above, Bridgman argues that the primary motivation of the "public" account of science is the need to account for the willingness of most scientists to accept the consensus of the scientific community in the way that Bridgman (hypothetically) accepted the mathematical results of his neighbors. However, Bridgman reports that accepting the consensus is justified only as an "easier method of getting the result to which my own activity would have led me or as a means of checking against my own mistakes" (*ibid.*). Thus the public nature of Bridgman's acceptance of and reliance on the work of his peers is grounded on an argument from convenience. In an earlier work, Bridgman acknowledged the fact that it is preferable to agree with others and that we should not doubt their results without good reason to question the mental integrity of the investigator. However, this only serves to ground Bridgman's epistemic standard in individual experiences and judgments: "in the last resort, every individual must be his own judge of what he shall accept to be satisfactory evidence of competence in the other" (Bridgman 1936, p. 14).

On this view, the result of an action can be judged as correct when it "enable[s] me to deal with my own experience with consistency and [results in] success in certain respects…" (*ibid.*). Using the example of "buying and selling," Bridgman implies that the ability to manage capital in a thoughtful and informed manner, e.g., "not run into bankruptcy," provides a practical definition of what counts as successful (*ibid.*). Of this account of successful science, Bridgman says, "the important thing is not the precise form of the criterion, but that the criterion is applied by me" (*ibid.*). And, on Bridgman's account, theoretical "success" is justified at least in part by the fact that the individual understands why he or she was successful in applying the theory.

Bridgman's argument against a public conception of science extends beyond a criticism of public accounts of scientific theories into his analysis of scientific theories, where he endorses methodological solipsism[102].

> The process that I want to call scientific is a process that involves the continual apprehension of meaning, the constant appraisal of significance, accompanied by a running act of checking to be sure that I am doing what I want to do, and of judging correctness or incorrectness. This checking and judging and accepting, that together constitute understanding, are done by me and can be done for me by no one else. They are as private as my toothache, and without them, science is dead (Bridgman 1940, p. 41).

Aware of the fact that this position breaks with the practice of science where consensus and acceptance are prized, Bridgman insists that this account of understanding is essential for scientific practice. Bridgman argues that the acceptance of theories by the scientific community does not preclude an eventual discovery of a disastrous logical paradox, and that broad acceptance is thus insufficient to show that a theory is correct (Bridgman 1940, p. 44)[103]. This does not imply that Bridgman regards the large-scale acceptance of

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[102] See also Bridgman 1940, discussed above, where he refers to his theory as solipsistic.

[103] Bridgman provides additional arguments to support his claim, but they are also weak. He argues that the understanding of a constant "a" in a logical formula depends on a mental activity where the "sameness" of

scientific theories as unimportant; rather he sees it as the result of an aggregate of individual practices that reach the same conclusion. Bridgman's point is only that acceptance itself is not what justifies the theories.

Lastly, Bridgman's analysis of the language of science reflects this individualism. The individualism and methodological solipsism are so profound that they result in every scientist having his or her own unique scientific theory.

> The issue is not just whether it is more profitable to emphasize the private or the public aspect of science. The issue is the deeper one of the unavoidably dual character of many of the words that we use in describing what happens to us and our fellows...Insofar as science is an activity we have to recognize the same duality: "my science" is different from "your science." If we were willing to deal with this situation by completely reforming our language so as to emphasize the dual character of all these words, then we would not be concerned very much with whether science is private or public...It is my point that the differences between "my science" and "your science" are particularly important and necessary to emphasize. It is only "my science" that is alive. Until "your science" becomes "my science" it is as dead and sterile as substitution of numbers into a formula by a high school student (Bridgman 1940, p. 45).

This view implies that there are as many sciences as there are scientists. Bridgman's individual sciences need not be incommensurable because on Bridgman's view we can always confirm[104] each other's results by replicating the experiments of other scientists. We are never really justified in trusting each other's work, rather it must always be

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each "a" is added to words on the text. Thus, understanding that "a" is the same constant representing the same quantity, which is in turn significant for some logical operation, requires "a background that is not contained in what I see on the paper" (Bridgman 1940, p. 42). Surely, Bridgman insists, the sameness could not be physical for "microscopic differences could certainly be found in the two "a's" (*ibid.*). The "complexity" also cannot be accounted for on the paper alone (*ibid*). We all understand that two letters look the same, and the entire system of education is built on the assumption that I can explain to you what the variable "a" stands for.

[104] Admittedly, this is not a traditional account of confirmation because, on Bridgman's account, each hypothesis is distinct. For the sake of clarity I will continue to use the term "confirmation," even though we do not mean *exactly* what Bridgman meant by this term.

accepted as an article of faith. Bridgman allows for both second- and third- person discussions, but even this allowance emphasizes the epistemic difference between "your science" and "my science," since Bridgman advocates for reforms to language that mark the distinction between your science and mine. As the reader may now expect, Bridgman does not give both forms of science equal status; "my science" is superior, because until any science becomes my science it is "dead and sterile" (*ibid.*)[105].

Bridgman's individualist account of science was not limited to his analysis of the activity and language of science. It also figured prominently in his analysis of the social responsibility of scientists. In Bridgman's 1947 paper *Scientists and Social Responsibility* he argued that the scientist should be understood as an enlightened individual, engaged in a process of creative discovery. As a result, it would be wrong to ask scientists to restrict their research as a result of moral concerns.

> To say that "scientists are responsible for the uses made of scientific discoveries" implies, according to what I believe is the usual usage, that each and every scientist has a moral obligation to see to it that the uses society makes of scientific discoveries are beneficent… It means that I have a moral obligation, and that if I do not meet the obligation I shall be deemed culpable by society and may justifiably be disciplined. The discipline that would be imposed is the natural and obvious one, namely, loss of scientific freedom (Bridgman 1947, p. 149).

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[105] Indeed, Bridgman's account of science reflects his general account of language and meaning: "The issue is not just whether it is more profitable to emphasize the private or the public aspect of science. The issue is the deeper one of the unavoidably dual character of many of the words that we use in describing what happens to us and our fellows. The words denoting sensation obviously have this character: 'my pain' and 'your pain,' or 'my consciousness' and 'your consciousness' are evidently different because the experiences are different, and the things that we do in order to decide whether to say 'my pain' or 'your pain' are different. The same duality extends to many words dealing with our activities: 'I think' is different from 'you think.' and 'I desire' from 'you desire.' In so far as science is an activity we have to recognize the same duality: 'my science' is different from 'your science'" (Bridgman 1940, p. 149). This suggests that, for whatever reason, Bridgman seems not to have been troubled by the fact that this account undermines communication. While the implications of this view are indeed interesting, they are not directly related to my account here and should be explored in a separate work.

On Bridgman's account, the loss of scientific freedom is not just a potential punishment, but an actual effect of insistence on the social responsibility of scientists; social responsibility results in a loss of scientific freedom. Because Bridgman's philosophy of science is grounded on a personal, private account of science, it is unsurprising that he did not hesitate to construe the social responsibility of scientists as a fundamentally misplaced responsibility: "The cry of responsibility is often no more than the cry of the lazy man to get someone else to do for him what he ought to for himself" (Bridgman 1947, p. 151). Bridgman's elitist account reduces the social responsibility of sciences to "the right of the stupid people to exploit the bright" (*ibid.*).

Bridgman argues that it is fundamentally wrong to hold that "some *deus ex machina* would stop scientific discoveries from being put to bad uses" (*ibid*.).

> Whereupon the human race, with its capacity for wishful thinking and rationalization, needs only this hint to invent the legend of the responsibility of the scientists for the uses society makes of their discoveries. Let society deal with this situation by the means already in its hands, means by which it deals with similar situations. If it truly believes that the peculiar qualifications necessary to deal with the misuse of scientific discovery are to be found among the scientists, which I, for one, very much doubt, then let it create mechanisms and make opportunities by which those scientists who can do this sort of work well will be attracted to this field, rather than to insist on its right to the indiscriminate concern of all scientists with this problem. And let the scientists, for their part, take a long-range point of view and not accept the careless imposition of responsibility, an acceptance which to my mind smacks too much of appeasement and lack of self-respect (Bridgman 1947, pp. 151-152).

It is the job of society to regulate scientific activity, and not of the scientists themselves. On Bridgman's view, society should be "tolerant" and allow scientists to pursue their research unfettered by moral concerns (Bridgman 1947, p 153).

Bridgman's arguments were inspired by the following question: Was it right of scientists to preform the basic research that resulted in the creation of the atomic bomb? To answer this question, Bridgman indicates that society should not have funded the research that led to the creation of an atomic bomb if it did not want to create an atomic bomb (1947, p. 151). However, this is a very narrow vision of social responsibility. This narrow vision does not seem to acknowledge that there are many instances in which scientists performed immoral research when they should not have, despite the fact that the basic research might have produced useful scientific information (e.g., the Tuskegee syphilis study, Nazi experiments on hypothermia). Bridgman argues that imposing social responsibility on scientists requires that the scientists evaluate not only their research but also "all the consequences that may be initiated by the act of the individual" (*ibid.*). That is not a charitable understanding of the requirement of social responsibility; for we can anticipate possible moral hazards by appealing to broad ethical guidelines, such as informed consent. Ethical oversight might indeed restrict the freedom of scientists, but that restriction of freedom must be weighed against the potential for violations to the rights of others and against the fairly simple ideal of basic human dignity.

Furthermore, given that Bridgman understands "my science" as a personal activity, it is difficult to imagine how such a view of science would be able to account for the practice of science, especially as it currently exists. Specifically, the reliance of scientists on the work of their colleagues is deeply undermined in Bridgman's view. If knowledge extends only as far as the experimental process carried out by a single individual, then science itself is reduced to an individualistic pursuit of personal knowledge. Given Bridgman's position, it would seem that there is no good justification for accepting anyone else's research *as true* without reproducing their work. From this, all researchers would be obliged, should they wish to appeal to the work of another, to do so on non-epistemic grounds, such as convenience. Perhaps in some cases certain results could be easily checked. But it would be nearly impossible to replicate the results of clinical trials or ethnographic analysis, for each of those methods requires large amounts of time, money, and even larger amounts of research capacity (e.g., staff, participants, community partners, travel, etc.). The point here is not that Bridgman denies the practical value of these research activities, but rather that he denies that their results (i.e., the knowledge produced) can be shared with others.

Such an account stands in stark contrast to Frank's attention to social factors, and his concern with the status of scientific theories in the public sphere. Frank's writing on education and the unity of science requires an understanding of science as an inherently collaborative practice, but also one that is built around empowering society to meaningfully engage in public debates about science (Frank 1946/1949, 1947a/1949). That is, a complete understanding of the history of scientific thought must also include discussions of the ways in which politically motivated rejections or misinterpretations of scientific theories were intended to "direct human conduct."

The task of science, then, on Frank's account, is twofold: To understand, as much as possible, the world through an empirical, unifying structure and to do so in a manner that promotes "liberal and humane values" (Frank 1950, p. 104):

> A greater emphasis must be placed on the integration of human knowledge. We must drop or at least minimize an opinion which has been popular among science teachers – that science is just an agglomerate of curious technical facts. If we accomplish this, the large space devoted to science in the modern college curriculum will no longer fill the minds of men and women with a huge body of "facts" indifferent to human goals and human aspirations. (Frank 1950, p. 105).

Thus, on Frank's account, socially responsible science (e.g., science that is not indifferent to human goals and aspirations) does not interfere with scientific progress. This understanding is in line with many important aspects of Frank's thought in the Harvard period. For example, Frank stressed that "the citizen needs...an understanding of how the mind of the scientist works in getting results, and along with it, in what sense those results are 'valid' or 'reliable' and can be used as a basis for judgment" (Frank 1962, p. xxi). As the Director of the Institute for the Unity of Science and long-time advocate for the movement itself, Frank was eager to emphasize the importance of promoting understanding between the sciences by "coordinating" the language of science (Frank 1947a/1949, p. 161), and indeed Frank seems to assume that such coordination was possible.

Frank was motivated by Bridgman's treatment of the activity/behavior of scientists, but he did not draw the same conclusions as did Bridgman. In *Science, Facts and Values* Frank presents operationalism as a kind of pragmatism, to be used in the analysis of actions (II, Ch. VI, pp. 6-9). In Frank's (1950, p. 27) exposition of operationalism, he avoids the problems associated with a private view of science by arguing that operations must be evaluated in groups, thus implying that one operation does not uniquely define one concept. Later, Frank argues that operations should not be reduced to an individual's particular mental state, because such a reduction results in the mistaken temptation to interpret scientific claims in common sense language (which often leads to error).

