

American Philosophy and the Intellectual Migration

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Volume 1

American Philosophy and the Intellectual Migration

Pragmatism, Logical Empiricism, Phenomenology,
Critical Theory

Edited by
Sander Verhaegh

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The refugee arrives only after he has gone through experiences perhaps unparalleled in the history of modern mankind... Only his education and his former status are left as bases of self-respect and emotional balance... The American is at an utter loss to understand the émigré's tenacious clinging to the old occupation.

—Gerhart Saenger, refugee philosopher
(“The Psychology of the Refugee,” 1940, 265–267)

I have been having a rather hectic time... Half of Europe seems to be passing through New York. And I have been seeing a sizeable number of these... Some of the ... people I have met ... have harrowing experiences to relate; and seeing so many splendid men splendidly equipped intellectually, but without any prospects, one does not quite know what to do with one's own despair.

—Ernest Nagel, U.S. philosopher (letter to Charles Morris, Oct. 20, 1938)

Preface

This volume collects papers presented at the conference “Euro-American Migration and the Development of Postwar Philosophy,” which took place at Tilburg University in August 2022. This conference marked the start of the ERC/NWO research project “Exiled Empiricists: American Philosophy and the Great Intellectual Migration” (2022–27). Both this volume and the conference would have been impossible without financial support from the European Research Council (grant 2021-STG, 101039904) and the Dutch Research Council (grant VI.Vidi.201.115) as well as Tilburg University’s Department of Philosophy, which helped to make this volume open access. I am very grateful for this support. In addition, I would like to thank the participants at the conference, the contributors to this volume, and the members of the *Exiled Empiricists* Research Project: Claudia Cristalli, Eugenio Petrovich, Fons Dewulf, Gregor Bös, Nina IJdens, and Ties van Gemert. Finally, I would like to thank Christoph Schirmer and the external referees who helped reviewing the book proposal and the manuscript for De Gruyter; and Catherine Kendig, Lydia Patton, and Alan Richardson, editors of De Gruyter’s History of Philosophy and Science book series.

Sander Verhaegh
May 2024

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Archival Sources

ASP	Alfred Schütz Papers, Beinecke Rare Book and Manuscript Library, New Haven, CT
CHP	Carl Gustav Hempel Papers, Archives of Scientific Philosophy, University of Pittsburgh
CUA-OPR	Columbia University Archives, Office of the President records
CUA-OVPR	Columbia University Archives, Office of the Provost and Vice President for Academic Affairs records
ECFSR	Emergency Committee in Aid of Displaced Foreign Scholars Papers, New York Public Library, Archives & Manuscripts
ENP-ASP	Ernest Nagel Papers, Archives of Scientific Philosophy, University of Pittsburgh
ENP-CUA	Ernest Nagel Papers, Columbia University Archives
FJEWPP	Frederick James Eugene Woodbridge Papers, Columbia University Archives
H DPR	Harvard University Department of Philosophy Records (1906 – 1979), Harvard University Archives
HFP	Herbert Feigl Papers, University of Minnesota Archives, Minneapolis & Philosophical Archive (Philosophisches Archiv) of the University of Konstanz
HRP	Hans Reichenbach Papers, Archives of Scientific Philosophy, University of Pittsburgh
HSP	Harlow Shapley Papers, Harvard University Archives.
JPC	Journal of Philosophy Correspondence, 1892 – 1943, Columbia University Archives.
MFP	Marvin Farber Papers, University Archives: The State University of New York at Buffalo
MRC	Max Rieser Collection, Leo Baeck Institute
ONN	Otto Neurath Nachlass, Wiener Kreis Archiv, Rijksarchief in Noord-Holland, Haarlem
RCP	Rudolf Carnap Papers, Archives of Scientific Philosophy, University of Pittsburgh
RVMP	Richard von Mises Papers, Harvard University Archives
SHP	Sidney Hook Papers, Hoover Institution Library & Archives, Stanford University
SPSL	Archive of the Society for the Protection of Science and Learning, Bodleian Archives & Manuscripts
USCPR	University of Southern California School of Philosophy Records, USC Libraries Special Collections
WHC	William Ernest Hocking Correspondence, Houghton Library, Harvard University

Other Abbreviations

AHA	American Humanist Association
APA	American Philosophical Association
CP	Peirce, Charles Sanders: <i>Collected Papers of Charles Sanders Peirce</i>
DDEP	Digitale Datenbank Exilphilosophie
IEUS	International Encyclopedia of Unified Science
IIE	Institute of International Education
IISR	International Institute for Social Research
IPS	International Phenomenological Society

XII — Table of Abbreviations

IU	Indiana University
NSSR	The New School for Social Research
NYRB	<i>New York Review of Books</i>
PROC	Proceedings of the American Philosophical Association
PPR	<i>Philosophy and Phenomenological Research</i>
SDS	Students for a Democratic Society
SPEP	Society for Phenomenology and Existential Philosophy
UCLA	University of California, Los Angeles
UIUC	University of Illinois Urbana-Champaign
UNCCH	University of North Carolina at Chapel Hill
USC	University of Southern California

Sander Verhaegh

1 Introduction: American Philosophy and the Intellectual Migration

In the 1930s and 1940s, thousands of European intellectuals sought refuge in the United States, hoping to find academic employment and a safe haven across the Atlantic (Fermi 1968; Breitman and Kraut 1987). Some of them had been dismissed from their jobs after the NSDAP seized power in Germany. Others were escaping the growing hostilities toward Jews and other minorities following the rise of fascism in Austria, the *Kristallnacht*, and the start of World War II. A 1934 press release estimates that “more than 1,300 scholars” and “some 7,000 students” were displaced after the German Reichstag passed the Law for the Restoration of the Professional Civil Service.¹ Many more fled when the threat of war intensified in the late 1930s. In total, the Emergency Committee in Aid of Displaced Foreign Scholars received about 6,000 applications from “displaced scholars and professional persons” between 1933 and 1945. Only a fraction of them, less than six percent, were granted financial assistance.²

Among the intellectual migrants were more than one hundred philosophers from a variety of schools, many of whom would have a significant impact on the American intellectual climate. Prominent logical empiricists (e.g. Rudolf Carnap, Carl Gustav Hempel, and Hans Reichenbach) helped shape and institutionalize U.S. philosophy of science. Members of the Frankfurt school (e.g., Theodor Adorno, Max Horkheimer, and Herbert Marcuse) had a formative influence on sociology and the New Left. Phenomenologists (e.g., Aron Gurwitsch, Herbert Spiegelberg, and Alfred Schütz) contributed to the reception of what we nowadays call ‘continental’ philosophy. And a group of political philosophers (e.g. Hannah Arendt, Leo Strauss, and Eric Voegelin) influenced the development of American political theory. All in all, dozens of migrants acquired positions at elite universities such that, in 1953, eight out of the eleven most prestigious U.S. philosophy departments had émigrés on staff.³

1 “7500 Academic and Kindred Refugees from Germany,” Emergency Committee in Aid of Displaced Foreign Scholars, July 21 1934, cited in Löhr (2014, 231). The University of Berlin alone terminated 278 out of 797 contracts after the law took effect (Grüttner and Kinas 2007).

2 Emergency Committee in Aid of Displaced Foreign Scholars Records, NY Public Library Archives & Manuscripts; <https://archives.nypl.org/mss/922> (accessed December 2023).

3 They were Rudolf Carnap (University of Chicago), Erich Frank (University of Pennsylvania), Carl Gustav Hempel (Yale University), Werner Jaeger (Harvard University), Jan Kalicki (UC Berkeley), Walter Kaufmann (Princeton University), Paul Kristeller (Columbia University), Hans Meyerhoff

It is no exaggeration to conclude that the intellectual migration was one of the most formative events of twentieth-century philosophy. The brain drain helped shift the academic center of gravity from the German-speaking to the Anglophone world, which saw an influx of dozens of scholars who had dictated the philosophical conversation during the interbellum. The encounters between European and Anglo-American philosophers led to a cross-fertilization between two intellectual cultures, thereby giving rise to many new approaches, perspectives, and schools of thought. Especially the rise of analytic philosophy in the 1950s is inextricably tied to the intellectual migration. Logical empiricism had been a relatively minor movement in the German-speaking world, where the philosophical debate was dominated by phenomenology and *Lebensphilosophie*, but the roles were remarkably reversed in the United States (Hardcastle and Richardson 2003; Verhaegh 2020a, 2020b and 2020c). Beyond the analytic mainstream, there was a lively reception of critical theory, psychoanalysis, and phenomenology, inside and outside philosophy departments, in the decades following the war (e.g. Burnham 2012; Ferri and Ierna 2019; Strassfeld 2022; Wheatland 2009).

This volume explores the impact of the intellectual migration on twentieth-century philosophy by bringing together historians specialized in a variety of scholarly traditions: pragmatism, idealism, naturalism, analytic philosophy, neo-realism, phenomenology, feminism, logical empiricism, humanism, critical theory, Straussianism, and Christian conservatism. Whereas existing work on the effects of the migration tends to focus on individual refugees and schools of thought,⁴ this volume presents twelve case studies on a variety of philosophers and movements, thereby offering a broader perspective on a period that has shaped the development of philosophy in the last half century. The twelve chapters reconstruct how encounters between different philosophical movements generated new traditions. But they also show how certain perspectives were forgotten or marginalized, and how cultural misunderstandings led to philosophical confusions that still affect the discipline today. This introductory chapter, finally, presents a list of 178 ex-

(UCLA), Hans Reichenbach (UCLA), and Paul Schrecker (UPenn). Some philosophers (broadly defined) were based at the same universities but had positions in other departments. They include Egon Brunswick (Berkeley), Philipp Frank (Harvard), Hermann Fränkel (Stanford), Kurt von Fritz (Columbia), Else Frenkel-Brunswick (Berkeley), Erich von Kahler (Cornell), Ernst Kapp (Columbia), Hans Kelsen (Berkeley), Hans Loewald (Yale), Herbert Marcuse (Columbia), Richard von Mises (Harvard), Franz Neumann (Columbia), Friedrich Solmsen (Cornell), Yves Simon (Chicago), Frederic Spiegelberg (Stanford), Leo Strauss (Chicago), Alfred Tarski (Berkeley), Robert Ulich (Harvard), and Joachim Wach (Chicago). On the most prestigious mid-century philosophy departments, see Strassfeld (2020).

⁴ See this chapter's list of references for a select overview. A notable exception is Beck and Coomann (2018).

iled philosophers (section 1.1), argues that historians ought to develop a broader perspective on the effects of the migration (sections 1.1–1.2), and provides an overview of the twelve chapters in this volume (section 1.3).

1.1 Quantifying the Philosophical Migration

Several historians and sociologists have tried to quantify the intellectual migration. Yet estimates of the number of exiled academics, students, and scholars widely differ, depending on the period of investigation, method of computation, and definition of intellectual. A 1947 report from the Committee for the Study of Recent Immigration from Europe estimates that about a quarter million refugees settled in the United States between 1933 and 1945 and that more than twenty thousand of them were engaged in “professional” occupations—a broad category including, among others, architects, chemists, and clergymen (Davie 1947, xvi–xviii). Between 5,322 and 5,469 of them are classified as “professor and teacher” or as “scientist and literary person,” depending on the precise method of estimation (Davie 1947, 41). Laura Fermi, author of one of the first book-length publications on the intellectual migration worked with a narrower definition of ‘intellectual’ and studied “the men and women who came to America ... with ... Ph.D.’s or diplomas from art academies ... and who continued to engage in intellectual pursuits in this country” as well as “a group of younger people” with a “degree from a European institution of higher education” but who had not “yet entered a career ... when they left Europe” (Fermi 1968, 4, 12). She arrives at a list of “about 1,900 names” but explicitly notes that this underestimates the total “size of the intellectual wave” as she limited her study to migrants who were successful enough to be included in reference works such as *Who’s Who in America* (Fermi 1968, 11–13). Bat-Ami Zucker, finally, has calculated the number of immigrants who arrived in the U.S on a special visa for professors and ministers (section 4d of the Immigration Act of 1924) or students (section 4e) and concludes that 19,082 immigrants in these categories—i.e. 13,322 students, 944 professors, 2,184 ministers, and 2,632 family members of these professors and ministers—entered the United States between 1933 and 1941.⁵

Estimates of the number of migrant *philosophers* are equally divergent. The aforementioned report from the Committee for the Study of Recent Immigration from Europe classifies 62 refugees as philosophers, including 44 professors, *Privatdozenten*, *Dozenten*, assistants, researchers, and lecturers as well as 18 scholars

⁵ House Committee on Immigration and Naturalization. *Restriction of Immigration*. 68th Congress, Rep. No. 350, pt. 1, 16; Zucker (2001, 159).

“whose records did not show former affiliation with a European institution or university” (Davie 1947, 314–316, 320–321). Fermi’s book includes a short section on migrant philosophers but lists just a few dozen of the most influential émigrés—e.g. Adorno, Arendt, and Carnap—in this section or elsewhere in her book. A 1942 inventory of “exiled scholars in the field of philosophy” compiled by the American Philosophical Association, finally, lists 77 migrant philosophers looking for a position at a U.S. college or university. The APA Committee on Exiled Scholars, chaired by Columbia philosopher Horace L. Friess, distributed this inventory to all members of the Association in the hope that it would “serve as more than a mere report, and that in our present common struggles for a humane civilization these scholars can be aided through our efforts to give the fullest service to the profession and to the nation.”⁶

All these existing lists underestimate the number of exiled philosophers, however. Fermi exclusively discusses the most successful migrants and the APA inventory only includes philosophers who had (a) turned to the association for help and (b) had not yet found a position by 1942. Philosophers who had been dismissed from their jobs in 1933 are largely absent, for example, as many of them had been in the country for almost a decade. Nor does the APA inventory include philosophy students who had started their education in Europe and were taking their advanced degrees in philosophy in the United States.⁷ The 1947 Report from the Committee for the Study of Recent Immigration from Europe, finally, underestimates the number of exiled philosophers as it relies on a relatively narrow definition of who to count as a philosopher. Refugee scholars were assigned just one field of study even though many of them had degrees in multiple fields or “had to change disciplines in order to meet American market requirements” (Davie 1947, 315). Prominent philosophers who had found positions in mathematics (e.g. Tarski), psychology (e.g. Erich Fromm), or political science departments (e.g. Strauss), for example, are not included in their number.

Table 1 provides a more extensive list of 178 exiled philosophers (broadly conceived) supplementing the 1942 APA inventory with more than one hundred philosophers who had already found a position or never turned to the association for assistance. Many of these names were kindly provided by a research team at the University of Jena which is currently building a *Digitale Datenbank Exilphiloso-*

6 “An Inventory of Personnel,” APA Eastern Division’s Committee on Exiled Scholars, April 1942, Max Rieser Collection, Leo Baeck Institute (hereafter, MRC), Box 1, Folder 5. All 77 migrants on the APA list were already in the U.S. The committee also published a second inventory of philosophers who were still in Europe but hoped to come to North America.

7 “Report of The Eastern Division’s Committee on Exiled Scholars for 1939,” December 29, 1939, MRC, Box 1, Folder 5.

sophie (DDEP).⁸ A few dozen additional names were included after consulting the vast secondary literature on the lives and works of philosophical émigrés (see the final column of Table 1), and a number of handbooks and guides, including the inventory *German Jews in the United States: A Guide to Archival Collections* (Wilhelm 2008), *Biographisches Handbuch der deutschsprachigen Emigration nach 1933–1945* (Röder and Strauss 1999), *The Dictionary of Modern American Philosophers* (Shook 2005), and a list of 300 intellectual migrants appended to Fleming and Bailyn (1969).⁹ The table is intended to be as complete as possible but should not be treated as an exhaustive list. Philosophers from France and Poland may be underrepresented, for example, as migration from these countries has been less well documented than migration from their German-speaking neighboring countries.¹⁰

In compiling the list, I followed the DDEP in working with a broad conception of ‘philosophy.’ Table 1 includes both academics with rich careers as philosophy professors and scholars who formally worked in different fields—e.g. physics, history, or sociology—but who had a degree in philosophy or regularly contributed to debates on (the history of) philosophy. The philologist Ernst Kapp, for example, is included because he published on the development of ancient logic (Kapp 1942; 1965), while chess world champion Emanuel Lasker is listed because he studied philosophy and published two philosophical monographs in the 1910s (Lasker 1913; 1918). Table 1 also includes scientists, humanists, and mathematicians who were closely affiliated with philosophical movements such as the Frankfurt School, the Berlin Group, and the Vienna Circle (e.g. Leo Löwenthal, Kurt Lewin, and Philipp Frank). Finally, I included displaced philosophy students in case they had already obtained a degree in Europe before they completed their studies in the United States. These include, among others, Walter Cerf, Robert Hartman, Ernst Maier, and Werner Marx. Migrants who arrived in the United States in their teens and obtained their first philosophy degree in their new home country—e.g. Adolf Grünbaum, Nicholas Rescher, and Guy Stern—are not included. I also excluded philosophers who arrived in the United States after 1945—e.g. Werner Falk, Hans Jonas, Benita Luckmann, and Rose Rand—even if they had spent a period

⁸ I would like to thank Max Beck, Nicholas Coomann, and Roman Yos for their help and comments on a first version of this introduction.

⁹ This literature was also used to collect some of the other information included in Table 1: i.e. birth year, alma mater, doctoral advisor (if available), year of arrival, year of return (if applicable), and U.S. affiliations. I included a maximum of two affiliations per philosopher.

¹⁰ Though see e.g. Mehlman (2000) and Jaroszyńska-Kirchmann (2004). At first sight, Italian philosophers appear to be underrepresented, too. A database of displaced Italian academics, kindly shared with me by Erika Luciano, however, suggests that very few Italian scholars ended up in the United States. Most of them sought refuge in Europe or South America.

in exile in another country before their arrival, as well as intellectual migrants who moved to North America before 1931.

Naturally, each of these criteria may be contested. One will end up with quite a different list if one includes all student migrants or omits refugees who never obtained a position as a philosophy professor. Still, Table 1 helps convey the magnitude of the philosophical migration. The American Philosophical Association (APA) had just some six hundred members in the late 1930s, meaning that the community of philosophers grew with a least a quarter in just a few years' time (APA 1938). Moreover, the inventory may contribute to a more detailed study of the population of migrant philosophers. It reveals, for instance, that most philosophers (approx. 47 percent) arrived in the period between 1939 and 1941, that a large number of émigrés (approx. 86 percent) was affiliated with at least one American institute of higher education, and that only a relatively small fraction of the refugees (approx. 19 percent) returned to Europe after the war.¹¹ Information about the émigré's alma maters, doctoral advisors, and U.S. affiliations (though incomplete), finally, can help historians reconstruct networks of institutional connections between migrant philosophers, both in Europe and in the United States.

Most importantly, Table 1 can help us to shift and broaden our perspective on the intellectual migration. Histories of this period tend to focus on the success stories, but it is important to keep in mind that just a small number of migrants went on to have successful academic careers in the United States. Though many migrant philosophers found temporary employment as instructor or lecturer, relatively few of them obtained a tenured position, let alone a professorship at a prestigious university. This is not just due to the large number of philosophical migrants; it is also an effect of the state of the U.S. economy at the time. Most refugees arrived in the late 1930s, a period when the academic job market was still plagued by the effects of the Great Depression. This economic downturn especially affected the humanities as students were massively choosing more 'practical' majors and courses (Verhaegh forthcoming). In 1938, the APA Committee on Opportunities for Employment reported that "depression conditions have made administrations susceptible to questions of numbers of students enrolled in department of philosophy," noting that this development had "seriously affect[ed]" the philosophical job market (APA 1938, 187).

Finally, we should not forget the large group of philosophers who wanted to move to the United States but who never made it, either because they were refused

¹¹ Approximately 19 percent arrived in the period 1933–35 and 31 percent in 1936–1938. The remaining philosophers arrived before 1933 or in the period 1942–1945. In estimating the number of philosophers who returned to Europe, I only included philosophers who had moved back by 1955.

Table 1: List of displaced European philosophers (broadly defined) who spent at least a few years in the United States between 1931 and 1945

Name	Born	Alma Mater	Doc. Adv.	Period in US	US Affiliation(s)	More information
Adorno, Theodor	1903	Frankfurt	Cornelius	1938 – 1949	Inst. for Social Research, Columbia; UC Berkeley	Wheatland (this volume)
Anders, Günther	1902	Freiburg	Husserl	1936 – 1950	i. a. NSSR; US Office of War Information	van Dijk (2000)
Arendt, Hannah	1906	Heidelberg	Jaspers	1941 –	i. a. UChicago; NSSR*	King (2015)
Ascoli, Max	1898	Rome		1932 –	i. a. NSSR*; editor of <i>The Reporter</i>	Camurri (2010)
Bamberger, Fritz	1902	Berlin	Spranger	1939 –	i. a. Coll. of Jew. Stud., Chicago; Leo Baeck Inst.	Meyer (2013)
Baumgardt, David	1890	Berlin		1939 –	Consultant, Library of Congress	Sternglass & Frank (1963)
Beck, Maximilian	1887	Munich	Pfänder	1938 –	i. a. Yale Un.; Central College, Little Rock*	De Santis (2023)
Berger, Klaus	1901	Göttingen	Geiger	1941 –	Northwestern Un.; Un. of Kansas (art history)*	Roussel (1982)
Bergmann, Gustav	1906	Vienna	Mayer	1938 –	Un. of Iowa*	Egidi & Bonino (2008)
Bespaloff, Rachel	1895	Paris		1942 –	Mount Holyoke Coll.	Benfey & Remmler (2006)
Bloch, Ernst	1885	Würzburg	Külpe	1938 – 1949	None (writer)	Schmieder (2012)
Blücher, Heinrich	1899	None	None	1941 –	i. a. NSSR; Bard College*	Köhler (1996)
Bondy, Curt	1894	Hamburg	Stern	1940 – 1949	Coll. of William & Mary (psychology)*	Guski-Leinwand (2018)
Borghesi, Lamberto	1907	Pisa		1940 – 1948	Student	Schwarz & Francesconi (2007)
Brann, Hellmut	1903	Berlin	Spranger	1941 –	i. a. Rutgers Un. (German); NY Staats-Zeitung	Hardt (1982)
Brecht, Arnold	1884	Leipzig		1933 –	NSSR (political science)*	Friedrich & Hula (1954)
Brendel, Otto	1901	Heidelberg	Curtius	1938 –	i. a. IU Bloomington; Columbia Un. (art history)*	Calder III (1976)
Broch, Hermann	1886	Vienna	None	1938 –	None (writer)	Lützel (2003)
Brunswik, Egon	1903	Vienna	K. Bühler	1937 –	UC Berkeley (psychology)*	Tolman (1966)
Bühler, Charlotte	1893	Munich	Becher	1940 –	i. a. MLPS Gen. Hosp.; USC (psychiatry)*	Woodward (2012)
Bühler, Karl	1879	Freiburg	von Kries	1940 –	i. a. St. Thom. Coll.; USC (clin. psychology)*	Bolgar (1964)
Camap, Rudolf	1891	Jena	Bauch	1935 –	i. a. UChicago; UCLA*	Verhaegh (2020a)
Cassirer, Ernst	1874	Marburg	Cohen	1941 –	Yale Un.; Columbia Un.	Gawronsky (1949)
Cerf, Walter	1907	Bonn		1936 –	i. a. Un. of Minnesota; Brooklyn Coll.	Sanders (2004)
Cleve, Felix	1890	Vienna		1940 –	Long Island University; NSSR	Luftschtein (2018)
Deku, Henry	1909	Berlin [†]	Dessoir	1940 – 1945	Yale Un. (divinity school)	Schönberger (2023)
Drucker, Peter	1909	Frankfurt		1937 –	Bennington Coll. NY Un. (management)*	Flaherty (1999)

Table 1 (Continued)

Name	Born	Alma Mater	Doc. Adv.	Period in US	US Affiliation(s)	More information
Duncker, Karl	1903	Berlin	Wertheimer	1937 –	Swarthmore Coll. (psychology)	King et al. (1998)
Eckstein, Walter	1891	Vienna	Jodi	1938 –	American Ethical Union	Gutmann (1973)
Edelstein, Ludwig	1902	Heidelberg	Regenbogen	1934 –	i.a. Johns Hopkins Un.; UC Berkeley (Greek)*	Diller (1966)
Erdos, Leslie	1905	Vienna	Gomperz	1939 –	Essex Jr. Coll., Newark (music)	Weber (2005)
Fales, Walter	1896	Berlin	Petersen	1940 –	Lincoln Un.*	Edgcomb (1990)
Falkenfeld, Hellmuth	1893	Berlin	Troeltsch	1938 –	Mount Sinai Hospital, NY	Heuer (1998, 478 – 481)
Feigl, Herbert	1902	Vienna	Schlick	1931 –	i.a. Un. of Iowa; Un. of Minnesota*	Neuber (this volume)
Flesch, Philipp	1896	Vienna		1939 –	Jewish Theological Seminary (library)	Ritchey (2008)
Förster, Friedrich W.	1869	Freiburg	Reihl	1940 – 1963	None (writer)	Wirth (1999)
Francès, Madeleine	1906	Paris		1941 – ?	Unknown	Schuhl (1960)
Frank, Erich	1883	Heidelberg		1939 –	i.a. Harvard Un.; Bryn Mawr College	Lang (1994)
Frank, Philipp	1884	Vienna	Boltzmann	1938 –	Harvard Un. (physics; mathematics)	Reisch & Tuboly (this vol.)
Fränkel, Hermann	1888	Göttingen	Pohlenz	1935 –	Stanford Un. (classics)*	Pearson et al. (1977)
Frenkel-Brunswick, Else	1908	Vienna	K. Bühler	1938 –	UC Berkeley (Inst. of Child Welfare)	Valsiner & Abbey (2006)
Freund, E. Hans	1905	Freiburg	Heidegger	1940 –	Pendle Hill School; Penn State Un.*	Oumma (2009)
Freund, Ludwig	1898	Leipzig		1934 – 1959	Columbia Un.; Roosevelt Un. (political science)*	Walk (1988)
Fritz, Kurt von	1900	Munich	Schwartz	1936 – 1954	Reed College; Columbia Un. (classics)*	Obermayer (2008)
Fromm, Erich	1900	Frankfurt	A. Weber	1934 –	i.a. NSSR; Michigan State Un. (psychology)*	Wheatland (this volume)
Gawronsky, Dimitri	1883	Marburg	Cohen	Unknown	None (writer)	Ferrari (2011)
Geiger, Moritz	1880	Munich	Lipps	1933 –	Vassar College*	Fabiani (2010)
Geiringer, Hilda	1893	Vienna	Wirtinger	1939 –	i.a. Bryn Mawr Coll.; Wheaton Coll. (maths)*	Richards (1987)
Glatzer, Nahum	1903	Frankfurt	Buber	1938 –	i.a. Hebrew Thl. Coll.; Brandeis Un. (Jew. Stud.)*	Sheppard (2004)
Gödel, Kurt	1906	Vienna	Hahn	1940 –	IAS, Princeton (mathematics)*	Budiansky (2021)
Goldberg, Oskar	1885	Munich		1941 – 1949	None (worked as a doctor)	Friedlander (1992)
Goldschmied, Albin R.	1897	Prague	Ehrenfels	1939 –	Chapel Hill School, Waltham; Middlesex Un.*	Schweitzer (2018)
Gomperz, Heinrich	1873	Vienna	Mach	1935 –	Un. Southern California	Stadler (2015)
Grab, Hermann	1903	Heidelberg	Salomon	1940 –	David Mannes Music School	Cramer (1994)

Table 1 (Continued)

Name	Born	Alma Mater	Doc. Adv.	Period in US	US Affiliation(s)	More information
Günther, Gotthard	1900	Berlin	Spranger	1940 – 1972	i.a. Widener Library; UIUC (engineering)*	Paul (2000)
Gürster, Eugen	1895	Munich		1941 – 1952	None (writer)	Mendelssohn (1980)
Gurvitsch, Georges	1894	Paris		1940 – 1945	NSSR*	Balandier (1974)
Gurwitsch, Aron	1901	Göttingen	Geiger	1940 –	i.a. Johns Hopkins Un.; Brandeis Un.*	Strassfeld (this volume)
Gutkind, Eric B.	1887	Berlin	None	1933 –	i.a. NSSR; City Coll. of NY	Finch (1969)
Haas, William S.	1883	Munich		1940 –	i.a. Un. of Colorado, Denver; Columbia Un.	Kamaly (2020)
Hartman, Robert S.	1910	Northwestern		1941 –	i.a. Wooster Coll.; Ohio State Un.*	Edwards (2005)
Hartmann, Gerda	1908	Kiel		1937[78] –	Indic Studies, Library of Congress	APA Invent. Exiled Phil. (1942)
Helmer, Olaf	1910	Berlin	G. Feigl†	1937 –	i.a. UChicago; Rand Corporation	Rescher (2006)
Hempel, Carl Gustav	1905	Berlin	Köhler‡	1937 –	i.a. Yale Un.; Princeton Un.*	Jeffrey (1995)
Hertz, Paul	1881	Göttingen	Abraham	1938 –	None	Gosh & Arden (2013)
Hildebrand, Dietrich v.	1889	Göttingen	Husserl	1940 –	Fordham Un.*	von Hildebrand (2000)
Hirschhorn, Samuel	1900	Vienna		Unknown	Unknown	APA Invent. Exiled Phil. (1942)
Hirschlaff, Ernest	1908	Berlin	Pringsheim	1937 – 1947	UChicago (physics)	APA Invent. Exiled Phil. (1942)
Honigshaim, Paul	1885	Heidelberg		1938 –	Michigan State Un. (sociology/anthropology)*	Leser (1968)
Horkheimer, Max	1895	Frankfurt	Cornelius	1934 – 1949	Institute for Social Research, Columbia	Wheatland (this volume)
Husserl, Gerhart	1893	Freiburg		1936 – 1952	Union Theological Seminary*	Böhler (1992)
Immerwahr, Heinrich	1916	Florence		1939 –	i.a. Yale Un.; UNCCH (Classics)*	Khang (2013)
Jaeger, Werner	1888	Berlin	Wilamowitz	1936 –	UChicago; Harvard Un.*	Burstein (2018)
Jospe, Alfred	1909	Breslau		1939 –	Hillel Foundation	Jospe & Jospe (2000)
Kahler, Erich von	1885	Vienna	Jodl	1938 –	i.a. NSSR; Cornell Un. (German lit.)*	Corngold (2022)
Kantorowicz, Ernst	1895	Heidelberg	Gothein	1938 –	UC Berkeley; IAS, Princeton (history)*	Lerner (2017)
Kaplan, Simon	1893	Jena		Unknown	St. John's College (Hebrew)*	APA Invent. Exiled Phil. (1942)
Kapp, Ernst	1888	Freiburg	Schwartz	1939 – 1955	Columbia Un. (classics)*	Obermayer (2008)
Kaufmann, Felix	1895	Vienna	Kelsen	1938 –	NSSR*	Strassfeld (this volume)
Kaufmann, Fritz	1881	Freiburg	Husserl	1937 –	Northwestern Un.; Un. Buffalo*	Strassfeld (this volume)
Kayser, Stephen S.	1900	Heidelberg	Rickert	1938 –	i.a. NY Public Library; Un. Judaism, LA*	Jennings (2023)

Table 1 (Continued)

Name	Born	Alma Mater	Doc. Adv.	Period in US	US Affiliation(s)	More information
Kelsen, Hans	1881	Vienna	Jellinek ³	1940–	Harvard Un.; UC Berkeley (political science)*	Dyzenhaus (this volume)
Klatzkin, Jakob	1882	Bern		1941 – 1947	College of Jewish Studies	Hotam (2008)
Klein, Jacob	1899	Marburg	Hartmann	1938 –	St. John's College*	Williamson & Zuckerman (1985)
Köhler, Wolfgang	1887	Berlin	Stumpf	1935 –	Swarthmore Coll.; Dartmouth Coll. (psychology)*	Neisser (2002)
Korsch, Karl	1886	Jena	Gerland	1936 –	i. a. Tulane Un.; IISR	Haliday (1970)
Koryé, Alexandre	1892	Paris		1941 –	École libre des hautes études; IAS, Princeton	Bensaude-Vincent (2018)
Kracauer, Siegfried	1889	Berlin		1941 –	Mus. of Modern Art; Columbia Un. (soc. sci.)*	Koch (2000)
Kraft, Julius	1898	Göttingen	Nelson	1939 – 1954	i. a. NSSR; Washington and Jefferson Coll.*	Popper (1962)
Kristeller, Paul Oskar	1905	Heidelberg	Hoffmann	1939 –	Yale Un.; Columbia Un.*	Monfasani (2001)
Kroner, Richard	1884	Freiburg	Rickert	1940 –	i. a. Union Theological Seminary; Temple Un.*	Gadamer (1985)
Krzyszinski, Andrew J.	1884	Cracow		1939 –	Laval Un., Quebec; Fordham Un.	Krzyszinski (ca. 1953)
Kuhn, Helmut	1899	Breslau	Hönigswald	1938 – 1949	UNCCH; Emory Un.*	Gadamer (1992)
Lasker, Emanuel	1868	Göttingen	M. Noether	1937 –	None (writer; chess teacher)	Hannak (1991)
Lederer, Max	1881	Vienna		Unknown	i. a. Coe College; Library of Congress	APA Invent. Exiled Phil. (1942)
Lévi-Strauss, Claude	1908	Sorbonne	Mauss	1941 – 1948	École Libre des Hautes Études; French Embassy	Loyer (2019)
Lewin, Kurt	1890	Berlin	Stumpf	1933 –	i. a. Un. of Iowa; MIT (psychology)*	Marrow (1969)
Loewald, Hans	1906	Freiburg	None	1939 –	Un. of Maryland; Yale Un.* (psychiatry)	Wayne Downey (1994)
Löwenthal, Leo	1900	Frankfurt		1934 –	i. a. Inst. for Soc. Res.; UC Berkeley (sociology)*	Bogart (1993)
Löwi, Moritz	1891	Breslau	Hönigswald	1938–	Connecticut College for Women	Grassi & Richart-Willmes (1997)
Löwith, Karl	1897	Munich	Geiger	1941 – 1952	Hartford Theological Sem.; NSSR*	Wolin (2001)
Maier, Joseph	1911	Columbia	Horkheimer	1933 –	Rutgers (sociology)*	Marcus (1999)
Manasse, Ernst Moritz	1908	Heidelberg	Regenbogen	1938 –	North Carolina Central Un.*	Schweitzer (1996)
Marck, Siegfried	1889	Breslau	Kühnemann	1939 –	YMCA Coll., Chicago; Roosevelt Coll., Chicago*	Schulz (2014)
Marcuse, Herbert	1898	Freiburg	Witkop	1934 –	i. a. Inst. for Social Research; Brandeis Un.*	Wolin (2001)
Marcuse, Ludwig	1894	Berlin	Troeltsch	1939 – 1962	Office of War Information; Un South. Cal.*	Lamping (1987)
Margolius, Hans	1902	Hamburg	Cassirer	1939 –	Miami Dade Junior Coll.; Florida Atlantic Un.	Schwarz (1988)
Maritain, Jacques	1882	Paris		1940 – 1952	i. a. École Libre des Hautes Étud.; Princeton Un.*	Collins (1963)

Table 1 (Continued)

Name	Born	Alma Mater	Doc. Adv.	Period in US	US Affiliation(s)	More information
Marx, Werner	1910	NSSR	Reizler	1938 – 1958	NSSR	Kähler & Nenon (1996)
Merlan, Philip	1897	Vienna		1940 –	Un. Redlands; Scripps College*	Solmsen (1968)
Metzger, Arnold	1893	Jena	Eucken	1941 – 1952	Simmons College	Henckmann (1994)
Michaelis, Anne L.	1912	Basel		1937[7/8] –	Un. North Carolina; Brenau Coll.	APA Invent. Exiled Phil. (1942)
Mises, Richard von	1883	Vienna	Hamel	1939 –	Harvard Un. (Aerodynamics)*	Siegmund-Schultze (2004)
Mokre, Hans	1901	Graz	Mally	1939 – 1948	St. Louis Un.; Barat Coll. (physics; mathematics)*	Müller (1993)
Neumann, Franz	1900	Frankfurt	Mayer	1936 –	i.a. Inst. for Social Res.; Columbia Un. (pol. sci.)*	Wheatland (this volume)
Neumann, Norbert	1903	Vienna		Unknown	Unknown	APA Invent. Exiled Phil. (1942)
Neumann, Sigmund	1904	Leipzig	Freyer	1934 – 1949	i.a. Off. of Strat. Serv.; Wesleyan Un. (soc. sci.)*	Graf (2021)
Oppenheim, Paul	1885	Giessen		1939 –	None (private scholar)	Rescher (1997)
Oppenheimer, Oscar	1900	Bonn		Unknown	i.a. Earlham Coll.; Central Michigan Un.*	APA Invent. Exiled Phil. (1942)
Raphael, Max	1889	Berlin	None	1941 –	None (private scholar)	Lachman (1994)
Reichenbach, Hans	1891	Erlangen	Hensel	1938 –	UCLA*	Verhaegh (2020b)
Reichenbach, Maria	1909	Freiburg		1939 –	UCLA; Los Angeles City College*	Maynes & Gimbel (2022)
Reik, Theodor	1888	Vienna	Freud	1938 –	National Psychological Assoc. for Psychoanalysis	Natterson (1966)
Rieser, Max	1893	Vienna		1939 –	Common Council for American Unity	Keller (2008)
Riezler, Kurt	1882	Munich	Pöhlmann	1938 – 1954	NSSR*	Thompson (1980)
Roenau, Ernst	1888	Vienna		Unknown	Unknown	APA Invent. Exiled Phil. (1942)
Rosen, Arthur	1891	Berlin		Unknown	Unknown	APA Invent. Exiled Phil. (1942)
Rosenstock-Huessy, E.	1888	Heidelberg	Schröder	1933 –	Harvard Un.; Dartmouth College*	Cristaudo (2020)
Rougier, Louis	1889	Sorbonne	Lalande	1941 – 1943	NSSR	Dewulf & Simons (2021)
Saenger, Gerhart	1910	Basel		Unknown	i.a. City College NY; Newark Un.	APA Invent. Exiled Phil. (1942)
Salomon, Albert	1891	Heidelberg	Lederer	1935 –	NSSR*	Grathoff (1995)
Scheerer, Martin	1900	Hamburg	Stern	1936 –	i.a. Montefiore Hosl.; Un. Kansas (psychology)*	Scheerer (1966)
Schrecker, Paul	1889	Berlin		1940 –	NSSR; Un. Pennsylvania*	Morrow (1963)
Schumann, Alfred	1899	Munich		Unknown	Unknown	APA Invent. Exiled Phil. (1942)
Schutz, Alfred	1899	Vienna		1939 –	NSSR*	Jacobs (this vol.)

Table 1 (Continued)

Name	Born	Alma Mater	Doc. Adv.	Period in US	US Affiliation(s)	More information
Schwarz, Balduin	1902	Munich	v.Hildebrand	1941 – 1964	i. a. Fordham Un.*	Gertzen (2015a)
Seidenmann, Alfred	1895	Freiburg		Unknown	Institute for Social Research, Columbia	APA Invent. Exiled Phil. (1942)
Simon, Yves	1903	Paris	Maritain	1938–	Un. Notre Dame; UChicago*	Kuic (1999)
Solmitz, Walter	1905	Hamburg	Cassirer	1940 –	Bowdoin College*	Schäfer (2023)
Solmsen, Friedrich	1904	Berlin	Jaeger	1937 –	i. a. Cornell Un.; Un. Wisc.-Madison (classics)*	North (1994)
Sonnemann, Ulrich	1912	Basel	Salin	1941 – 1955	NSSR (psychology)*	Fiebig (2010)
Speier, Hans	1905	Heidelberg	Mannheim	1933 –	NSSR; Un. Mass. Amherst (sociology)*	Bessner (2018)
Spiegelberg, Frederic	1897	Tübingen		1937 –	i. a. Un. Rochester; Stanford Un. (Asiatic Stud.)*	Kabil (2012)
Spiegelberg, Herbert	1904	Munich	Pfänder	1938 –	i. a. Swarthmore Coll.; Washington Un. St. Louis*	Ierna (2019)
Stefansky, George	1897	Prague	Sauer	1939 –	i. a. City College NY; Columbia Un. (sociology)	Gertzen (2015b)
Steinhardt, Käthe	1894	Vienna	Schlick	Unknown	Unknown	APA Invent. Exiled Phil. (1942)
Stenzel, Joachim	Unkn.	Florence ^{II}	Pasquali	Unknown	Antioch Coll.	APA Invent. Exiled Phil. (1942)
Stern, Alfred	1899	Vienna	Reiniger	1944 –	California Inst. Technology*	Hull (2013)
Stern, William	1871	Berlin	Lazarus	1934 –	Duke University	Büttner (2013)
Stocker, Helene	1869	Bern	Walzel	1942 –	None	von Bockel (1991)
Straus, Erwin	1891	Göttingen	Bonhoeffer	1938 –	Black Mountain Coll.; Un. Kentucky*	Barbaras (2004)
Strauss, Eduard	1876	Berlin		1939 –	Beth David Hosp.; New York Un. (chemistry)	Meiners (2005)
Strauss, Leo	1899	Hamburg	Cassirer	1937 –	i. a. NSSR; UChicago (political science)*	Schlessner (this volume)
Sturzo, Luigi	1871	Rome		1940 – 1946	None (informant)	Acanfora (2021)
Tagliacozzo, Enzo	1909	Naples		Unknown	Harvard Un.	APA Invent. Exiled Phil. (1942)
Tarski, Alfred	1901	Warsaw	Leśniewski	1939 –	UC, Berkeley (mathematics)*	Feferman & Feferman (2004)
Tillich, Paul	1886	Breslau	Kühnemann	1933 –	i. a. Union Theol. Sem.; Harvard Un. (religion)*	Pauck & Pauck (1976)
Toch, Ernst	1887	Heidelberg	Kroyer	1935 –	i. a. NSSR; Un. Southern California*	Erwin (1987)
Toller, Ernst	1893	Munich	None	1937 –	None (writer)	Newberry (1980)
Ulich, Robert	1890	Leipzig	Goetz	1934 –	Harvard Un. (education)*	Miller (2023)
Voegelin, Eric	1901	Vienna	Kelsen	1938 –	i. a. Louisiana St. Un. (gov.)*; Hoover Inst.	Dyzenhaus (this volume)
Wach, Joachim	1898	Leipzig		1935–	i. a. Brown Un.; U. Chicago (theology)*	Waugh (2005)

Table 1 (Continued)

Name	Born	Alma Mater	Doc. Adv.	Period in US	US Affiliation(s)	More information
Wahl, Jean	1888	ENS Paris	Boutroux	1942 – 1945	École Libre des Hautes Ét.; Mount Holyoke Coll.	Gansen (2020)
Weil, Hans	1898	Göttingen	Nohl	1940 –	i. a. Care; Columbia Un.	Feidel-Mertz (1992)
Weinschel, Herbert	Unkn.	Unknown		Unknown	St. Louis Un. (government)*	APA Invent. Exiled Phil. (1942)
Weixgärtner, Wolfgang	Unkn.	Unknown		Unknown	Unknown	APA Invent. Exiled Phil. (1942)
Werner, Heinz	1890	Vienna		1933 –	i. a. Un. Michigan; Clark Un. (psychology)*	Valsiner (2005)
Wertheimer, Max	1880	Würzburg	Külpe	1933 –	NSSR (psychology)*	King & Wertheimer (2005)
Weyl, Helene	1893	Göttingen	None	1933 –	None (translator)	Weyl (2019)
Weyl, Hermann	1885	Göttingen	Hilbert	1933 –	IAS, Princeton (mathematics)*	De Bianchi & Catren (2018)
Wiener, Max	1882	Breslau		1939 –	i. a. Hebrew Union Coll.; Theodor Herzl Society	Weidner (2005)
Wind, Edgar	1900	Hamburg	Panofsky	1939 – 1955	i. a. UChicago; Smith College*	Sorensen (2023)
Wolff, Kurt	1912	Florence	Mannheim	1939 –	i. a. Ohio State Un.; Brandeis Un. (sociology)*	Zachary (2012)
Wolff, Werner	1904	Berlin	Wertheimer	1939 –	i. a. Vassar Coll.; Bard Coll. (psychology)*	Gainer & Murphy (2014)
Zilsel, Edgar	1891	Vienna	Gomperz	1939 –	Hunter College; Mills College	Raven (2003)
Zimmer, Henry Robert	1890	Berlin	Lüders	1940 –	Columbia University	Case (1994)
Znaniecki, Florian	1882	Krakow	Rauh	1940 –	Columbia Un.; Un. Ill. Urbana (sociology)*	Hensoldt (2020)

* Confirmed tenure-track or tenured position.

† Henry Deku (born Heinz Dekuczynski) was never officially awarded his doctorate. See Schönberger (2023).

‡ Hempel and Helmer were students of Reichenbach. Wolfgang Köhler and Georg Feigl took over as advisors after Reichenbach was dismissed from his position in 1933.

§ Kelsen obtained his PhD after passing examinations (instead of a dissertation) at the University of Vienna. He wrote his Habilitationsschrift under Jellinek at Heidelberg (Robles 2020, 3).

|| It is unclear whether Stenzel formally obtained his doctorate. The APA inventory only makes note of “graduate studies under G. Pasquali.”

a visa or because they were imprisoned or killed before they could leave Europe. The United States had strict immigration quota designed to preserve America's "racial status quo" and this policy disproportionately affected Jewish immigrants and academics from Eastern Europe.¹² As a result, philosophers only had a realistic chance to be allowed entry if they acquired a *non*-quota visa, meaning that they needed a job offer from a U.S. institution. In order to qualify for such a visa, immigrants were required to have "at least two years" of experience as "professor of a college, academy, seminary, or university" (Leff 2006, 3), which was particularly problematic for junior scholars of Jewish descent since many of them had been unable to find an academic post. One scholar tragically affected by this policy was Janina Hosiasson-Lindenbaum, a Polish-Jewish philosopher who worked on inductive logic and probability in Warsaw. Despite efforts by, among others, Nagel, Quine, Hempel, and Tarski to find an American college or university that would take her, Hosiasson never found a position in the United States. In 1942, she was shot by the Gestapo after seven months of imprisonment.¹³ Other philosophers who never made it to North America include Walter Benjamin, Salomo Friedlander, Adhémar Gelb, and Gertrud Kantorowitz.

1.2 Toward a More Integrated History of the Intellectual Migration

Present-day academic philosophy in the Global North is characterized by a deep divide between 'analytic' and 'continental' approaches. Analytic philosophers typically view themselves as members of an intellectual tradition going back to Gottlob Frege, G. E. Moore, and Bertrand Russell and are particularly influential in the Anglophone world (Beaney 2013, 2). Continental philosophy is an amalgam of a long list of mostly German and French schools—e.g. idealism, (neo-)Kantianism, phenomenology, existentialism, hermeneutics, *Lebensphilosophie*, Marxism, critical theory, structuralism, post-structuralism, psycho-analytic theory, feminism, and postmodernism (e.g. Critchley 2001, 13; Glendinning 2006, 58–65)—and is more dominant in mainland Europe. Both labels are notoriously vague (e.g. Kearney 1994; Glock 2008) but the distinction is so deeply engrained that philosophers working on similar topics on each side of the divide are often unaware of each other's work. Analytic and continental philosophers have organized themselves in

¹² House Committee on Immigration and Naturalization. *Restriction of Immigration*. 68th Congress, Rep. No. 350, pt. 1, 16.

¹³ Cheryl Misak briefly discusses her work and fate in chapter 2. See also Sznajder (ms.).

separate societies, attend different conferences, and publish work in distinct journals and book series.

One particularly problematic consequence of the analytic-continental divide is that it has affected the historiography of twentieth-century philosophy. Historians tend to exclusively focus on one of the two traditions and to have little knowledge about developments on the other side of the intellectual iron curtain. And while there is nothing wrong with historians specializing in the ideas of a particular philosopher or school of thought, it is problematic to neglect ties between philosophers we *nowadays* view as belonging to different intellectual traditions. Such an anachronistic perspective might lead one to read Carnap's *Der logische Aufbau der Welt* as a foundationalist work in the analytic-empiricist tradition, while a more contextualist approach reveals that the book was strongly influenced by neo-Kantian debates in the early 1920s (Richardson 1998; Friedman 1999). Or it might lead one to ignore the historical connections between phenomenology, the Lwów-Warsaw school, and the Cambridge school of analysis, which all emerged from or had strong ties to the Brentano school (Horgan et al. 2002; Textor 2013; Brożek 2020). While philosophers also organized themselves into distinct groups and movements before World War II, the social divisions were not as rigid as those between analytic and continental philosophers today. Philosophers from different schools regularly engaged with each other's work and journals tended to be more pluralistic (Katzav and Vaesen 2017). It is well established, for example, that there were fruitful interactions between logical empiricists and phenomenologists before they were rebranded as paradigmatic analytic and continental schools of thought. Hermann Weyl, Arthur Eddington, Hans Reichenbach and Moritz Schlick were all entangled in debates about the implications of relativity theory in the 1920s (Ryckman 2005), while phenomenologists like Felix Kaufmann and Robert Neumann were closely associated with the Vienna Circle (Stadler 2015).

Historians concerned with the effects of the intellectual migration should be especially aware that labels such as 'analytic' and 'continental' are relatively recent inventions. Though the origins of the split can be traced back to nineteenth-century responses to Immanuel Kant as well as to debates between Russell and Henri Bergson (Vrahimis 2022) and Carnap, Cassirer, and Martin Heidegger (Friedman 2000), philosophers only began to *identify* as 'analytic' in the late 1940s. In fact, our present-day bifurcation between analytic and continental approaches may very well be an effect of the intellectual migration. While the term 'analytic' was still mostly employed to designate a philosophical style associated with the Cambridge school of analysis in the 1930s, American philosophers began to use it to denote a broader range of schools and perspectives, including logical empiricism, Oxford ordinary language philosophy, and scientific pragmatism in the years after the war (e.g. Pap 1949; Feigl and Sellars 1949). In the early 1930s, C. I. Lewis could still write

that logical positivism was one of “the most promising of present movements in Continental philosophy.”¹⁴ Twenty years later, logical positivism had become a paradigmatic analytic school of thought, while the predicate ‘continental’ was exclusively used as a derogatory term to refer to the types of philosophy practiced on the other side of the Atlantic (e.g. Rieser 1954).¹⁵

If we want to understand the effects of the intellectual migration in philosophy, therefore, it is not very helpful to project our current labels back onto the 1930s and 1940s. On the contrary, a broader, more integrated study of this period might help us better understand why we distinguish between analytic and continental philosophy today. Such a study should appreciate that the United States had become a melting pot of philosophical traditions, including both American schools (e.g. pragmatism, naturalism, neo-realism, and process philosophy) and European movements (e.g. logical empiricism, phenomenology, and critical theory). And it should recognize that the distinctions between these traditions were much more fluid than we are used to today. The aforementioned Nagel, for instance, is a perfect example of someone who still engaged with a broad range of schools and traditions in the 1930s. He figures in eight of the twelve chapters in this volume as he was a student of the pragmatist John Dewey (chapter 2), who played an important role in the development of the Columbia school of naturalism (chapter 4) as well as the reception of logical empiricism in North America (chapters 8 and 10) but was also involved in debates with phenomenologists (chapter 7), Horkheimer’s Frankfurt School in exile (chapter 11), and Leo Strauss (chapter 12). Ironically, Nagel was one of the scholars who helped coin the phrase ‘analytic philosophy’ (Nagel 1936) and would eventually become one of America’s best-known philosophers of science in the analytic tradition. Only a less compartmentalized approach to the study of this period will help us understand how these changes came about.

1.3 Overview

The present volume aims to contribute to a broader, more integrated study of the effects of the intellectual migration by bringing together historians specializing in a range of philosophical schools and traditions. The chapters in this book present twelve case studies illustrating the diverse ways in which the migration affected

¹⁴ Recommendation letter for Herbert Feigl, April 14, 1931, Herbert Feigl Papers, University of Minnesota Archives, 03–53–01.

¹⁵ See Frost-Arnold (2017), Strassfeld (2022), and Verhaegh (forthcoming) for a more detailed history of the origins and development of these labels.

the development of postwar philosophy. These studies focus on the lives and views of well-known philosophers as well as scholars who are typically excluded in work on this period. They cover movements that have their roots in both European and distinctly American schools of thought. And they reveal how encounters between European and American research cultures stimulated the development of new perspectives while simultaneously contributing to the marginalization of others.

This volume is organized into four sections. Part I focuses on the impact of the migration on *American* philosophical movements. In recent decades, historians of U.S. philosophy have often presented the intellectual migration as a development that led to the eclipse of pragmatism, arguably the United States' best-known school of thought. These historians tend to present pragmatism as a rich and diverse intellectual tradition that was eclipsed by a wave of rather crude, analytically-minded philosophers from Europe, in particular the logical empiricists, such that it was no longer "a respectable subject of interest" by the late 1950s (Thayer 1968, 559). The three chapters included in Part I all challenge some aspects of this received view. Cheryl Misak (chapter 2) argues that the eclipse narrative has led to a false understanding of the relation between pragmatism and logical empiricism. Focusing on the work of Harvard philosopher C. I. Lewis and some of his students, Misak shows that there were fruitful interactions between the two traditions, such that the development of postwar analytic philosophy is perhaps best viewed as an amalgam of pragmatist and logical empiricist ideas. Joel Katzav (chapter 3), on the other hand, argues that the eclipse narrative is too narrowly focused on the fate of American pragmatism, and identifies a rich but largely forgotten tradition of *speculative* philosophy. He argues that speculative philosophers of science working in the first decades of the twentieth century introduced many ideas and topics that are nowadays attributed to logical empiricists or analytic philosophers and uses it to reconsider the impact of logical positivism on American philosophy of science. My own contribution, finally, seeks to qualify the eclipse narrative by studying the rise and fall of John Dewey's school of naturalism (chapter 4). Focusing on the naturalists' interactions with German philosophers in the 1930s, I argue that it is better to speak about *a split within* than *the eclipse of* the Columbia school, showing that one faction of analytically-minded naturalists was able to incorporate the views of the émigrés, developing the naturalist tradition into new directions, while another faction of historically-oriented naturalists was overshadowed by the analytic movement.

Part II comprises three studies on the development of *phenomenology* in North America. Carlo Ierna (chapter 5) traces the American reception of Brentano's and Husserl's work in the decades before the migration in order to shed light on the climate that phenomenologists encountered when they sought refuge in the United States in the 1930s. He accepts the standard view that American philosophers were

strongly influenced by their German colleagues at the turn of the century but argues that there were important differences between the two intellectual cultures. While the Brentano school had a significant impact on the development of philosophy in the German-speaking world, American philosophers and psychologists were more interested in German Idealism and Wundt's experimental psychology. There was quite some attention for Husserl's early work, Ierna submits, but it was mostly read through the lens of the new realist movement, such that Americans mostly picked up on his views on the nature of logic rather than his phenomenological method. Jonathan Strassfeld (chapter 6), picks up Ierna's thread in the 1930s and reconstructs the development of phenomenology in the years just before and after World War II. He traces the American careers of several phenomenological migrants—e.g. Moritz Geiger, Aron Gurwitsch, Felix Kaufmann, Fritz Kaufmann, and Alfred Schütz—and analyzes the growing network of phenomenologists in the United States. One crucial figure in the network, Strassfeld argues, was Marvin Farber, a U.S. philosopher who had studied with Husserl in the early 1920s. Hanne Jacobs (chapter 7), finally, studies the evolution of phenomenological thought during the second half of the twentieth century, tracing the influence of one particular approach—Alfred Schütz's phenomenological sociology—in modern standpoint epistemology. Jacobs shows that Schütz developed his perspective on the methodology of the social sciences in conversation with Talcott Parsons' and Nagel's competing views and reconstructs the former's influence on two key thinkers in contemporary feminism—Dorothy E. Smith and Patricia Hill Collins—thereby demonstrating that the ideas of one phenomenological immigrant played a crucial role in the development of a standpoint epistemological approach that is influential to this day.

Part III turns to the evolution and reception of *logical empiricism* in the United States. In discussions about the impact of the intellectual migration, logical empiricism is frequently presented as a unique success story. Positivist ideas about meaning, method, and metaphysics had a significant influence on American philosophy and helped pave the way for the analytic turn in the 1950s and 1960s. Logical empiricism scholars, however, tend to emphasize that members of the Vienna Circle and affiliated groups defended a wide variety of perspectives and that only one, rather narrow-minded variant of the position flourished in the United States (Friedman 1999; Richardson 1998; Verhaegh 2024). Part III discusses three aspects of logical empiricism that were defended by prominent exiled empiricists but never made it into the philosophical mainstream. George Reisch and Adam Tamas Tuboly (chapter 8) trace the first years of Phillip Frank in North America and reconstruct his attempts to advocate a less technical, more accessible brand of positivism in line with the social ambitions of the movement's founding members. Matthias Neuber (chapter 9) studies Herbert Feigl's scientific humanism and

reconstructs his ties with Roy Wood Sellars and the American Humanist Association, suggesting that logical empiricism's humanist ideology may have been its strongest link with American philosophy. And Fons Dewulf (chapter 10) argues that a central feature of Carnap's and Reichenbach's perspective—their voluntarist conception of rationality—was ignored and subsequently forgotten by the American philosophical community. Together, these chapters demonstrate that though Carnap, Feigl, and Reichenbach were, academically speaking, among the most successful philosophical migrants, there is still plenty of room to question to what extent they got their message across.

Part IV, finally, collects three studies concerning the development of *social and political theory* in the years following the intellectual migration. Thomas Wheatland (chapter 11) discusses the failed reception of the Frankfurt School in American philosophy in the 1930s. He argues that there was quite some common ground between critical theory and pragmatism but that Horkheimer's polemics prevented the two communities from developing a fruitful exchange of ideas. The Frankfurt School did have a significant impact on U.S. philosophy in the 1960s but this was mostly due to Marcuse, Neumann, and Fromm, who, Wheatland argues, all developed a more strategic approach in their interactions with American philosophers. Eric Schliesser (chapter 12) continues Wheatland's focus on polemics in his reconstruction of the reception of Leo Strauss, arguing that analytic philosophy's opposition against Straussianism aided in its self-constitution as an intellectual tradition. Zooming in on two analytic responses to Strauss, Schliesser reconstructs both Nagel's analysis of the thesis that social science cannot be value-free and Felix Oppenheim's criticism of Strauss's position concerning the principle of sufficient reason. David Dyzenhaus (chapter 13), finally, continues Schliesser's discussion of the value-free ideal and turns to the debate between Eric Voegelin and his teacher Hans Kelsen—the former a prominent member of 'Black Vienna,' the latter a liberal philosopher of law who had helped write the Austrian constitution. Dyzenhaus reconstructs their back-and-forth and its ramifications for the political climate today, arguing that Voegelin should be viewed as one of the most important members of a small group of refugee philosophers who paved the way "for the toxic blend of militant Christian conservatism, libertarianism, and anti-liberalism that drives the Republican Party in the Trump era."

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I American Philosophy

Cheryl Misak

2 Lewis and the Exiled Empiricists

Abstract: The Second World War caused many of the finest minds in Austria and Germany to flee, largely to America. In the discipline of philosophy, the most profound effect was the arrival of the logical positivists. This paper explores the relationship between those logical positivists and the American pragmatist C. I. Lewis. It traces the important commonalities and differences between the two traditions and suggests that where the two traditions diverged, pragmatism took the better path. The paper also explores the tragic missed opportunity for American pragmatism when the country turned back Janina Hosiasson, leaving her to be murdered by the Nazis.

2.1 Introduction

The Second World War shattered lives. It also scattered talent, largely to America. In philosophy, some take the arrivals from Vienna and Berlin to have altered the intellectual landscape like an industrial-scale coal mine, stripping the land bare to expose a hard seam of logically minded scientific philosophy, permanently damaging the richness of the homegrown American philosophy called pragmatism. H. S. Thayer seems to have initiated this eclipse narrative in 1968 when he asked:

How, then, did it happen that in the nineteen-forties pragmatism was so quickly eclipsed by the movement of logical positivism and analytic philosophy? The change was so complete that a decade later pragmatism was not even a respectable subject of interest in most departments of philosophy. (Thayer 1968, 559)

But the eclipse narrative is a false understanding of the relationship between the exiles and the pragmatists. There may be something to the story with respect to John Dewey, who never got the hang or the point of the new symbolic logic, which was central to logical positivism. While there was an early bond of respect between Dewey and the logical positivists, it soon frayed.

But the other great pragmatist of that generation, the Harvard philosopher Clarence Irving Lewis, was an excellent logician and could fully engage the new-

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comers. The exiled empiricists Rudolf Carnap, Carl Hempel, Herbert Feigl, Philipp Frank, and others met a brand of philosophy in Lewis that had commonalities with their own, with some important differences. Carnap, for instance, in correspondence about his volume in the *Library of Living Philosophers*, wanted a piece on “the influence on American philosophy by the European empiricists (who have emigrated here).”¹ In this paper I will draw out the influences, running in both directions, and suggest that, where the logical positivists did not end up following Lewis, they took wrong turns.