> The attempts to give direct common sense interpretation to the principles of relativity theory and quantum theory have played a considerable role in our time. Thus the expression "length relative to a certain system of reference" has frequently been replaced by "length relative to an individual observer" who was even occasionally referred to as "Peter" or "Paul." According to the theory of relativity there is an influence of motion upon yardsticks. Hence, every

> description of a length measurement has to contain the speed of the yardstick. [Frank seems to mean that every description of length has to include a description of the velocity of the yardstick with respect to the frame of reference.] But by referring to "length" as the sense impression of "Peter" or "Paul" the dependence of length upon the system of reference becomes a dependence upon the mental state of "Peter" or "Paul." This is the short circuit by which the expression "length with respect to a system of reference" becomes an expression of common-sense language; but now it asserts that there is an influence of optical sensation upon the mental state of "Peter" or "Paul." As soon as we replace the expressions that occur in the theory of relativity by these anthropomorphic common-sense terms, the physical theory becomes a kind of psychological theory. From this common sense interpretation the result is derived that physics actually speaks about the mental phenomena of individuals (Frank 1958, p. 61).

Individualizing our discussion of a theory risks inadvertently psychologizing it, and hence reducing our theory to an account of mental phenomena rather than an account of physical objects. Additionally, Frank did not hold an individualist view like Bridgman's, for he specifically indicated that the meaning of scientific statements should be public: "In order to be sure that a method of verification is 'shareable,' science prefers 'observations' that consist in, e.g., in seeing at a certain point of space, at a certain instant of time, a red spot…" (Frank 1957, pp. 299). This reference to protocol sentences and their "sharability" shows that Frank held that the language of science ought to be intersubjective, as was required by Neurath during the protocol sentence debate (Neurath 1932/1983).

But, despite these important differences, Frank did not ever attack Bridgman's view. Partial endorsement is typical of Frank's writing. Rather than seeking to contrast his work with that of his peers, Frank actively sought out intellectual allies, frequently suggesting how each view worked to complement the other. As I argued in Chapter 1, this way of writing is especially appropriate if we construe Logical Empiricism as an intellectual *movement* and not the mere amalgamation of diverse threads of academic thought.

4.4 Operationalism and the Pragmatics of Scientific Theories

One important but unresolved issue for Frank's use of Bridgman involves the distinction between the pragmatics and semantics of scientific theories. Grünbaum (1954) convincingly argues that Bridgman problematically equates the pragmatics of scientific theories with the semantics of scientific theories.

> Before considering relativity, it is necessary to articulate operationalism in terms of distinctions that are familiar from the general theory of signs. Students of semiotic [the theory of signs which is generally regarded as an aspect of the pragmatics of scientific theories] have found that it is both valid and important to distinguish between pragmatics and semantics when giving an account of an interpreted system of signs such as a physical theory. It is admittedly illuminating to study psychologically, sociologically, and otherwise the activities of scientists in their pursuit of science and to investigate the relationships between scientific symbols and their human users. Studies of this kind constitute the field of pragmatics. But, from the standpoint of the *logic* of physics, our concern must be *not* with pragmatics but rather with the relationships between physical theory on the one hand and the designata or denotata of that theory in the realm of nature on the other (Grünbaum 1954, p. 84).

As Grünbaum explains, the semantics of scientific theories, which he describes as the "logic of physics" (*ibid.*), must investigate the relationship between the signs and whatever it is that they denote. Conversely, the pragmatics of scientific theories investigates the activities of the scientist, how the theory is interpreted, and issues surrounding the application of the theory. In this paper, Grünbaum provides a sophisticated argument against Bridgman's operationalism, in which he attacks Bridgman's treatment of special relativity. Given that Bridgman's operationalism set out to defend special relativity (Bridgman 1927), Grünbaum's attack targets the foundation of Bridgman's theory.

Grünbaum's argument against Bridgman's thesis focuses on what justifies physical theories and, relatedly, questions whether Bridgman has correctly understood the proper subject matter of physics:

> …the objects that physics talks about are neither the manual nor the conceptual activities of physicists; instead, physics is concerned with the postulational and observational ascertainment of the attributes and relationships that characterize various kinds of physical entities or processes (Grünbaum 1954, p. 85).

So Grünbaum's concern is not merely a question of categorization. If Bridgman is correct, then the proper subject matter of physics is observer-dependent operations, rather than "relationships that characterize various kinds of physical entities or processes." In short, Bridgman's view makes scientists rather than the physical world the subject matter of physics. This may appear to make Frank's reliance on operationalism in his theory of meaning problematic. If his work is carefully examined, however, we will see that Grünbaum's arguments do not apply to Frank's use of operationalism.

First, we will more fully flesh out Grünbaum's argument. He begins by spelling out the difference between the Newtonian and relativistic accounts of time.

> Accordingly, in the Newtonian theory, every two events of nature stand in a determinant, unambiguous temporal relationship to each other: either they can be the termini only of causal chains of *infinite* velocity, as in the case of gravitational action-at-a-distance, or they will be the termini of causal chains of finite velocity, however large. In the former case they are absolutely simultaneous and in the later case they are absolutely non-simultaneous. In restricted [special] relativity...no influences can propagate themselves faster than light, and therefore every class of physical events in which each member sustains an objective relationship of temporal order to every other member will be only a proper subclass of the totality of physical events (Grünbaum 1954, p. 87 italics original).

In other words, of the set of all physical events, there will always be some events that do not have objective causal relationships to at least one other physical event. Thus, there exist "pairs of physical events whose members cannot be the termini of any influence chain and therefore are not related temporally by any objective criterion" (*ibid.*).

Human activity does not play a role in the above description until "we assign numerical names to these events" (*ibid.*). Recall that issues relating to naming are usually classified as semiotic, and hence pragmatic. Grünbaum's point is that "relativistic temporal order of nature neither is generated by, nor derives its meaning from, *our* hypothetical or actual signaling activities or from any other operations performed by human beings…" (Grünbaum 1954, p. 87, italics original). In relativity theory, simultaneity is usually discussed in terms of light signals, which do not require that we make reference to our individual experiences. This is why Grünbaum intentionally avoided discussing signaling as an activity. His argument shows that we should not take uses of light signaling to suggest that it is necessary to refer to observers or other kinds of human activity in our discussion of the semantics of scientific theories.

In constructing his argument in the way outlined above (i.e., avoiding all talk of the activity of scientists), Grünbaum has shown that human activity is not necessary to explain the relationship of physical events. Indeed, human activities need not be mentioned at all.

> It is because no relations of absolute simultaneity *exist* to be measured that *our* measurement cannot disclose them; it is not the mere failure of *our* measurements to disclose them that constitutes their nonexistence, much as the failure is *evidence* for their non-existence.
>
> Thus, the upshot of Einstein's analysis concerning the issue before us is *not*, as Bridgman would have it, that the concepts of science refer to our operations instead of to the properties and relationships of physical events (italics original, Grünbaum 1954, p. 88).

This does not mean that every aspect of Bridgman's view is without merit. Rather Grünbaum has shown only that he should not conflate the pragmatics and the semantics of scientific theories; the *process* of measurement is not the same as the quantity being measured. Observer-dependent operations, which can be important in pragmatic discussions, need not be referred to in semantic analyses.

Unlike Bridgman, Frank does not regard scientific knowledge as reducible to experimental practice, and for this reason Frank's view does not suffer from the same problems faced by Bridgman's account. To understand why this is, it is helpful to examine Frank's treatment of Reichenbach's account of "interphenomena" (Reichenbach 1944)[106]. Reichenbach defines inter-phenomena as the set of unobservable occurrences that result from a particular theoretical interpretation.

> We then shall consider as unobservable all those occurrences which happen between the coincidences, such as the movement of an electron, or of a light ray from its source to a collision with matter. We call this class of occurrences the interphenomena. Occurrences of this kind are introduced by inferential chains of a much more complicated sort; they are constructed in the form of an interpolation within the world of phenomena, and we can therefore consider the distinction between phenomena and interphenomena as the quantum mechanical analogue of the distinction between observed and unobserved things (Reichenbach 1944, p. 21).

Interphenomena represent the set of occurrences implied by a particular theory, used in various kinds of experimental designs, but which cannot be observed. Rejecting the notion that one theory of quantum mechanics uniquely explains the phenomena and is thus the correct interpretation, Reichenbach argues that both interpretations posit, "a class of equivalent descriptions of interphenomena, each of which is equally true, and all of which belong to the same world of phenomena" (Reichenbach 1944, p. 23).

Frank absorbs this aspect of Reichenbach's work into his account, and the resulting argument prohibits an interpretation that is consistent with Bridgman's theory of meaning. Frank argues that interphenomena are not measured by operations, for they

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[106] While the shift from relativity theory to quantum mechanics is a bit jarring for the reader, it is one that Grünbaum (1954) himself makes when he cites the *Philosophical Foundations of Quantum Mechanics*.

result only from the application of one or another *interpretation* to a particular system. The experiment, operations, and the resulting “world of phenomena” remain unchanged, for they are independent of the interpretive interphenomena. Frank says:

> In the parlance of this book, we would say rather that the law of these interphenomena...can be formulated without knowing the whole experimental arrangement; we do not need to know, for example, whether the diaphragm is fixed or movable relative to the inertial frame (Frank 1957, p. 225).

In this way, knowledge gained from the experimental process does not exhaust what we know on Frank’s account. And insofar as the interphenomena are not dependent on a particular experiment or on their ability to be observed, they are not observer-dependent in the way required by Bridgman’s account of operationalism. Frank adds further distance between his account and Bridgman’s by treating the predicate “observable” as independent of an actual observer: “the observer is only introduced as a figure of speech, and can be eliminated without changing the meaning of statements” (Frank 1957, p. 230). So, operations are not observer-dependent on Frank’s view and the meanings of statements are not grounded in the practices of the scientist.

As Grünbaum remarks at the end of his paper, his analysis does not undermine the prospects for operationalism to function as a useful tool in the pragmatics of scientific theories. Despite the mistakes discussed above, Bridgman’s interest in scientific methodology remains important. So, we should now ask: to what extent has the role of scientific methodology been overlooked? To what extent might returning to methodological questions be useful in the contemporary context? And how can the pragmatics of scientific theories aid such a return? These questions will be explored in the next section.

4.5 Hasok Chang's Grammar of Scientific Practice

Even though Frank does not invoke some of the most controversial aspects of Bridgman's theory, operationalism still looms large in much of his work during the Harvard period. As we have seen, Frank defines semantics as the study of the "relations between physical object and signs or symbols" (Frank 1957, p. 349). He does not endorse Bridgman's particular brand of operationalism, which treats the actions of physicists as part of semantics. When Frank is concerned with the activity of science, it usually has to do with the pragmatic component, which studies "the relationship between the scientist and his signs" (*ibid.*). There are many ways that pragmatics may conceivably affect scientific practice. For example, Frank follows his discussion of interphenomena with an entire chapter devoted to "Metaphysical Interpretations of the Atomic World" (Frank 1957, pp. 232-259). He begins this chapter by introducing the following concerns:

> Although from the purely scientific aspect the new theories used in subatomic physics, the quantum theory and quantum mechanics, are very different from the theory of relativity, the metaphysical interpretations of both types of theories, relativity and quantum, are in many respects very similar to one another. The claim has been made that a "mental element" is introduced into the physical world and that "materialism" is refuted. However, while relativity theory has been interpreted as supporting a belief in predestination, quantum theory has been regarded as a support for the doctrine of "free will" (Frank 1957, p. 232).

So the discussion of interphenomena revealed the locus of Frank's concern; he was worried about misinterpretations of scientific theories.

In this section, I will argue that contemporary discussions of operationalism would benefit by explicitly including Frank's pragmatic concerns. In particular I will focus on Hasok Chang's "Philosophical Grammar of Scientific Practice" (2011). In this paper, Chang argues that by attending to the activity of science, we can show how seemingly philosophical problems are in fact pseudo-problems. He targets mistakes that he says are perpetrated by the majority of analytic philosophers (as indicated in the quote below). The meta-philosophical issue introduced by Chang will be addressed in the next section (4.6).