It is a complex story to tell. My own first attempt was in the 2013 *The American Pragmatists*.² I remain largely satisfied with my account of how the logical positivists (with their ally in England, Bertrand Russell) rejected the pragmatism of William James and how Dewey received the exiled empiricists (warily, at first agreeing to write a volume on value for the *Unified Encyclopedia of Science*, then turning away from the Circle, eventually so bitterly that he told his students not to read them). In this paper, I will expand on an important part of the story that I (and others) have not properly explored: the interaction between the exiled empiricists and Lewis.

The relative inattention to Lewis is a striking omission in the history of logical empiricism and American pragmatism.³ For many of the luminaries in the next generation of American philosophers were the direct intellectual offspring of Lewis and the positivists: W. V. Quine, Nelson Goodman, Morton White, Wilfrid Sellars, and Arthur Pap. I will suggest that while they have genetic material of both their parents, some of the traits the next generation acquired from the logical positivists were not good ones—a penchant for reductionism, a preference for sparse metaphysical landscapes, and a disregard of ethics.⁴

2.2 Pragmatism

We must start with a quick summary of pragmatism. Peirce describes its birth:

It was in the earliest seventies [1870s] that a knot of us young men in Old Cambridge, calling ourselves, half-ironically, half-defiantly, ‘The Metaphysical Club,’—for agnosticism was then

¹ Carnap to Feigl, Nov. 14, 1951, Rudolf Carnap Papers, UCLA, 01-CC03. Charles Morris wrote a piece for the volume called “Pragmatism and Positivism,” which was perhaps intended to fill this bill.

² Others have tackled it as well. See Verhaegh (2020) for an excellent recent account.

³ Some notable exceptions are Dayton (1995), Hookway (2008), O’Shea (2017), and Sinclair (2022).

⁴ White and Pap escaped these traits (see Misak 2022) and Goodman grew out of most of them (see Misak 2013).

riding its high horse, and was frowning superbly upon all metaphysics—used to meet, sometimes in my study, sometimes in that of William James. ... The type of our thought was decidedly British. I, alone of our number, had come upon the threshing-floor of philosophy through the doorway of Kant, and even my ideas were acquiring the English accent. (CP 5.12, 1907)⁵

When Peirce says “The type of our thought was decidedly British,” he means that pragmatism took its inspiration from the empiricism of Locke, Berkeley, Hume, and Bain. The basic idea of British empiricism is that our beliefs must be connected to experience. There have been various ways of expressing this basic thought. *Reductionism* has it that our beliefs must be grounded in an indubitable foundation of experience. *Verificationism* has it that all meaningful beliefs must be such that some experience can show them to be true or false. Those two empiricist ideas were central to logical positivism. The expression given by *pragmatism* holds that the connection between belief and experience is less strict—meaningful beliefs and beliefs aimed at truth must be responsive to experience. Pragmatism also takes from Bain the idea that belief and action are intimately connected—the idea that belief is something upon which we are prepared to act.

On all these versions of empiricism, we get an anti-metaphysical, more scientific, method for doing philosophy. Here is Peirce: we “must look to the upshot of our concepts in order to rightly apprehend them”: in order to get a complete grasp of a concept, we must connect it to that with which we have “dealings” (CP 5.3, 5.416). This method delivers down-to-earth concepts of, for instance, truth. Truth is not what is in the mind of God or what corresponds to a believer-independent world of objects. Truth is the best beliefs or habits we human beings could have—beliefs that would guide our actions successfully. Peirce required that success be connected to the force of experience:

it is one of the essentials of belief, without which it would not be belief ... that a man could hardly be considered sane who should wish that though the facts should remain lamentable, he should believe them to be such as he would wish them to be.⁶

A belief that is “determined by any circumstance extraneous to the facts” should be doubted (Peirce 1900–, Vol. 3, 253). Experience impinges upon us and we must take it seriously.

James wobbled on this point. He articulated the pragmatists account of truth thus:

⁵ Here and hereafter, CP refers to Peirce 1931–58, cited as “CP volume.page.”

⁶ MS 673, 11, 1911, Charles S. Peirce Papers, Houghton Library, Harvard University.

any idea upon which we can ride ... any idea that will carry us prosperously from any one part of our experience to any other part, linking things satisfactorily, working securely, simplifying, saving labor, is ... true ... (James 1975 [1907], 34)

But he at times disagreed with Peirce that a successful belief has to be connected to the facts:

Satisfactorily, means more satisfactorily to ourselves, and individuals will emphasize their points of satisfaction differently. To a certain degree, therefore, everything here is plastic. (James 1975 [1907], 35)

In the “Will to Believe,” James said that it might be rational to believe in God if that belief made one’s life go better. Peirce and Lewis were not keen on this more subjective brand of pragmatism.

Peirce is right that he brought Kant to pragmatism. For one thing, he argued that all facts and experience come clothed in our concepts. As Dorothy Emmet put it, there is a difference between experience’s being brute and stubborn, à la Peirce, and its being bare and naked, à la the logical positivists (Emmet 1994, 186). Another Kantian move concerns the regulative assumptions of inquiry. Central to pragmatism is a theory of inquiry that holds that an inquirer has a body of settled beliefs, which are in fact not doubted. Peirce argued that such beliefs take a variety of forms: they may be ordinary empirically confirmed beliefs, mathematical and logical beliefs, or deeply-engrained beliefs whose origins are intangible. There are also regulative assumptions of the practice of inquiry, things we must assume if we are to continue with it. For instance, we need to assume there is a truth of the matter to the question at hand, otherwise it would make no sense to inquire into it. But Peirce was clear that needing to assume something does not confer upon the assumption the status of necessity. Peirce wanted to naturalize Kant—do away with transcendental deductions and move to talking about the requirements of belief and inquiry. Peirce said he was “not one of those transcendental apothecaries ... —they are so skilful in making up a bill—who call for a quantity of big admissions, as indispensable *Voraussetzungen* of logic” (CP 2.113, 1902).

We shall see that Lewis, Peirce’s true successor in the next generation of pragmatists, developed a pragmatism with both the British and Kantian elements as well.

2.3 The Exodus

In the interwar period, the scientific empiricist philosophy of the Vienna and Berlin circles was branded dangerous and Jewish by the rising right-wing. These log-

ical positivists were on the whole a politically progressive group.⁷ Their philosophy aimed to sweep away metaphysical and ideological authority in favor of scientific rigor and clarity. Most of the purported answers to age-old questions about essences, the Absolute, or the thing-in-itself were to be declared meaningless by showing that they are not reducible to observation. Ethics, religion, and politics were to be reimagined as scientific or factual statements about people's desires. This did not go over well and, at best, the careers of Vienna and Berlin Circle members would be hopeless. At worst, their very lives were at stake. Many fled to America, where the founders of American pragmatism were already dead, Peirce in 1914 and James in 1910. It was next generation of American pragmatists who greeted the émigrés: Dewey and Ernest Nagel at Columbia; Lewis and his former students Nelson Goodman, Morton White, V. W. O. Quine at Harvard.

Feigl was an early arrival. Antisemitism prevented him from getting a university post in Vienna after his doctorate and he went to Harvard in 1930 on a temporary Rockefeller Foundation grant, landing a job at the University of Iowa, and eventually moving to Minnesota. Others came when the political situation became more threatening. Carnap secured a job at the University of Chicago in 1936 and finished his career at UCLA, with periods at Harvard and at Princeton's Institute for Advanced Study. Hempel first visited Chicago to join Carnap in 1937, made his way to Queens College in New York, and then to Princeton for the bulk of his career. Reichenbach moved to UCLA in 1938 from a temporary perch in Turkey. Once safe in America, the exiles and the locals banded together to bring more people over. Philipp Frank was helped to Harvard in 1938 by Carnap and Nagel. A letter the Carnaps wrote to the Nagels describes the not-untypical arrangements:

Frank writes us that he has a passage on the 'Europa' leaving Bremen on the 28th of September. I believe that means that he will be in New York around the 3rd of October. Does Ernest intend to meet him at the boat? Frank has some money deposited with us which he will need when he arrives. Will you let us know about your plans? And might we send you a check to be cashed by you and to be handed over to Frank?⁸

Such private help was a supplement to official arrangements of universities looking for talent and the Rockefeller Foundation, one of the major movers of displaced academics to America.

⁷ See Edmonds (2020).

⁸ Letter from the Carnaps to the Nagels, Aug. 23, 1938. Ernest Nagel Papers, MS 0915, Box 1, Columbia University Archives (hereafter, ENP-CUA).

2.4 Early Friendly Encounter with Lewis

While the exiles knew a little about Peirce and James before they arrived in America, one of the earliest and most substantial points of contact with the pragmatists was with Lewis.⁹ From 1912, Lewis had been writing papers critical of the account of material implication in Russell and Whitehead's *Principia Mathematica*, a book that was important to the Vienna Circle. Lewis's *A Survey of Symbolic Logic* was published in 1918 and was well known (Carnap cites it in his 1928 *Der logische Aufbau der Welt*). In 1930, an American visitor to the Vienna Circle, Albert Blumberg, was likely the one who introduced *Mind and the World Order* to the Circle.¹⁰ Feigl said that, before 1930, "Most of us in the Vienna Circle were largely ignorant of American philosophy" (Feigl 1969, 69). But from that year, they knew about Lewis's pragmatism.

Feigl met Lewis in 1930, during his year at Harvard. Lewis spotted the similarities between his brand of pragmatism and logical positivism straight away. He said in his replies in the volume of the *Library of Living Philosophers* devoted to him that "In ... the early documents of the neopositivists—particularly in Carnap's *Der logische Aufbau der Welt* ... I found an empiricism and an analytic method which were congenial to my own persuasions" (Lewis 1968a, 664). They shared the idea that verification by experience was key in knowledge and meaning. Lewis saw that they had to address a similar set of problems, for instance, how we can verify statements about the past. In *Mind and the World Order*, he argued that we must understand them as referring to the circumstances that *would* verify them. He was eager to point out that *Mind and the World Order* was published the year that the Vienna Circle was founded (Lewis 1968b, 664). That is, Lewis did not get his empiricism from the logical empiricists, but rather, his was a "pragmatic empiricism" born with Peirce (Lewis 1968b, 664; 1956 [1929], xi).

Lewis and Feigl got on well and Lewis actively helped Feigl secure an academic position. His letter of reference for him in 1931 ended thus:

Feigl is one of the group—with Carnap, Reichenbach and Schlick—who represent the newly-formulated 'neo-positivism', which represents what we in America are sure to regard as the most promising of present movements in Continental philosophy. The manner in which this doctrine draws upon recent tendencies in exact science, and Dr. Feigl's particular competence

⁹ See Misak (2013).

¹⁰ See Christoph Limbeck-Lilienau(2010), who tells us that Blumberg had mentioned Lewis's 1929 book in his thesis.

in these fields, have rendered his discussions in our little group at Emerson Hall particularly pleasant and profitable for us.¹¹

The little group might well have been what Feigl referred to as the *Langer Zirkel*. Feigl wrote to Schlick in 1930:

I was delighted to meet Susanne Langer, who is a professor here at Radcliffe College ... She is an excellent woman and her versatility is admirable... She ... speaks German as well as English.... We (i.e. a group of young people who are interested in logic and philosophy...) meet at her place every Monday evening for discussions on the Viennese model.¹²

The Langer Circle met at Langer's house, not in Emerson Hall. But notice that Lewis says "the little group at Emerson Hall," which could well mean that it was a group of Harvard philosophers who were based in Emerson Hall, but met somewhere other than Emerson Hall. In any event, it is clear that Lewis and Feigl had significant philosophical conversations in 1930. Lewis read a draft of Feigl's "Logical Positivism," co-authored with Albert Blumberg.¹³ In December of that year, Feigl wrote to Schlick that Lewis's "conceptual pragmatism" is "barely distinguishable from our positivism" and said that *Mind and the World-Order* is "the best epistemology in the English literature."¹⁴

2.5 Lewis's Kantian Pragmatism

In *Mind and the World-Order*, Lewis, following Peirce, argued for a pragmatism that takes human inquiry as the starting point in philosophy, yet has that inquiry shaped by the brute force of experience. He rejected an ahistorical, transcendental, or metaphysical theory of truth, but was nonetheless committed to doing justice to the objective dimension of human inquiry. That objectivity is delivered to us through experience—simple feelings that impinge upon us, but which require interpretation before they become perceptual beliefs. Our body of beliefs is like a pyramid, with the most comprehensive, such as those of analytic definition and logic, at the top, and singular judgments at the bottom. Everything in our body

¹¹ Correspondence, Lewis, C. I., 1931–1933, Box 7, Herbert Feigl Papers, University Archives, University of Minnesota (hereafter, HFP).

¹² Quoted and translated by Verhaegh (2022). Feigl saw that Langer, with Lewis and Percy Bridgman, shared the logical positivist's tendencies. (Blumberg and Feigl 1931, 281).

¹³ See Verhaegh (2020).

¹⁴ Quoted and translated by Verhaegh (2020, 113).

of knowledge is connected in some way to experience through this network of beliefs, and everything is revisable.

While Lewis claimed that all experience “is either a specific quale (such as the immediacy of redness or loudness) or something analyzable into a complex of such,” he did not hold, as did many of those in the European empiricist movement (Schlick, Russell, Wittgenstein), that all knowledge is built up of qualia about which we cannot be mistaken.¹⁵ With Peirce, Lewis argued that we cannot get down to the first impressions of sense. What we have are our judgments of them. Knowledge is not built up from infallible atoms, but merely responds to the impinging of experience which is immediately interpreted. Lewis, like Peirce, had a Kantian inflection to his empiricism. Our concepts shape our experiences and certain of those concepts—those without which our practices of experience and inquiry could not continue—are to be assumed to be true. But everything in our body of belief is connected in some way to experience through this network of beliefs and any belief can fall to experience, as long as we are willing to make the ensuing revisions throughout the pyramid. That includes *a priori* beliefs such as logical truths and definitions. We are not quick to revise such beliefs, as the reverberations would be felt all the way down. But they nonetheless are revisable. In effect, Lewis was saying that there is no *a priori*, as it is traditionally conceived. There are no “self-illuminating propositions” which are true with certainty (Lewis 1970 [1923], 231). *A priori* statements are disconfirmable by experience—not directly, as is *The swan coming around the bend will be white* or *All swans are white*. But they can be shown to be mistaken, for instance, when the tide of experience makes us question a cherished assumption. Lewis was set against the quest for certainty and the constructivism/reductionism of the Vienna Circle.

He argued that it is not the *a priori* that is compelling, but the *a posteriori*: “It is given experience, brute fact, the *a posteriori* element in knowledge which the mind must accept willy-nilly.” The *a priori*, on the other hand, “represents an attitude in some sense freely taken, a stipulation of the mind itself, and a stipulation which might be made in some other way if it suited our bent or need” (Lewis 1970 [1923], 231). The *a priori* is the “uncompelled initiative of human thought”—the human-made net of categories and definitions without which we cannot “interrogate” or “capture” experience (Lewis 1970 [1923], 237).

We hold such “categorical principles” firm, even if experience seems to go against them, until we stop holding them firm because they no longer work or serve our purposes. Our *a priori* categories and definitions “are subject to alteration on pragmatic grounds when the expanding boundaries of experience reveal

¹⁵ See, for instance, Lewis 1956 [1929], 60).

their infelicity as intellectual instruments”; they “may be subject to gradual transition and even to fairly abrupt alteration” (Lewis 1956 [1929], 228). These changes might be brought on by the brute impinging of facts, but also by the developing mind. We investigate, revise, and re-invent our framework.

We have, for instance, discovered that there are several logics, each self-consistent on its own terms. The choice is a “pragmatic” one, the criteria for which will be drawn from our “human bent and intellectual convenience” (Lewis 1970 [1923], 233). Take the law of excluded middle (for any proposition either it or its negation is true). It

formulates our decision that whatever is not designated by a certain term shall be designated by its negative. It declares our purpose to make, for every term, a complete dichotomy of experience, instead—as we might choose—of classifying on the basis of a tripartite division into opposites (as black and white) and the middle ground between the two. Our rejection of such tripartite division represents only our penchant for simplicity. (Lewis 1970 [1923], 232)

The laws of logic are “principles of procedure, the parliamentary rules of intelligent thought and speech” (Lewis 1970 [1923], 232). Like definitions, they are “addressed to ourselves” and “represent no operations of the objective world, but only our categories of mind.” Both “are peculiarly social products, reached in the light of experiences which have much in common, and beaten out, like other pathways, by the coincidence of human purposes and the exigencies of human cooperation” (Lewis 1970 [1923], 239). Our conceptual framework thus both has the feel of a “fiat” and of “deliberate choice” (Lewis 1956 [1929], 213).

The *a priori* is not, however, “arbitrary in the sense of being capriciously determined” or in the sense that it answers to no criteria. (Lewis 1956 [1929], 237). Since knowledge has “a practical business to perform,” all our concepts and interpretations answer to “our need to understand, in the face of an experience always more or less baffling, and ... our need to control” (Lewis 1956 [1929], 237). Empirical judgments answer to the criterion of observation. Mathematics answers to self-consistency. Ethical judgments answer to something different. Lewis included ethics in our body of knowledge or under our cognitive scope, from the 1920s and later developed this line of thought, saying that in ethics, humans “are mostly served by cooperation with others” and that our ethical beliefs must answer to those requirements (Lewis 1956 [1929], 238). The philosopher “seeks to reveal those categorical criteria which the mind applies to what is given to it, and by correct delineation of these criteria to define the good, the right, the valid, and the real” (Lewis 1956 [1929], 36).

Lewis met the solipsism challenge to any empiricism head-on. We do not know whether the qualia or content of what is given to each mind is similar or not, but we do know that human beings have similar basic needs which will cause them to

take similar attitudes to what they experience and to act in similar ways. Our common world is thus created by common need.

2.6 Further Engagement of the Exiled Empiricists with Peirce and Lewis

Carnap said in 1936, the year he landed in America: “It seems to me there is agreement on the main points between the present views of the Vienna Circle ... and those of Pragmatism, as interpreted e.g. by Lewis” (Carnap 1937 [1936], 427). Peirce was the subject of a talk at the fifth International Conference for the Unity of Science, which Charles Morris and Quine organized at Harvard in 1939, the first of the conferences to be held in America. Nagel, who did not like to call himself a logical empiricist or a pragmatist, but who had strong connections with both, gave a masterful paper titled “Charles S. Peirce: Pioneer of Modern Empiricism.” He pointed out the affinities between logical empiricism and pragmatism: the antipathy to metaphysical speculation, the emphasis on cooperative scientific research, and the fact that the pragmatic/verificationist maxims were “offered to philosophers in order to bring an end to disputes which no observation of facts could settle because they involved terms with no definite meaning” (Nagel 1936, 73).

Reichenbach saw the connection as well. He learned about Peirce’s work from Sidney Hook, who sent him some of Peirce’s papers in 1932. Reichenbach admired Peirce’s work on induction (Reichenbach 1938 187–188) and saw that his own vindication of induction walked hand in hand with Peirce’s idea that “Inductive inference cannot be dispensed with because we need it for the purpose of action” (Reichenbach 1938, 346). The only way of justifying induction is that it is required for the success of action. That is one of Peirce’s regulative assumptions of inquiry. If we are to know anything at all, we must rely upon inductive inference.

But there was much that divided them. The first published critical engagement was Lewis’s 1930 “Pragmatism and Current Thought,” followed by his 1933 Presidential Address at the American Philosophical Association, published in 1934 as “Experience and Meaning.” Lewis thought there were insurmountable problems with the phenomenalist foundation employed by Carnap and others in the logical positivist camp.¹⁶ He noted that “if your hours as felt, are twice as long as mine,

¹⁶ In 1930, as Verhaegh (2020) has shown, the Vienna Circle was in agreement about the nature of the foundation. They took Wittgenstein’s view in the *Tractatus*: all meaningful propositions can be reduced to elementary propositions, the form of which must conform to the form of the phenomena. Later there were disagreements.

your pounds twice as heavy” that makes no testable difference (Lewis 1930, 242–243). Carnap, in the *Aufbau*, had argued that classes of experience are constructed out of individual time slices of sensory experiences; then concepts such as blue are built up; then objects; then higher concepts. Lewis noted that Wittgenstein, in the *Tractatus*, also accepted the unacceptable solipsistic implication (Lewis 1970 [1934], 261–262).¹⁷ But Lewis thought that the attempt to terminate knowledge claims in what is actually present to a person here and now (Carnap’s “autopsychological basis”) doesn’t work, for knowledge and meaning would then “collapse into the useless echo of data directly given to the mind at the moment” (Lewis 1970 [1934], 263).¹⁸

The problem is that it is impossible to communicate such private qualitative content to others—the experiencer seems to be trapped in his or her own world. Lewis held that solipsism of the present moment has to be avoided by any theory, if it is to be compatible with the idea of the validity of knowledge. An empiricism that has meaning and truth bottoming out in phenomenological qualia cannot say how we actually get to the world beyond our experience of it. Since we are supposed to be talking and thinking about that world, this is a major problem. We have seen that Lewis addressed the challenge of solipsism by appealing to our shared basic needs. That is a distinctly pragmatist answer that the logical positivists, at least at this stage in their development, were unwilling to offer.

Lewis argued that the meaning of an expression is not reducible to immediate experience, rather, it consists in the expectation of a future possible experience (Lewis 1970 [1934], 268). Meaning resides in prediction or expectation. One salutary effect of this account of meaning is that it allows Lewis, unlike Carnap and the rest of the Vienna Circle, to include general propositions and inferred conclusions in our body of meaningful belief. The open generalization *All humans are mortal* goes beyond any actual experience, but, as Ramsey put it, it is a rule with which we meet the future.

¹⁷ Wittgenstein was silent about what elementary sentences were, but it was reasonable to interpret him as thinking they were along the lines of *blue, here and now*. Frank Ramsey, the pragmatist in Wittgenstein’s orbit, made the same objection to Wittgenstein and Carnap. See Misak (2016, 226).

¹⁸ For Schlick and Carnap, it is only the qualia which are private, but the structure and relation between the qualia are communicable. There are major problems with the structuralist idea as well. See Newman (1928).

2.7 The Move towards Lewis

As Limbeck-Lilienau (2012) argues, immediately after the *Aufbau* was published, Carnap distinguished degrees of verifiability. He was already starting, that is, to revise the position Lewis was challenging. Carnap wrote a draft of a reply to Lewis, sent it to Schlick and Morris, and later incorporated it into the 1937 “Testability and Meaning.” He called it his “Lewis paper” (Creath 1991, 190). Carnap’s pragmatist move was first published in his 1934 *Logical Syntax of Language*, where he argued that when an increasing number of consequences of the hypothesis agree with experience, the hypothesis is increasingly confirmed (Carnap 1937 [1934], 318). Carnap’s response to Lewis’s charge is to allow for partial verification and for the meaningfulness of generalizations.

It was also in this book that Carnap’s pragmatist principle of tolerance—his move to a pluralism about theoretical frameworks—made its debut.¹⁹ He said that he had in the past “overlooked the fact that there is a multiplicity of possible languages” (Carnap 1937 [1934], 245). Internal questions, which can only be raised from within a linguistic framework, must be straightforwardly verifiable. An external framework of abstract concepts, beliefs, methodological principles, on the other hand, is chosen on pragmatic grounds. In the 1947 *Meaning and Necessity* he put the point in distinctly pragmatist terms: We accept a framework on the grounds of whether it is “expedient, fruitful [or] conducive to the aim for which the language is intended” (Carnap 1956 [1947], 214). Our frameworks are tested by their success or failure in practical use. Here is Carnap sounding even more like Lewis:

No rule of the physical language is definitive; all rules are laid down with the reservation that they may be altered as soon as it seems expedient to do so.... In this respect, there are only differences in degree; certain rules are more difficult to renounce than others. (Carnap 1937 [1934], 318–319)

Carnap said in an autobiographical essay that as a result of pragmatist influences, he put more emphasis on the social factor in knowledge and on the idea that a “conceptual system” involves practical decisions (Carnap 1963, 861). It is

¹⁹ See Richardson (2007), Uebel (2017) and Misak (2013) for the full argument about the pragmatist nature of the principle of tolerance. Richardson and I also note the striking similarity between Neurath’s holism and pragmatism, but since Neurath was not one of the empiricists who ended up in America, this essay is silent on him.

clear that there is a direct and acknowledged link from Lewis to Carnap. But we shall see that a significant difference undercuts this agreement.

Philipp Frank also adopted something like Lewis's holism, arguing that the meaning and the truth of a statement are matters of the statement fitting into a system—into a theory. He argued that theories are under-determined by the data—more than one theory can account for what we experience—hence the choice of theory involves considerations such as social utility.²⁰

Arthur Pap was even more Lewisian. He occupies an interesting place in the story of the exiled empiricists, tending (falsely) to be more identified with logical positivism than pragmatism. Pap's family fled Europe in 1941, when Arthur was 19. He hadn't encountered the new scientific philosophy during his high schooling in Zurich—he was a Hegelian when he arrived in New York, ready for his undergraduate studies and then a PhD at Columbia in 1944. Pap was influenced by the logical positivists—by Charles Stevenson, who argued that ethical terms do not have cognitive or observational meaning, but are mere expressions of emotions and Carnap, with whom he became friendly with in 1947 while they were both instructors at the University of Chicago. In 1952–53, after translating Viktor Kraft's history of the Vienna Circle, Pap went on a Fulbright to the University of Vienna.

But Pap was more heavily influenced by pragmatism. His PhD thesis, supervised by Ernest Nagel, was published in 1946 as *The A Priori in Physical Theory*.²¹ In it, Pap acknowledged his indebtedness to Lewis's pragmatic account of the a priori. Necessity is not an all-or-nothing game that radical Platonists (voting “all”) and radical empiricists (voting “nothing”) would have us believe—“both the empiricist and the rationalist fail to account for the interaction of empirical and formal knowledge” (Pap 1943, 458). Pap put forward a “hypothetical” conception of necessity—one that treats necessity as being a “prospective” and “functional” condition for something else (Pap 1943, 449). A hypothetical necessity is not a self-evident axiom that stands without alternatives. The “determination of the *essential* or def-

²⁰ See Frank (1951, 19) and (1950).

²¹ Nagel was also an early exile who had a foot in both pragmatist and logical positivist camps. He emigrated from present-day Slovakia to the United States at the age of ten and received a BSc from the City College of New York in 1923, and a PhD from Columbia 1931. In letters to Sidney Hook in 1945, he wondered (at length): “I should like to know whether I am a naturalist or not. I gather that the chief qualification is respect for scientific method. The doubts begin to arise when I ask ... what this method means. Apparently one of the requirements is that any ‘responsibly held’ proposition should be ‘publicly verifiable’” (ENP-CUA, MS Coll 0915, Box 1, 24). It was the verificationist requirement of the logical positivists that made him wary. He preferred the Lewisian position and argued that “any verifying process has evidential value only *within* a framework of pre-existing knowledge” (Nagel 1956 [1954], 5).

initory properties of kinds ... is not a discovery, but a choice” and that choice is based on “pragmatic reasons” (Pap 1943, 452).

Scientific change, on Pap’s view, is grounded on “the best possibility,” “best relative to the functional or teleological context in which it arises” (Pap 1943, 450):

Thus the ptolemaic hypothesis was good for explaining the astronomical facts or phenomena; its rejection in favour of the Copernican Hypothesis had no logical ground, but only a functional ground: the latter did the job better than it, being simpler and hence more convenient. (Pap 1943, 451)

Pap argued that as principles of science become accepted, they become fixed definitions and criteria for further inquiry. This is a core insight of all pragmatists, from Peirce onwards: we need a body of belief, not doubted, by which to judge other beliefs and by which to make sense of experience. But that whole body of belief is fallible. Pap added that what was once empirical can become *a priori* and vice versa, and that too is open to change down the road.

Pap was set against the logical positivist’s “static” model of science, in favor of the pragmatist’s “dynamic” model (Pap 1946, vii). As David Stump puts it, the main idea in Pap’s functional *a priori* is that “one considers the role that a sentence is playing in a physical theory, rather than its origin” (Stump 2011, 279). That is as nice a way as any of summing up a crucial difference between the Vienna Circle (with its focus on origin) and pragmatism (with its focus on function).

In what follows, I shall outline what I take to be the further salient differences between the Lewis position and that of the logical positivist. If I had enough time and space, I would argue that Lewis comes out better on each and every one.

2.8 Five Points of Disagreement

Lewis disagreed with the exiled empiricists on some important and related matters. First is the one identified by Pap: Lewis focuses on the function of our statements, rather than on their origins being grounded in the certainty of experience. Not all the logical positivists held that, but those who did were wrong, argued Lewis.

The second is Lewis’s antipathy to the idea that knowledge is reducible to experience. As Lewis puts it, the pragmatist asks “What empirical confrontations would confirm this statement?” whereas Carnap asks “To what class of observable predicates is this term reducible?” and “What other terms are synonymous with this one?” (Lewis 1970 [1941], 95). The logical positivist’s project is to try to eliminate or reduce away what they considered inflated entities in favor of a sparse and sci-

entific landscape. But Lewis thinks that “words and sentences without associated imagery are marks and noises without significance” (Lewis 1970, [1941], 96). In wanting to “logicize all problems, [the logical positivists] try to get rid of some of the most important problems in philosophy and life” (Lewis 1970 [1941], 96).

One of the things that is reduced away on the logical positivist’s project are value statements, and this was what Lewis identified as the “strongest” difference between them and him. In 1933, he was writing to Feigl, worrying that “a plain implication of the Circle’s theory of meaning is that ethics can be only descriptive—a sort of branch of sociology or behavioristic psychology.”²² He later said that “one of the strangest aberrations ever to visit the mind of man” is the idea that value-predictions are not about matters of fact but are merely expressions of emotion and hence are not true or false (Lewis 1970 [1941], 107; 1971 [1946], 366). The fact that human beings are the judges of what is right and wrong does not mean that “the evaluations which the fool makes in his folly are on a par with those of the sage in his wisdom.” Human beings “stand in need of all that can be learned from the experience of life in this natural world” (Lewis 1971 [1946], 398–399); “empiricism in epistemology and naturalism in ethics do not imply relativism and cynicism” (Lewis 1971 [1946], viii).

Another difference is the treatment of analytic or what Lewis calls *a priori* statements. The logical positivists needed a sharp analytic/synthetic distinction, as they had to explain how definitional truths and the truths of logic and mathematics were legitimate on their picture. Their solution was that such truths are analytically true—they are not about the world, and so they are not subject to empirical verification and need not meet the verification requirement. Lewis, we have seen, did not think that definitional, mathematical, and logical truths are cordoned off from empirical inquiry. They are part of our corpus of knowledge, subject to revision by experience.

Quine, in his 1951 “Two Dogmas of Empiricism,” attacked Carnap over the matter of analyticity and dragged Lewis into the dispute. Quine argued that, contrary to the dogma of the analytic-synthetic distinction, no statement is immune from revision. Our sentences form a web of belief, and each sentence is more or less revisable, depending upon how peripheral or central its position is in the web. There is only an *appearance* of some sentences being analytic because they are in the center of the web and so are given up, if ever, only under extreme pressure from the peripheral forces of experience. But no sentence is immune from revision and so no sentence is analytic. As Carnap put it, when it came to the analytic,

²² Feigl Papers, HFP, Box 7. Lewis’s comment seems to have been about Schlick’s *Questions of Ethics*.

Quine thought that “at best, a distinction of degree could be made,” not a distinction of kind (Carnap 1963, 64–65). Quine’s account of knowledge and the *a priori* picture is lifted straight out of Lewis’s *Mind and the World Order*, without attribution. He blithely set out Lewis’s epistemology as his own and then attacked him, along with the Vienna Circle, on the matter of the analytic. Joining in the attack were two of Lewis’s other students, Morton White and Nelson Goodman. As White told me late in life, they were not fond of their teacher.²³

The final disagreement between Lewis and the exiled empiricists has to do with frameworks. Carnap thought that analytic statements could be revised, but not by experience and not in the course of inquiry. Questions internal to a language are subject to rational assessment in terms of the rules of that language. Once we are working within a language, we can engage in the project of constructing our theories from pure, primary, materials. But questions external to the framework—whether to adopt a phenomenalist or physicalist language or whether to adopt classical or intuitionist logic—are not resolved by the rules of the language.²⁴ They are a matter of choice and are “non-cognitive,” because meaning lies only within a framework (Carnap 1956 [1947], 214). Carnap, that is, thought that pragmatic factors, such as simplicity and usefulness, play a role in the choice of language, but not in decisions about what scientific theory within a language to accept. We can make a radical change only by choosing a different language. Lewis thought that rules and definitions are part of our web of belief, not something meaningless that stand outside it. He argued that we can make a radical change *within our framework*—for instance, by changing a law of logic—without it becoming quite literally a different language. For Lewis, choice is ever present in ordinary scientific revision. We do not need a revolution in order to employ the values of simplicity, fit for purpose, and so on.

Lewis surely has the better, more realistic, position here. Carnap’s suggestion that we can switch frameworks employs an odd sense of *can*. We do not in fact switch frameworks. What would it be to do so? Moreover, as Russell first noted, Carnap’s pluralism about frameworks is in tension with his aim, an aim internal to his own framework, of coming to objective and grounded true beliefs. Pluralism about frameworks unhooks truth from reality. As Russell put it, “Various questions which I should regard as questions of fact are, for [Carnap], questions as to the

23 “C. I. Lewis was not a nice man,” he told me. See White (1999). In Misak (2013) and (2020), I explore more fully this rather shocking moment in the history of philosophy.

24 Lewis was unhappy with Carnap’s switch to the choice of a physical language as the foundational base in 1932, which seemed to Lewis a mere “semantic ghost of direct experience” (1968b, 664). It is the language of experience, rather than the language of physics, that is important (1970 [1941], 103).

choice of language” (Russell 1997 [1945], 154). Another way of putting this disagreement is that the pragmatist thinks that there is only one framework, which we all inhabit. As Donald Davidson argued, if you can communicate across conceptual schemes, then there is only one of them.

Towards the end of his career Lewis, tired and frustrated with being taken as a reductionist logical empiricist who bought into the myth of the given, became hostile to logical positivism. He was now convinced that where the positivists disagreed with him—especially regarding reductionism and ethics—they were having a deleterious effect on the new generation of students.²⁵

2.9 One Who Didn’t Make It: Janina Hosiasson

I have argued elsewhere that the most compelling lineage of pragmatism moves from Peirce to his successors Lewis and the British pragmatist Frank Ramsey. Because Ramsey died at the age of 26 in 1930, his work unfinished and hard to interpret, he has been taken to be part of the Vienna Circle. (His undergraduate thesis, “The Foundations of Mathematics” included a move the Circle took to be part of “the turning point in philosophy”—the argument that mathematics, as well as logic was tautologous or analytic.) But Ramsey was explicit that he was a kind of a Peircean pragmatist.²⁶

Ramsey’s great addition to Peirce and Lewis is that once we think of belief in terms of habit, we can understand and measure partial belief. Peirce had argued that “our beliefs should be such as may truly guide our actions so as to satisfy our desires” (CP 5.375). Ramsey provided us a way of thinking about how belief, desire, and action are connected. He imagines a hill walker who needs to make a decision:

I am at a cross-roads and do not know the way; but I rather think one of the two ways is right. I propose therefore to go that way but keep my eyes open for someone to ask; if now I see someone half a mile away over the fields, whether I turn aside to ask him will depend on the relative inconvenience of going out of my way to cross the fields or of continuing on the wrong road if it is the wrong road. But it will also depend on how confident I am that I am right; and clearly the more confident I am of this the less distance I should be willing to go from the road to check my opinion. (Ramsey 1990 [1926], 70–71)

²⁵ Christian Damböck has some interesting work in progress where he argues that the later Carnap’s position on all points of apparent disagreement (reductionism, value, framework, analytic) was actually much closer to Lewis’s than he or Lewis saw. If Damböck is right, Carnap was for some reason reluctant to make that clear.

²⁶ See Misak (2016) and (2022) for the full story.

When the walker heads off to the right, he has some degree of belief or confidence, however he might have come to it, that this is the correct road, as well as some preferences about the relative inconveniences of getting lost and of going out of the way to ask directions. In order to make sense of probability in such contexts, we need to be able to measure degrees of belief.

The received view was that “belief and other psychological variables are not measurable” or, if they are measurable, we must try to measure them introspectively, with each person looking inside his own mind and assessing the intensity of feeling that accompanies his belief (Ramsey 1990 [1926], 62). Ramsey thought the introspective method could not provide enlightenment. Our perceptions of what goes on inside our minds are not only hard to access and study, but they are also unreliable. Even if we *could* ascribe numbers to intensity of feelings, we would get wildly inaccurate measurements. For instance, the beliefs we hold most strongly—the ones we take for granted—are often “accompanied by practically no feeling at all” (Ramsey 1990 [1926], 65). But if we think of belief as habit, we have something to go on. We assess habits in terms of their success. Ramsey’s subjective account of probability showed how degrees of belief conform to the logic of the probability calculus, giving rise to rational decision theory (although he himself thought that a too-highly idealized model for human rationality).

Pragmatists have only recently cottoned on to the fact that Ramsey was one of them and that he brings significant insight to pragmatism. This realization could have unfolded in America much earlier, had there not been a tragic missed opportunity. The brilliant Polish theorist of subjective probability (and Jewish communist), Janina Hosiasson, had spent 1929–30 in Cambridge, during and after Ramsey’s sudden illness and death. While in Cambridge, she realized that she and Ramsey had come to the same theory of probability and method for measuring partial belief. In 1931 she published “Why Do We Prefer Probabilities Relative to Many Data?,” which argues for something very much like Ramsey’s idea of mathematical expectation and noted that her position made sense only from a pragmatist perspective. Partial beliefs can be represented as probabilities and that the way people make decisions based on their partial beliefs is by maximizing expected utility. It follows that the way we can measure people’s partial beliefs is by studying their habits or betting strategies.

Hosiasson returned to Europe and when the war came, she needed to flee. Feigl, Hempel, Nagel, Sidney Hook, G. E. Moore (then waiting out the war at Smith College), and others tried to bring her to the New School for Social Research, via a Rockefeller grant. As her letters of support put it, she was “gravely in need of

assistance.”²⁷ Her CV, written by someone on her behalf, lists under “Additional Remarks”: “Starving; endangered.” On October 18, 1940, the Rockefeller Foundation wrote to the New School to say that they were “not inclined at the present time” to consider a grant for Hosiasson. An appeal was lodged, but it was turned down. Quine weighed in with his support, saying that Hosiasson held “a distinguished place in the annals of [the] important topic” of probability theory and inductive inference. In 1941, the group contacted individual presidents of universities with the suggestion that Tarski would personally find funds for two years to support Hosiasson, at no cost to whatever university would take her. They had success at the University of Washington. The Rockefeller Foundation, however, wrote to the president, asking whether the University could make a commitment of a continuing appointment. Hearing that it could not, the matter was over. Letters amongst her supporters continued to fly, but this was in effect the end of Janina Hosiasson. She stayed on the run from the Nazis, but was arrested in 1941 and shot in 1942. Astonishingly, she managed to write and publish (excellent) papers during this time. Had Hosiasson made it to America, she would have transformed the empiricisms of all parties, for she would have brought a sophisticated account of partial belief to America. As it transpired, this version of pragmatism, which would have been congenial to logical positivism, got buried when Ramsey and Hosiasson met their early deaths and is only being excavated now.

2.10 Conclusion

In one sense, the eclipse narrative is true: the émigrés eclipsed Deweyan pragmatism, until it was resurrected by Rorty in the 1970s. But Lewis’s pragmatism survived in the work of Pap, Quine, Goodman, and White.²⁸ And parts of it survived in Carnap’s position, although I have suggested in this paper that while Carnap learned some important things from Lewis, he did not go far enough with Lewis on the path of pragmatism.

The tragedies and vagaries of human existence influence the course of thought as well as the course of lives. Lewis’s students didn’t like him and so they attacked him after adopting his position and (except for White), refused to call themselves pragmatists. Ramsey died at the age of 26 and Pap at 38, their pragmatist positions

²⁷ The passages in this paragraph are from the archive collected by Marta Sznajder, who will publish the full story of Hosiasson. While we wait for the larger work, see Sznajder (2022). I thank her for sharing the material with me.

²⁸ Goodman’s vindication of induction is pragmatist in all but name and White was explicitly pragmatist, although he tended not mention Lewis.

not brought to fruition. Hosiasson's pragmatism was buried when she was murdered by the Nazis. Philosophy in Germany and Austria suffered from losing their brain trust during the war. As Feigl wrote to Nagel in September 1954 "Vienna philosophy is [other than Paul Feyerabend] in terrible shape."²⁹ Feyerabend, who as a young man served in the Austrian army, would also leave Vienna the following year, eventually landing at Berkeley.

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29 Feigl to Nagel, Sept. 16, 1954 (ENP-CUA, MS Coll 0915, Box 1).

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Joel Katzav

3 Speculative Philosophy of Science vs. Logical Positivism: Preliminary Round

Abstract: I outline the theoretical framework of, and three research programs within American speculative philosophy of science during the period 1900–1931. One program applies verificationism to research in psychology, one investigates the methodology of research programs, and one analyzes scientific explanation and other scientific concepts. The primary sources for my outline are works by Morris Raphael Cohen, Grace Andrus de Laguna, Theodore de Laguna, Edgar Arthur Singer Jr., Harold Robert Smart, and Marie Collins Swabey. I also use my outline to provide a partial comparison of American speculative philosophy of science and 1930s logical positivism. My comparison suggests that logical positivism was a proposal for substantially narrowing down and winding back American philosophy of science and was based on positions that were already problematized in the American context.

3.1 Introduction

(North-)American philosophy of science developed substantially during the early decades of the twentieth century. Key figures, starting with James Edwin Creighton and continuing with Edgar Arthur Singer Jr., Morris Raphael Cohen, and others, articulated conceptions of philosophy that equated it with, or at least intimately tied it to, speculative philosophy of science. They also used these conceptions in training substantial numbers of philosophers of science.¹ Creighton's location at Cornell University was particularly significant, since it played a central role in populating American philosophy departments (Cohen 1910; Schneider 1946, 471; Katzav and Vaesen 2022). In line with the ongoing professionalization of philosophy, the work of philosophers of science was prominent in the first two professional American philosophy journals, *The Philosophical Review* and *The Journal of Philosophy, Psychology and Scientific Methods* (Katzav and Vaesen 2022). More broadly, a sub-

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¹ There were about 500 people registered as members of the American Philosophical Association in 1931, with about 6% of them identifying the philosophy of science as their primary research focus (PROC 1932).

stantial body of work in the philosophy of science was produced by dozens of American philosophers of science. This work included work on many of the topics subsequently important within analytic philosophy of science. By far the largest group of philosophers of science working in America were speculative philosophers of science, with only a small minority of these identifying as pragmatists (Katzav and Vaesen 2022).

There is, however, still no detailed examination of American speculative philosophy of science's research or of the bearing of this research on the development of philosophy of science after the arrival of logical positivism in America in the 1930s. Standard histories of the field start with the arrival of logical positivism and continue with what is assumed to be its internally driven development into logical empiricism and, eventually, the broader analytic philosophy of science of the late twentieth century. Insofar as pre-logical positivist American philosophy of science is recognized, it is the work of a few key pragmatists, most notably Charles Sanders Peirce and John Dewey.

I aim to enrich our understanding of American philosophy of science by taking a closer look at American speculative philosophy of science. I will present this tradition's conception of philosophy of science, especially of the logic of science, in more detail. Some input from the work of Cohen and of Filmer Stuart Cuckow Northrop aside, I do this using the work of some of Creighton's students, especially Harold Robert Smart but also Grace Andrus de Laguna, Theodore de Laguna (Grace's husband), and Marie Collins Swabey. So too, I will describe the development of three of speculative philosophy of science's research programs in the logic of science during the period 1900–1931, starting in each case with the work of Singer and then examining developments in the work of either the de Lagunas or Smart. One program applies verificationism to research in psychology, one investigates the methodology of research programs, and one provides logical analyses of scientific explanation and other scientific concepts. I will, finally, use my history to reconsider the impact of logical positivism in America.

My focus is on speculative work around Singer and Creighton not only because of their centrality to American philosophy of science and because of their own influence but because their work and that of their students is representative of a broader swath of American philosophy of science. The focus on work by speculative philosophers will also facilitate reconsidering the impact of logical positivism in America. I will conclude that logical positivism appears to have been a proposal for narrowing down the scope of and winding back American philosophy of science. Logical positivism also appears to have rested on positions that had already been problematized in the American context.

In section 3.2, I outline American speculative philosophy of science's view of philosophy of science. In section 3.3, I outline three of its research programs in

the logic of science. I then, in section 3.4, compare American speculative philosophy of science with logical positivism. Section 3.5 is my conclusion.

3.2 Speculative Philosophy of Science and the Logic of Science

American speculative philosophy of science was part of the broader tradition of twentieth-century Anglophone speculative philosophy. Roughly, what characterized speculative philosophers as speculative was their insistence on the epistemic independence of philosophy from established opinion, including from science, in developing perspectives on reality. Speculative philosophers are contrasted with critical ones, according to whom philosophy aims to answer its questions by appealing to established opinion and somehow analyzing or unpacking it rather than going beyond it. In the American context, the schools associated with critical philosophy were those of new and critical realism (Katzav and Vaesen 2017).² Logical positivism was a form of critical philosophy (Katzav and Vaesen 2022).

On the speculative conception of philosophy of science, philosophy of science comprises the logic of science and speculative metaphysics (Benjamin 1936). Speculative metaphysics aims to offer visions of reality that include a depiction of humans and of how they, and their distinctive characteristics, fit into the broader scheme of things. Moreover, these visions are to be developed in light of research in the logic of science while nevertheless going beyond science in what they envisage (see, e.g., Creighton 1902 and 1912; Cohen 1910 and 1930; Northrop 1925).

Within the speculative tradition, the logic of science had as its object the systematic organization of scientific knowledge. Thus, the logic of science was concerned with how judgment—conceived of as a positive epistemic attitude towards hypotheses and other representations—and inference are exemplified in the structures of scientific knowledge, including in classification, explanation, experimentation, and theory (Smart 1931, 25–26). Similarly, inquiry into the system of knowledge was taken to require, and often involved, an examination of whether there is only one kind of scientific judgment/inference or whether, for example, each special science came with its own kind of judgment/inference (Smart 1931, 31). The question whether there are multiple kinds of scientific judgment brought with it the question of how the judgments of the different sciences relate to each other. Asking this last question involved considering whether the concepts and laws of

² Not all members of these schools, however, were opposed to speculative philosophy. William Pepperell Montague, for example, was a new realist and a speculative philosopher (Sheldon 1954).

any science are reducible to those of any others (Smart 1931, ch. 2). Not surprisingly, speculative philosophers of science investigated the range of the special sciences, including formal logic and physics (de Laguna and de Laguna 1910; Northrop 1925; Smart 1931).

The concern with judgment and inference also brought with it a concern with the status of scientific judgments, specifically with whether such judgments are either epistemically or ontologically fundamental (Smart 193, 35–44; Swabey 1930, ch. 3). The epistemic question here is to what extent scientific judgments are evaluated against empirical evidence holistically, that is, as members of systems of inferentially related judgments, rather than individually, one at a time. The related ontological question is whether it is concepts, judgments or inferences that are ontologically fundamental.

One of the key theses shared by many speculative philosophers of science was that scientific judgment involves abstraction, including idealization. For this reason, it was often assumed that the logic of science should include an examination of the ways in which judgment is abstract as well as how this affects scientific inference. In other terms, the logic of science should involve a critique of scientific judgment, in the sense of an examination of the extent to which it provides us with less than the full truth (Cohen 1930; Smart 1926, 92; 1931, 217–225).

Let me bring out four more features of the speculative logic of science. First, its questions were approached descriptively, that is, as part of an investigation into how science actually is, and normatively, that is, as part of an investigation into how it ought to be. Thus, for example, the critique of science did not merely describe the limitations of scientific inference but also included suggestions about overcoming some of these (e.g., Cohen 1931). Second, how to understand judgment was often informed by evolutionary ideas, including Hegelian and Darwinian ones. This led, for example, to views according to which judgments have evolved to have characteristic functions and thus are to be understood in terms of these functions (de Laguna and de Laguna 1910, part III). Third, not unrelated, the nature of judgment was often taken to be substantially illuminated by empirical investigation into its evolution. Answering philosophical questions in the logic of science was thus not limited to logical analysis, conceived of as the articulation of conceptual truths (de Laguna and de Laguna 1910, part III; Swabey 1930, preface). Fourth, speculative philosophers working on the logic of science often saw no tension between empiricist, or even verificationist, views about the logic of science and speculative metaphysics (e.g., G. de Laguna 1942).³

³ The logic of science is broader than what Katzav and Vaesen call the critical component of speculative philosophy of science. The critique of science and empirically informed, speculative an-

3.3 Research Programs in the Speculative Logic of Science: 1900 – 1931

3.3.1 Methodology of Psychology and Verificationism in the Work of Singer and de Laguna

Our first excursion into the speculative logic of science starts with Singer's "Choice and Nature" (1902). Singer there considers whether there is legitimate room for psychological, ethical, and other non-epistemic considerations in scientific inference, more specifically in deciding which scientific hypotheses to accept. In his terms, the question is what role there is for free choice in scientists' decisions about how to interpret nature. William James argued that, where evidence is lacking, we can freely decide what to believe (James 1897). Singer's response to James and similar positions is that, in the absence of empirical evidence for or against a hypothesis, scientists ought to acknowledge their uncertainty about it. And if there is no possible empirical evidence that bears on the hypothesis, it is meaningless:

Before those who really claim the right to believe in unsupported possibilities, science can only plead its inability to grasp their meaning. 'Either,' it says, 'your so-called beliefs are conceivably capable of confirmation or they are not. If they are, they await the event to be confirmed or refuted, as my doubts await it to be resolved. If they are not, but pose as faith in bare possibilities, they escape all chance of destruction by abandoning every vestige of content.' (Singer 1902, 74)

Here Singer deploys a verificationist criterion of significance for scientific judgments, that is, one that ties their meaningfulness to the existence of conditions in which they can be individually tested. On his criterion, for a scientific judgment to be meaningful, it must "be capable of confirmation or refutation from an indefinite series of other points of view" (Singer 1902, 77). Further, Singer takes his criterion to be empirical; it is based on observing scientific practice (Singer 1902, 73–74; 77–78).

One of the main areas where Singer applies his verificationist criterion of significance is the philosophy of mind. His view is that a consistent empiricism cannot allow one to infer the existence of other people's mental states if these are conceived of as being essentially subjective. Conceiving of them in such a way would

swers to philosophical questions are not part of the critical component of philosophy of science (Katzav and Vaesen 2022).

render claims about other minds unverifiable. As he puts it, some philosophers endorse

that curious bit of reasoning commonly known as the ‘analogy argument’ which runs somehow thus: I am aware, and I alone am aware, that certain of my bodily acts are accompanied by mental states. When I observe similar acts in other bodies I infer that they too are accompanied by like states of mind. (Singer 1911, 180)

But, retorts Singer,

An inference from a single case ... has ... no value at all ... no series of observations, no probable error; no ground for inference; no meaning as a datum. (Singer 1911, 181)

The claim here is that, if we can only make a single observation of a kind of correlation to support a judgment that the correlation holds more broadly, and thus cannot test the judgment from multiple perspectives, we cannot estimate the judgment’s probable error. In other terms, we cannot provide a probably correct estimate of how far our judgment might be from the truth. Such a judgment is supposedly meaningless.

Singer’s view of scientific judgment is part of his broader view of the mental, and both views illustrate the widespread speculative goal of understanding the mental functionally. According to Singer, mental states, including judgments, are goal-oriented (teleological) dispositions to behavior that are fully public and social rather than private; sufficient observation will make each state fully visible to others (Singer 1911). Each type of mental state is thus supposedly differentiated by its function. As we have seen, scientific judgments are partly characterized by verifiability, which is part of their function.

We will see how Singer thinks verifiability fits with some of the broader goals of science in the next section. The remainder of this section considers how his research program in the logic of science was developed further by (Grace) de Laguna. She explicitly acknowledged the influence of Singer’s work on her philosophy of mind (G. de Laguna 1927, xii) and, in particular, was drawn by the idea that knowledge of other minds is not knowledge of other essentially private mental states but of functional states. She pursues this idea in the context of a methodological investigation of Margaret Floy Washburn’s psychology.

Washburn used ontological dualism to underpin her book *The Animal Mind* (1908). In this book, she assumes that mental states are essentially private and, in accord with this assumption, proceeds to inquire what it is like for various organisms, including single-celled organisms, to have such states. In a review of the second edition of Washburn’s book, de Laguna argues that, contrary to Washburn, psychology should not have essentially private mental states as its objects of study,

partly because claims about such states are not verifiable. In support of the unverifiability of essentially private mental states, de Laguna deploys what later, when Ludwig Wittgenstein offered a similar argument, came to be called a “private language” argument. Could one, for example, fix the meaning of “being angry” by taking it to refer to some essentially private mental process? Not according to de Laguna. She writes,

No psychologist, I venture to assert, ever discriminated such a process and mentally labelled it ‘anger’ for purposes of scientific reference and comparison. Suppose he had done so, and tried to classify later experiences as ‘anger’ or ‘not-anger’ by comparison with this. He would find himself in serious perplexity, first, because it is very difficult to recall a past emotional state for purposes of comparison; and second, because he would probably find himself using the term in an arbitrary way, and making statements which could not be verified by others. (G. de Laguna 1918a, 621–622)

De Laguna’s arguments lead her to conclude that psychology can only have as proper objects of study factors that are functions of the standardized conditions of the experimental setup. Mental states are, accordingly, to be conceived of in terms of their causal role in standardized conditions. As she puts it,

It is an essential condition of scientific investigation of any phenomenon that observations made by one individual shall be verifiable by others. Otherwise indeed a phenomenon is not even identifiable. This was the point of my argument that psychological phenomena investigated experimentally ‘become in effect functions of the factors constituting the standardized conditions of the experiment.’ (G. de Laguna 1919, 297)

De Laguna moves beyond Singer in that her use of verificationism is methodological. She is arguing that scientific hypotheses need to have verifiable implications about their subject matter if that subject matter is to be identifiable and thus if scientific progress is to be possible at all. Thus, de Laguna is not using verificationism as a criterion of significance. Indeed, she makes clear that it is the task of metaphysics to take up questions that science must, because of its methodological commitments, leave aside, including about the ultimate nature of mental phenomena (G. de Laguna 1919; 1927, 127–128).

Plausibly, what lies behind this shift in the use of verificationism is the sophistication of de Laguna’s holistic theory of meaning. She and her husband, Theodore de Laguna, argue that concepts, which they took to be judgments, never regulate behavior directly by connecting stimuli and responses but via logical interrelationships within systems of concepts. How we respond to any situation depends on relevant systems of concepts, including such systems’ internal logical structure. For the scientist, the relevant system is primarily the system of concepts of their own science, along with these concepts’ closely knit inferential interrelationships.

Because of this, a concept's meaning cannot be specified in terms of correlations between stimuli and behavioral responses, such as observation claims, but also requires specifying its logical relations to other relevant concepts (de Laguna and de Laguna 1910, especially chapters 2 and 5 of part III; Katzav 2022a). Thus, on the de Lagunas' view, it is only within systems that concepts have meanings and generate predictions. So, the meaningfulness of a concept depends on being part of a system of concepts rather than on being individually verifiable. They accordingly reject verificationist criteria of significance, including Singer's criterion, which requires that each judgment be individually verifiable. Further, (Grace) de Laguna argues that there are, in addition to concepts, other forms of representation, such as perception, that have content that cannot be fully captured using concepts. Part of her motivation is that she recognizes that we do have introspective (phenomenological) knowledge that appears to resist conceptualization (G. de Laguna 1927, 290). So, even if we cannot make conceptual sense of some phenomenon, it does not on her view follow that we cannot make sense of the phenomenon.

Not surprisingly, given de Laguna's theory of meaning, she can develop an account of mental phenomena that is less naïve than Singers', where they are equated with observable behavior. Her account treats them as theoretical entities (G. de Laguna 1918a, 626). More specifically, she thinks about mental phenomena in terms similar to what later came to be known as "functionalism": an explanation of the nature of a mental state is, roughly, to be given in terms of its causes, effects and relations to other mental states, when fulfilling its proper function (G. de Laguna 1927; Katzav 2022b).

Singer's verificationist research program was also developed by other figures, including quite a few of those who, unlike de Laguna, were his students. These students included, for example, the philosopher of science C. West Churchman (1948) and the logician Henry Bradford Smith (1928). De Laguna's own methodological approach had an impact on the development of psychology. In particular, a father of modern cognitive science, Edward Chase Tolman, is convinced by her argument that psychology ought to offer functionalist accounts of mental phenomena (Tolman 1922, 45).

3.3.2 The Methodology of Scientific Research Programs

Let me turn to another early twentieth-century research program in the logic of science, one that also starts with Singer's discussion of the role of non-epistemic considerations in hypothesis choice. This second program is what, in the second half of the twentieth century, came to be called "the methodology of research programs."

Despite Singer's appeal to a verificationist criterion of significance, he does, in the end, allow room for free choice in deciding which scientific claims to accept. Here too, he deploys verificationism to support his position. Roughly, his argument is as follows: for an observation claim to be potentially confirmable and falsifiable (testable), and thus meaningful, the claim must represent two kinds of error, namely probable error (which, for observation claims, amounts to what we would call "measurement error") and constant error (the conditions under which the observation is expected to be correct). So, for example, for some report about the length of a rod to be testable, it must include a margin of error as well as a specification of the temperature, stress, and other standard conditions under which the claim about the rod is supposed to be correct. Singer accordingly takes it that the simplest kind of scientific judgment, the observation one, is hypothetical; it tells us roughly what would be observed in certain standard conditions. It is not categorical, stating that such and such is the case. Similarly, Singer holds that scientific laws are conditional in their application (Singer 1902, 78–80).

Still, science must make categorical claims. Singer calls the sets of assumptions that underpin, and so make possible, categorical applications of law and observation claims "classification systems." These specify research questions for the scientist and, in doing so, guide the acceptance of scientific claims. Classification systems thus set up what we would call "research programs." Further, since there are always multiple classification systems from which the scientist might choose and since they underpin accepting laws and observations, there is always a choice in how scientists respond to observed exceptions to their laws (Singer 1902, 81–82). Thus, while Singer's criterion of significance implies that each scientific claim must be empirical, it also leads to recognizing that choice has a role in determining every scientific fact. His resulting position is a form of idealism in which choice plays a role in constituting nature. Regarding science, he tells us,

Whatever is required to account for the way in which one of its stages follows on another is essential to the nature of experience. And since at any stage of our growing knowledge at which we try to tell what Nature is, the describer is presented with a choice, and since no stage can be found which does not embody past choices, I take it that this series of choices is involved in anything we do or can mean by Nature. (Singer 1902, 82)

Singer uses a series of case studies drawn from the history of science to support his claims about the role of classification systems and to explain how they are selected. The replacement of a classification system, and of the laws that presuppose it, is guided by the goal of systematicity or unity. When empirical evidence suggests exceptions to our laws, we are not satisfied with accepting the exceptions. The goal of a global unified system of knowledge drives us to formulate new, maximally uni-

fied, and thus relatively simple, schemes of classification and corresponding sets of laws. In this way, we deepen our understanding of nature (Singer 1902, 82–90).

Among the speculative philosophers who further develop the discussion of research programs after Singer, we find the de Lagunas.⁴ One of the main ways in which they go beyond positions such as Singer's is in their variant of holism. We have seen that they had a holistic view of conceptual meaning. They also had a holistic view of hypothesis evaluation. On the de Lagunas' view,

The validity of a universal principle is not a matter of its own individual adequacy as a description of reality; nor, again, is its validity relative to the whole existing body of human knowledge (if, indeed, we can speak of such a thing). It may correctly enough be said that the validity of such a principle depends upon its place in the developing structure of our knowledge, if we remember that this place is not definitely determined, but is exceedingly variable. A law is not judged as true because it marks the limit of human knowledge and because we are not able to correct any given formulation of it. Its truth is always a matter of context. It is valid if we find a certain harmony between the character and degree of its abstractness and the character and definiteness of the conclusions in view of which it is asserted. (de Laguna and de Laguna 1910, 153)

The de Lagunas, then, agree with Singer that laws are not abandoned merely because they confront counterexamples but rather partly due to relevant background assumptions including the goal of theoretical cohesion. To this extent, they all agree that hypothesis evaluation is holistic. However, the evaluation of laws in science is to some extent a local affair, one tied to the specific theoretical and empirical state of knowledge within given disciplines. Counterexamples to laws will be tolerated by scientists within a given context as long as the laws are sufficiently well articulated in order to manage the complexity of available empirical data. It is only when available theoretical systems of laws are too crude to do this that alternatives will be sought. Here the de Lagunas disagree with Singer or, at least, note a serious lacuna in his treatment of hypothesis evaluation. He fails to recognize that the goal of a unified system of all science is too distant to play a central role in explaining hypothesis choice. In addition, the de Lagunas present their development of the theory of judgment as one that takes further the evolutionary account of judgment. A proper evolutionary account, on their view, needs to recognize that judgment itself is not just functional but also that its function evolves and thus that, for example, the standards of judgment will change in different contexts (de Laguna and de Laguna 1910, 135–148). Singer does not recognize such evolution.

⁴ Another important starting point for the de Lagunas' work on research programs is that of Creighton (Katzav 2022a).