To understand Chang's proposal, we must first consider his discussion of Bridgman. This discussion is focused on the activity of scientists rather than propositions about that activity:

> I begin with the recognition that all scientific work, including pure theorizing, consists of actions – physical, mental, and 'paper-and-pencil' operations, to put it in Percy Bridgman's (1959, 3) terms. Of course, all verbal descriptions we make of scientific work must be put into propositions, but we must avoid the mistake of only paying attention to the propositional aspects of the scientific actions. That is a sure path to disconnection from practice, and it is precisely the path that analytic philosophers on the whole have taken. What I am complaining about is our habit of focusing on descriptive statements that are either products or presuppositions of scientific work, and our commitment to solving problems by investigating the logical relationships between these statements. I take heed of Bridgman's conviction that 'it is better, because it takes us further, to analyze into doings or happenings rather than into objects or entities' (Bridgman 1954, 76; Chang 2009b). (Chang 2011, p. 208)

Chang argues that we can begin to heed Bridgman's advice by taking contemporary topics in the philosophy of science (e.g. confirmation, explanation, demarcation, etc.) and seeking to understand those topics in terms of actions. So, rather than asking what "confirmation" is, we ought to ask how and when scientific theories are confirmed[107].

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[107] Here Chang is not arguing that these are *essentially* different questions. Instead he urges his readers to attend to the fact that the manner in which we ask questions or express propositions about science might implicitly suggest particular kinds of answers. Readers might be concerned that Chang's questions (e.g., how do we confirm theories?) seem to presuppose answers to other questions that he wants to dismiss (e.g., what is confirmation?). However, such concern would miss Chang's point, because he is concerned with the structure, or the grammar, of our questions. By asking what confirmation is, we will usually be led to a high-level, top-down answer. In focusing on scientific practice, Chang seems to suggest a bottom-up approach. And, indeed, this approach might eventually lead to saying what confirmation is. But, alternatively, it might lead us to think that our focus on confirmation was initially misguided or that it attempted to account for the wrong sorts of issues or phenomena. I believe that Chang's approach intends to foster the latter sort of conclusion.

The basic unit of analysis is "epistemic activity," or a "coherent set of mental or physical actions (or operations) that are intended to contribute to the production or improvement of knowledge in a particular way" (Chang 2011, p. 209). Such an analysis involves a normative component and a social component. The normative component is required because philosophical accounts are "evaluated in light of their conformity with rules" (*ibid.*) that govern the epistemic activity. And the evaluation of practice will require some understanding of the social organization of science.

Chang argues that "we need to recognize teleological or purposive behavior in the agent," when the agent "carries out her intentions, which are formulated on the basis of her desires and beliefs: the agent takes the kind of actions that she believes will contribute towards the satisfaction of her desires" (p. 210). (The reader should note the strong similarity between this remark by Chang and Frank's discussions of the pragmatics of scientific theories.) Such an analysis requires that we take into account the "plurality of desires" (Chang 2011, p. 211). In order to best capture that plurality, Chang develops nine evaluative criteria of scientific practice:

1. Activity: What is being done in the practice in question?
2. Purpose: Why?
3. Agent: Who is doing it?
4. The second person: To/with whom?
5. Ontological principles: What must the world be like, in order for this activity to be intelligible?
6. Capabilities: What must the agent be capable of, in order to carry out this activity?
7. Resources: Which tools are necessary for this activity to be successful?
8. Freedom: What kind of choices does the agent make?
9. Evaluation: Who is judging the results, and on what criteria? (Chang 2011, p. 217)

These categories not only cover a wide breadth of scientific activity, but they also help to clarify how Chang would transform the grammar of scientific practice by changing how we *actually* talk about science. Chang's proposal is intended to address issues that arise from misleading forms used in scientific questions. Rather than asking about the underlying ontological status of the entities referred to in scientific theories, Chang encourages us to ask questions about the methodological assumptions that we must make in order to make sense of ontological principles. Like Chang, Frank argued that philosophical problems could emerge from how we talk about science. For example, Frank's worries about misinterpretations of physics are related to Chang's concerns. As we saw above, both quantum mechanics and relativity were thought of, by some, as justifying one metaphysical system or another. And as we saw in Chapters 2 and 3, these misinterpretations are established by the creation of auxiliary concepts, or commonsensical ways of speaking. These concepts can then make metaphysical interpretations seem as if they are absolutely true, even though they are not.

The spirit of Frank's work is most evident at the end of Chang's essay. There Chang finds common cause with the 1947 mission statement of the Philosophy of Science Association (PSA). (This mission statement was adopted at the same meeting at which Frank became the first elected president of the PSA.) In 1947 following objectives were approved:

> The objects of the Association are the furthering of the study and discussion of the subject of philosophy of science, broadly interpreted, and the encouragement of practical consequences which may flow therefrom of benefit to scientists and philosophers in particular and to men of good will in general (Philosophy of Science Association 1948, p. 176).

It is difficult to imagine that Frank's views did not influence the 1947 statement. As

Heather Douglas points out, however, within a decade the mission of the PSA[108] changed significantly:

> The objects of this Association shall be the furthering of studies and free discussion from diverse standpoints in the field of philosophy of science, and the publishing of a periodical devoted to such studies in this field (Philosophy of Science Association 1959, p. 63; quoted in Douglas, PSA).

This change is significant for it ushered in a new era for the philosophy of science, one that was marked by increasing "professionalization and institutionalization" (Howard 2003, p. 66). These trends are marked by the development of "clearer criteria for community membership" which function to exclude those who do not fit those criteria from the academy and its associated institutions (Howard 2003, pp. 72-73). As Don Howard explains, formerly common themes such as science and values, political ideology, science planning/policy, and the sociology of knowledge almost entirely disappeared from the pages of *Philosophy of Science* (2003, pp. 66-70). At the height of the cold war, it is also likely that any leftist activity would have been viewed as highly suspicious (Howard 2003; Reisch 2005).

Insofar as Chang harkens back to the 1947 mission statement, he is invoking the memory of the oft-forgotten tradition briefly discussed above, the tradition associated with Philipp Frank. It is important to bear in mind, as Douglas (PSA) and Howard (2003) note, that the pragmatist wing of logical empiricism did not find a significant audience among philosophers of science. As a result, the central questions that defined this approach to the philosophy of science (such as the relationship between science and society, pragmatics of scientific theorizing, ethics/values in science) were not regarded as

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[108] Douglas's history is undated and may be found at the following address: http://www.philsci.org/about-the-psa/history-of-the-association

important problems. But as Chang indicates, by attending to the practice of science we might better demonstrate the relevance of the above issues, and hence of our discipline, to the concerns of society at large (Chang 2011, p. 218). This is where careful attention to Frank's contributions is likely to be useful. His account of how social factors influence scientific theories can help us to meet the challenge of demonstrating the discipline's relevance to society at large by attending to its practices.

Both the positive and negative forms of Frank's account of social factors (that is, identifying how ideology biases our understanding of facts and acknowledging the roles played by values and social beliefs in shaping our scientific theories) help to capture what Chang has called "purposive behavior," or why we take particular actions or pursue particular theories. Frank's account of the role played by social factors in science was an attempt to account for these motivations, especially when they factor into theory acceptance and rejection. Although Frank's special concern with socially and politically motivated theory rejection was arguably a product of his cultural *milieu*, his analysis is broad enough to capture this aspect of Chang's view. This is because both accounts require the identification of what Chang calls "proximate aims" (2011, p. 211) and what I have above called non- or extra-scientific motivations. While Frank and Chang certainly do not offer the same theories, their approaches do indeed urge us to take extra-scientific motivations/proximate aims seriously in our philosophies of science.

Recall that, according to Frank, taking into account positive social factors allows us to accept certain metaphysical claims as meaningful if they can be operationalized via appropriate connection to actions. On his account, the "validity of a scientific theory cannot be judged unless we ascribe a certain purpose to that theory" (Frank 1957, p. 359).

Appealing to the sociology of science, where it is possible to understand the beliefs and motivations of proponents of metaphysics, enables us to pin-point potential problems and to develop solutions to issues related to metaphysical accounts of science. These sociological and pragmatic questions extend beyond the "scientific criterion in the narrower sense," and provide an empirical, scientific way of understanding problems associated with the practice of science. Again, this is mirrored in aspects of Chang's account:

> ...instead of thinking about the nature of a definition, we can consider what one has to do in defining a scientific term: formulate formal conditions, construct physical instruments and procedures for measurement, round people up on a committee to monitor the agreed uses of the concept, and devise methods to punish people who do not adhere to the agreed uses. (Chang 2011, p. 208).

For an example of how Frank often discussed issues pragmatically, consider his Frank's work on education. He does not engage in debates about how "education" should be defined. Rather, he locates a particular problem, such as politically motivated misinterpretations of scientific theories, and then understands education as both a possible source of that problem and a possible solution to it. In this example, Frank mobilizes current research on education in order to address a pragmatic problem having to do with how science is used. So, it is better to understand Frank's work on education as a proactive attempt to ensure that scientific concepts are well-understood by teaching students to be sensitive to their role in the system of a scientific theory. It would be a mistake to construe educational problems as problems that originate from how we define education. Frank's thought is similar to Chang's in that it focuses our attention on *pragmatic* problems and avoids erroneously casting those problems as purely theoretical questions.

As Frank argues, citizens cannot make informed decisions about scientific research unless they are scientifically literate.

> In our time, the government has to devote a great deal of attention and financial support to scientific research. In a democracy, no government could embark on such a program unless it were supported by its citizens; but they would not support it unless they understood what it was all about. The problem arose of how citizens could learn to judge reports of experts, e.g., about appropriations for research projects, without being specialists in science themselves (Frank 1957, p. xxi).

This problem that Frank discusses above does not have to do with the meaning of scientific terms *per se*. Rather, the problem lies in how the results of scientific theories are communicated to members of the public, how the public can be made capable of understanding these theories, and how they can judge their worthiness of support (financially and otherwise). As we saw in chapter 2, Frank often appeals to Conant in making these arguments.

> Many people have believed that this goal could be achieved by popularizing the results of science, by adult education courses in which intelligent and interested men and women could absorb the "facts" discovered by scientists in a digestive way. Conant, however, made the point that by absorbing "results" and "facts" laymen could not acquire any judgment about the reports of scientists. What the citizen needs rather is an understanding of how the mind of the scientist works in getting results, and along with it, in what sense are those results are "valid" or "reliable" and can be used as a basis for judgment (Frank 1957, p. xxi).

Understanding the mind of the scientist "in a systematic way" Frank says, "is the task of the philosophy of science" (*ibid.*). We should also note that Frank presents this task in an active way, not unlike Chang. Frank's interest is distinct from Chang's, but I believe that it serves to add value to Chang's proposals. This is because he asked something like "how do we understand?" rather than "what is there to understand?" From this, it becomes clear that we must pay attention to processes that promote understanding. And these remarks help to make clear what kind of "practical consequences" Frank might have had in mind in 1947.

In this section I presented Chang's arguments that we should reform the way that we discuss issues in the philosophy of science in order to better attend to methodological and pragmatic issues. These arguments were influenced by Bridgman's operationalism. And, like Bridgman and Chang, Frank also appreciated the importance of pragmatic issues in the philosophy of science. But, unlike Bridgman, Frank does not neglect the important difference between pragmatism and semantics in his discussions of scientific theories. Instead Frank's arguments suggest that philosophers of science ought to pay more attention to pragmatic problems, such as those produced by misinterpretations of scientific theories. In the next section I will show how Frank (during his Harvard period) challenged orthodox logical empiricism by arguing for a more pragmatic orientation of the discipline, which he called "active positivism."