Like Singer, the de Lagunas use case studies to support their claims about the treatment of laws in science (de Laguna and de Laguna 1910, 149–161). I, however, want to emphasize a further distinctive feature of their work on research programs. While Singer recognizes but says little about the psychological and sociological factors that play a role in the evolution of research programs (Singer 1902, 80), the de Lagunas say quite a bit on this topic. Grace says more in “Cultural Relativism and Science” (1942). Theodore says more in his *The Factors of Social Evolution* (1926).⁵ Here is one particularly striking summary of his views from that book:

Often enough, when our principles are contradicted, we simply deny the accuracy of the new observation or the veracity of the report. More often, perhaps, we ascribe the apparent contradiction to the operation of unknown disturbing causes. Nothing is more familiar to us than that a rule should have exceptions. The proverb even has it that ‘the exception proves the rule.’

But if the exceptions become frequent, and especially if they begin to exhibit a certain regularity, the whole complexion of the matter changes, for the principle itself becomes charged with the fault. It may not be at once given up—in fact, it is extremely unlikely that it should; for the extensive correlation of detail that it formerly accomplished, it still accomplishes, and there is nothing as yet to take its place. But a condition of instability is produced. Attempts are continually being made to correct the principle in question so as to accommodate the troublesome exceptions; but too often the new formulae fail to cover much that was satisfactorily accounted for by the old. A division between conservative and radical parties occurs, just as in the case of a moral or political issue. And, despite all differences of detail, the final settlement is reached in fundamentally the same fashion. Comparative shortcomings must be appreciated, not counted; and the importance ascribed to each is, in the last resort, determined by tastes and prejudices. (Th. de Laguna 1926, 94–95)

This quote includes within it much that became so familiar from Kuhnian philosophy of science, including, in its statement that the new formulae fail to cover everything that the old ones did, a recognition of what came to be called “Kuhn-loss,” and, at the end of the quote, an account of the socio-political factors operative in scientific revolutions.

In closing this subsection, let me emphasize that the discussions of research programs by Singer and the de Lagunas are examples from a broader discussion during the early decades of the twentieth century. I could equally have used authors other than the three I have chosen to show that these decades included a rich discussion of this topic. I could have followed Cohen’s 1931 discussion in his

⁵ I note that this book’s discussion of whether there are innate, race-related differences in intelligence (1926, 99–133) uses problematic language that reflects the prejudices of the time, although the discussion’s aim appears to be to reject the case for such differences.

Reason and Nature (1931, 80–146), Smart's discussion from the same year (1931, 34–45), or other discussions.

3.3.3 Scientific Explanation and Other Topics in Logical Analysis

The already presented examinations of methodology in psychology and of scientific research programs is substantially empirical. Thus, Singer's statement of a verificationist criterion of significance is an empirical claim about science. At least part of the time, he seems to be reporting on scientists' conception of meaningfulness. So too, his case studies provide empirical support for his claim about the role of classification systems in science. The de Lagunas' discussion of research programs in science is clearly partly empirical, concerning which standards of acceptance are actually used in science. In this section, I will present a speculative research program in which the focus is more on logical analysis conceived of as the provision of logical or conceptual truths. The program concerns the nature of scientific explanation and, once again, at least partly goes back to Singer's work.

Singer's paper "On Mechanical Explanation" (1904) is concerned with whether there is ultimately one kind of scientific explanation. More specifically, Singer's question is whether the mechanical ideal can be realized, that is, whether ultimately all explanation in the natural sciences is mechanical explanation. His response is a partial one and is that, if all scientific knowledge in the natural sciences can be conceptually reduced to that of mechanics, all scientific explanation in the natural sciences will be mechanical explanation. He also argues against what he takes to be some of the strongest objections to the possibility of such a reduction.

Singer offers the following account of what it is for one science to be reduced to another:

any science x, dimensions abcd, is reducible to any science y, dimensions abc, when it may be shown in any manner that the term d is expressible as a function of abc. (Singer 1904, 271)

By "dimensions of a science," Singer means the kinds of independent measurements that need to be made for its formulae to yield definite predictions. In mechanics, according to Singer, these dimensions are measurements of mass, length, and time. It follows, since he thinks that reduction to mechanics will vindicate the mechanical ideal, that he thinks that mechanical explanations are functional explanations in terms of mass, length and time. And the mechanical ideal of explanation will be achieved when the apparently extra dimensions of the natural sciences can be expressed as functions of mass, length and time, so that these last

three are the only real, independently measured quantities, and thus the only dimensions of all the natural sciences (Singer 1904, 267).

Keeping in mind Singer's verificationist criterion of significance suggests further that he thinks that, when a dimension, d , is expressed as a function of others, our judgments about d are shown to be the same as our judgments about the others. So, Singer's verificationist criterion of significance goes along with a corresponding verificationist criterion of sameness of meaning. For, by hypothesis, no judgment about d can have any measurable implications that are not already captured by judgments that are about the other dimensions. Thus, any judgment putatively expressing something distinct about d from the judgments of the reducing science would have no empirical implications and thus be meaningless. It seems that reduction of a science involving d to one not involving d means that our judgments about d say nothing more than what is expressed by judgments about other quantities. This is why, for Singer, successful reduction of the natural sciences other than mechanics to mechanics is conceptual and will vindicate the mechanical ideal.

Singer goes on to argue that the facts of biology, including teleological ones, cannot serve to demonstrate the unrealizability of the mechanical ideal. He also expresses the (unargued) view that, if the facts of biology cannot thus be used, no facts from another science can (Singer 1904, 282–283). I want to focus, however, on the (partial) support he offers for the mechanical ideal. This ideal came under repeated attack by later philosophers of science within his tradition.

The de Lagunas' meaning holism challenges Singer's attempt to vindicate the mechanical ideal. Their holism permits, because it tells us that logical structure has a role in fixing concept meaning, multiple theoretical systems with the same predictions but without the same content. So, their view would be that reducing a science to mechanics, in Singer's sense of reduction, does not automatically imply that the two have the same content. If anything, doing so threatens to change the meanings of the involved scientific terms by changing their logical interrelationships. A reduced science may not be saying the same thing as it said before reduction and thus may not be able to explain what it used to explain.

Indeed, in directly critiquing the mechanical ideal, (Grace) de Laguna argues that neither the concepts of physics nor those of psychology can begin to express what all the sciences express. One of her arguments involves observing that the principles of classification of physical science do not even allow identifying social kinds, such as election victories or bank collapses. She notes that the different sets of physically described events—redistributions of mass and energy—that embody the class of electoral victories by the USA's democratic party cannot be classified by physics as events of a single kind. So, one cannot identify, never mind describe or explain, democratic victories using physical terms (G. de Laguna 1917a). Similar-

ly, psychological kinds, such as resembling classes of experiences, do not generally map onto physiological, never mind physical, kinds (G. de Laguna 1918b). Conversely, physical kinds do not generally map onto corresponding psychological kinds (G. de Laguna 1917b).

De Laguna, however, does not explicitly offer a theory of scientific explanation. Smart does and also, like de Laguna, rejects the idea that there is only one kind of explanation in science.⁶ Smart's discussion of explanation in science starts with an argument for thinking that, contrary to positivism, scientific judgment in the mathematical sciences is explanatory (Smart 1931, ch. 2). This, in turn, leads him to ask what the nature of explanation in these sciences is and eventually to argue that, and consider how, explanation in the mathematical sciences differs from explanation in other sciences.

Smart articulates his view of scientific explanation in the mathematical sciences using a conceptual distinction between descriptive and explanatory laws:

We must distinguish between two types of law, the empirical or descriptive, and the theoretical or explanatory. Empirical laws do merely describe the 'how' of things; but the real problem of science is to discover some theoretical basis for things. Thus why comes to mean ... an explanation in terms of natural conditions, or ... in terms of systematic organization. (Smart 1931, 60)

On this analysis, what makes an explanatory law explanatory (in the mathematical sciences) is not just its generality but also its distinctive inferential role. Such a law allows us to derive empirical laws or generalizations and assign them to natural classes. Explanatory laws thus show empirical laws to be necessary and, in doing so, explain them. For example, Newton's law of gravitation determines whether a planet's path will be elliptical, hyperbolic or parabolic just given a planet's initial velocity, without an initial direction. So, the law of gravitation delimits three natural classes of empirical laws and thus allows us to see the laws that belong to each of these classes as necessary in respect of the type of conic section they follow. This is why the derivation of these empirical laws is explanatory (Smart 1931, 55–56).

So far, Smart has in effect pointed to a lacuna in Singer's discussion. Singer seems to assume that any general principle, or in his terms any function, is explanatory and thus neglects the important distinction between explanatory and empirical laws. Disagreement proper arises when Smart considers explanation in biolo-

⁶ Smart, unlike de Laguna, never mentions Singer. This should be no surprise. Smart, following citation customs in his milieu, offers limited citations and does not tend to cite those in his own research community. The small size of the American philosophy of science community, however, means that Smart will have known Singer's work.

gy. Smart rejects the view that “ultimately the biological sciences must seek the same type of answers to their problems, as satisfy the physicists” (Smart 1931, 61). While mechanical explanation should be pursued as far as possible in biological science, we find that biological science’s primary aim is still the classification of individuals. Moreover, a proper understanding of how this affects biological explanatory principles should convince us that biology should not, even in the long run, aim solely at explanatory laws of the kind found in physics. Physics views the individual merely as the exemplification of laws and thus abstracts from concrete reality in a way that diminishes the individuality of the phenomena with which it is concerned. Biological science is concerned with classificatory judgments that relate individuals to classes. And individuals are understood to belong to their classes as a function of the totality of their organization, so that biological sciences are still inevitably concerned with individuals as wholes (Smart 1931, 61–63; 159–162). Regarding the individual as a whole, explanation “means the tracing of an evolutionary process of chance, and possible reference to purpose” (Smart 1931, 63).

I have used the work of Singer, the de Lagunas, and Smart to illustrate some of the positions being developed in the logical analysis of scientific explanation within American speculative philosophy of science. The work of other figures, such as that of Cohen (1931) or A. Cornelius Benjamin (1927; 1936), could equally illustrate views of scientific explanation. So too, while I have focused on the logical analysis of scientific explanation, I could equally have focused on logical analysis relating to measurement, probability, confirmation, the problem of induction, the observation-theory distinction, realism about theoretical entities, idealization, or causation. Each of Cohen (1931), Smart (1931) and Swabey (1930) offer us relevant discussions of all these topics, except for Smart who, as far as I am aware, has no discussion of measurement or of probability.

3.4 Speculative Philosophy of Science and Logical Positivism: Initial Comparison

3.4.1 A Comparison of the Scope and Development of Speculative Philosophy of Science with That of Logical Positivism

Katzav and Vaesen (2022) observe that early twentieth-century speculative philosophy of science provided logical analyses of general methodological scientific concepts. Such analyses only start to appear in logical positivism in the 1930s and, in

key cases, only appear with its demise and the development logical empiricism in the 1940s and 1950s. The project which logical positivism brought with it to America, and which was first clearly articulated in 1928 by Rudolf Carnap (1967), was centered on establishing the unity of science via various verificationist, reductivist projects and on the verificationist elimination of metaphysics (e.g., Blumberg and Feigl 1931; Carnap 1934; Reichenbach 1938). Discussions of induction and probability only begin to emerge within logical positivism in the 1930s. And analyses of confirmation and of scientific explanation that are influenced by logical positivism only emerge in the 1940s,⁷ with the work of Carl Gustav Hempel, the associated demise of logical positivism, and the development of logical empiricism (Giere 1996).⁸ What the previous sections, especially 3.3.3, suggest that we add to this story is that the speculative discussions of the logic of explanation not only preceded logical positivism but were part of a long-standing research program, one that started as early as the turn of the twentieth century and that exhibited substantial theoretical development. This program in the analysis of explanation was, further, part of a broader collection of programs of analysis, one that arguably encompassed all the main projects of analysis of later philosophy of science.

Similarly, while logical positivists were aware of the difficulty of deciding when to accept falsifying evidence (e.g., Neurath 1931; Carnap 1937, 317), they do not themselves develop a substantive research program investigating these difficulties. It is only in the wake of Karl Popper's 1959 publication of the English translation of his 1934 book *Logik der Forschung* and the publication of Thomas Kuhn's *The Structure of Scientific Revolutions* in 1962 that research into the methodology of research programs enters what became analytic philosophy of science along-

7 Carnap does discuss the problem of confirmation in the 1930s. However, he then thinks this problem is the same as that of determining the verification conditions of propositions and is not concerned with analyzing the concept of confirmation (Carnap 1936, 420; Giere 1996, 340).

8 I here adopt a use of "logical positivism" and "logical empiricism" that roughly follows the periods of their introduction into widespread use (Feuer 1941; Salmon 1999). On my use, "logical positivism" refers to a body of principles that was brought to America by immigrant philosophers of science and played a central role in the subsequent development of philosophy of science in that country; the principles are those attributed to the logical positivists in this and the next section. "Logical empiricism" refers to the dominant philosophy of science that developed in the 1940s and 1950s when the principles of logical positivism fell out of favor. While it is sometimes assumed that "logical positivism" and "logical empiricism" are coextensive and substantially broader in their extension (e.g., Creath 2023), it is important for my comparative purposes to distinguish between what influential immigrant philosophers of science brought with them to America in the 1930s, what was proposed by some immigrants but never had an impact there (see footnote 9), and what was found there in later decades.

side logical empiricism.⁹ Yet, as Katzav and Vaesen (2022) observe, and as we saw in section 3.3.2, work on the methodology of research programs already had a place within the earlier speculative tradition. What section 3.3.2 allows us to add here is that speculative work on research programs went back at least to the start of the twentieth century and exhibits substantial theoretical and methodological development. My discussion of speculative work on research programs also indicates that speculative logicians of science tended to gain philosophical insights from case studies drawn from the history of science, unlike 1930s logical positivists.¹⁰

Section 3.3.1 indicates further that even verificationism and its implications, supposedly key, distinctive foci of logical positivism, were integral parts of speculative philosophy of science and were developed within one of its long-standing research programs. To be sure, many have noted the affinity between classical pragmatism's criteria of significance and verificationist ones (e.g., Misak 2005; Uebel 2015). But the pragmatist criteria tie the meaningfulness of ideas to their practical consequences, that is, to how accepting an idea would or should in general affect behavior or, even more broadly, affect us in any way (Legg and Hookway 2021). Singer's principle, and the de Lagunas' dissent from it, focus specifically on empirical testability just as do logical positivist criteria of significance.

We thus find that the most prominent aspects of analytic philosophy of science in America during the second half of the twentieth century—logical positivist verificationism and worries about it, logical empiricist analysis of general concepts in science, and discussions of research programs—were developed research programs within an earlier, largely forgotten speculative tradition. But we ought to remind ourselves, there was much more to this tradition. It included extensive dis-

9 In 1934, Popper recognizes the fallibility of falsification and incorporates it into his view of diachronic scientific development (1959). He thus makes a start at developing a methodology of research programs. Despite appearing to mistakenly complain that Popper does not recognize the fallibility of falsification, Neurath not only does not go beyond pointing out that when to revise falsification claims is not straightforward but appears to suggest that there is no relevant philosophical issue that needs to be addressed (Neurath 1931; 1935).

10 My claim is not that the logical positivists were uninterested in the history of science but only that they did not tend to use it to evaluate philosophical claims. Edgar Zilsel, to be sure, has been taken to be a logical positivist and arguably did endorse the idea of a case-study based argument for the unity of science. However, whether for practical reasons or because he came to reject this idea, Zilsel never actually used his work in the history of science to support the thesis of the unity of science (Raven and Krohn 2003, xlix and li–liii). Moreover, he neither accepted the logical positivist view that philosophy is logical analysis nor had an impact in America (Raven and Krohn 2000, xix and xliii). While some of Neurath's work from the 1910s can also be taken to include case-study based philosophy of science, it is unclear whether it does (Zemplén 2019, 219). In his logical positivist phase, he at most permits that philosophy has a role in conceptual analysis (Neurath 1931).

cussion of idealization in science, of how idealization affected hypothesis selection, of speculative visions of reality, and of work on the logic of individual special sciences. American logical positivists were apparently proposing a substantial narrowing down and winding back of the philosophy of science.¹¹

3.4.2 Logical Positivism and Some Problem Situations in American Philosophy of Science

How did the arrival of logical positivism in America affect problem situations in American philosophy of science, that is, affect its range of questions, as well as the range and viability of available answers to these questions? To begin answering this question, we need more detail about logical positivism in its American context. My further presentation of it will center on five theses which were shared by key logical positivists—Rudolf Carnap, Herbert Feigl, and Hans Reichenbach—during at least the 1930s, when their work came to be known in America. To begin with, Carnap (1934; 1937), Feigl (Blumberg and Feigl 1931; Feigl 1943) and Reichenbach (1938) embraced verificationist criteria of significance and of sameness of meaning. In *The Unity of Science*, which originally appeared in 1932 and was translated into English in 1934, Carnap divided meaningful statements into protocol statements, i.e., observation statements, and non-protocol statements. He was undecided about whether protocol statements were best understood as requiring no verification or as directly verifiable by experience. However, he thought that a meaningful non-protocol statement must be (to some degree) verifiable by the protocol statements it entails (Carnap 1934, 42–50). Further, non-protocol statements which implied the same protocol statements were supposed to have the same meaning (Carnap 1934, 51). Carnap presents similar commitments in *The Logical Syntax of Language*, published in 1934, though there his focus is on distinguishing meaningful and meaningless sentences and thus on syntactical categories (Carnap 1937, 319–320). Feigl's views were, during this period, akin to those of Carnap (Feigl 1943, 392–393). Reichenbach, on the other hand, endorses a probability theory of meaning. On this theory, “a proposition has meaning if it is possible to determine a weight, i.e., a degree of probability, for the proposition,” where weights are determined by observation. Further, two statements have the same meaning “if they obtain the same weight, or degree of probability, by every possible observation” (Reichenbach 1938, 54).

¹¹ Whether logical positivists' views of formal logic might have been more novel is not something I take a stand on here.

Alongside criteria of significance and of sameness of meaning, Carnap, Feigl and Reichenbach shared reductivism about science, the view that the meanings of all scientific statements could be reduced to those of some privileged set of statements. At the outset of his logical positivism, Carnap aimed to use explicit definitions to fully translate all the statements of all the sciences into protocol statements about similarity classes of sense data, though he also proposed the possibility of an alternative translation scheme into protocol statements about observable physical objects. He quickly came to prefer a reduction base of physical statements and to propose a less ambitious form of reductivism. On this less ambitious proposal, non-protocol statements have their meaning partially specified by entailing, with the help of stated correspondences between what their non-observational terms describe and what observational ones describe, protocol sentences (Carnap 1967, preface). The development of Feigl's views of reduction (Feigl 1943) are similar to Carnap's. Reichenbach, however, thought that a statement, p , is reduced to a set of statements, S , if p is coordinated with S , either by definitions or empirically, so that p and S have the same meaning according to the probability theory of meaning (Reichenbach 1938, 94–95, 216–217). Further, according to Reichenbach, all statements, including, e.g., those of sociology, are reducible to observation statements about physical objects (Reichenbach 1938, 211–217).

The introduction of logical positivist verificationism into the USA could not itself have amounted to philosophical progress. As we have seen, verificationist criteria of significance and of sameness of meaning already had an, at least, decades long history there. Moreover, verificationism was still on the scene in the period 1920–1940. The papers by Singer that I have been discussing were all republished in his 1924 book *Mind as Behavior and Studies in Empirical Idealism*, and his work is extensively discussed by his students and other prominent American philosophers (Clarke and Nahm 1942). These discussions could hardly have gone unnoticed in the small community of American philosophers of science.

Indeed, the reductivist forms of verificationism positivists brought with them would very plausibly seem, from the American perspective, at best to require substantial work. There were, to begin with, the challenges that the de Lagunas' meaning holism suggest for verificationist criteria of significance and sameness of meaning. Their holism also challenges positivist reductivism. It is not just that, as pointed out in discussing Singer's reductivism, the idea of reducing various sciences to some fundamental one is threatened but also the idea of reducing all the statements of a science to its observation statements. If the de Lagunas are correct, the concepts of a science have their meaning partly by virtue of their inferential role in the system of the science's concepts. This applies equally to the concepts used in observation statements, so that the positivist goal of using them as primitives in terms of which all others are to be analyzed appears to be blocked.

(Grace) de Laguna, further, explicitly challenges reductivism about science, partly by noting the absence of straightforward mappings of sociological kinds onto physical kinds. If, as she suggests, this implies that these sciences must talk about different things, even a partial translation of sociology into physics must fail. For similar reasons, even a partial translation of physics into psychology must fail. Such challenges came later to be recognized as key challenges to reductivism about science (Katzav 2022b). Smart supports de Laguna's perspective with his detailed examination of the types of explanation provided in mechanics and biology, arguing that the latter rightly offers a kind of explanation that rests on concepts that cannot be captured in physical terms.

Two final logical positivist theses which I will discuss are the restriction of philosophy to logical analysis and the rejection of metaphysics (Blumberg and Feigl 1931; Carnap 1934, 1937; Feigl 1943; Reichenbach 1938). According to the first of these, philosophy is epistemology and is solely concerned with the logical analysis of the structure of scientific knowledge. Again, Carnap's views are illustrative. He writes that "the activity of philosophy consists ... in clarifying the notions and statements of science" (Carnap 1934, 33). He adds that everything other than logical analysis that has been a traditional part of philosophy is a "confusion of non-scientific pseudo-problems" (Carnap 1934, 23). To be sure, Carnap and other logical positivists permit the empirical investigation of knowledge, but they are clear that such investigation cannot answer the questions of philosophy. Carnap tells us that the empirical investigation of knowledge is merely an investigation of the origin of knowledge by psychology; it is not an investigation of the nature of knowledge (Carnap 1934, 22–24). Finally, there is, as part of the rejection of traditional philosophy, the rejection of all metaphysics. Not even logical analysis is allowed to contribute to metaphysics. More specifically, metaphysics is meaningless or, at least, almost entirely meaningless; exceptions to the meaninglessness of metaphysics allowed by Feigl (1943, 385) comprised metaphysical statements that are "disreputable" inductions from available observations.¹²

Speculative philosophers of science recognized logical analysis as part of epistemology but also included empirical research within it. As we have seen, they informed their work on the methodology of research programs by empirical considerations and offered explicitly empirical hypotheses about such programs. Now, while logical positivists thought that empirical considerations could only contribute to the causal understanding of knowledge and not to epistemology, specula-

¹² One can, despite the logical positivists' claims, view their goal of unifying science via reductivist projects as a metaphysical one. In what follows, however, I will follow their lead and use "metaphysics" to refer to work that aims to make substantive claims about reality rather than merely to provide logical analyses.

tive philosophers of science were well motivated in thinking that empirical considerations were relevant to epistemology. For example, according to the de Lagunas, types of judgments are types of evolved, functional states. As a result, such types tend to have been selected for specific functions and can thus be understood by an examination of how they were selected and of the purposes for which they were selected. An understanding of their functions should bring with it an understanding of their success conditions and thus of how they are to be evaluated (de Laguna and de Laguna 1910; Katzav 2022a). As far as I can tell, logical positivists did not criticize these positions.

The verificationist criterion of significance is the explicit motivation logical positivists offered for their rejection of metaphysics (e.g., Carnap 1937, 278). Those who had already examined and rejected verificationist criteria of significance would rightly not have been impressed by appeals to them. (Grace) de Laguna, for one, persisted in promoting speculative metaphysics (Katzav 2022b). But even someone like Singer would have seen no reason to conclude that metaphysics is meaningless. Singer's deployment of verificationism is, we have seen, part of an investigation that aims to determine to what extent scientific evidence leaves room for choice in how scientists represent nature. It is as a result of this investigation that he concludes that choice of classification system reflects scientists' freedom and thus points to the hypothesis that scientists mold nature through their choice of classification systems. Thus, for Singer, idealist metaphysics is the result of a verificationist exploration of science. To be sure, some variants of the verificationist criterion of significance could be deployed against Singerian metaphysics. His idealism cannot plausibly be said to have been strongly confirmed by empirical evidence, so that it would be rejected as meaningless by a criterion according to which only fully or strongly verified hypotheses are meaningful. But these variants of verificationism were quickly recognized to be unacceptable, even by logical positivists.

3.5 Conclusion

When logical positivism arrived in America, it was in effect proposing a substantial narrowing down of philosophy of science, one encompassing not only the rejection of metaphysics-related philosophy of science and empirical philosophy of science but also of the logical analysis of science. What was proposed for elimination, further, was part of an established tradition of work with decades of development behind it. This proposal for philosophy of science went along with the proposal of the adoption of a logical positivist approach to philosophy. While the standard historiography of philosophy of science sees this approach as largely unopposed by

local philosophy of science, if only because it supposedly did not exist, my work suggests that the local tradition had its own established, evolving speculative approach. My work also suggests that it is not obvious how the speculative approach was challenged by what the logical positivists brought with them. If anything, it seems that the logical positivists' proposals were, when viewed from the American perspective, poorly argued, and based on an already problematized set of assumptions, including verificationism, reductionism, and methodological misgivings about empirically informed philosophy.

I have, to be sure, only provided the very beginnings of a comparative examination of the local and immigrant approaches to philosophy of science. I have not looked at much detail or substantially evaluated arguments. Nor have I looked at all the challenges posed by speculative philosophy to logical positivism, or at all the ways in which these schools might be contrasted. For example, there were philosophers of science, such as Swabey, who aimed to provide synthetic a priori justification of key inferential and ontological scientific principles (Swabey 1931). Their arguments were developed in response to critiques of synthetic a priori knowledge and thus provided a ready-made challenge to the empiricist immigrants. Similarly, one can compare work by speculative philosophers on the special sciences with imported work. For example, one can contrast the metaphysics-driven philosophy of physics of Filmer S. C. Northrop and Andrew Ushenko with the philosophy of physics of Reichenbach. There remains much work to be done before we understand what happened to the philosophy of science with the arrival of logical positivism in America, never mind how it affected the subsequent development of the field.

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4 Columbia Naturalism and the Analytic Turn: Eclipse or Synthesis?

Abstract: Historical reconstructions of the effects of the intellectual migration are typically informed by one of two conflicting narratives. Some scholars argue that the logical positivists contributed to the demise of distinctly American schools of thought. Others reject this “eclipse view” and argue that analytic philosophy can best be characterized as a synthesis of American and positivist views. This paper studies the fate of one of the most influential schools of U.S. philosophy—Columbia naturalism—and argues that both narratives are part of a larger story. First, I reconstruct the rise of the school, focusing on its naturalist analyses of science, morality, and religion. Next, I trace some of the naturalists’ contacts with German philosophers and show that they encountered a bifurcation between historical and scientific philosophy in their discussions. I argue that a similar distinction gradually infected debates between naturalists, eventually resulting in a split within the Columbia school itself. The historically-oriented naturalists were overshadowed by the analytic movement, while the science-minded naturalists incorporated the views of the émigrés, thereby developing the tradition in new directions.

4.1 Introduction

A philosopher’s centennial is usually an occasion for reflection and commemoration. But when Columbia University organized the John Dewey Centenary in 1959, the participants had little to celebrate. Dewey’s school had played a central role in the development of American thought but most participants realized that Columbia’s department of philosophy had lost “the enviable position it once held.”¹ Though most Columbia philosophers were direct students of Dewey, the school had become badly split not even seven years after his death. Speakers at the event tried to keep the ceremony “publicly solemn”—commemorating Dewey’s

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1 “Dewey Centenary Commemorated,” *Columbia Daily Spectator*, October 21, 1959; “Report of the Ad Hoc Committee on the Promotion of George Kline,” ca. May 1959, Office of the Provost and Vice President for Academic Affairs records, Series I. Office Files, 1939–2006, Columbia University Archives (hereafter, CUA-OVPR), Box 9, Folder 6.

“philosophy of growth” and “democratic faith in human nature”—but privately complained about the “backbiting and conniving” behind the scenes.²

The causes of the conflict were many. An internal report commissioned by Columbia president Grayson Kirk describes a number of tensions and disagreements within the school. In addition to several “long-standing” and “fairly deep-seated personality conflicts,” the professors disagreed about the department’s hiring policy. Most philosophers preferred to appoint Columbia students trained in the naturalist tradition but a small group of professors, led by Ernest Nagel, believed this policy had led to intellectual inbreeding and preferred to “invite outsiders who ... represent philosophical positions other than [our] own.”³ The most important source of conflict, however, was the future of philosophy itself. Most Columbia philosophers worried about the growing popularity of logical positivism and affiliated schools of analytic philosophy. They were convinced that philosophical problems are human problems and that it is misleading to address those questions in a strict analytic vacuum, divorced from any cultural-historical context. Historical instead of logical analysis ought to be the “very essence of ... philosophy” since philosophical ideas emerge in specific communities in specific historical periods (Randall 1939, 83). Irwin Edman disqualified the positivists’ “formalisms” as “barren” and “divorced from a subject-matter” and John Herman Randall Jr. said that there was no philosophical position to which he was “more opposed than the one known as ‘analysis’.”⁴ Their opponents, however, sympathized with the analytic approach and felt that the department overemphasized “historical philosophy.”⁵ They believed that the “vigorous and technically precise” methods of the logical empiricists could be “salutary stimuli” to the Columbia school (Nagel 1956, xii) and urged the department to hire more philosophers with a background in logic and philosophy of science.

This paper investigates the split within Columbia’s department of philosophy through the lens of the intellectual migration. Philosophical reconstructions of the

2 “Dewey Centenary Commemorated”, *Columbia Daily Spectator*, October 21, 1959; Sidney Hook to Ernest Nagel, October 12, 1959, Ernest Nagel Papers, Archives of Scientific Philosophy, University of Pittsburgh (hereafter, ENP-ASP), Box 3, Folder 34; Hook to Nagel, October 20, 1959, ENP-ASP, Box 3, Folder 34.

3 “Report of the Committee on the Future Planning of the Philosophy Department,” May 25, 1960, Office of the President records, Series I: Central Files, 1895–1971, Columbia University Archives (hereafter, CUA-OPR), Box 379, Folder 20; “Report of the Ad Hoc Committee on the Promotion of George Kline,” OVPR, Box 9, Folder 6.

4 Edman (1941, 562); Randall to Hook, October 25, 1951, Sidney Hook Papers, Hoover Institution Library & Archives, Stanford University (hereafter, SHP), 22.09. See also Jewett (2011).