4.6 Frank's Meta-philosophical Critique

It is easy to forget that when the Vienna Circle entered its public period in 1929 analytic philosophy was only beginning to establish itself as a discipline. Logical empiricism first developed independently and then symbiotically with the emerging analytic tradition. Members of the Vienna Circle and its periphery, including Rudolf Carnap, A.J. Ayer, Hans Reichenbach, and Carl Hempel, all became important figures in analytic schools of thought. However, some philosophers – including Frank – became concerned about this new research trajectory. Frank in particular worried about the waning interest in the pragmatic or "real world" potential of philosophy. This section will explore the nature of Frank's concern by examining his critique of the "purely analytic attitude" (Frank 1949, pp. 36-37). I will argue that Frank's critique of analytic philosophy was characteristic of divisions between American pragmatism and logical empiricism as outlined by Alan Richardson (2002). Richardson describes two approaches to philosophy that roughly correspond to logical empiricism and American pragmatism. The first approach is "demarcationist," and seeks to separate what science is from what it is not. This approach is characteristic of logical empiricism. The second is "imperialist," and (notwithstanding the negative implications of this particular moniker) seeks to apply the methods characteristic of scientific reasoning to other kinds of inquiry. I will argue that not only is it correct to place Frank in the latter camp, but also that doing so will help to explain a peculiar criticism that he made of Carnap's work in 1963 (Frank 1963).

As I have argued throughout, Frank's understanding of logical empiricism is different from what has become the orthodox view. Frank often wrote about science in an expansive context, taking into account the historical role of empiricist thought (1949a), the social context of the development of Einstein's thought (1963), and the democratic potential of the scientific attitude (1950). Prior to each of these works, in the first decade of the Harvard period, Frank explicitly called into question the aims of analytic approaches to philosophy:

> Our original Viennese group and particularly Neurath were not satisfied with ascribing to our new philosophical group mainly critical and analytical objectives. We knew well that man is longing for a philosophy of integration. If the new philosophy refuses to serve the cause of integration, a great many people, including even scientists, would rather return to traditional metaphysics than be restricted to a purely analytic attitude. As a matter of fact, the traditional goal of "philosophy," through thousands of years of human knowledge, has been integration (Frank 1949a, pp. 36-37).

The "original Viennese group" in this quote refers to the first Vienna Circle (i.e., Frank, Neurath, and Hahn). Frank specifies what he means by "integration" in *Relativity, a Richer Truth*, published one year after the quote above. In that work, integration is defined as a task unique to philosophical inquiry that aims to bring together science and the humanities, as well as the various specializations within these fields of study (Frank 1950, p. 58). In line with Frank's pragmatic leanings, he describes "integration" as a kind of activity: "If we want to work for the integration and synthesis of knowledge we must make use of scientific methods to their limits. We must try to discuss the presuppositions with the same rigour and by the same methods that have proved reliable in other fields of science" (Frank 1950, p. 66). The task of the philosopher in achieving "an integration in the field of sciences" is to then "help produce a language that can be used in several different fields" (Frank 1950, p. 70).

Frank's conception of "integration" is related to the unity of science, the belief that it is possible to bring together diverse branches of science through the development of a new (or several new) language(s) of science. Among logical empiricists, there were varying opinions on what this language, and hence the unity of science, would look like. However, most logical empiricists did come to prefer the development of formal languages that could be used to analyze natural language, an approach that is now called the logical analysis of language[109]. The restrictive "analytic attitude" that Frank described then seems to refer to that approach. Frank does not just argue that the "analytic attitude" undermines the unity of science. He also suggests that pursuit of analytic objectives was not the primary aim of the First Vienna Circle:

> Our group [referring to the First Vienna Circle] did not wish to stress the work on analysis in contrast to the creation of a new synthesis. We never regarded the logic and analysis of science as a goal in itself; we believed strongly that this analysis is a necessary part of obtaining an unprejudiced outlook on life (Frank

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[109] Indeed many logical empiricists did regard the development of the logical analysis of language as related to larger social goals. Carnap discusses this in his 1963 autobiography. However, as Frank's late work suggests, logical analysis is unlikely to be of any practical use in achieving these goals. I will explicate Frank's critique of logical analysis throughout the remainder of this section.

1949, p. 37)[110].

Implicit in this reference to the First Vienna Circle is a call for logical empiricism to move beyond attempts to understand and reconstruct the language of science. Frank does not suggest that the analytic methods of logical empiricism are faulty; rather he is emphasizes their limited scope.

To understand these remarks, it is important to know that two years prior to this discussion of analytic philosophy there was a significant shift in his thought. In 1947 Frank "became more and more interested in the actual meaning of the metaphysical interpretations of science" (Frank 1949a, p. 51). At this time Frank began his study, "within the framework of logico-empirical and socio-psychological analysis," of the meaning of metaphysical beliefs (Frank 1949a, p. 52). He developed an interest in these issues between 1940 and 1947, when he began to attend the "Conference of Science, Philosophy, and Religion," organized by Harlow Sharply among others (*ibid.*). In these meetings Frank engaged regularly with a variety of thinkers, many of whom he described as "rather critical of the scientific outlook and its contribution to human welfare" (*ibid*.).

For whatever reason, a majority of the other attendees did not share Frank's hopeful and optimistic attitude towards scientific progress. In fact, Frank's impressions of the conference (Frank 1949a, 1950) suggest that the atmosphere was actively hostile towards science. Frank provides the following description:

> The great majority of the members of this Conference were churchmen, educators, social workers, philosophers and historians. Only a small sprinkling were scientists. Belonging to this small latter group has turned out to be a very valuable experience for me. Scientists as a group usually take it for granted that science is an end in itself. It was therefore of great profit to me to be brought into

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110 The "unprejudiced outlook on life is similar to the "critical attitude" discussed at the end of Chapter 2.

> such close personal and intellectual contact with those representatives of groups who consider science *a means toward* shaping a desirable way of life (Frank 1950, p. xiv).

The first topic addressed by Frank in *Relativity, A Richer Truth* (Frank 1950, p. 3) was a concern raised by many of the conference attendees: that science actively undermines democratic values. As a result, Frank's post-1947 project acquired an evangelizing tone. Arguing against the preferred accounts of absolute truth, Frank defended a relativized notion of truth, where our perception of the truth of a claim is dependent both on the set of information available to us at a particular time and on our goals. Frank believed that this tolerant attitude would foster democratic deliberation insofar as we can debate what is true only if at least one interlocutor is willing to change his or her mind (Frank 1950, pp. 7-11).

Frank's attention to how other disciplines regard scientific methodology is reflective of one kind of difference between logical empiricists and American pragmatists, described by Alan Richardson. He suggests that this difference is characterized "less [by] philosophical theses, and more [by] philosophical goals, commitments, and aspirations" (Richardson 2002, p. 39). Emphasizing the role of distinct sets of goals and aspirations, Richardson argues that one of the differences between the work of logical empiricists and American pragmatists lies in how each understood "the motivational elements of scientific philosophy" (*ibid.*). Those divisions are characterized by Richardson as either "demarcationist" or "imperialist":

> We may call the rhetoric of logical empiricism *demarcationist:* the point was to demarcate the proper role of science and to find the scientifically acceptable replacement for a core of traditional philosophy...Dewey's characteristic rhetoric is *imperialist:* the point is to bring scientific rigor into all areas of philosophical concern (Richardson 2002, p. 46, italics original).

Richardson's conception of pragmatic imperialism was developed with particular attention to the work of John Dewey, who (in *Reconstruction in Philosophy*) discusses "socially conscious engineering philosopher[s]" (Richardson 2002, p. 42). On Richardson's analysis, Dewey's view is distinct from that of the logical empiricists, who were primarily advancing a philosophical program that was "technical and esoteric, theoretical rather than practical" (Richardson 2002, p. 41). Singling out the Left Vienna Circle, Richardson says:

> Otto Neurath, Philipp Frank, Rudolf Carnap and others believed that traditional projects in metaphysics were not simply nonsense, but nonsense with a political agenda: talk of transcendent values served to confuse people, propping up illegitimate structures of political authority with stories that no one could truly understand. The rejection of metaphysics had political consequences; it was a rationalizing and modernizing social movement. (Richardson 2002, p. 44)

If we take metaphysics to be politically problematic, then it is not just a philosophical problem, but it is also a problem in the world. And for real world problems to be addressed, our work must involve more than philosophical reflection. Put another way, merely undermining metaphysical arguments is not sufficient to bring about social or political change if our opponents are unwilling to change their minds. If all of this is true, then it follows that we must actively evangelize on behalf of our position; we must become imperialists (to use Richardson's phrase).

As Richardson concludes, the differences between logical empiricists and American pragmatists were not well appreciated by either school of thought. Instead, each preferred to see the other as an ally in a broader fight against "conservative religiously oriented philosophy" (Richardson 2002, p. 45). However, after 1947, Frank advocated a new approach for the philosophy of science, which he termed "active" or "proud positivism" (Frank 1951, pg. 28). Frank's conception of proud positivism combines aspects of pragmatism and logical empiricism. As we saw in Chapter 3, Frank's account of meaning was very similar to that of most other logical empiricists, but Frank applied to it a unique set of issues by appeal to sociology. Proud positivism relies on that account of scientific theories to insist that logical empiricists use their philosophy in order to address pragmatic issues arising from non-scientific interpretations of scientific theories.

Non-scientific (e.g. theological, political, etc.) interpretations of science can be critically examined using Frank's conception of social factors. He defines "active" or "proud" positivism as a method of "examining the decisions favored by social and educational influences and attempts to interpret them as acts which can be derived from the laws of human behavior" (*ibid.*). On Frank's account, the task of science was not simply the "collection of material" to be studied and interpreted by philosophers, theologians, and politicians. Rather, the task of active positivism was to cooperatively develop science by bringing together the many fields of scientific research. The problem of metaphysics is in part a problem of scientific organization:

> The real meaning of these [metaphysical] generalizations can be found within science if we include also the social sciences in the system of sciences. The idea of a superscience called philosophy or metaphysics arises only if we mean by "science" some special science instead of the system of all sciences (Frank 1951, p. 30).

Here Frank is looking beyond the issue of demarcation. Merely pointing out what is and is not scientific will not be sufficient to convince proponents of non-scientific (or even anti-scientific) interpretations of scientific theories that their philosophical project is incorrect. This is especially true when the proponents of these positions do not accept even the most basic premises of so-called scientific philosophy.

Frank's program for action is not a new one, for the unity of science was an established aspect of logical empiricism since Neurath began that project in 1934

(Cartwright et al. 1996). However, his particular call to action does seem to be unique among his logical empiricist colleagues. In fact, Frank grounds the concept of “active philosophy” not within the context of logical empiricism, but within the context of Dewey’s work. Frank ends his 1951 argument by giving Dewey’s “strong and impressive” account the last word:

> The history of philosophy will take on a new significance. What is lost from the standpoint of would-be science is regained from the standpoint of humanity. Instead of disputes among rivals about the nature of reality, we have the scene of human clash of social purpose and aspirations (Dewey 1920, quoted in Frank 1951, p. 30).

In terms of Richardson’s distinction then, Frank’s philosophy seems to be closer to the imperialist approach than it is to the demarcationist approach. But, a careful reading of Frank’s own writing on the subject suggests that he did not understand the work of his colleagues in terms of anything like Richardson’s demarcationist/imperialist distinction.

Frequently Frank’s criticisms of logical empiricism are cast as a move away from the philosophical project of his colleagues. However, Frank’s own writing seems to suggest that he never thought of his work, or even that of Dewey, as distinct from the mainstream of logical empiricism:

> Since an “attitude towards life” certainly influences human actions, metaphysical propositions have, according to Carnap, a meaning if we understand “meaning” in the sense of Neurath. We remember that John Dewey emphasized again and again that metaphysical propositions are not propositions about the objective reality of the physical universe, but propositions about human aspirations. Essentially, what Carnap calls “attitudes toward life” is not very different from what Dewey called “human aspirations.” Practically the Vienna Circle shared the opinion of the pragmatists about the “meaning of metaphysics” (SFV, II, Ch. V, p. 7).

Thus both schools of thought, on Frank’s account and as suggested by Richardson, shared the analytical tools and conceptual basis necessary to meet the challenges posed by metaphysical interpretations of science. Frank’s remarks are not dismissals of the demarcationist project. Instead it seems that Frank wanted his colleagues to use their tools of demarcation to engage in an imperialist fight, to use Richardson's terminology once more.

These insights regarding the purpose of philosophy help to make sense of Frank's somewhat bizarre 1963 discussion of Carnap's "The Elimination of Metaphysics Through Logical Analysis of Language." Published in the well-known Schilpp volume dedicated to a retrospective assessment of Carnap's work, Frank's essay purported to be a commentary on the role of pragmatism in Carnap's work. Contrary to what might be expected from an essay on pragmatism, Frank wrote largely about the dismissal of Carnap's work by a Soviet-era philosopher, V. Bushlinsky. Frank quoted Bushlinsky's criticism at length, taking up nearly two and a half pages. This excessive quotation and the rest of Frank's commentary surprised many of his colleagues[111]. Gerald Holton (2014) and Robert S. Cohen (2014) did not understand why Frank made such an obscure work the centerpiece of his discussion.

However, by attending to the differences between the demarcationist and the imperialist approaches to the philosophy of science we may understand Frank's criticism of Carnap as an imperialist criticism that sought to expand the domain of philosophical inquiry. In other words, Bushlinsky's essay proved to be an example of a view that was hostile to logical empiricism and also used metaphysics "to support a certain way of life, a certain political or religious creed" (Frank 1963, p. 163). So, following Frank's argument, we may ask whether or not the demarcationist approach was able to

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[111] Don Howard diplomatically refers to this event as "curious" (2003, p. 62).

accomplish its own goals: What is the purpose of having an anti-metaphysical philosophy that does not eliminate metaphysics in practice?[112]

In Frank's commentary, he asks whether or not Carnap truly achieved his goal of eliminating metaphysics. Frank's answer is a resounding "no." This is because Frank understands the elimination of metaphysics not only as an intellectual exercise (e.g., an evaluation of the reasons to believe metaphysical claims), but also as an exercise in social construction (e.g. showing how a particular metaphysical thesis is used to justify some ideological position). As Frank argues the "purpose of eliminating [metaphysics] would be to destroy this undesirable ideology" (Frank 1963, p. 163). Despite Frank's apparent frustration, he correctly understood that Carnap's idea of the elimination of metaphysics was intellectual and not social. But Frank's criticism of Carnap questions the value of non-pragmatic eliminations because metaphysical beliefs have real-world effects; just because metaphysical misinterpretations of scientific theories should not happen does not mean that they do not happen (Frank 1957, p. 355). Additionally, Frank blamed logical empiricism's reputation, as either "cynical skepticism" or "intolerant dogmatism," on essays such as Carnap's "Elimination of Metaphysics Through the Logical Analysis of Language" (Frank 1963, p. 159).

We should acknowledge that Frank's criticisms were unlikely to succeed because he accuses Carnap of not engaging in a philosophical project that never really interested him to begin with. As Carnap says, as a result of the "division of labor" within the Vienna Circle he is "compelled to leave the detailed work in this direction to

[112] This goal is indeed lofty, but it seems to have been Frank's concern here.

philosophically interested sociologists and sociologically trained philosophers" (Carnap 1963, p. 868). Even though Frank and Carnap did not hold radically different interpretations of logical empiricism, the differences in their work suggest that they did envision different ways of applying logical empiricist thought. Recall that by 1947 Frank was primarily interested in what made metaphysics meaningful and he understood this investigation as pragmatic.

As we saw above, Frank regards the elimination of metaphysics as a genuine social occurrence, an actual elimination. On Carnap's account, where pragmatic considerations are regarded as extra-linguistic, there would have been no room to explore Frank's concerns within the logical analysis of language. On this reading, Frank's argument is not simply a criticism of Carnap's position, but an expression of a different goal for philosophical inquiry that requires philosophers to look beyond formal modes of inquiry.

> In order to understand this objection from a broader background, we have to remember that Pragmatists with strong social interests raise similar objections. John Dewey, e.g. pointed out that one cannot overcome metaphysics fully by proving that it is meaningless but by understanding its meaning fully and exposing it. The word "fully" means: "Including the pragmatic component." The lack of attention given to the pragmatic component brings about, according to the soviet philosophy, a lack of co-ordination between theory and practice and, in connection with it, an exaggerated importance to the logical component (Frank 1963, p. 164).

While much has been said about the nature of Frank's criticisms of logical empiricism, it should be remembered that his meta-philosophical critique prescribes a different course of action, although it does not prescribe a fundamentally different philosophical system.

4.7 Conclusion

In this chapter we explored both Bridgman's operationalism and Frank's use of it in his analysis of scientific theories. Bridgman's account was hyper-individualistic, private, and suffered from an inability to adequately distinguish between the pragmatics and the semantics of scientific theories. Frank's use of Bridgman, however, does not suffer from the same set of problems. Frank's account is public, concerned with democratic values, and emphasizes the pragmatics of scientific theories. By paying attention to the pragmatics of scientific theories we can recover an often-overlooked part of the history of the discipline of philosophy of science. Additionally, by more closely attending to the pragmatics of scientific theories we may begin to reshape how we understand some topics in the discipline. If we follow Frank on this subject, we can understand that problems that apparently have to do with education reflect broader, social concerns. Lastly, by looking closely at his work in the pragmatics of scientific theories, we begin to understand the sense in which Frank can be said to be a pragmatist.

5 Chapter Five

5.1 Introduction

In this chapter, I will end my discussion of Frank's work as Frank himself often ended it: by reflecting generally on how his work contributes to our understanding of theory acceptance and rejection. In Frank's discussions of these issues, he examines how "extra-scientific" factors affect scientific thought, particularly in the context of acceptance and rejection.

> Scientists and scientifically minded people in general have often been inclined to say that these "nonscientific" influences upon the acceptance of scientific theories should not happen; but since they do happen, it is necessary to understand their status within a logical analysis of science. We have learned by a great many examples that the general principles of science are not unambiguously determined by the observed facts. If we add the requirements of simplicity and [agreement with] common sense, the determination becomes narrower, but it does not become unique. We can still require their fitness to support desirable moral and political doctrines. All of these requirements together enter into the determination of a scientific theory (Frank 1957, p. 355).

This text is particularly interesting, for in it Frank states his intention to examine the status of extra-scientific influences[113] within the context of theory acceptance and rejection. Frank's appeal to "logical analysis" and "observed fact" is common among logical empiricists. But, as we see in the quote above, Frank adds to that requirement because he urges his readers to acknowledge the important role played by extra-scientific factors. On Frank's account, "the validity of a scientific theory cannot be judged unless

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[113] Note that by "non-scientific influences" Frank means anything that is not empirically justified, and that would not be considered to be scientific on a logical empiricist account. Frank interchangeably uses the terms "extra-scientific" and "non-scientific." I will continue to use "extra-scientific" for the sake of consistency.

we ascribe a certain purpose to that theory" (Frank 1957, p. 359). By "validity" Frank is not referring to an ideal justification of a scientific theory, but rather to the actual practice of how we judge the merit of a scientific theory. As we saw in Chapter 3, Frank regards understanding the purpose of a theory as a pragmatic task. Therefore, in order to understand the role of extra-scientific factors, the pragmatics of scientific theories must also be investigated.

In his discussions of theory acceptance and rejection, Frank is clear that logical analysis alone cannot answer his question of validity: "the validity of a theory cannot be judged by 'scientific' criteria in the narrower sense: agreement with observations and logical consistency" (*ibid.*). In addition to these modes of analysis, Frank recommends the adoption of "a sociology of science" or a "humanistic background of science" (*ibid.*) in order to address extra-scientific factors within science. To understand what Frank might mean by extra-scientific factors, we can point to his account of simplicity above, where simplicity functions as an epistemic value. However, that leaves a larger question: Do non-epistemic values play a role in Frank's philosophy of science?

In Chapter 2 I argued that although he certainly invites value-laden discussions and critiques of scientific theories, Frank stops short of offering a positive role for values in science (i.e., Frank does not provide a theory of values). I shall suggest here that those findings do not sufficiently capture important aspects of Frank's thought with respect to the question of values in science. As we saw throughout this thesis, Frank frequently wrestled with the question of value, even though he did not engage in outright normative, ethical theorizing. However, in the context of theory acceptance and rejection, as we just saw, Frank's analysis suggests some role for a pragmatic, value-laden evaluation of the validity of scientific theories. So, in order to explain Frank's apparent agnosticism on the question of values in science, I argue that Frank *never endorses the fact/value dichotomy* that is often used to justify the thesis that science is value-free. Instead he seems to avoid this distinction, as evidenced by his remarks in the first chapter of *Science, Facts, and Values*, where he suggests that values be seriously studied.

> One says often that "science" treats only a very poor skeleton of the world, because human personality and human values are eliminated by a far-reaching process of abstraction. But this is only true if we restrict ourselves to logic and semantics neglecting the pragmatic component. The pragmatic studies of science includes the influence of social, philosophical and religious factors upon the invention of scientific symbolism. … By using the pragmatic approach to scientific theories of a certain period, we are impelled to study the human values of this period. For this reason a chief topic of the present book will be the pragmatic approach to present day science including the line of descendance from which our present science has originated. If we want to study this approach, we have to study the symbols of science not only from the scientific angle. We have not only to study the actual meaning of symbols for the purpose of predicting future facts or for producing gadgets; we have also to study the meaning of these symbols as expressions of human aspirations. Thus, the variety of meanings which have been attributed to scientific symbols is a main topic in the present book. (Frank SFV, I, ch. 1, p. 12).

Testifying to the continued influence of this concern, at least six years prior to *Science, Facts, and Values*[114], Frank expressed a similar sentiment in the early sections of *Relativity: A Richer Truth* (1950, pp. 3-6).

In this conclusion, therefore, I will explore Frank's account of the value-free ideal. Specifically, I will argue that Frank rejected the fact/value dichotomy in his early work and that even though this did not result in a positive account of the role of values in science, it did undermine the naïve forms of objectivity, value-freedom and value-neutrality that are often attributed to logical empiricists. Such an approach does not argue

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[114] I say "at least six years" because in the introduction of *Science, Facts, and Values* Frank cites newspapers dated in 1956.

that science is, or should be, value-free (as was done after Frank's lifetime), but it does serve to challenge the standard accounts of logical empiricism.

Locating Frank's work within the context of early debates about value-neutrality is not without precedent. Indeed, Heather Douglas again provides helpful background, indicating that Frank's work was closely connected to the work of other value-freedom and value-neutrality dissidents:

> The most prominent arguments against the view of science as a value free enterprise were developed by C. West Churchman, Philipp Frank, and Richard Rudner. Between 1948 and 1954, both Churchman and Rudner presented widely read arguments that social and ethical values are required components of scientific reasoning, while Frank supported these endeavors. In their view, the scientist as public advisor and decision maker was an important role for scientists, and this role necessitated the use of ethical values in scientific reasoning. The Churchman-Rudner thesis forced philosophers of science into a dilemma: either accept the importance of values in science or reject the role of the scientist as public decision maker. Rather than accept values in science, the field chose the latter option, thus rejecting a careful consideration of the public role of science. This allowed the value free ideal to take up its position as the widely accepted doctrine it became: that scientists should consider only internal values when doing science (Douglas 2009, p. 50).

As Douglas explains, both Churchman and Rudner were concerned with how scientists ought to assess risk. At the dawn of the atomic age, the need for such assessments was urgent. Each concluded that assessments of risk necessitated the use of ethical considerations when accepting scientific theories (Douglas 2009, pp. 50-53). However, the reader should note that the Churchman-Rudner thesis locates values in the context of theory acceptance and specifically when risk is assessed (Steel 2014, pp. 147-149).

In this chapter I argue that Frank's use of values extends beyond the limits of risk assessment characteristic of the Churchman-Rudner thesis. Since assessing Frank's position on the value-neutrality thesis will require appeals to the conclusions reached in previous chapters, we will begin by reviewing some of those arguments (5.2). Prior to arguing that Frank did indeed offer a more robust account of the role of values, we will examine the arguments provided by Churchman and Rudner against the value-neutrality thesis. Following that discussion, we will then see that Frank's arguments against the value-free ideal are ultimately more successful. Specifically, the strength of Frank's arguments lies in his descriptive account of theory choice, where he identifies how metaphysical beliefs lead to misinterpretations of scientific theories. By emphasizing the importance of this pragmatic aspect of theory acceptance, Frank is able to recommend actions that may be taken in order to address the problems posed by metaphysical misinterpretations (5.3). In part, this section will serve to expand the account offered by Douglas, in which Frank seems to play a marginal role (*ibid*.).

5.2 Key Findings

Throughout the Harvard Period Frank developed his mature philosophy of science and paid particular attention to social and political issues. In this section, findings relevant to the role of values in Frank's philosophy of science will be revisited in order to better explicate the nature of his rejection of the value-free ideal in section 5.3. So far, we have traced the development of Frank's philosophy of science, developed a conception of his work as an early example of socially engaged philosophy of science, assessed his account of meaning, and evaluated his use of operationalism. Frank's interest in values was not a late development nor was it a response to the critics of logical empiricism working in the United States. Instead, and contrary to standard accounts of logical empiricism, Frank's interest in values began early and was tied up in his interests in history, sociology, and pragmatism.