5 “Report of the Committee on the Future Planning of the Philosophy Department,” CUA-OPR, Box 379, Folder 20.

effects of the migration are typically shaped by one of two conflicting narratives. Some historians argue that European refugees, in particular the logical positivists, contributed to the demise of distinctly American schools of thought. They believe that U.S. pragmatists, realists, idealists, and naturalists developed a unique and refined philosophical culture that was simply eclipsed by the overly technical, analytic approach of the exiled empiricists in the 1930s and 1940s (Thayer 1968, 559). Others have argued that this “eclipse narrative is demonstrably false” and emphasize the *continuities* between European and American schools of thought. Prominent postwar philosophers such as Hilary Putnam and W. V. Quine, they argue, were influenced by *both* pragmatism and logical empiricism and used this dual heritage to create a fruitful new approach to philosophy (Talisie 2007, 133). This paper studies the impact of the intellectual migration on the Columbia naturalists and argues that both narratives are part of a larger story. First, I reconstruct the rise of the Columbia school, focusing on its analyses of science, morality, and religion as well as its contributions to the history of ideas (sections 4.2–4.3). Next, I trace some of the naturalists’ contacts with German philosophers and show that they encountered a strong bifurcation between historical and scientific philosophy in their discussions (sections 4.4–4.5). Finally, I argue that a similar distinction gradually infected debates between naturalists, eventually resulting in a split within Columbia’s department itself (sections 4.6–4.7). The historically-oriented naturalists, I argue, were overshadowed by the analytic movement, whereas the science-minded naturalists were able to incorporate the views of the émigrés, thereby developing the tradition in new directions.

4.2 Woodbridge and Dewey

The story of the Columbia naturalists starts in 1902, when the department appointed F. J. E. Woodbridge to replace Nicholas Murray Butler, who had just been elected president of the university. Butler had built a department which aimed to replace “dogmatic philosophy” with “historical, critical, and interpretative teaching” and relate its study “to the results of modern scientific research.”⁶ Woodbridge perfectly fit the profile because he combined a science-minded philosophy with a historical approach. He had studied with the German psychologist Hermann Ebbinghaus and was known for defending a realist metaphysics at a time when U.S. philosophy was dominated by idealism. He believed that modern philosophy was built on a

6 “The Department of Philosophy at Columbia,” *Columbia University Quarterly*, December 1901, 143–144. Butler also identified a third aim, viz. to apply “philosophy ... to the subject of education.”

misleading dichotomy between subject and object, or man and nature, and was convinced that recent scientific results challenged such dualist modes of thinking. Yet he was also well-versed in the history of philosophy and took much inspiration from Aristotle, whom he interpreted as a “sober naturalist” who could help twentieth-century philosophers “transcend the assumptions of modern philosophy” (Randall 1957, 117, 128). In 1904, Woodbridge and his colleague J. M. Cattell created *The Journal of Philosophy, Psychology, and Scientific Methods* (later *Journal of Philosophy*). The periodical was modeled after German science journals and became an important venue for publications of the Columbia school during its heydays in 1930s.⁷

Dewey followed Woodbridge to Columbia shortly after the publication of the first issue of the journal. The philosopher and educational reformer had had a conflict at the University of Chicago and Cattell was quick to see that he would be a major asset to the department. “Scarcely anything ... so favorable to our work in philosophy, psychology and education” could have happened, Cattell wrote in a letter to Butler, correctly predicting that Dewey’s arrival would have an “appreciable effect on the influence and prestige of the university.”⁸ Dewey, unlike Woodbridge, had started out as an idealist but had gradually “drifted away from Hegelianism,” replacing his speculative approach with a “biological” one in the 1890s.⁹ In his seminal address “The Influence of Darwinism on Philosophy,” Dewey explained how the evolutionary framework had transformed his perspective on “the logic of knowledge” (Dewey 1909, 2). Whereas traditional philosophy rests on a “logic of the changeless, the final, and the transcendent,” the Darwinian logic had led him to forswear inquiry into wholesale essences and to replace it with questions of how particular changes serve concrete purposes (Dewey 1909, 7). Just as a species is not a fixed and final kind but a constantly adapting entity responding to environmental changes and contingent selective pressures, the philosopher is not in the business of answering divine, immutable questions but responding to specific queries raised by our evolving society and body of scientific knowledge.

Together, Woodbridge and Dewey built the Columbia school, though they rarely identified as “naturalists” at first. In the early 1900s, philosophers still associated the label with crude reductionist theories, defining naturalism as the view that “mental and moral processes may be reduced to the terms and categories of the natural sciences” (Dewey 1901, 139–140). Yet much of the opposition to the

7 “The Journal of Philosophy, Psychology and Scientific Methods,” *Journal of Philosophy Correspondence*, 1892–1943 (hereafter, JPC), Box 1, Folder “James.”

8 Cattell to Butler, April 26, 1904, CUA-OPR, Box 320, Folder 7.

9 Dewey (1930, 20); Dewey to Robet, May 2, 1911, (*Correspondence*, no. 01991).

label evaporated after the publication of George Santayana's *The Life of Reason* (1905–06), which developed a naturalist but non-reductive theory about man's place in the universe. Dewey first described his philosophy as an “empirical naturalism” in the second edition of *Experience and Nature* (Dewey 1925, 1) and Woodbridge called for “a thoroughgoing naturalism” in an address titled “The Nature of Man” (1932).¹⁰ While traditional religions and modern philosophers try to *separate* man and nature by appealing to the transcendent or the supernatural, Woodbridge maintained, we should not believe that we are an “exception in the natural history of the world.” In the face of our best scientific discoveries, “it has become intellectually impossible to believe that man is not a natural being in the same sense as animals, plants, and atoms” (Woodbridge 1932, 86, 89). It is the naturalist's job to investigate how humanity's evolving ideas and values both shape and are shaped by their cultural, social, and historical environments. In doing so, philosophers have “no private store of knowledge or methods for attaining truth” but must utilize “the best available knowledge of [their] time and place,” such that their “road is the subject-matter of natural existence as science discovers and depicts it” (Dewey 1925, 408).

4.3 The Columbia School

Through Woodbridge and Dewey's influence, the Columbia school gradually developed into something more than just a loose collection of philosophers. By 1931, the department employed a substantial number of professors and instructors—Herbert W. Schneider, Edman, Randall, Horace L. Friess, Richard McKeon, James Gutmann, Corliss Lamont, and Nagel—who identified as naturalists and were direct students of the two.¹¹ At this point, Columbia naturalism had become more than a philosophical view about man's place in the universe. It was at once an intellectual stance, a worldview, and an emancipatory movement. The second generation typically classified naturalism as an intellectual “temper” or as a disposition to understand every branch of human behavior—scientific inquiry, moral deliberation, social interaction, artistic expression, and religious experience—as a natural phenomenon (Nagel 1931, ii; Edman 1935; Randall 1944, 355). Dewey's philosophical

¹⁰ Ironically, it may have been Santayana's accusation that Dewey's position constituted only a “half-hearted and short-winded” naturalism that led the Columbia philosopher to embrace the label. See Santayana (1925, 680) and Dewey (1927).

¹¹ “Department Budget 1931–32,” Coss to Butler, November 3, 1930, CUA-OPR, Box 343, Folder 8. Some of Dewey's students who would come to play an important role in the naturalist school—most notably Sidney Hook—had positions elsewhere in New York. See also Jewett (2011).

studies had always been intertwined with his progressive political agenda and work on educational reform, and many of his students followed him in his footsteps. Hook was involved in the American Workers Party and advocated the role of science in education, arguing that “method should be central in educational activity” (Hook 1946). Randall was one of the signatories of the humanist manifesto (Kurtz 1973). Nagel regularly published in progressive journals and was convinced that the ideals realized through scientific inquiry “are also the ideals which are indispensable to the successful operation of any society of free men” (Nagel 1954, 306). And Friess and Schneider worked on the cross-section between naturalism and religion, pioneering the empirical study of religious movements (Friess and Schneider 1932). The 1944 volume *Naturalism and the Human Spirit*, often viewed as a manifesto of the school, served as joint public statement on the multifaceted aims of naturalism, displaying, in Randall’s words, “a community of temper, of method, and even of general outlook, rather remarkable in any group of writers so crotchety and individualistic as professional philosophers” (Krikorian 1944; Randall 1944, 355).

Yet the naturalists were not just known for their progressive politics and systematic studies of science, morality, art, and religion. The school was equally famous for its work in the *history* of ideas. Combining Dewey’s adaptationist perspective on the origin of philosophical problems with Woodbridge’s attempts to use history to free philosophy from its dualist dogmas, many naturalists presupposed a genetic approach to the study of philosophy. “If men’s minds are a mosaic or palimpsest of belief upon belief,” Randall wrote in *The Making of the Modern Mind*, “it is of the highest importance that they understand the life-history of those beliefs, why they are there, and whether they are justified in being there” (Randall 1926, 6). Many second-generation naturalists had written dissertations on historical figures such as Schleiermacher (Friess), Spinoza (McKeon), Aristotle (Edel), and Schelling (Gutmann), or were known for their contributions to the history of philosophy (Randall 1926; Edman 1928; Hook 1936; Schneider 1946). The “Columbia school” was celebrated for its “historical studies” (Murphy 1937) and published several volumes of their *Studies in the History of Ideas* series (1918–1935). Even Nagel, who would come to play an important role in contesting Columbia’s emphasis on “historical philosophy,” published a host of papers on the history of logic because he believed that the discipline’s problems will be “more persuasive” if we examine the context in which they emerged (Nagel 1979, 196; Mormann 2021).

The Columbia naturalists, in sum, challenged many dichotomies that were taken for granted at the turn of the century. Not only did they reject deeply-engrained philosophical distinctions between man and nature, subject and object, or self and society, they also aimed to integrate the study of philosophy and its his-

tory. Dewey, Woodbridge, and their students saw historical work as integral to philosophical inquiry because philosophical problems are contingent problems that emerge in specific historical contexts. It is deeply ironic, therefore, that the school eventually split into two factions itself: one which viewed philosophy as a humanistic discipline and emphasized the role of “historical philosophy” and one which saw it as a scientific field and focused on what they called “theoretical or ‘creative’ philosophy.”¹² It were the naturalists’ encounters with German philosophers that helped put this distinction on the philosophical agenda.

4.4 Hook’s Year in Europe

The Columbia school worked in relative isolation during the first years of its existence. While German philosophy had had a major impact on American thought in the late nineteenth century, the First World War hampered international communication for more than a decade. Academics from allied countries organized a boycott on German scholarship and banned their former colleagues from international conferences until the mid-1920s.¹³ Only at the World Congress of Philosophy in 1924, international communication started to be restored. Guido Della Valle, president of the organization committee, listed the “renewal of friendly relations” as one of the event’s important goals as only “national or interallied” congresses had been organized for such a long period (Della Valle 1924, 391). A year later, American academia reinstated the tradition to have its most talented scholars travel to Europe for a year of study when the John Simon Guggenheim Memorial Foundation launched its now famous fellowship program to foster “international understanding.”¹⁴

Sidney Hook was the first product of the Columbia school to be awarded a Guggenheim fellowship. In 1928–29, he spent an academic year in the Weimar Republic to write a “philosophic history of the period from Hegel to Marx with emphasis on the social and political forces which controlled the evolution of ideas.”¹⁵ Hook

12 “Report of the Committee on the Future Planning of the Philosophy Department,” CUA-OPR, Box 379, Folder 20.

13 By “labeling a conception, a policy, or a mode of conduct ‘German,’” Frank Thilly wrote a few years after the end of the conflict, philosophers were able “to put the quietus on it: whatever was German was wrong” (Thilly 1920, 185). On the boycott, see Grundmann (1965) and Cock (1983). On Dewey’s response to German philosophy during the war, see Dewey (1915) and, for a discussion, Campbell (2004).

14 *New York Times*, February 23, 1925.

15 Guggenheim Foundation to Hook, March 13, 1928, SHP, 16.18

was both a student of Dewey and a committed Marxist and was convinced that there are strong similarities between their views. Both Dewey and Marx had started their careers as left-Hegelians but gradually came to naturalize the dialectic in order to do justice to the philosophical implications of Darwinism (Hook 1935, 224). To the extent that they were different, Hook believed, “dialectical materialism must take its cues from the scientific pragmatism of Dewey” (Hook 1928, 154). The New York philosopher felt that his reading of Marx would open the door to a more democratic and more American version of socialism, which he tried to implement through his activities for the American Workers Party in the early 1930s. Dewey, in turn, was impressed by Hook’s work and viewed him as “one of the most promising students” he had met in “forty years of teaching.”¹⁶ He regularly consulted his protégé on philosophical questions and told a former colleague that he almost felt ready to retire as Hook had “not only got the point but sees many implications I hadn’t.”¹⁷

Hook arrived in the Weimar Republic in July 1928 and spent most of his year in university libraries to study archival material concerning the development of Marx. Yet his correspondence reveals that he was equally interested in contemporary philosophical developments in the German-speaking world.¹⁸ Throughout the year, Hook audited courses and visited a large number of philosophers in Munich, Berlin, Heidelberg, Bonn, Cologne, and Frankfurt, summarizing his findings in a paper (“A Personal Impression of Contemporary German Philosophy”) in *Journal of Philosophy*. As one of the first such accounts from a scholar steeped in the “methods and traditions” of American philosophy (Hook 1930a, 141), the paper played an important role in shaping Columbia’s reception of postwar German philosophy in the years after the boycott.

Hook’s paper and correspondence reveals that he was deeply disappointed with recent developments in German philosophy. Although he was impressed with the “dramatic quality” of the lectures he audited (Hook 1930a, 150), he was disturbed by the philosophers’ ignorance about science and logic. Even at the University of Berlin, where Einstein and Schrödinger had been revolutionizing physics, philosophers were indifferent and sometimes even outright hostile to the activities of their colleagues in the natural sciences (Hook 1930a, 147). In letters to his friend Ernest Nagel, Hook complained that there was “really very little” to

¹⁶ Dewey, Recommendation letter, February 4, 1926, SHP, 174.4.

¹⁷ Dewey to G. H. Mead, cited in Levine (1989, vii); Phelps (1997, 29).

¹⁸ See in particular the 34 letters and postcards Hook sent to Nagel between July 1928 and August 1929 (ENP-ASP, Box 3, Folders 10–11) and the 32 postcards sent to his family (SHP 3.12 and 133.16).

be gained from listening to German philosophers as most of them had “no interest in logical analysis and critical thinking”:

The longer I stay here the more contemptuous do I become of current philosophy in Germany ... It seems ... that the technical philosophers in evaluating a man's capacities, put down to his credit whatever historical philosophy he knows and then *subtract* his knowledge of math, physics and logic to get the total.¹⁹

The problems of philosophy, Hook complained, were almost exclusively “presented in terms of their history, not in terms of their logic” (Hook 1930a, 145).

Hook was particularly disappointed with the development of phenomenology, without doubt Germany's most dominant school of philosophy at the time. He had always admired Husserl for his work on the philosophy of logic but discovered that the Freiburg professor had turned to a transcendental-idealist position in the 1920s. In a letter to Nagel, Hook complained that the phenomenologists' arguments “are palpably weak and grounded in the faith that what is immediately perceived, felt, or experienced has absolute significance.”²⁰ Already in his first month in Germany, Hook was “resolved to pen an attack on the basic assumption of the phenomenological school.”²¹ He published a paper on “Husserl's Phenomenological Idealism” in *Journal of Philosophy* (Hook 1930b) and criticized Heidegger's *Sein und Zeit*, published just a year before his arrival, in his “Impressions” paper. While Heidegger's anthropocentric focus on “life-of-man-in-the-world” reminded him of Dewey, his book was “such a jungle of arbitrarily-invented technical terms, that only the natural belief that where there is so much smoke there must be at least a little fire, keeps the reader at the grueling task of trying to make sense out of its pages” (Hook 1930a, 154). Phenomenology, for Hook, had turned into yet another version of idealism, Germany's national obsession. Whenever a professor exclaimed “*Aber, meine Herren, das ist Naturalismus,*” he meant his students to understand that the position had been reduced to absurdity (Hook 1930a, 145).

The one major exception to Hook's negative assessment was Hans Reichenbach. While few German philosophers had “the stature of ... Dewey,” Reichenbach was clear-headed, open-minded, and deeply engaged with recent scientific findings.²² Hook audited his lectures on probability and philosophy of science and was delighted to find a German philosopher whose views were “congenial” to his “pragmatic naturalism” (Hook 1978, 33). He got to know the Berlin philosopher when they

¹⁹ Hook to Nagel, July 11, 1928; May 8, 1929; May 30, 1929, ENP-ASP, Box 3, Folder 10.

²⁰ Hook to Nagel, July 29, 1928, ENP-ASP, Box 3, Folder 10.

²¹ Hook to Nagel, July 29, 1928, ENP-ASP, Box 3, Folder 10.

²² Hook to Nagel, October 2, 1928; November 16, 1928, ENP-ASP, Box 3, Folder 10.

both attended a conference of the *Kant Gesellschaft* in Halle and learned that Reichenbach defended a “naturalistic interpretation of the *a priori*” and a pragmatic interpretation of probability (Hook 1930a, 159). In his “Impressions” paper, Hook introduced Reichenbach’s philosophy of science to the American philosophical community, writing about his naturalism and his most recent book *Philosophie der Raum-Zeit-Lehre*, which he described as “the most lucid and comprehensive exposition of the philosophical implications of the theory of relativity that has yet appeared in Germany” (Hook 1930a, 159).

4.5 Woodbridge and Nagel

Hook was not the only Columbia naturalist to be charmed by Reichenbach’s approach. In 1931, Woodbridge traveled to Europe to take up a position as Roosevelt Professor at the University of Berlin, where he regularly exchanged ideas with the German philosopher. The Roosevelt chair was part of an exchange program between Columbia and Berlin and had been created by Butler in order to stimulate the “intellectual bonds” between Germany and the American people.²³ Woodbridge was the first Roosevelt professor since 1914, when the program had been discontinued, and he used his year in Berlin to promote American philosophy. He lectured on American naturalism and realism but also kept a close eye on political developments. In letters to Butler, who had just been awarded the Nobel Peace prize, Woodbridge regularly reported on German politics, sensing that the country was at a “cross-roads” and could use some “realistic thinking.”²⁴

It is no coincidence that Woodbridge and Reichenbach got acquainted during the former’s year as Roosevelt professor. About a month before Woodbridge arrived in Berlin, Reichenbach had written Hook that he “would very much like to gain more contact with American philosophy.” The logical empiricist was convinced that the United States would be more fertile ground for his scientific philosophy than Germany, where he and his colleagues “always [had] to fight against historically-oriented *Schulphilosophie*.”²⁵ Hook had informed Reichenbach that Woodbridge would be coming to Berlin and said that his teacher’s naturalism

23 Butler to Woodbridge, July 31, 1931, CUA-OPR, Box 342, Folder 8.

24 Woodbridge to Butler, December 14, 1941, Frederick James Eugene Woodbridge papers, 1884–1950, Columbia University (hereafter, FJEWPP), Box 1, Folder: “Butler, Nicholas Murray, 1931.”

25 Reichenbach to Hook, August 20, 1931, Hans Reichenbach Papers, Archives of Scientific Philosophy, University of Pittsburgh (hereafter HRP), 014–51–28, my translation. See also Verhaegh (2020, section 5).

was “in fundamental agreement” with the German’s scientific philosophy.²⁶ Naturally, Reichenbach was excited to meet the editor of *Journal of Philosophy*—a periodical he and his colleagues had been reading “with great interest”—and invited Woodbridge to give a talk at his *Gesellschaft für empirische Philosophie*.²⁷

Woodbridge visited Reichenbach’s society in January 1932 and gave a talk titled “Der Empirismus in der amerikanischen Philosophie” to a crowd of academics and philosophically-minded Berliners.²⁸ The sixty-three-year-old professor appears to have been impressed by Reichenbach and his society as he described it as “an active and progressive philosophical movement” in a letter to Stephen Duggan, whom he asked to arrange an American lecture tour for the German philosopher.²⁹ After the event, he began studying Reichenbach’s work on the theory of relativity and wrote him that he hoped that they would have more time to talk about “the connection between Space and Geometry” in the future.³⁰ Though Woodbridge believed that philosophers should mostly “look for enlightenment” in the “biological sciences” as “philosophy is the outcome of human living rather than of physical movements,” he was interested in the philosophical implications of relativity, which philosophers had been debating in his *Journal of Philosophy* for more than a decade.³¹ He himself had been working on the topic “for some time” and hoped to learn more about recent developments from Reichenbach.³²

Hook did not just help establish a connection between Reichenbach and Woodbridge. He also stimulated Nagel to engage with Reichenbach’s work. During his year in Europe, Hook regularly mentioned the Berlin philosopher in his letters and postcards, urging his friend to read the German’s publications on probability and the theory of relativity.³³ Nagel was completing a dissertation on “the logic of measurement” and later recalled that he studied Reichenbach’s work “with enormous profit” (Nagel 1978, 42). Whereas Dewey and his other teachers provided only informal characterizations of concepts such as probability and measurement, Nagel adopted an axiomatic approach, just as Reichenbach had done in his books on relativity. He published a paper on measurement in *Erkenntnis* and regularly

26 Hook to Reichenbach, August 29, 1931, HRP, 014–51–27.

27 Reichenbach to Woodbridge, September 11, 1931, FJEW, Box 1, Folder: “Correspondence ‘R.’”

28 Woodbridge to Reichenbach, November 3, 1931 and March 1, 1932, FJEW, Box 1, Folder: “Correspondence ‘R.’”; “Chronik,” *Erkenntnis*, Vol. 2, 1931, 310.

29 Woodbridge to Duggan, January 18, 1932; March 1, 1932; FJEW, Box 1, Folder: “Corr. ‘D.’”

30 Woodbridge to Reichenbach, March 16, 1932, FJEW, Box 1, Folder: “Correspondence ‘R.’”

31 Woodbridge to Paul J. Tomlinson, FJEW, Box 1, Folder: “Correspondence ‘T.’” See Verhaegh (2024) on the reception of relativity in American philosophy.

32 Woodbridge to Reichenbach, March 16, 1932, FJEW, Box 1, Folder: “Correspondence ‘R.’”

33 Hook to Nagel, November 16, 1928 and May 30, 1929, ENP-ASP, Box 3, Folders 10–11.

cited the German philosopher in his first published papers (Nagel 1929, 176; 1933, 538).

In 1934, it was Nagel's turn to travel to Europe on a Guggenheim fellowship.³⁴ By this time, however, his interests had largely shifted from Reichenbach to Carnap. The news about the Vienna Circle, in particular Carnap's *Der logische Aufbau der Welt*, had reached American shores and Nagel had been one of many U.S. philosophers to respond to the latter's ideas about meaning and verification (Nagel 1934). In September 1934, he met the German philosopher at the International Congress of Philosophy in Prague, where several members of the Vienna Circle were present. Carnap invited Nagel to come back to Prague sometime after the conference so that the two could discuss each other's ideas about "logic and methodology" in more detail.³⁵ Nagel was excited about the opportunity and spent a few weeks with the Carnaps in November 1934. In just a short period of time, Nagel was swayed by Carnap's technical approach, wondering whether it did not offer a more solid foundation for naturalism than the work of his teachers. In a letter to Hook, he favorably compared Carnap's method to Dewey's, writing that "Columbia's philosophy department" suddenly seemed like "a home for poets who have missed their vocation":

At this distance, and under the influence of the positivists, Dewey's psychologizings and failures to come to grips with the detailed structure of scientific theories seem very serious shortcomings, and I am sure 'our brand' of naturalism will be better served by overcoming them.... But this is perhaps a passing mood, induced by contact with Carnap. He really has shown me that a man can have a larger vision, without being simply ecstatic or, as in the case of Dewey very muddy.³⁶

Importantly, Nagel also commented on Carnap's *ahistorical* approach. In a report about Europe's emerging analytic movement, published in two installments in *Journal of Philosophy*, Nagel wrote that the philosophers he met in Vienna, Prague, Lviv, Warsaw and Cambridge had little interest in historical analysis. Instead of asking why philosophers such as Kant and Hegel had held "the ideas they do," like the Columbia naturalists, Carnap and his colleagues simply dismissed their views as logical "blunders." Nagel had been educated in an environment that emphasized a contextualist perspective but saw the "analytic" approach as "a welcome relief from the transcendental pose assumed by so many American writers in approaching systematic philosophy" (Nagel 1936, 7). While he believed it inaccurate

³⁴ See Verhaegh (2021) for a reconstruction of Nagel's background and year in Europe.

³⁵ Nagel to Carnap, November 10, 1934; Carnap to Nagel, November 11, 1934, Rudolf Carnap Papers, Archives of Scientific Philosophy, University of Pittsburgh (hereafter, RCP), 029–05–20/2.

³⁶ Nagel to Hook, December 3, 1934, SHP, 22.08.

rate to dismiss “all of traditional philosophy [as] a mistake,” he described the method as “exhilarating to an unusual degree” (Nagel 1936, 7). Philosophers like Carnap were only interested in valid arguments, not in historical context.³⁷

4.6 Historical and Logical Analysis

Hook, Nagel, and their naturalist colleagues were science-minded academics who saw historical work as integral to philosophy. In Germany, however, the two encountered a strong bifurcation between historical and scientific perspectives. Hook, we saw, observed how German idealists presented philosophical problems exclusively “in terms of their history, not in terms of their logic” and learned that Reichenbach had a hard time finding a job as a scientific philosopher in a country dominated by “historically-oriented *Schulphilosophie*.”³⁸ Nagel described analytic philosophy’s ahistoricism in his report for *Journal of Philosophy*, signaling the lack of interest in “the genesis of doctrines” by the Vienna Circle and like-minded groups (Nagel 1936, 6). When Nagel, a few months after his return, mentioned that he was writing a paper on the “growth of modern conceptions in logic,” he was again confronted with this ahistorical stance. Carnap, Nagel wrote, “expressed a strong distaste for the project,” telling him that he would be “wasting [his] time.” Better to solve logical problems, than to study their history.³⁹

It is precisely some such distinction that eventually led to a split within Columbia’s department of philosophy. While Nagel believed that the “vigorous and technically precise” methods of the logical empiricists would be “salutary stimuli” (Nagel 1956, xii) and attempted to convince his colleagues about the value of this approach, most of them came to see logical positivism as a dangerous development. Nagel expected that his colleagues would find Carnap’s ideas “congenial and stimulating” but Edman qualified the latter’s “formalisms” as “barren,” describing the logical empiricists as a “philosophical cult” that reduced philosophy to a “series of definitions, postulates, [and] logical relations.”⁴⁰ Randall was even

³⁷ Note that I have exclusively focused on Nagel’s impressions here. Some logical empiricists seem to have had more subtle ideas about the value of historical philosophy. See Dewulf (2020) for an overview.

³⁸ Hook (1930a, 145); Reichenbach to Hook, August 20, 1931, HRP, 014–51–28.

³⁹ Nagel to Hook, June 28 and July 5, 1936, SHP, 22.09.

⁴⁰ Nagel to Coss, November 28, 1934, ENP-ASP, 029–05–19; Edman (1941, 562; 1934, 477). Naturally, I do not want to suggest that Nagel and Hook only encountered the distinction between logical and historical analysis in the German-speaking world. Both had been students of Morris R. Cohen at the City College of New York before they enrolled at Columbia graduate school. Cohen had a

more “hostile” to his German colleagues (White 1990, 30) and wrote Hook that he saw the analytic approach as *the* biggest threat to the discipline:

There is no respectable philosophical position today to which I am more opposed than the one known as ‘analysis’.... I have reluctantly become convinced that ‘philosophical analysis’ would if it could kill the philosophical enterprise completely... ‘Analysis’ is opposed to any serious consideration of any of the philosophic issues which seem to me important.⁴¹

Hook, in turn, replied that “if a battle-line is to be drawn between ‘respectable philosophic positions,’” there is “no doubt in my mind that it is on the side of the analytical philosophers that I belong.” The analytic approach, Hook claimed, is strongly committed to “careful and clear statement,” which is “indispensable condition for responsible philosophic activity.”⁴²

It is important to note that Hook and Nagel were not blind to the tensions between analytic and naturalist approaches to philosophy. Hook clearly explains the limits of a purely analytic perspective in an unpublished lecture, read at the 1939 Unity of Science conference at Harvard. “By taking statements in isolation from their historical contexts,” Hook explained, “it is easy to show that they do not conform to any known scheme of logical grammar.” Logical analysis is a useful method to demonstrate that a claim is ambiguous or meaningless but such a conclusion should not be the end but the *starting point* of an investigation. Naturalists see it as their task to understand the contexts which “have given rise to ... conflicting metaphysics and ideologies” as well as “to locate their meaning by correlating statements ... with behavioral responses to specific situations in which other forms of conflict arise.”⁴³ Likewise, Nagel believed that “the historical approach, when wisely cultivated, can frequently produce the same kind of intellectual catharsis and *dissolution* of pseudo-problems as does the analytic method” (Nagel

more analytic approach to philosophical problems and regularly criticized the Columbia approach as too “anthropocentric” (Cohen 1940). See e.g. Verhaegh (2021, section 5). At the very least, Cohen’s teaching seems to have played a role in helping Hook and Nagel appreciate the analytic approach they encountered in Germany. Indeed, it seems no coincidence that the strongest opponents of the analytic approach had all been at Columbia since college.

⁴¹ Randall to Hook, October 25, 1951, SHP, 24.09. Even Dewey eventually soured on analytic philosophy and logical empiricism. While Nagel and Hook had helped Otto Neurath persuade Dewey to write a contribution for the empiricists’ International Encyclopedia of Unified Science at first, he began to dismiss logical empiricism as an overly “scholastic” approach to philosophy in the early 1940s (Lamont 1959; 13; Randall 1953, 7). See Reisch (2005, ch. 4) for a reconstruction of Dewey’s interactions with the Unity of Science movement.

⁴² Hook to Randall, October 27, 1951, SHP, 24.09.

⁴³ “John Dewey and Logical Empiricism,” unpublished ms., ca. 1939, SHP, 34.30.

1936, 7) and he regularly criticized Carnap for his exclusive focus on “ahistorical evaluation[s]” (Nagel 1979, 3). A naturalist should not just identify the “rationality of science with the use of exclusively formal canons,” they should also critically assess the canons of rationality through a study of their development (Nagel 1979, 3).

But while Hook and Nagel believed that the analytic method could *supplement* the historical approach, their colleagues would have none of it. As several major representatives of the logical empiricist movement found prominent positions at U.S. institutions, their former teachers dug in their heels. Randall and his colleagues observed that analytic philosophers were mostly “unsympathetic towards the history of philosophy” and perceived the movement as an existential threat.⁴⁴ “Logical positivism,” Nagel concluded, had made “a decided impression” upon “the younger men in the profession” but the “old-timers ... distrust it, dislike it, and pretend that it has nothing very important to say.”⁴⁵ Nagel regularly asked for new hires to strengthen the department’s profile in analytic philosophy but had little sway with his colleagues. He was unable to convince them that Columbia should invest more in “foundations of mathematics” and got a “strongly negative reaction” when he suggested Reichenbach for one of the department’s open positions in a faculty meeting.⁴⁶ Edman, the department’s chair, preferred to hire a “pronounced ‘humanist’” and suggested that Columbia already had a specialist in philosophy of science, viz. Nagel himself.⁴⁷ The department hired seven new assistant and/or associate professors in the late 1940s, and all of them had written a historically-oriented dissertation: Charles Frankel (on the French Enlightenment), Justus Buchler (on C. S. Peirce), Joseph Leon Blau (on the Christian interpretation of the Cabala in the Renaissance), John R. Everett (on John Bates Clark), Paul Kristeller (on Plotinus), Ernest Moody (on Ockham), and Albert Hofstadter (on Locke). Moreover, six of the appointees had a Columbia PhD, much to the annoyance of Nagel, who had long felt that his colleagues are “so damned smug that [they think] all philosophical virtue has been conceived in Morningside Heights.”⁴⁸

⁴⁴ Paul Kristeller to Philipp Wiener, April 13, 1957, cited in Dewulf (ms.).