In Chapter 1, we saw the evolution of Frank's pragmatic thought, and how

Frank's behavioral account of pragmatism contributed to his interest in extra-scientific factors, or issues relating to value, history, and sociology. Of the many developments in Frank's thought, his early work on conventionalism proved to be the most important in this regard. Frank's 1907 paper, "Experience and the Law of Causality" concludes with the following remark: "With the question of world conception in the ethical-religious sense, all this has nothing whatsoever to do" (Frank 1907/1949, p. 60). This brief remark proved crucial for Frank's intellectual development. Following two incisive criticisms, Frank eventually reformed his early account of causality through the introduction of pragmatic considerations and his conviction that this account had nothing to do with ethical or religious beliefs.

Einstein's criticism came first and convinced Frank that his 1907 account of causality was flawed, because the conventional element in the law of causality can be (and in many cases, should be) constrained by pragmatic considerations.

> [Einstein] agreed with me that, whatever may happen in nature, one can never prove that a violation of the law of causality has taken place. One can always introduce by convention a terminology by which this law is saved. But, it could happen that in this way our language and our terminology might become highly cumbersome. What is *not* conventional in the law of causality is the fact that we can save this law by using relatively *simple* terminology: we are sure that state *A* has reoccurred when a small number of state variables have the same values that they had at the start. This "simplicity of nature" is the observable fact which cannot be reduced to a convention on how to use some words. I realized the Poincaré's conventionalism needs qualifications. One has to distinguish between what is logically possible and what is helpful in empirical science (Frank 1949a, pp. 10-11, italics original)[115].

This exchange led Frank to seriously consider how pragmatic concerns might function

115 Here as in Chapter 1, Frank is presenting his own view of Poincaré. Frank's account is not a reliable account of Poincaré's philosophy.

within the context of empirical science. As a result, Frank's conventionalism was adapted, and eventually came to include pragmatic considerations. He no longer regarded epistemic values, such as simplicity, to be mere conventions. Rather, he came to understand simplicity as a "helpful" conceptual tool.

But Frank did not introduce pragmatic constraints only to augment his account of conventionalism. As a result of the second criticism – Lenin's "unfavorable" discussion – Frank was ultimately led "to investigate the role that metaphysical interpretations of scientific theories have played in support of political and religious philosophies" (Frank 1949a, p. 11). That is, Frank recognized that pragmatic concerns play another important role; they help to shape socially informed accounts of facts. As Frank emphasizes throughout his work, what we regard as a fact may be shaped by other deeply held beliefs. Accordingly, he began to consider how pragmatic aims originate and how they might affect science.

> He [Mach] was accused [by Lenin] of agnosticism, subjectivism, and relativism. It was alleged that Mach had denied that science could know anything about the objective world. In this way, it seemed, the door was opened to other ways of finding truth, particularly the ways of traditional religion (Frank 1949a, p. 17).

Social, religious, or political views may appeal to pragmatic concerns, such as the ability of a doctrine to support moral behavior, in order to accept or reject a scientific theory. So, when the Catholic Church rejected Copernican theory, it was motivated by a desire to preserve its worldview, which rested on the idea that mankind has a special, dignified status that is reflected in the structure of the universe (i.e., in the geocentric solar system).

So, on Frank's account, it is essential to answer pragmatic questions. Since our explanations of the facts are intrinsically linked to what we want to do, our account of facts must reflect the role played by worldviews and interpretation. Extrapolating from his pragmatic descriptions of the physical world, Frank went on to argue that if we want to understand why social and political actors behave as they do, we have to understand their account of "facts." He concluded that any such understanding must identify the practical aims of these groups. It is in this way that Frank's account of facts is socially embedded. Here, "socially embedded" means that our conception of facts is rooted in other beliefs. So, those other beliefs must be understood in order to understand particular instances of theory acceptance and rejection.

As we saw in Chapter 2, Frank's attention to the idea that values may play some role in scientific thought and his interest in political and religious systems of belief have been used by scholars (Howard 2003, 2009; Uebel 2005, 2011; Romizi 2012) to argue that the Left Vienna Circle (LVC) was distinctly politically engaged. Additionally, LVC scholars have argued that this sort of political engagement was somehow a result of the LVC's philosophy of science. However, as Sarah Richardson (2009a/b) argued, the LVC seems not be politically engaged enough for twenty-first-century purposes. She argues further that when we claim that the LVC scholarship shows us how we might reconstitute some form of politically or socially engaged philosophy of science, we continue to ignore and marginalize other political philosophies of science (such as feminist approaches). Even though Frank's philosophy of science is not explicitly political in the way that Richardson would prefer, it does invite and encourage discussions of value, contrary Richardson's criticisms. Frank achieves this by introducing "the critical point of view" (Frank 1917/1949, p. 74), as discussed below.

With the LVC debate in mind, we can examine how Frank's philosophy of science was political and how it incorporated values. In Chapter 2, I argued that Frank certainly contextualized his philosophy of science as political during the Harvard Period. Primarily this was done in his work on education, where he aligned his work in the unity of science with Conant's General Education Program. Specifically, Frank understood education as the means by which students would be prepared to function in democracies. These educational proposals are politically engaged enough that they may supplement Philip Kitcher's[116] concept of tutoring in well-ordered science. Even though Frank's educational proposals require that philosophers of science recognize the social and political potential of their work, he does not require them to augment their accounts of the philosophy of science with social and political beliefs. This is because while Frank's educational proposals rely on a dynamic picture of science, they are not tantamount to a normative system of ethics.

Frank's dynamic picture of science is rooted in a 1917 discussion of Ernst Mach, wherein he argues that Mach is an exemplar of enlightenment philosophy. In that essay, "The Importance for Our Times of Ernst Mach's Philosophy of Science," (1917/1949) Frank develops an account of "the critical point of view" (Frank 1917/1949, p. 74). In trying to understand this critical point of view – how we resist the tendency to reify scientific concepts – Frank turns to Nietzsche. And in so doing, he pays particular attention to Nietzsche's account of perspective and subjectivity.

> That things have a quality in themselves, quite apart from any interpretation and subjectivity, is an idle hypothesis: it would presuppose that to interpret and to be a subject are not essential, that a thing detached from all relations is still a thing (Nietzsche, *The Will to Power*, No. 289 quoted in Frank 1917/1949, p. 77).

Frank's later discussions echo this interest in Nietzsche's account of subjectivity. Recall

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[116] See Section 2.5 or Kitcher 2001 pp. 117-133 for discussions of Kitcher's account of tutoring.

that in *Relativity a Richer Truth,* Frank argues that the "*jobs of finding facts and of interpreting facts are indivisible,*" (Frank 1950, p. 71, italics in the original). Interpretation is an essential part of Frank's account of how we identify what counts as a fact, and interpretation is subjective. There is no "view from nowhere," as we would now say, and there is no ideal position from which we can judge science. Subsequently, objectivity itself can become a problematic value. So it seems that Frank's analysis results in the following conclusion: we can find agreement based on the phenomena, but we can never speak simply about *what* we observe, for we must also address *how* we observe.

Based on the arguments provided so far, it would be possible to view Frank's efforts as isolated attempts to address specific philosophical problems rather than as manifestations of a more integrated line of argumentation. We shall see, however, that Frank's efforts are not isolated but are instead united by his reliance on an adaption of verificationism. Most relevant to the line of thought presently being pursued is Frank's attempt to identify and understand the meaning of metaphysical statements.

> The salient point is that metaphysical propositions about the physical universe are actually meaningful propositions about human behavior or, in other terms, propositions of sociology... The point is that, according to the opinions of the Vienna Circle, metaphysical propositions about the physical world are meaningless within the system of physical concepts, but have meaning within the wider "universe of discourse" that embraces physical and sociological concepts. (Frank SFV, Part II Chapter 5 section 4 p. 8)

On Frank's account, metaphysical beliefs shape our understanding of facts, and as the quote above indicates, metaphysical beliefs can be meaningful if they are interpreted sociologically. This is important because it shows that Frank's account of facts, values, and politics are all understood through his account of meaning.

On this account, metaphysical statements may be understood by appeal to social factors. These factors are the means by which Frank's account of meaning can make sense of metaphysical statements and beliefs. I divided these factors into positive and negative forms, with each representing a unique application of logical empiricist thought. Negative social factors are associated with situations in which scientific theories might be misinterpreted for extra-scientific and, hence, pragmatic reasoning. Positive social factors allow us to understand human behavior as an empirical category within the science of sociology. According to Frank's account of positive social factors,, behaviors that are associated with metaphysical beliefs can be identified and measured (i.e., operationalized) and can be used to empirically interpret metaphysical statements. In different ways, Frank's discussions of these factors show how different social and political worldviews shape our understanding of the world.

Throughout Frank's account of meaning, he frequently appeals to Bridgman's operationalism. Despite this, his account is quite different and does not rest on Bridgman's problematically individualist assumptions. If Frank had completely appropriated Bridgman's view, then his account would be rendered inconsistent. But, as we saw, Frank offered a more pluralistic account of operationalism that in turn supported his interest in socially engaged philosophy, exemplified through the idea of "active positivism."

Frank's socially engaged account of active positivism encourages us to actively support science:

> In our time, the government has to devote a great deal of attention and financial support to scientific research. In a democracy, no government could embark on such a program unless it were supported by its citizens; but they would not support it unless they understood what it was all about. The problem arose of how citizens could learn to judge reports of experts, e.g., about appropriations for research projects, without being specialists in science themselves (Frank

1957/2004, p. xxi).

The problem lies in how the results of scientific theories are communicated to members of the public and whether or not citizens are capable of understanding those results or of judging them as worthy of support (financially and otherwise)[117]. Considering the broad implications that the interpretation of scientific theories has for public life, Frank took a broad view of the philosophy of science and sought to promote its use in contexts beyond the familiar applications to logic and physics.

In this section we have paid close attention to how the pragmatics of scientific theories may contribute to our understanding of what a socially engaged philosophy of science might achieve and to how social and political beliefs affect our understanding of facts. But these achievements are not sufficient to undermine the value-free ideal in Frank's thought. In the next section we will examine Churchman's and Rudner's arguments against the value-free ideal and will see that Frank provided an argument that does not agree with either of theirs.

5.3 Churchman, Rudner, and Frank on the Value-Free Ideal

As discussed in section 5.1, the value-neutrality thesis was entertained in a lively and important debate within the philosophy of science community. This was done despite the fact that many philosophers of science would go on to accept the value-free ideal, relegating scholarly work on science and values to the margins of the philosophy of

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[117] As we saw in chapter 2, Frank often appeals to Conant in making these arguments: "Many people have believed that this goal could be achieved by popularizing the results of science, by adult education courses in which intelligent and interested men and women could absorb the "facts" discovered by scientists in a digestive way. Conant, however, made the point that by absorbing "results" and "facts" laymen could not acquire any judgment about the reports of scientists. What the citizen needs rather is an understanding of how the mind of the scientist works in getting results, and along with it, in what sense are those results "valid" or "reliable" and can be used as a basis for judgment" (Frank 1957, p. xxi).

science community (Douglas 2009). During the late 1940's and into the 1950's, the role and status of values in science were actively debated and explored, even though such explorations were becoming increasingly rare (Reisch 2005). Two of the best-known positions in this debate were those of Churchman and Rudner. In two recent discussions of these positions, offered by Steel and Douglas (Steel 2014; Douglas 2009), papers dating from 1948 are discussed. In this section I will examine the arguments from 1954, which were published in Frank's edited volume, *The Validation of Scientific Theories.* This is because I wish to examine how Frank's views might have interacted with those of C. West Churchman and Richard Rudner. To the best of my knowledge, theirs are the only papers that we can be sure Frank had read and carefully considered (assuming that these anthologized papers were indeed read and carefully considered by the editor). In this section, I will first address Churchman's arguments, then Rudner's, and finally, Frank's.

Prior to beginning this discussion, we must note the strained relationship between the logical empiricists (including Frank) and Churchman (Reisch 2005, pp. 283-289). This strain was exacerbated by the death of William Malisoff in 1947, at which time Churchman assumed the role of editor for the journal *Philosophy of Science* (Reisch 2005, p. 283). Churchman's assumption of this role preceded Frank's election to the office of President of the Philosophy of Science Association (PSA). Churchman's editorship was met with some trepidation on the part of the logical empiricists. The following passage, co-authored by Churchman, provides an example of the views that inspired this trepidation:

> To the logical positivists, investigation into the nature of logic, or as we should prefer to say, formal science, is quite independent of investigation in the nature of

> empirical science. They regard experimental science as dependent upon logical analysis but *they fail altogether to show in what way logical analysis is dependent upon experiment.* This means that for them logical analysis can proceed regardless of experiment, that this method is wholly non-experimental. The result is that the method becomes discursive, verbal, arbitrary, commonsensical, and scholastic (Churchman and Cowan 1945, p. 219, quoted in Reisch 2005, p. 286 italics original).

As Reisch correctly points out, it seems that Churchman misunderstood the explicit attempts by logical empiricists to connect their work with experimental science (Reisch 2005, p. 287). However, Churchman seemed to have a larger, and arguably, more legitimate target:

> There is too much talk and too little action. Action will arise from consideration of such problems as the following:
> 1. What is an operational (i.e., experimental) definition of value?
> 2. How do we determine experimentally the value of any act or sets of actions?
>
> We need more of a science of value and less talk of the value of science.
> (Churchman and Ackoff 1947, p. 271 quoted in Reisch 2005, p. 289).

As we saw in Chapter 4, Frank sought to address at least some of these claims, for he did offer an operational account of democratic values within science. Following this criticism – and perhaps because of it – Frank offered an account of "active positivism," requiring philosophers of science to attempt to understand the social role of science (Frank 1951, p. 26). As we will see, it seems that Frank took Churchman's criticisms to heart.

In Churchman's 1954 paper, "A Pragmatic Theory of Induction," he argues for an account of induction where "the problem is parsed in terms of the relationship between evidence and decisions" (Churchman 1954, p. 18). In arguing that actions are often influenced by sets of beliefs, Churchman emphasizes the importance of how systems of belief may shape our actions, and hence our policies. However, Churchman does not employ a contemporary account of policy, which is intended to guide the application of scientific theories at a high level. Rather, he locates values in small-scale scientific practices that guide data collection, which he refers to as "evidence gathering" (*ibid.*). Both large-scale applications of theories and small-scale data collections are identified as value-laden because they involve actions based on value-laden decisions.

Churchman explicates his account of value-laden decisions via the pragmatic problem of induction: "… it is the determination of the optimum relationship between evidence gathering and the determination of policies" (Churchman 1954, p. 19). Turning his attention to the question of efficiency, by which Churchman means policy optimization, he explores how we might develop processes by which we could, with increasing levels of certainty, achieve our aims (whatever those may be). Throughout his discussion of policy Churchman emphasizes the importance of employing values in our appraisals of scientific theories in order to assess and mitigate potential risks that may be associated with both investigating and implementing theories. These values constrain the set of acceptable solutions for a given problem or implementation (Churchman 1954, p. 23). In this way, our values ground our decisions by delimiting what choices constitute either acceptable or unacceptable risks.

Following Churchman's departure, Rudner took over the editorship of *Philosophy of Science*. Rudner's relationship with logical empiricism seems to have been less vexed than Churchman's (Reisch 2005, p. 341). His position as editor was supported as a result of his expertise in social sciences, and this expertise might have rounded out the specializations that had previously dominated the PSA. Under Rudner's leadership, however, there was a precipitous decline of papers on either social science or science and values in *Philosophy of Science* (Howard 2003). This decline may have resulted from Rudner's own positivist account of social science, which he restricted "to ‘methodological’ problems encountered by social science” (Reisch 2005, p. 357). So, given that Rudner’s administrative contributions seem to have discouraged research into science and values, it is surprising that his 1954 arguments are so strongly in favor of including values into the methodology of science.

While Churchman locates values in the decisions made about data collection and implementation, Rudner locates values in the “validation” of scientific theories. He states that values play ethical, psychological, and logical roles in the process of theory acceptance (Rudner 1954, p. 24). But, Rudner emphasizes that he is specifically interested in identifying the way in which values may be constitutive of scientific practice: “*How sure must we be before we accept a scientific hypothesis depends on how serious a mistake it would be*” (Rudner 1954, p. 26 italics original). So, values are discussed in the context of theory application, and therefore have implications for the acceptance and rejection of scientific theories. By attending to the acceptance and rejection of scientific theories, Rudner preserves a central role for values within the scientific method: “the scientist must make the decision that the evidence is *sufficiently* strong or that the probability is *sufficiently* high to warrant the acceptance of the hypothesis” (Rudner 1954, p. 26 italics original). The mistakes that Rudner emphasizes are to be found in possible applications of a theory, and he argues that we must evaluate the risks associated with these mistakes before accepting a theory. Thus, Rudner gives an account of theory acceptance wherein our ethical evaluations of the possible risks associated with theory acceptance bear on the epistemic credence that is afforded to a theory.

Theory acceptance has both ethical and epistemic aspects on Rudner’s account.

He explicitly rejects the idea that evaluations of applications and acceptance may come apart; he worries that doing so would undermine the role of "scientist qua scientist to validate hypotheses or theories" (Rudner 1954, p. 27).

> The determination that the degree of confirmation is, say, *p* or that the strength of the evidence is such and such, which is on this view the indispensible task of the scientist *qua* scientist, is clearly nothing more than *the acceptance, by the scientist, of the hypothesis that the degree of confidence is p or that the strength of the evidence is such and such;* and, as these men have conceded, acceptance of hypotheses does require value decisions (Rudner 1954. p. 27 italics original).

Therefore, it is the responsibility of the scientist to determine the strength of the evidence for a given hypothesis or theory and to recommend for or against its acceptance based in part upon an assessment of the risk associated with a particular theory.

Like Churchman, Rudner is also concerned with risk, albeit in a more limited context. On his view, we can conclude that confirmation alone is not sufficient to determine whether the risk associated with the acceptance and implementation of a particular theory is morally permissible. And, because the evaluation of risk requires some kind of valuation we may no longer assume the classical ideal of objectivity:

> What seems necessary...is nothing less than a radical reworking of the ideal of scientific objectivity. The naive conception of the scientist as one who is cold-blooded, emotionless, impersonal, and passive, mirroring the world perfectly in the highly polished lens of his steel-rimmed glasses is no longer, if it ever was, adequate.
>
> What is proposed here is that objectivity for science lies at least in becoming precise about what value judgements are being made and might have been made in a given inquiry—and, stated in the most challenging form, what value decisions ought to be made (Rudner 1954, p. 28).

Appealing to his cultural milieu, Rudner frames this discussion by referring to both the Manhattan Project and the "horror" with which most scientists regarded the intrusion of religious values into scientific thought (Rudner 1954, pp. 27-28). Given the deep problems associated with each, objectivity itself must be subject to scientific analysis lest scientists cede this responsibility to non-experts, ideologues, or worse, chance.

Both Churchman and Rudner argue that if we acknowledge the importance of risk analysis in the acceptance of scientific theories, then we must also acknowledge an important role for values in science. However, both accounts focus primarily on risk, and how the assessment of risk requires appeal to values. On Churchman's account, values help to solve the problem of induction because values may serve as a tool by which we can evaluate evidence and theories. So, on his account, if a particular risk is morally impermissible, then we should reject the theory. And, on Rudner's account, our ethical evaluations restrict our epistemic evaluations because when we evaluate the application of a theory we must also decide to accept it or not. So, in both cases, morally significant risk is involved in recommending for or against theory acceptance in the epistemic context.

Churchman's and Rudner's arguments are, however, highly problematic. Despite the importance of considering morally significant risk, doing so should not guide our account of theory acceptance. Theory acceptance is based upon the epistemic status of a theory (e.g., true, empirically adequate, etc.). Deciding whether or not to use a theory can and should involve moral considerations. However, if we run these two problems together, as Churchman and Rudner suggest, then our accounts of theory acceptance and of theory implementation will become irreparably damaged. There are many instances in which morally significant risk results from scientific knowledge. For example, developing deadly, highly infectious strains of flu poses a serious humanitarian risk even though developing a vaccine prior to the natural evolution of that particular strain of flu may result in a humanitarian benefit. When we ask whether or not such a strain of flu should be developed, we should undertake an ethical evaluation. Such a humanitarian risk does not, however, change the epistemic status of our theory. Since this problem does not cross into the epistemic realm of theory acceptance, the severity of the moral threat does not change the fact that our theories of virology are well justified. We should also note that Churchman's account of evidence gathering does not avoid this problem, since he values evidence gathering precisely because it helps to determine how we calculate risk.

And although Rudner acknowledges the epistemic and ethical aspects of theory acceptance, he does not meaningfully distinguish them because doing so would undermine the scientist's moral responsibility to evaluate risk. In other words, on Rudner's account, the scientist should evaluate the theory on both epistemic and on moral grounds. Both of those evaluations should then factor into whether or not we accept a given hypothesis. One might argue that when Rudner discusses theory acceptance, he means something akin to theory application. And, if this is true, then Rudner is not mistaking the epistemic issue of acceptance (whether or not we should believe in a theory) for the ethical aspect of theory acceptance (whether or not we should apply the theory given its morally significant risk).

While such a position would indeed be stronger, it is not the position pursued by Rudner. Instead, on his account, ethical considerations are understood as a part of the justification of scientific theories: "…the making of such judgements [moral/ethical judgements] is *logically* involved in the validation of scientific hypothesis; and consequently that a logical reconstruction of this process would entail the statement that a value judgment is a requisite step in the process" (Rudner 1954, p. 24, italics original). Indeed, Rudner wants to show that ethical considerations relating to risk are "involved in the scientific method as such" (Rudner 1954, p. 25). Since "no scientific hypothesis is ever completely verified," ethical considerations must be included in our account of theory acceptance. So it seems unavoidable that, on Rudner's account, the epistemic status of a theory is tied to its moral risk.

Churchman's and Rudner's positive accounts of the role of values are not tenable. Frank never offers the kind of full-throated endorsement of ethical and moral assessments in the context of justifying scientific theories that Churchman and Rudner provided. I shall argue that his view is ultimately stronger and more tenable than theirs. As we already saw, Frank appeals to values at the level of the interpretation of evidence, and hence of theory acceptance. By virtue of his account of interpretation, Frank avoids the kind of problem discussed above. Furthermore, this view leads to Frank's rejection of the value-free ideal because "facts" are always interpreted and there is no neutral ground from which we can judge either evidence or theories. Recall that Frank is skeptical of the notion of objectivity, since he worries that both objectivity and truth may lead to a static account of science that reifies scientific concepts and thus eschews the "critical perspective" that encourages a dynamic picture of science. This seems to be a largely descriptive account of the role of values in science, for Frank's only normative recommendation is that we should avoid misleading notions of truth and objectivity that incorrectly reify scientific concepts, hence promoting false certainty.

Specifically, Frank explores why scientific values, such as simplicity[118], have different interpretations (Frank 1954b, p. 4) in the contexts of different worldviews. On

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[118] Simplicity is the only value under discussion here because it is the only value that Frank discusses at length.

Frank’s account simplicity is a non-epistemic value because our specific accounts of simplicity will vary in accordance with our worldviews (or world-conceptions, in the case of the logical empiricists). In the remainder of this section I discuss Frank’s account of simplicity and show that this account attempts to make disputes about theory acceptance understandable.

For example, even though mathematical simplicity is a good scientific reason to accept the heliocentric model of the solar system, it will not be convincing to those who prize consistency with their religious worldviews (Frank 1954b, p. 5). This is because a distinct notion of simplicity is at work in these accounts. Mathematical simplicity functions as an epistemic value since it promotes an intellectually fruitful model of the solar system. Likewise, religious accounts also locate simplicity in an epistemic context, but they define “simplicity” as consistency with first principles and with religious dogma. In this dispute the term “simple” takes on two distinct meanings. Both meanings purport to make epistemological claims, but the conflict is not just about epistemic matters. According to Frank, we cannot understand either the scientific or theological account of simplicity without appeal to the social, political, or historical background of those beliefs.