⁴⁵ Nagel to Neurath, January 2 and October 13, 1936, Otto Neurath Nachlass (hereafter, ONN), Wiener Kreis Archiv, Noordhollands Archief, Haarlem, Item 275.

⁴⁶ Nagel to Fackenthal, April 12, 1944, Ernest Nagel Papers, Columbia University Archives (hereafter ENP-CUA), Box 1, Folder 6; Nagel to Hook, November 25, 1946, SHP, 22.09.

⁴⁷ Edman to Gutmann, May 17, 1945, James Gutmann Papers, Box 1; Nagel to Hook, November 25, 1946, SHP, 22.09.

⁴⁸ Nagel to Hook, November 25, 1946, SHP, 22.09.

4.7 The End of History

The opposition between historical and analytic approaches became even more pronounced in the 1950s, when the positivists and affiliated schools of analytic philosophy started to dominate American philosophy. Nagel had become a prominent figure in analytic circles and this significantly changed the balance of power within the department. The philosopher of science began to receive increasingly generous offers from prestigious universities and his colleagues realized that the department's "reputation ... would ... be damaged immeasurably if [he] were to go."⁴⁹ They faced the "constant possibility" that Nagel would leave and went through great lengths to keep him in New York. When the Columbia professor received invitations from "several colleges" in 1955, for example, the department created a special John Dewey Chair and approached a number of donors to collect money for a fund that should guarantee Nagel "a salary worthy of John Dewey's name."⁵⁰ By 1959, when the department celebrated the Dewey centenary, Nagel earned substantially more than his senior colleagues, some of whom had been hired a decade before him.⁵¹

At first, this shifting balance of power had little effect on the course of the department itself. Nagel was a rising star in American philosophy but the Columbia school kept investing in historical philosophy. After James Gutmann took over as the department's executive officer in 1954, the philosophers hired six new assistant professors and most of them were, again, historically-oriented scholars with a Columbia PhD: Sidney Gelber (who had written a dissertation on John Grote), George Kline (on Spinoza), Stanley Newburger (on Lalande), and James Walsh (on Aristotle).⁵² But this simmering conflict inevitably came to a head. In 1959, when the department had to decide whether or not to promote Kline, one of these recent hires with a historical approach, the faculty was "seriously divided," with seven professors favoring and four professors opposing the promotion.⁵³ An *ad hoc* committee was appointed to resolve the situation and concluded that the disagreement had less to do with Kline than "with the future of the department." A majority (Gutmann, Randall, Friess, Buchler, Kristeller, Blau, and Cumming) was "content with the *status quo*" whereas "an important minority" (Nagel, Hofstadter, Frankel,

⁴⁹ Gutmann to Barzun, February 27, 1958, CUA-OPR, Box 437, Folder 23.

⁵⁰ Gutmann to Walter D. Fletcher, January 21, 1955, CUA-OPR, Box 437, Folder 22.

⁵¹ Cumming to John M. Mullins, June 9, 1960, CUA-OVPR, Box 9, Folder 6.

⁵² The only exceptions were Sidney Morgenbesser, a philosopher of science from the University of Pennsylvania and Richard Kuhns, a philosopher of art with a Columbia PhD.

⁵³ Report of the Ad Hoc Committee on the Promotion of George Kline," CUA-OVPR, Box 9, Folder 6.

and Cooley) was “convinced that the department ha[d] been steadily deteriorating” and had “lost the important position it once held in American philosophy.”⁵⁴ Importantly, the committee sided with the minority and used the report to warn the administration about the declining reputation of the Columbia school of philosophy in an increasingly analytically-oriented intellectual landscape:

the *ad hoc* committee came to share the conviction of the minority group about the parlous state of the Department of Philosophy. Both at home and abroad several of us ... heard the statement that Columbia [has] lost its place in the philosophical sun. A majority of our senior professors in the department have little reputation outside New York. Most of them were trained by either Dewey or Woodbridge, to one or the other of whom they seem to have—as one of our informants put it—a ‘father fixation’. They have not only been living largely upon the reputation of their teachers, but have tended to build up a department that is seriously inbred ... Indeed, the majority have opposed various attempts to invite outsiders who would represent philosophical positions other than their own. This is a serious condition in any department and likely to be fatal in a department of philosophy.⁵⁵

Kirk, the president of the university, received the report in 1959 and responded to its alarming conclusions by installing a new committee which was to conduct a more in-depth investigation of the future of the school.⁵⁶ This second committee published a detailed report a year later, revealing that the conflict regarding Columbia’s hiring policy was rooted in a more fundamental disagreement regarding, i.a., the opposition between historical and analytic approaches to philosophy. In a section titled “Ideological Conflicts,” the committee writes:

There is a clear division of opinion about the emphasis which should be given to the history of philosophy, on the one hand, and theoretical philosophy, on the other: The Columbia Department has long held an enviable position in the history of philosophy. Some members of the Department feel that there is an undue emphasis upon theoretical or ‘creative’ philosophy. The most severe criticism comes from ... the minority group [... which] stresses the over-emphasis upon historical philosophy.⁵⁷

54 Gutmann to Barzun, May 14, 1959, CUA-OVPR, Box 9, Folder 6; Report of the Ad Hoc Committee on the Promotion of George Kline,” CUA-OVPR, Box 9, Folder 6. Cooley was the department’s logic instructor for years but had been promoted to assistant professor in 1953. Hofstadter started as a historical philosopher but had turned to more analytic topics in the 1950s (e.g. Hofstadter 1951; 1953). Frankel’s remarks about scientific philosophy in “Philosophy and History,” finally, offer some background concerning his stance in the debate (1957, 363–367).

55 Report of the Ad Hoc Committee on the Promotion of George Kline,” CUA-OVPR, Box 9, Folder 6.
56 Nicolson to Kirk, February 12, 1960, CUA-OPR, Box 379, Folder 20.

57 “Report of the Committee on the Future Planning of the Philosophy Department,” CUA-OPR, Box 379, Folder 20.

The minority, led by Nagel, sympathized with the analytic approach and felt that the department overemphasized “historical philosophy.” The majority, led by Randall, resisted the analytic turn and complained that the others overemphasized “the philosophical fashions of the day: the philosophy of science and symbolic logic.”⁵⁸ Importantly, this ideological conflict did not just inform the disagreement concerning Columbia’s hiring policy. It also led to opposing views concerning the editorial policy of *Journal of Philosophy*, the periodical edited and published by the Columbia school. On the one hand, the minority complained that the journal had “lost the respected position it once held in the philosophical world” as it was “rare to find a single historical paper” in other philosophical periodicals. The majority, on the other hand, felt that there was *not enough* space for historical research and worked toward starting a new journal exclusively focused on the history of philosophy.⁵⁹

The 1960 report significantly changed the course of Columbia philosophy. The committee recommended a number of reforms and the administration responded by appointing a new chairman, promising him new hires on the condition that the department would use the new funds to improve the “‘balance’ ... between ‘analysts’ and ‘humanists’.”⁶⁰ The department had a discussion about its “tendency to ‘inbreeding’” and decided to terminate the contracts of three assistant professors.⁶¹ In the years following its publication, the department hired more outsiders in senior positions (e.g. Richard Taylor, Robert Paul Wolff, and Charles Parsons) and many of them had a systematic rather than a historical approach to philosophy. Combined with the retirement of Randall, Friess, and Gutmann, this led to a different, more analytically-oriented profile. *Journal of Philosophy*, finally, developed a more analytic profile, too. Arthur Danto, Morgenbesser and Walsh took over as editors in the mid-1960s and the journal virtually stopped publishing his-

58 “Report of the Committee on the Future Planning of the Philosophy Department,” CUA-OPR, Box 379, Folder 20.

59 “Report of the Committee on the Future Planning of the Philosophy Department,” CUA-OPR, Box 379, Folder 20. See Dewulf (ms.) for a reconstruction of Randall’s, Schneider’s and Kristeller’s attempts to found a new periodical, which eventually resulted in the creation of *Journal of the History of Philosophy*.

60 Richard Herpers to Gutmann, December 5, 1958, CUA-OPR, Box 437, Folder 23; Barzun to Cumming, June 22, 1960, CUA-OPR, Box 9, Folder 6; Cumming to Mullins, June 9, 1960, CUA-OPR, Box 9, Folder 6; Cumming to Nagel, May 16, 1960, ENP-CUA, Box 1, Folder 20.

61 They were Newburger, Sommers, and Kuhns, who all held a Columbia PhD. Kuhns’s contract was eventually retained and he stayed at Columbia until 1993. See “Report of the Committee on the Future Planning of the Philosophy Department,” CUA-OPR, Box 379, Folder 20; and Gutmann’s letters to Newburger, Kuhns, and Sommers, February 6, 1960, CUA-OPR, Box 9, Folder 6.

torical papers in the period thereafter.⁶² The one major remaining naturalist opposing the analytic tradition—Justus Buchler—left the department because he became increasingly “disturbed by the overall changes in the University’s intellectual climate” (Gelber 1991, 12). Though the school still employed a large share of scholars working in the naturalist tradition—e.g. Nagel, Morgenbesser, and Isaac Levi—most of them had an analytic approach to philosophy.⁶³

4.8 Conclusion

American philosophers today tend to associate “naturalism” with the views of W.V. Quine, an analytic philosopher with pragmatist *and* positivist roots. Quine had no direct ties with the Columbia school but started using the label in the 1960s to defend the position that philosophy is not “an a priori propaedeutic or groundwork for science” (Quine 1969, 126). Ironically, Quine first identified as a naturalist in his John Dewey Lecture “Ontological Relativity,” held at Columbia in 1968. Though Quine admitted that he was “not much of a Dewey scholar,” he told his audience that he shared the latter’s view “that knowledge, mind, and meaning are part of the same world that they have to do with” and that they ought to be studied “in the same empirical spirit that animates natural science” (Quine 1968, 26).⁶⁴

The popularity of Quine’s ideas in postwar academic philosophy shows that the analytic turn did not lead to an eclipse of naturalism as such. On the contrary. Jaegwon Kim has suggested that naturalism is contemporary philosophy’s dominant “ideology” (Kim 2003, 83) and a recent survey shows that about fifty percent of philosophers accept a naturalist position in metaphilosophy, thereby following Quine’s suggestion that philosophy ought to be “continuous with science” (Bourget and Chalmers 2023, 7; Quine 1969, 126). Still, the popularity of Quinean naturalism also reveals that something *was* lost in the analytic turn. For the Harvard philosopher, like many of his analytic colleagues, never saw the value of a historical approach. While Randall, Edman, and even Hook and Nagel all shared the view that (1) philosophical problems are contingent problems that emerge in specific historical contexts and (2) that historical reconstructions can help us excavate the roots of our ideas, methods, and assumptions, Quine was known for joking “that there are two sorts of people interested in philosophy: those interested in philosophy

⁶² See Katzav (2018) for a reconstruction.

⁶³ See also Strassfeld (2022, 125) who notes that Columbia’s hires in the 1960s were “overwhelmingly analytic in orientation.”

⁶⁴ See Verhaegh (2018, 155–160) for a reconstruction.

and those interested in the history of philosophy.”⁶⁵ Both advocates and opponents of the “eclipse view,” therefore, tell an important part of a larger story. Naturalist approaches still prominently figure in contemporary analytic philosophy but to this day *Journal of Philosophy*, the brainchild of a Columbia naturalist who once wrote that “the serious study of history is characteristic of a certain maturity of mind” (Woodbridge 1916, 1), explicitly warns prospective authors that it “does not publish papers that are primarily historical.”⁶⁶ Ironically, only historical research can help us uncover and contextualize what the historically-oriented branch of the Columbia school contributed to the development of naturalism before it was overshadowed by the analytic movement.

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⁶⁵ See MacIntyre (1984, 39–40).

⁶⁶ <https://www.journalofphilosophy.org/memotoauthors.html> (accessed December 2023).

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II Phenomenology

Carlo Ierna

5 Was North America Fertile Ground for the Early Phenomenological Movement?

Abstract: Phenomenology arrived in North America during the first decades of the twentieth century. On the continent, it had flourished particularly where the terrain had been prepared by the School of Brentano. Did something similar occur in North America or were the background and context completely different? I will try to show the possible role of some of the competing elements in the late nineteenth and early twentieth century that favored and opposed phenomenology in North America. Despite significant German influences, North America was not receptive to phenomenology in the beginning. Philosophy and psychology were closely connected, in Germany as well as the US, but the prevailing imported approaches of Wundt and Herbart were not congenial for the reception of Brentano and Husserl. Moreover, Husserl's work was initially taken up in the context of debates about realism in the US, with limited interest in the original contributions of his phenomenological method.

5.1 Introduction

Before phenomenology started to take root in North America in the 1930s and '40s, already some seeds had been sown during the first decades of the twentieth century. On the continent, such seeds had fallen into fertile ground particularly where the terrain had been prepared by the School of Brentano. Did something similar occur in North America or were the background and context significantly different? In this contribution, I will try to show the possible role of some of the competing elements in the late nineteenth and early twentieth century that might have favored or opposed the early adoption of phenomenology in North America.

The large waves of German migrants as well as the numerous American students in Germany would suggest a receptive context for the German-language works of the early phenomenological movement during the last decades of the nineteenth century and the first decades of the twentieth. And yet, phenomenology did not really take root in American soil until much later.¹ The question then is: why was this not fertile ground for phenomenology?

¹ The Chronology for the reception of phenomenology in the Anglo-American context in Spiegelberg (1982) starts with Husserl's London Lectures in 1922. Strassfeld (2022, 47) lists Farber's 1925

On the one hand, while philosophy and psychology were closely connected at the time, in Germany as well as the US, the prevailing imported approach of Wundt was opposed to the phenomenologies of Brentano and Husserl. On the other, Husserl's works were initially interpreted and taken up in the context of "Logical Realism" (Parkhurst), "Logical Objectivism" (Ryle) and as a "defence of a realist theory of universals" (Knight). Given the contingent particularities of the succession of philosophical movements in the US, the early arrival of the phenomenological movement seems mostly to have been untimely.

5.2 Brentano in the Background

In order to contextualize the reception of phenomenology in North America, it is informative to start with how Edmund Husserl himself characterized his own position. In various prospective and retrospective texts Husserl gives grand visions of what phenomenology aims to be, for instance in 1922/23 he claims that: "Phenomenology is the utmost consummation of rationalism, but can just as well be termed the utmost consummation of empiricism" (Husserl 2002, 288). This might seem to be mostly hyperbole at first glance, but considering the development of phenomenology from the 1900/01 *Logical Investigations*, we can see what Husserl might have meant by such grand claims. Not so much that there were no differences, ultimately, between rationalism and empiricism (as some kind of philosophical horseshoe theory), but that phenomenology aims at addressing and (ideally) answering or overcoming the fundamental questions of both (De Palma 2020, 74). This is consistent with the mission statement for phenomenology at its inception in 1900/01: finding and founding the relation between the subjectivity of knowing and the objectivity of knowledge. However, this concern is much older than the *Logical Investigations*, originating in the 1890s and Husserl's early "pre-phenomenological"² philosophy of mathematics. His first works were indeed also con-

dissertation (published 1928) *Phenomenology as a Method and as a Philosophical Discipline*, as "the first systematic treatment of Husserl's thought in the English language" and Winthrop Bell's 1927 course "Husserl and the 'Phenomenological' Movement" as the first course in phenomenology in America.

2 This term only makes sense if we confine "phenomenology" to Husserl's own theories. In a broader sense, as we will be using the term here, it also encompasses the positions of Husserl's teachers Brentano and Stumpf. See e.g. Spiegelberg (1982, 6 and 28f.): "How far is it legitimate to begin the history of the Phenomenological Movement with Franz Brentano? ... the main reason for crediting Brentano with having prepared the ground for phenomenology must be sought in specific elements of his philosophy which have influenced and even permeated the fullfledged phenomenology of Husserl and his successors."

cerned with “how to reconcile the objectivity of mathematics, and of all science in general, with a psychological foundation for logic” (Husserl 1900, 6). By “psychological” here he means Franz Brentano’s descriptive psychology. Brentano considered his psychology to be empirical, but not necessarily experimental; subjective, but not at all introspective; and scientific, interdisciplinary, and non-reductionist. Brentano also already struggled with the same problem: how to reconcile a posteriori empirical natural science and a priori logic and mathematics. He tried to navigate a middle course between “extreme” empiricism, exemplified by Mill, and “extreme” idealism, exemplified by Kant.³ In order to characterize his position, it might be tempting to just go along with the title of his main work: *Psychology from an Empirical Standpoint*. Brentano would be first and foremost an empiricist: “experience alone will be my teacher,” “the foundations of psychology as well as of the natural sciences are perception and experience” (Brentano 1874, 35).⁴ However, this does not do justice to his extensive discussions of the nature of philosophy and science as well as their shared methodologies: “Philosophy is a science like other sciences and therefore it must also have a method that is in essence identical to the method of the other sciences” (Brentano 1895). While Brentano did not begin his academic career as a mathematician as Husserl did, he nevertheless saw a fundamental role for mathematics:

Mathematics reveals in a clear and understandable way the fundamental nature of all true scientific investigation. There is no better field of study for gaining one’s first clear view of laws, deduction, hypothesis, and many other important logical concepts. (Brentano 1874, 34; 1995, 21)

For Brentano mathematics is both logically as well as chronologically prior to the sciences: it has come to maturity first and can serve as the model. Brentano is very clear about the status of mathematics itself: “Mathematics is not an inductive, but a purely deductive, and in this sense, a priori science” (Brentano 1870–1917, *Megethologie* 40025 f.). For Brentano mathematics is deductive, a priori, and analytic. Yet, this does not produce a tension with approaches in the empirical sciences, but supports them: “Indeed, were it not, then there would be no science at all, neither deductive nor inductive” (Brentano 1870–1917, *Megethologie* 40025 f.). In order to yield scientific knowledge and laws, inductive a posteriori experience needs a foundation that is not itself based again on experience.

³ For Brentano’s strategic positioning between German Idealism and British Empiricism, see Ierna (2022).

⁴ Translations, unless otherwise indicated, are my own.

It is clear that the whole attempt to justify induction by induction itself, leads to a vicious circle. It must clearly be proven a priori, hence in a purely deductive way. And this deductive way is through the calculus of probability. (Brentano 1870–1917, *Megethologie*, 40024f.)

Mathematics gives us tools like the calculus of probability and statistics, which we can apply to experience to justify inductive knowledge, but mathematics itself is founded analytically a priori. This is the way that Brentano sought to conciliate a priori and a posteriori knowledge, deductive and inductive methods. According to Brentano's approach, from the common starting point of our sensations, we can proceed inductively in both directions, outward and inward, in finding the laws of coexistence and succession of all phenomena. Laboratory experiments as well as research on children, animals, or so-called "primitive" populations could also all contribute to the science of psychology (Ierna 2014). Yet, this descriptive psychology or psychognosy or phenomenology (as Brentano variously called it) had the advantage of the epistemic privilege of inner perception. Alongside mathematics, the absolute certainty of inner perception constituted the second column of Brentano's scientific architecture.

This brief outline of the similarities of Husserl and Brentano at a methodological level (how to reconcile the various dichotomies of subjectivity and objectivity, induction and deduction, a priori and a posteriori, analytic and synthetic, in epistemology and philosophy of science) are meant to illustrate how a previous reception of Brentano and his other students facilitated the uptake of Husserl's as well.

One of the reasons for the fertile ground in Munich and for the origination of the phenomenological movement precisely there, was that here there was a generally congenial atmosphere. Throughout the years the people in Munich, more than at any other German university, had engaged very positively with Brentano and his School; i.e. with that circle of thinkers to which Husserl himself also belonged.⁵ (Schuhmann 1988, 97)

This talk of "fertile ground" and "a generally congenial atmosphere" then forms the background for the core issue of the present contribution: Was North America (and more specifically: the USA), fertile ground for the early phenomenological movement? Was there a comparable previous reception of Brentano and other Brentanists? Was there a "congenial atmosphere," a sympathetic audience, which had "engaged very positively" with other thinkers from the same broad phenomenological movement? Or, rather, was the soil more suitable to other transplanted people

⁵ Similar arguments can be given for Husserl's reception in Great Britain, see e.g. Spiegelberg (1982, 662): "Before World War I the interest in Husserl's phenomenology was overshadowed by that in Franz Brentano, promoted chiefly by the analytical psychology of G. F. Stout, and in Alexius Meinong, stirred by Bertrand Russell's three articles in *Mind* (1906)."

and ideas? Was there competition for the nutrients in the intellectual soil? Did the new environment change or selectively enhance the invasive exotic species, rendering them more congenial to their new home?

5.3 Land of Opportunity?

There are multiple reasons why one would at first expect a fertile soil in the “land of opportunity.” During the nineteenth century there had been an ongoing import and transformation of German ideas, values, products, not to mention the mass migration from Germany into the United States. In the period going from 1820 to 1900 approximately 5 million German immigrants arrived in the United States, on a total of approximately 20 million immigrants (Faust 1909 I, 585). This should already make it plausible that among the various foreign influences, the German one was likely the largest. Moreover, it also makes sense to compare this to the overall population in the US.⁶ Starting with a mere 5 million total population in 1800 it grew to 76 million in 1900. Given the large influx of Germans and their descendants this also implies that:

By the end of the nineteenth century immigrants from German-speaking Europe constituted one of the largest ethnic groups, German was a popular foreign language to study, partly as the major language of modern science, and nearly a thousand newspapers in German were being published in America in the 1890s. (McClelland 2020, 207)

Besides a sharp rise in migration in the 1840s and 1850s in connection with events on the old continent, including the Irish potato famine and the various attempts at revolutions in 1848, German immigration peaked at the end of the nineteenth century, in the 1880s, when nearly a million and a half came to settle in the US. According to the US census bureau, in the 1890s nearly 15 percent of the population in the US was foreign-born and more than 40 million Americans self-reported to be of German ancestry in 2020. While the sinking of the British passenger ship *Lusitania* with 123 US citizens aboard by a German submarine in 1915 and WWI put a temporary crimp on German-American relations and immigration, migration

⁶ Regrettably, as far as I have been able to determine, most of these statistics merely concern the citizens and registered immigrants, not the native populations of North America (at least before 1860) nor those held in slavery. This means that all such “overall” numbers have a large margin of uncertainty and are *au fond* if not racist then at least Eurocentric.

from Germany to the US surged again immediately after WWI, during the 1930s, and in the aftermath of WWII, but without reaching the heights of the 1880s.⁷

The opposite movement is likewise relevant here. During the nineteenth century approximately 10,000 American students went to Germany to pursue their studies, often with a view to obtain a (prestigious) German diploma. This number peaked in the academic year 1895–96, when more than 500 Americans were officially enrolled at a German university (Veysey 1965, 130). Studying in Germany was so common that in 1925 Herbert Gardiner in his retrospective “The First Twenty-Five Years of the American Philosophical Association” could quite straightforwardly state: “For the Association in the next quarter of a century we may repeat the slogan familiar to many of us from our student days in Germany, *Vivat, crescat, floreat!*” (Gardiner 1926, 158) Even if one hadn’t gone to Germany, they would have been familiar with German academic culture through the many students and colleagues who had gone.

During the nineteenth century many American institutes of higher learning were founded and/or underwent massive growth. Johns Hopkins (1876) was explicitly modeled on the German research-university originally envisioned by Humboldt.⁸ Andrew D. White, founder and first president of Cornell, had studied in Berlin and brought this experience to bear on the design of Cornell University. Stanford was on its turn modeled on Cornell.⁹ Tappan, president of the University of Michigan, likewise believed in the superiority of the German approach:

On the other hand, in Protestant Germany, what an advance has been made! In no part of the world has University education been so enlarged, and made so liberal and thorough. The Universities of Protestant Germany stand forth as model institutions. (Tappan 1851, 39)

Harvard had of course been founded long before all that, in 1636, but the philosophy department had grown significantly in the second half of the nineteenth cen-

7 It would still take two years before the US entered the war, but the sinking of the *Lusitania* sparked a wave of anti-German sentiment which ultimately contributed not only to an overall change in political attitude in the US, but also a shift in the loyalties of German-Americans. See e.g. Trommler (2009). Münsterberg, who was ardently pro-German, was severely ostracized after the outbreak of the war and particularly after the sinking of the *Lusitania*. For this and other repercussions on philosophy, see Kuklick (1977, § 23).

8 “The spirit of research characteristic of the German university was now securely planted upon American soil, and so quickly bore fruit that Germany very soon recognized Johns Hopkins as a sister institution. Almost all the earlier members of the faculty were doctors of German universities” (Faust 1909 II, 229).

9 For a more extensive discussion of the influence of the German “Humboldt university” model on American higher education, see Muller (1976).

tury (Pochmann 1957, 666; Münsterberg 1901, 475) and begun to teach German philosophy:

The crucial innovation in the eighteen-seventies was the introduction of regular instruction in the domain of German philosophy. This occurred at Harvard in 1873–1874, when Professor Bowen for the first time conducted his course in modern German philosophy. Harvard's lead was followed in a few years by others of the larger schools.¹⁰ (Pochmann 1957, 310)

Up to now everything looks perfectly poised for an overall positive reception and rapid uptake of the German-language philosophies of Brentano and his students. Brentano's *Psychologie vom empirischen Standpunkte* was published in 1874, and it would seem at first blush that it would have been perfectly timed and suitable to be included in the curriculum. Yet, once we start looking at the actual content of the courses and the books used by Bowen and others, it becomes obvious why Brentano and his ilk might instead not have been such a good fit after all.¹¹

The books Bowen used in Harvard for his courses in German philosophy were of an entirely different orientation from Brentano's: Schwegler's *Geschichte der Philosophie im Umriss*, Kant's *Kritik der reinen Vernunft*, Hartmann's *Philosophie des Unbewussten*, Schopenhauer's *Die Welt als Wille und Vorstellung* (Pochmann 1957, 674). Given Brentano's vocal criticism of German Idealism and Kant as its (perhaps unwitting) initiator, a context in which Hegelians, Kantians, and their direct interlocutors were given center stage, would not have facilitated the uptake of his type of "Austrian" philosophy.¹² Bowen was certainly not an exception, if we look at the other courses offered by prominent philosophers at Harvard thereafter. Royce lectured on "German Ethics" in 1883/84, on the "Movement of German Thought 1770–1830" (the dates suggest a thoroughgoing engagement with the rise and fall of German Idealism), and a seminary in metaphysics on Hegel in the years 1890–93. James likewise held courses on Kantian philosophy in the years 1896–1899 (Pochmann 1957, 673).

This is partially preceded by and overlaps with the spread of German Idealism outside of academia. Indeed, the first reception of Kant and Hegel happened through

10 "Further evidence of the widespread adoption by the 1880s of the method and content of German philosophical study appears in the use of a large number and variety of German textbooks, some in the original and others in translation" (Pochmann 1957, 319).

11 "In American philosophy at the end of the nineteenth century there was small interest in Empiricism and almost no interest in Realism. ... In contrast to these fragmentary realistic tendencies, Idealism, both epistemological and ontological, was everywhere rampant" (Montague 1937, 140).

12 For this label and its opposition to "German" philosophy, see Smith (1994, 1) and Ierna (2020, 99f.).

the “St. Louis Hegelians” Denton Snider and William Torrey Harris who founded the *Journal of Speculative Philosophy* which ran from 1867 to 1887.

Kant and Hegel provided most of the inspiration for the American idealists. Before the Civil War idealism had gained more advocates outside the academic community than within it, and the specifically Hegelian idealism that developed in the United States after 1865 was first promoted by a group of non-academic thinkers, especially in the St. Louis area. (Veysey 1965, 193)

Nearly three quarters of the journal’s contents were dedicated to German philosophy, and of that again more than half to Kant and Hegel. It is hard to overestimate how popular Hegel was at the time. It is likely that beside the St. Louis journal also the influence of the popular history of philosophy manual by Schwegler contributed to this. Schwegler’s *Geschichte der Philosophie im Umriss* had appeared in German in 1848 and was translated into English twice. In Great Britain it was published as *Handbook of the History of Philosophy* in 1867 by James Hutchinson Stirling. Stirling added a “Note Conclusory” (Stirling 1867, 396) to Schwegler’s own text titled “Why the History of Philosophy ends with Hegel and not with Comte,” which also conveys the idea that German philosophy was in all respects superior to any other philosophy:

are we then in advance of Germany? Is Germany in any respect behind us? Is not the truth rather this, that at this moment Germany leads the whole world even in empirical science ? ... Can any empirical science be named, indeed, for which Germany writes not the text-books? ... And to what is this superiority owing? Why, to nothing else than the superior faculties, the superior ideas, and the superior terms, which have resulted from the hard discipline of German philosophy. (Stirling 1867, 414)

This estimation of German philosophy by the Scotsman Stirling is *grosso modo* also confirmed in practice by the list of authors of the most used handbooks and texts in America at the end of the nineteenth century: Zeller, Heeren, Elmendorf, Falckenberg, Fischer, Ueberweg, Windelband, Erdmann, Harm, Schlegel, Hegel, Schopenhauer, Kant, Külpe, Schwegler, Deussen, Lübke, Hartmann, Volkmann, Beneke, Höffding, Ribot, Lotze, and Wundt (Pochmann 1957, 674).

Please note the last name in that list: Wundt. Like Brentano, Wundt had published a monograph on psychology in 1874.¹³ Contrary to Brentano, however,

¹³ There were certainly important similarities between the projects of Brentano and Wundt (and others at the time), such as the idea of a scientific psychology “without the soul” (see i.a. Textor 2021, ch. 2, §§ 3–7).

Wundt was enthusiastically received in the US at the end of the nineteenth century, no doubt due to the intercession of his numerous American students.

In the 1890s, Wundt's earlier US visitors and disciples were now in place at key institutions, from where their own disciples would soon embark for Leipzig. ... US students clustered in Wundt's laboratory and lecture rooms in the early 1890s. Between 1890 and 1893, five prospective American psychologists obtained PhD degrees under Wundt's supervision. The example of the little group suggests how closely knit the early network of US psychologists was, and to what extent American Wundt students assisted one another in furthering their careers. (Werner 2013, 159f.)