Frank considers the choice between the geocentric and heliocentric models of the solar system and the issue of simplicity from the perspective of proponents of the geocentric model:

> Newtonian laws of motion only have a simple form if the sun is taken as a system of reference and not the Earth. But the decision in favor of the Copernican theory on this basis could be made only when Newton’s laws came into existence. This act requires creative imagination…(Frank 1954b, p. 7).

During the time of this debate it was widely taken to be “self-evident” that acceleration due to gravity always occurred in straight lines directed towards the center of the Earth.

Therefore, the choice was not just between religion and science. Rather, it was between Copernicus' system (which defied common sense, contradicted religious teaching, and undermined a thousand years of philosophy based upon the value of reasoning from self-evident principles) and an apparently more consistent and understandable system (Frank 1954b, pp. 7-8). Here Frank is not indicating that the theological reading is correct. Rather, he is indicating that the rejection of Copernican theory was based upon a distinct notion of simplicity. On this account, the Copernican notion of "fact" that seemed to explain some empirical evidence contradicted supposedly unassailable moral truths. Since simplicity means something different for the Ptolemaics and the Copernicans and because those understandings are rooted in distinct social situations, the dispute cannot be cast in purely epistemic terms, according to Frank.

Frank's account of theory acceptance is complex in part because it casts the debate as a result of genuine disagreements. The complexity lies in the fact that we cannot be clear what is meant by basic terms such as "simplicity" because evidence ("fact") is at least partly determined by social, historical, and religious beliefs. And in this way, non-epistemic values play some role in establishing what is meant within systems of belief and in how facts are identified and interpreted within that system. (Positive social factors are at work here.) But Frank does not present Copernicus' story merely as a cautionary tale:

> Looking at the historical record, we notice that the requirement of compatibility with common sense and the rejection of "unnatural theories" have been advocated with a highly emotional undertone, and it is reasonable to raise the question: What was the source of heat in those fights against new and absurd theories? Surveying these battles, we easily find one common feature, the apprehension that a disagreement with common sense may deprive scientific theories of their value as incentives for desirable human behavior. In other words, by becoming incompatible with common sense, scientific theories lose their fitness to support

> desirable attitudes in the domain of ethics, politics, and religion (Frank 1954b, p. 9).

As this passage indicates, values must also play an important role in our decisions about theories, particularly in cases of underdetermination (which Frank appeals to in this presentation of the Copernican conflict). These conflicts result from widespread beliefs about what constitutes desirable human behavior and on widely held social values that may play a pragmatic role in theory choice[119].

In line with Churchman and Rudner, Frank argues that because scientists are human, they are predisposed to hold some moral belief or another (Frank 1954b, p. 12). And, like Churchman and Rudner, Frank holds a pragmatic view of scientific theory choice and agrees that it is important for us to make informed decisions regarding how we should use our theories. But Frank's pragmatic view of theory acceptance is deeply social. He understands theory choice not as an individual matter, but as a group behavior. As a result, the practical choice of theories requires informed democratic decisions. Frank explicates his position in the context of science policy:

> The policy making authorities are, from the logical viewpoint, "free" to make their choice of which type of plane should be put into production. However, if we look at the situation from the perspective of unified science that includes both physical and social sciences, we shall understand how the compromise between speed and safety, between fun and endurance is determined by the social conditions that produce the conditioned reflexes of policy makers. The conditioning may be achieved, for example, by letters written to congressmen. As a matter of a fact, the building of a scientific theory is not essentially different from the building of an airplane (Frank 1954b, p. 15).

In the pragmatic context of theory application, Frank positions his account of

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[119] I do not think that Frank approves of the rejection of Copernicus' theory based on the moral/religious grounds described above. Rather, I believe that Frank wishes to cast the debate as reasonable. In so doing he suggests that we can come to understand the philosophical problems that produced the conflict (instead of continuing to investigate the many reasons why one theory was true and the other false).

scientific theories as democratic and participatory. On this view, we may use empirical information to argue for or against the application of a particular theory. Recall that on Frank's account "'agreement with observed facts' never singles out one individual theory" (Frank 1957, p. 355). But, unlike Rudner's position, this does not mean that we must rely on ethical considerations to decide whether or not we ought to accept a given theory for epistemic reasons (e.g., because it is technically correct). Rather, Frank suggests that we investigate the dispute itself by empirically understanding the different notions of simplicity and how they inform differing positions. Frank's empirical, descriptive account of values such as simplicity will not cause a change of mind in any party but it will help us to understand the social situation within which the dispute emerged. This improved understanding can help us develop new avenues of solution (e.g., writing letters to senators), particularly when the nature of the dispute is pragmatic and social and is not epistemic. But Frank does not limit his account to this pragmatic context and hence to issues arising from the applications of scientific theories.

On Frank's account, disputes must be resolved with respect to our aims. Epistemic disputes are resolved with respect to "the observed facts," and sociological disputes are resolved with respect to our beliefs about how the world should be. So, in evaluating a theory,

> …we have to ask the preliminary questions: What purpose is the theory to serve? Is it only the purely technical purpose of predicting observable facts? Or is it to obtain a simple and elegant theory that allows us to derive a great many facts from simple principles? We choose the theory according to our purpose. For some groups, the main purpose of a theory may be to serve as a support in teaching a desirable way of life or to discourage an undesirable way of life. Then, we would prefer theories that give a rather clumsy picture of observed facts…If we wish to speak in a brief and more general way, we may distinguish just two purposes of a theory: the usage for the construction of devices (technological purpose) and the usage for guiding human conduct (sociological purpose). (Frank 1954b, pp. 15-

16).

Frank's technological purpose maps onto what I have called the epistemic aspect of theory acceptance; for it judges the correctness of our theory relative to observed facts. And Frank's sociological purpose concerns the role that the theory might play in public life, and hence what I have called the pragmatic aspect of theory acceptance. By distinguishing between the pragmatic and the epistemic components of theory choice, Frank avoids the problems Churchman and Rudner face as a result of their recommendation that we evaluate the epistemic merits of evidence and theories based on moral evaluations[120].

Even though Frank did not provide his readers with a normative account of values, he did challenge the value-free ideal. In his discussions of values, Frank highlights the role that values can play in our disputes about scientific theories. To address this issue Frank argued for a more inclusive form of philosophy of science that seeks to better understand the complex nexus of social and political issues at play when we interpret and accept scientific theories. Careful interpretations and instances of theory acceptance must include a consideration of values that spans both the pragmatic and epistemic contexts. For these reasons, continued attention to Frank's work stands to benefit contemporary work on socially engaged philosophy of science. In Don Howard's paper, "Better Red Than Dead," he spells out the challenge for social engagement and a possible solution:

No one disputes the old empiricist ideal of letting the evidence do all of the work

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[120] The reader should note that we can be motived by both technological and social reasons on Frank's account, so long as we understand that each reason entails a different sort of justification.

> that it can, and everyone prefers evidence to prejudice. But the real challenge arises in those all too-common cases of real, if transient, underdetermination in settings where action is required. How do we proceed when the evidence equally warrants importantly different theories and models? Can the philosopher of science help?
>
> There are many ways in which the philosopher of science can help. If the philosopher of science respected as philosophical the perspectives afforded by history, sociology, and psychology, then the philosopher could bring to bear the lessons of history, sociology, and psychology to understand the forces moving individual scientists and whole communities this way and that. (Howard 2009, p. 206)

With regard to the question and the answer, Frank's work seems to be an ideal fit.

5.4 Conclusion

Logical empiricism is often remembered as an initially promising approach to the philosophy of science that eventually became bogged down in technical and, at times, esoteric discussions. When not recalled in this manner, it is remembered simply as a failed enterprise. In recent decades, however, some philosophers have begun to cast doubt on these myopic portrayals of logical empiricism. The works of Carnap, Neurath, Schlick, Hempel, and Reichenbach have been reread and reexamined with renewed interest. At the same time, despite his widely acknowledged importance to the development of logical empiricism, few works have been devoted to a careful study of Frank's philosophy of science. Given the diversity of his range of interests—sociology, values, history, and pragmatism—and the longevity of the caricatures of logical empiricism, this neglect is most unfortunate.

Frank's intellectual lineage would have allowed him to pursue much of the formal, technical work that the logical empiricists are remembered for today. He was handpicked by Einstein as his successor at the German University of Prague, and worked with many of the leading thinkers of his day, such as Mach, Boltzmann, and Hilbert. But despite this impressive academic pedigree, Frank opted for a different path. Along with his colleagues in the first Vienna Circle, Frank began his intellectual career not with small-scale technical work, but by pursuing a research question that was remarkably broad in scope: How can science and the humanities be brought together? On the one hand, this inquiry introduced him to the work of Abel Rey, from whom he learned of the crisis in philosophy (1.3). On the other, he began to study philosophy, which eventually resulted in his rigorous and widely-read analysis of causality (1.4). As a result of both these trajectories, Frank eventually became interested in the role that history, sociology, values, and pragmatism play in science.

In this thesis, I have attempted to reinforce and critically examine how history, sociology, values, and pragmatism figure into Frank's philosophy. He brought to bear rich historical resources on his philosophical work, sometimes to explicate an idea by appealing to an example. Oftentimes, however, Frank engaged historical literature in order to assess how our philosophical ideas shape our experience of the world. As we saw in Section 2.6, he encouraged us to critically examine our philosophical beliefs in order to guard against dangerous reifications of metaphysics. Moreover, due to the pragmatic bent of his thought, Frank encouraged us to scrutinize the manner in which metaphysics and misinterpretations of scientific theories can direct human conduct.

Frank's philosophy of science is also noteworthy for its attention to pragmatic issues in science and in science education. But it is important to remember in this context that Frank not only highlighted the value of pedagogy but also argued for the importance of *critical*, philosophical training. Recall that his alliance with Conant resulted in the development of a democratic account of science and that he frequently advocated for improvements in science education. By placing his educational proposals squarely in the pragmatic context, Frank highlighted one way in which philosophical thinking might benefit students preparing to become democratic citizens. In this way, Frank applied his philosophical insights in the real world by engaging with an important issue of his time.

In this chapter, and throughout this thesis, I have explored how Frank understood science in its social context. Frank's ambitious project is rooted in a desire to make scientific work broadly understandable. He used sociology in order to describe metaphysical systems, despite his continued adherence to modified forms of verificationism and operationalism. In this context, sociological information can be understood as meaningful if the theoretical statements of sociology can be connected to empirical content. This use of sociology allowed Frank to descriptively analyze the rejections and politically motivated interpretations of scientific theories. Further, he recognized that the source is not mysterious, but empirically discoverable (using the social factors described in Section 3.7); and if it is discoverable, we may come to recognize that our conception of "facts" is sometimes guided by social, political, and historically informed beliefs. To his credit, Frank did not demonize the tendency to misinterpret "facts" that conflict with deeply held metaphysical beliefs. Rather he sought to understand it.

Focusing on the hope for progress brought about by scientific advancement, Frank highlighted the need for improved public dialogue and, significantly, emphasized the promise of science to further promote human welfare. As Gerald Holton has argued, Frank's view remains valuable today:

> We are in some ways right back at the place where Philipp took up his battle on behalf of the rational analysis of science and its philosophy in the wider culture of

> the day. If he were now among us again, he would again have a hard missionary task, but he would also have many allies. Moreover, something new has been happening to help him and us. I mean that scientific research over these past few decades is reaching more and more for a new sort of unity or integration. This time this is not driven by philosophy of science or a search for unity with philosophy, but is appearing as if spontaneously in the progress and pursuit of science itself. There is a new and increasing coalescence of scientific disciplines in many areas. Thus the discovery of the structure of the genome not only required contributions from parts of physics, chemistry, biology, mathematics, and information technology, but in turn it lead to further advances in biology, chemistry, physics, technology, medicine, ecology, and even ethics. And all this scientific advance, as Philipp always insisted, led, as it should, to the hopeful betterment of the human condition, as had also been announced in the last sentence of the Vienna Circle's Manifesto of 1929 (Holton 2006, p. 309).

In order to address social issues arising from the misuse of science, we first have to understand these issues. While many of his positivist colleagues provided tools to promote the understanding of science itself, Frank used those tools to investigate how science can be used to benefit society. Ignoring the social value of science constitutes an important intellectual failure. As scholars continue to revitalize the spirit of logical empiricism, it would be a serious mistake to continue to ignore the socially engaged work of Frank.

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Blue and Grey Scholarship, The University of North Florida, 2004-2008.
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