Many of their names are indeed quite well-known: "Frank Angell (PhD Leipzig 1891), Edward Pace (1891), Edward Scripture (1891), Edward Titchener (1892, British), and Lightner Witmer (1893) knew one another at Leipzig" (Werner 2013, 160).

Among those who had previously studied at Wundt's lab in the 1880s we might also note James Cattell, Harry Gardiner, James Baldwin, George Mead, Hugo Münsterberg, and Stanley Hall, who was at Wundt's lab when it opened in 1879 and later became the first president of the psychological APA. James Creighton, who had also studied in Leipzig and Berlin in 1889/90 (Auxier 2005, 549) seems to have played an important role too: "His translation of the writings of Wundt materially aided in the introduction of German philosophy into America" (Pochmann 1957, 316). The "material aid" consists in having produced the first translation of Wundt's (1894) *Lectures on Human and Animal Psychology* together with Titchener, which went through numerous editions: 1894, 1896, 1901, 1907, 1912, etc., a further testament to the rapid spread and adoption of Wundt's paradigm in America. Creighton contributed to this in his various roles. For instance, Creighton contributed to the development of the Sage School of Philosophy at Cornell, which became the model for graduate education in philosophy across the US (Katzav and Vaesen 2022, 337; Auxier 2005, 550). Moreover, as one of the founders in 1901, in 1902 he became the president of the philosophical APA. The overlap between philosophy and psychology was also present at the institutional level: "The American Philosophical Association, founded in 1901, is, historically, an offshoot from the American Psychological Association founded ten years earlier" (Gardiner 1926, 145).

Hence, again, we see how in theory such an environment could have been fertile ground for a reception of Brentano and his school, but in practice, once we look at the details, was not. Brentano's absence is indirectly confirmed also by the fact that it took until 1921 for Titchener to discuss the rivalry between

Wundt and Brentano in any detail, which apparently had not been a concern in America until then:¹⁴

The year 1874 saw the publication of two books which, as the event has shown, were of first-rate importance for the development of modern psychology. ... one would look in vain for any sign of closer intellectual kinship between them; hardly, indeed, could one find a greater divergence either of tendency or of training. ... The student of psychology, though his personal indebtedness be also twofold, must still make his choice for the one or the other. There is no middle way between Brentano and Wundt. (Titchener 1921a, 108)

Brentano's approach had not been a serious rival in the US, while on the continent there had been several controversies between the School of Brentano and the followers of Wundt.¹⁵

The juxtaposition between Brentano's empirical (phenomenological) psychology and Wundt's physiological (experimental) psychology became the discriminating element between the 'German' and 'Austrian pathway' to scientific psychology. (Antonelli 2018, 13)

I think we can safely say that "Austrian psychology" didn't really have a significant impact on American debates at the time. Those who seriously engaged with Brentano all come from a later generation of students. A brief search of the *Dictionary of Modern American Philosophers* shows that all of those who engaged with Brentano on American soil, natives as well as migrants, critics as well as proponents, were born after the turn of the century: Hall (1901–60), Gurwitsch (1901–73), Findlay (1903–87), Spiegelberg (1904–90), Bergmann, G. (1906–87), and Chisholm (1916–99). All, except Alain LeRoy Locke (1885–1954), whose 1918 dissertation con-

¹⁴ In Titchener's (1912) article on "The Schema of Introspection," Brentano is mentioned only once, to distinguish his phenomenology from that of Husserl: "The phenomenology that I have in mind is, of course, that of Husserl, and not that of Brentano and Stumpf. An adequate discussion would require another article; I can here only express my opinion that no form of phenomenology—phenomenology of mind, *Gegenstandstheorie*, science of selves—can be truly scientific, for the reason that the implied attitude to experience is multiply motivated and fluctuating, while the *minimum* requirement of science is a fixed and constant point of view." Please also note that he, like others, classes phenomenology together with *Gegenstandstheorie*, even though Meinong is nowhere mentioned in the article.

In Titchener's "Functional Psychology and the Psychology of Act: I" he explicitly distinguishes the American and German approaches: "Functional psychology, in this sense, is especially American, and the psychology of act especially German" (Titchener 1921b, 519). Clearly act-psychology in Brentano's style had not become a serious competitor on the other side of the Atlantic.

¹⁵ See Ierna (2009, 498f.). Before studying with Brentano in Vienna and Stumpf in Halle, it might bear mention here that Husserl, very briefly, attended lectures in philosophy by Wundt while studying astronomy in Leipzig (Schuhmann 1977, 4).

cerned the theory of value, and Helen Huss Parkhurst (1887–1959), whose 1917 dissertation on “Recent Logical Realism” we will discuss in more detail later on.

We already noted how Brentano’s 1874 *Psychology* appeared seemingly at the right moment, when German philosophy was being introduced in the curriculum, and yet ultimately lost out due to the preference for Kant and German Idealism on the one side and Wundt on the other. What about Husserl? Did his works appear at a favorable moment to be translated and transmitted across the Atlantic?

While there might have been an important moment of impact and growth of German philosophy right after the publication of Brentano’s main work in 1874, the period following Husserl’s *Logical Investigations* in 1900/01, was much more self-consciously aware of this fact. Consider, for instance, the address held by William James at the opening ceremony of Harvard’s Germanic Museum on November 10, 1903: “Our university, like most American universities, is Teutomaniac. Its ideas of scholarship and of the scholarly character have been inspired by German rather than by French or English models” (James 1904, 32; Tatlock and Erlin 2005, 11). Or the general sentiment at the 1904 St. Louis World Fair, which also encompassed the International Congress of Arts and Sciences organized by Münsterberg: “a pivotal moment of American-German academic exchange” (McClelland 2020, 202). Indeed, many if not most of the international speakers were from Germany or had been educated in Germany (Münsterberg 1904, 6–7).

American academia by this point had certainly matured into a partner of comparable stature to its European inspirations. Rather than passively absorbing theories and methods imported from elsewhere, by this point there was a much more autonomous discourse in philosophy in the US. Thanks in no small part to the founding of institutes, organizations, and journals, such as the Sage school, the APA, and the *Philosophical Review* (all linked to Creighton), America had developed its own schools and movements, not just by picking and choosing fashions from the old continent, but grown on its own soil. Anything coming from across the pond would be selected and interpreted in light of ongoing local debates and discussions. Given that both Brentano as well as early Husserl (as well as the nascent current of analytical philosophy) were connected to a criticism of idealism (broadly understood) from a realist standpoint (broadly understood), we should take into consideration how the ongoing debates in America about realism¹⁶ affected the re-

16 These are also mentioned by Gardiner as part of the important developments of American philosophy and discussions at the meetings of the APA: “we have reflected in our meetings the best thinking of American philosophers in our time. The future historian, inspecting our records, would be able to derive from them a not inaccurate conspectus of the state of philosophy in America in the first quarter of the century. ... He would discover among those who had contributed to our discussions the names of those who have been leaders in some of the most significant philo-

ception of the early phenomenological movement. In which respects was it understood differently than on the continent, given the differing background and debates?

5.4 Phenomenology: Real and Realist

Starting at the turn of the century various works appeared with “realism” in the title (e.g. in Montague’s (1902) “Royce’s Refutation of Realism” and Perry’s (1902) “Royce’s Refutation of Realism and Pluralism”), which then bloomed into a movement self-identifying as “New Realism”: the 1910 “Program and First Platform of Six Realists”¹⁷ and the 1912 *New Realism: Co-operative Studies in Philosophy*. These were soon followed by Roy Wood Sellars’ (1916) *Critical Realism* and the 1920 collective volume *Essays in Critical Realism*.¹⁸ In various contexts, the development of New Realism was linked by commentators to earlier developments on the other side of the pond.¹⁹

sophical movements of our time; I refer, of course, to such movements as those of Pragmatism, of the ‘New’ and ‘Critical’ Realism, and the Theory of Value” (Gardiner 1926, 157).

17 “The group consisted of Perry and Holt from Harvard, Marvin and Spaulding from Princeton, Pitkin and myself from Columbia” (Montague 1937, 142). A connection between Husserl and the six realists was already noted by Gurwitsch in 1940, see Strassfeld (2022, 36), also see Willard (1995, 165 n. 26). The most extensive and integrated treatment of the realism(s) of Brentano, Meinong, early Husserl, Russell, Moore, and the Six Realists can be found in the introduction to Chisholm’s (1960) *Realism and the Background of Phenomenology*.

18 With contributions by Durant Drake, Arthur Lovejoy, James Pratt, Arthur Rogers, George Santayana, Roy Wood Sellars, and C. A. Strong.

19 Also see Textor (2021, ch. 9) for an interesting comparison of four types of positions with respect to the status of the elements of the tripartition “act content object,” as originally described by Russell in 1919. According to Textor (2021, 215–217): 1) Brentanists would accept the tripartition, 2) Cambridge Realists (including Russell pre-1918) would “reject the distinction between content and object, but argue that the distinction between act and object can be discovered in introspection,” 3) Neutral Monists such as Mach and the American Realists (including Russell post-1918) “reject the distinction between act and object as a fundamental distinction given in consciousness,” and finally 4) British Idealists “agree with Neutral Monists that experience is non-relational”, but do take it to be fundamentally mental. In Russell’s words: “American realists ... have rejected both the act and the content, and have kept only the object; while idealists, in effect if not in words have rejected the object and kept the content” (Russell 1919, 25). Russell himself declares that an analysis into act and object is no longer satisfactory: “I am at a loss to discover any actual phenomenon which could be called an ‘act’ and could be regarded as a constituent of a presentation.” The inspiration for this change was “reinforced by the arguments of James and the American Realists” (Russell 1919, 25f.).

there has arisen almost yesterday another variety of speculation—perhaps the fifty-seventh—known as the New Realism. Influenced largely by Franz Brentano, Meinong and Edmund Husserl in Germany; by Bertrand Russell in England.²⁰ (Siegfried 1926, 310)

Indeed, there certainly had already been a “revolt against idealism” on the continent in the second half of the nineteenth century, followed soon by the British revolt against idealism by Russell and Moore, who were inspired at least in part by Brentano and his students.²¹ The contention that the American developments might be a continuation of this broad trend is hence not very surprising, but needs to be examined in more detail.²² Can we find general characterizations of their positions that would match central aspects of early phenomenology as well as explicit links between the schools and movements on both sides of the Atlantic?²³

Frank Thilly (first president of the Western APA) seems to provide both elements:

The contemporary philosophies of the United States are largely characterized by their opposition to traditional idealism. ... The new realists accord full ontological status to things of thought (subsistences) as well as to physical entities (existences); things are real which do not exist, for to exist means to be in space and time. (Thilly 1926, 522, 526)

The opposition to German Idealism and an alignment with the natural sciences are key ingredients in distinguishing Austrian philosophy from German philosophy (Haller 1991, 50; Ierna 2020, 99 f.). The second passage in the quotation quite directly references the controversy between Brentano and Meinong, which was also central to several of Russell’s works at the beginning of the twentieth century. This analysis is echoed by Roy Wood Sellars:

²⁰ Husserl is not quoted in the book *New Realism*, Meinong only indirectly through Russell, and Brentano only by Holt for his analysis of color in his 1907 *Untersuchungen zur Sinnespsychologie*, and not the *Psychology from an Empirical Standpoint*.

²¹ Sellars (1927, 507): “Mr. G. E. Moore fired what is usually considered the opening gun of the attack upon idealism in his essay entitled, ‘The Refutation of Idealism’. This was published in *Mind* in 1903”; Also see Bell (1999) for the (Brentanian) background and context of this “coup” against idealism.

²² On Russell’s relationship to the new realists and the critical realists, also see Neuber (2024a). On Perry and the New Realists as precursors of analytic philosophy, see Neuber (2024b).

²³ “More than any other twentieth-century philosophers, Meinong and Brentano share the concerns both of British and American ‘realism’ and of what Husserl calls ‘phenomenology’. In this respect, they are less provincial than are subsequent philosophers in either tradition” (Chisholm 1960, 12).

This realistic movement is likely to seem tremendously important to the thinker who has participated in it. He sees it as a right-about-face in Anglo-American philosophy. Not only was it a protest against the perspective and assumptions of traditional idealism; it was also an effort to build afresh upon the foundations of the sciences. (Sellars 1927, 503)

The “about face” Sellars mentions is evident in the shift away from German Idealism and towards the newer logic as well as laboratory-backed psychology and philosophy of mind in the style of Wundt. Nevertheless, Sellars also sees a difference between American and English realism:

Thus we shall see that the English realists have had a somewhat different notion of mind and mental acts than have Americans. To what does this go back? Perhaps to the influence of G. E. Moore and Russell, and perhaps from them to the work of Meinong and Brentano? (Sellars 1927, 504f.)

The distinction upon which Mr. Moore puts so much stress is characteristic of this English type of realism. It is that between the mental act of awareness and the object of that act. ... Whether this distinction between act and object was suggested by Brentano and Meinong or was worked out by himself I do not know. (Sellars 1927, 507f.)

Sellars underscores here the kind of logical realism that accords a certain ontological dignity to the contents of a judgment, which is enabled by Brentano’s notion of intentionality.²⁴ The link between the British realists and the School of Brentano is clearly noticeable. Brentano himself already distinguished between act, content, and object of a mental act in his lectures and then his students took over and refined this distinction, e.g. in Meinong and Höfler’s (1890) *Logik* as well as in Twardowski’s (1894) “Content and Object.”

This was also noticed by Knight, who pointed to Husserl as the more recent and influential author and to Bolzano as a noted predecessor in this respect:

One of the chief characteristics of German philosophy since the beginning of the century has been the development of theories which hold that reality is not co-extensive with existence. Among the most influential of these theories has been that of Edmund Husserl. Husserl, now Professor of Philosophy at Freiburg, has himself been influenced by Franz Brentano, of whom he was a student, and by Bolzano. ... Great stress is laid by Husserl, as indeed by other Ger-

24 Linke (1926) also underscores this. Brentano’s notion of intentionality is the “key” to explain “how consciousness reaches objects.” Both early Husserl and Meinong fruitfully combined the doctrines of Brentano and Bolzano, but Husserl’s further development of (transcendental idealist) phenomenology in the 1913 *Ideas* “proved disappointing” because he moved away from the object side. The main tendency in German logic, according to Linke, is “anti-psychological” and based on a *Gegenstandstheorie*: “The men who deserve the greatest credit for the increasing success of this tendency are B. Bolzano and F. Brentano.”

man realists, on “intentionality,” in virtue of which the mind can know, and have other relations to, objects external to itself. (Knight 1927, 79f.)

We see here that “intentionality” is considered important not so much for the analysis of mental acts or as a demarcation criterion for the mind, but rather for the ontological (and metaphysical) issue of the relation between reality and existence. This, rather than “the notion of mind and mental acts” is what rendered Austrian Philosophy so interesting to a whole generation of British and American philosophers and is perhaps made most clear in the notorious lecture course proposed by Ryle:

I even offered an unwanted course of lectures, entitled ‘Logical Objectivism: Bolzano, Brentano, Husserl and Meinong’... I was right in thinking that their Meaning-theories would reflect some light on and borrow some light from the partly parallel doctrines of Frege and of Moore and Russell in their early Edwardian days. (Ryle 1970, 8)

Ryle is very explicit about the context in which he is reading Husserl, and that he has no interest in Husserl’s transcendental idealist phenomenology:

A good deal of phenomenology does indeed get into the second edition of Husserl’s *Logische Untersuchungen*, which is what I was reading alongside of Meinong, Frege, Bolzano, Moore and Russell.... I realized pretty soon that Husserl’s intentionalist, anti-psychologistic theory of Meaning/Nonsense, which was what interested me, owed nothing to his posterior Phenomenology, and bequeathed too little to it. ... Fairly soon in the 1930s I dropped my rather solitary studies of Husserl’s *Logische Untersuchungen* and of his phenomenology. (Ryle 1970, 9)

Ryle was intimately familiar with phenomenology at the time, having traveled to Freiburg and met Husserl and Heidegger, but his sympathies clearly lay with Husserl’s earliest phenomenology from around 1900. In his 1932 article on “Phenomenology” he proceeds to outline first and foremost Brentano’s descriptive psychology under this header; then Husserl’s early, realist phenomenology, and finally to criticize and disagree with his transcendental idealism:

Phenomenology seems to have turned in Husserl’s hands into an egocentric metaphysic. But this seems to be the result of one or two false theories which need never, and should never, have trespassed into the analysis of types of mental functioning. (Ryle 2009 [1932], 181).

In so far as Husserl is still working in the same tradition as Brentano and Meinong, Ryle endorses his phenomenology, in so far as he is moving into a more substantive (egological) metaphysics, Ryle rejects it. While Ryle certainly had a significant influence in later years (see e.g. Thomasson 2002, § 6), it is difficult to estimate in how far his views on “phenomenology” affected the American context in the 1920s.

Ryle is however quite explicit in his assessment of Husserl's influence in Great Britain:

[Husserl] aroused almost no interest in the English-speaking world at this time [between the *Logical Investigations* and the *Ideas*]. His thunder had been stolen by Meinong and Frege on the Continent, by Moore and Russell at Cambridge, and by Cook Wilson in Oxford. His message was *démodé* before it was heard of. (Ryle 2009 [1946], 227)

In this respect, Ryle would agree that early phenomenology, that is to say pre-transcendental pre-idealistic phenomenology, did not take root in the British Isles at the time, at least not under the banner of “phenomenology” (which Ryle considered an unfortunate and infelicitous label anyway). In Ryle's analysis, it is rather Husserl's theory of meaning in so far as it was continuous and blended in with other contemporary and local theories that allowed him any reception at all.²⁵

The early phenomenological movement and the core notion of intentionality were hence tied up with a very specific debate, mediated by the reception of Russell and Moore, linked to the theories of Bolzano and Frege, and brought in connection with “logical objectivism” and “logical realism.” Certainly this was a very different kind of environment for Brentano and the early Husserl than Munich. If we identify the origin of the Phenomenological Movement (in the stricter Husserlian sense) with the physical movement of his students from Munich to Göttingen in 1905 (Ierna 2020, 104; Parker 2021, 15), can we then not also find American students who took Husserl's theories from the continent to the US and kickstarted the movement across the Atlantic?

Indeed, there are several examples of students from the US, specifically also from Harvard, that went to study with Husserl at the beginning of the twentieth century. A famous example in this respect is Winthrop Bell (Bell 2019, 28f.; Strassfeld 2022, 37f.). After studying in Leipzig for a year (also attending lectures by Wundt), he became interested in phenomenology and went to Göttingen to study with Husserl from 1911 to 1914. However, due to the outbreak of WWI, Bell did not get the chance to travel back across the Atlantic to bring his new insights home. Indeed, he was arrested and imprisoned right at the outbreak of the war. The faculty went to examine him in prison and Bell was able to successfully defend his dissertation “Eine kritische Untersuchung der Erkenntnistheorie Josiah Royces,” but remained interned until the end of the war in 1918. He returned to Canada in 1920, officially received his doctorate only in 1922 and started to teach at Harvard, but left academia in 1927. Hence, Husserl's first North American doctoral

²⁵ Ryle's interest in and appreciation of the early phenomenological theory of meaning is also testified by his apparent closeness to Brentano, as argued by Dewalque (2021).

student, did not really have the opportunity to significantly influence the reception of phenomenology.²⁶ Also other examples that could be given from the 1900s and 1910s were abortive.²⁷ However, there is one other vector of transmission that is relevant and interesting to examine in this connection, which also probably is the first book-length treatment of the School of Brentano in the US.

This link is Helen Huss Parkhurst (Waithe 1995, 346–347; De Waal 2005, 1868–1869). She studied with Theodore and Grace de Laguna at Bryn Mawr, went to Europe in 1913–14 to study in Cambridge and at the Sorbonne. At that point her trajectory differs from Bell's, because when she was caught in the war zone, she did manage to return home and finish her dissertation in the US. She obtained her PhD at Bryn Mawr in 1917, would later become a professor at Columbia, was active in the APA, and collaborated repeatedly with Montague. What is of interest to us here is the content of her dissertation "Recent Logical Realism."

While Parkhurst originally wanted to write on aesthetics, we know from Russell's correspondence that there was little chance for her to work with someone on that topic.

Miss Parkhurst turned up safely, & I have found her a nice person, as you said she was. ... There is nobody here who can help her with aesthetics, & not much to be got in the way of ethics, but she seems to take to the sort of stuff I teach, & I hope it will be useful to her. (Russell to Donnelly, October 19, 1913; Forte 1988, 208)

The "sort of stuff" Russell taught was much more focused on logic, language, and mind, moreover, he thought that the best place to study (for her or perhaps in general) was Austria: "It seems to me folly for her to go to Paris ... On the Continent, I am inclined to think Austria the best country for philosophy at present."²⁸ At the time Meinong was a professor in Graz, and Masaryk and Marty (who died on October 1, 1914) at the German University of Prague. As Sebestik (1994, 215) reports: "Brentano's pupils occupied most of the chairs of philosophy in the Austro-Hungar-

26 "Other early American visitors included Walter B. Pitkin, Winthrop Bell (Canada) and Arthur Chandler, who, however, did not spread the word of phenomenology after their return" (Spiegelberg 1982, 663).

27 Consider Pitkin's failed attempt at translating the *Logical Investigations*, Münsterberg chasing Hocking away from Husserl, etc. and the other examples given in Strassfeld (2019, 4): "Thus, on closer examination the early history of phenomenology reveals itself as a story of lost opportunities on American shores."

28 Russell to Donnelly, April 18, 1913 (Forte 1988, 204). The suggestion to also visit Paris may have come from Donnelly herself, who had also studied at Oxford and the Sorbonne. A further factor might have been Parkhurst's interest in aesthetics and art.

ian Empire. Probably more Brentanians were active in Prague than anywhere else.”

In her dissertation, Parkhurst thanks Russell “for first directing my interest toward the subjects treated in my dissertation” (Parkhurst 1917, 67). These subjects include a critical discussion of “logical realism,” and as Parkhurst points out right at the beginning: “Of realism as of idealism there are many distinct varieties, and logical realism is only one of several outgrowths of the great contemporary realistic movement” (Parkhurst 1917, 5). In the context of this realistic movement, she links together precisely the various strands we also indicated above: “There is, for example, the realism of Külpe, of Russell, of the six Americans, of Mach, and of Frege, as well as that of Meinong and Husserl” (Parkhurst 1917, 5).

In order to bring out as forcefully as possible the fact that Husserl’s reception was not mediated by Brentano in the US as it was in Munich and that “phenomenology” was received in a completely different fashion, I present the following quote, in which the “School of Brentano” is presented not as the students of Brentano, but as the heirs of Bolzano:²⁹

It is only in recent years, with the revival of interest in Bolzano and in the kind of thinking which he initiated, that *Sätze an sich* have been reinstated. At present they go by a variety of names. Marty calls them *Urteilsinhalten*; Meinong, *Objective*; Husserl, *Bedeutungen an sich*; Stumpf, *Sachverhalten*. (Parkhurst 1917, 6)

The dissertation then focuses chiefly on Meinong, with Husserl’s *Logical Investigations* as a secondary source.³⁰ Parkhurst immediately points out that in this context the “fundamental dogma of logical realism” is intentionality, which gives her the one and only occasion to mention Brentano.

This doctrine, though it takes as its point of departure a particular view of the nature of consciousness, throws its main emphasis upon what is neither consciousness nor the product of

²⁹ The link between Husserl and Bolzano can be also found in Ewald (1911, 604): “Pure logistic (Logismus), as its renovator Husserl has pointed out, finds its real origin in Bolzano.” It was later discussed in more detail by Linke (1926), who explicitly links the endeavors by Husserl, Frege, Lotze, and Bolzano.

³⁰ Also see Ewald (1909, 529): “the phenomenological inquiries and theory of objects (*Gegenstandstheorie*) of such pure logicians as Husserl, Stumpf, and Meinong.” Early phenomenology and theory of objects were considered as parallel or even joint enterprises. Likewise Ewald (1914, 617, 626): “Meinong’s *Gegenstandstheorie* has many points of contact with Husserl’s phenomenology, and both authors recognize this relation.” Switching the place of prominence, but keeping the connection, much later Lanz (1924, 521) still could claim: “Thus Meinong’s *Gegenstandstheorie* appears as a mere corollary of the phenomenological principle.”

consciousness. Its concern is to establish the ‘something’ postulated as the invariable correlate of acts of judgment and conception. (Parkhurst 1917, 10)

The focus of the work is then not on the “nature of consciousness,” but on the objects of consciousness which allow the specific Bolzanian type of logical realism. Hence, it is not Brentano’s descriptive psychology that paved the way for Husserl, but merely an instrumental role of his conception of intentionality as well as a mediation of Bolzano’s theories. Indeed, Brentano often lamented that Meinong and Husserl had lost the way by misunderstanding him and misinterpreting Bolzano’s Platonism. It is neither orthodox Brentanism nor Husserl’s post-1913 “transcendental idealist” phenomenology that was of interest to the American philosophers in this period. Hence, in so far as there was a reception at all, it was co-opted by the local debates at the time. In Parkhurst’s dissertation there is no mention of “phenomenology,” but Husserl is recruited as support for Meinong’s *Gegenstandstheorie*. Given Russell’s role of introducing her to the subject, this is not so surprising. As Parkhurst argues, “the core of the whole doctrine” is that the objects about which we judge “do not depend upon the occurrence of the acts by which they are apprehended. They are self-subsistent”:

The god Jupiter who, as Husserl says, is not to be found as a constituent of the intentional experience which refers to him, is neither an ‘immanent’ mental object nor extra-mental. He is *not*. A centaur, adds Husserl, is nothing and nowhere. Yet Jupiter and centaurs, along with the round-square, the square root of -1, and the principle of the parallelogram of forces are subsistent *Gegenstände*. (Parkhurst 1917, 13)

While this argument is to be found in *Ideas I*, it is taken here to be broadly in agreement with the 1900/01 *Logical Investigations*.³¹ Meinong and Husserl are taken both to endorse *Gegenstandstheorie* as a realist, anti-psychologist approach: “Realism thus believes itself to be a doctrine of objects, not only as they appear in discourse, but as they are in themselves, in their non-spatial, non-temporal, non-psychical being” (Parkhurst 1917, 13). Parkhurst allows us hence to draw a quite straight line from Meinong (and Husserl), through Moore and Russell, to the American debates on realism, stating that the origin of modern realism lies not in Moore’s (1903) “The Refutation of Idealism”:

31 “Although Husserl was to accept a version of idealism, the origins of his phenomenology were not unlike those of British and American realism. The first volume of his *Logische Untersuchungen* (1900) is an important defense of a realistic—as opposed to a ‘psychologistic’—interpretation of the truths of logic” (Chisholm 1960, 12).

But in the opinion of this reviewer, our chronology of modern realism might better take its start with the year 1901, the date of the preface to the first edition of Meinong's *Über Annahmen*, which inaugurated the modern phase of Platonic or subsistential realism. (Parkhurst 1930, 46)

The connection of Husserl's early reception in the context of continental "logical realism" and American "new realism" to phenomenology, explicitly by name, is also made by one of the New Realists themselves:

what the New Realists called the domain of 'subsistence', and what Plato called the world of 'Ideas' was the subject-matter of what Husserl called 'phenomenology.' The philosophers just named had recognized the new phase of Being and made their bow to it. But to Husserl belongs the honor of initiating a systematic study of its nature and structure. (Montague 1939, 232)

This explicit connection to "phenomenology," however, should still be read in the context of Husserl's realist period. The "subsistence" and "logical realism" here serve simply as a reformulation of Husserl's anti-psychologism: "That $7+5=12$ is entirely explained by the natures of seven, of five, and of twelve, and not in the least by the nature of consciousness" (Montague 1937, 145).³²

5.5 Concluding Remarks

Can we answer our main question "Was North America Fertile Ground for the early Phenomenological Movement?" now that we have zoomed into this level of detail? We started with general considerations about the philosophical climate in North America. We saw that it was indeed receptive to German philosophy in the period when Brentano's major work, the *Psychology from an Empirical Standpoint* was published. If we take this work as the beginning of the phenomenological movement, as Spiegelberg also tends to do, calling Brentano the grandfather of phenomenology, then we must conclude that unfortunately it did not have a significant reception at the time. The atmosphere was more favorable to German Idealism and Wundt, both of which were opposed by Brentano and his students. Moving on from the 1870s and 1880s, the climate certainly remained favorable towards German philosophy and led to abundant exchanges of people, works, and theories.

³² This was already noted by Ewald (1906, 252): "Logical laws are independent of the fact of their being apperceived by an individual. ... In contradistinction to psychologistic investigations, which direct their attack against pure logic, Husserl has called his studies (undertaken in the service of pure logic) 'phenomenological'."

Yet, debates in the US had moved on to such topics, that Husserl's early works underwent a very one-sided reception. There was a secondary reception of Brentano's ideas through the works of Meinong, Russell, Moore, and Husserl, which became part of the debate about (logical) realism. Famously, at the "Fifth International Congress of Philosophy" to be held in London in 1915 (which of course never took place), a general session would have been dedicated to "Realism" with "Professors Perry, Meinong, Husserl, Strong, and Alexander."³³ Husserl's position was not fully recognized as "phenomenology" at the time.³⁴ The element that made it interesting to American authors in the early twentieth century was not an analysis of the intentional structure of consciousness or how the world was disclosed in experience, etc. but merely the fact that intentionality seemed to enable a specific realist position about logical entities. Husserl was invited in as a fellow traveler of Meinong (and as a disciple of Bolzano). Husserl's earliest reception before the 1930s and '40s, was generally not a reception of his phenomenology as such, but rather of his "phenomenology" as anti-psychologism and a form of *Gegenstandstheorie*. If by "early phenomenology" we understand (broadly) the theories of Brentano and Husserl up to his switch to idealism, then there barely was any reception of early phenomenology as such in the US and we cannot really speak of "fertile ground." Phenomenology as such took root in the American ecosystem only later, with students like Farber and Cairns who had studied with Husserl during the 1920s.³⁵ While it is certainly true that "Husserl did not go entirely unmentioned in American publications in the first two decades of the twentieth century either" (Strassfeld 2019, 12), it was not his project of phenomenology as a new foundational method that attracted the attention of the American Realists, it was rather his work on mathematics, logic, and the theory of meaning, the particular blend of Brentano and Bolzano he shared with Meinong and (up to a point) Russell: indeed a story of lost opportunities.

³³ See the announcement in *The Monist*, October 1914, 637.

³⁴ Perry (1914, 388) will even go so far as to speak of "the present-day revival of 'phenomenology' by Husserl, Meinong, and Russell," making it clear that the American realists are not using "phenomenology" in the same sense as Husserl himself did, but rather to reference to the broader movement started by Brentano, which influenced all three of the authors in the quote, and specifically in so far as it can contribute to their own concerns with (logical) realism.

³⁵ "Phenomenology could not take real root in the States until the early twenties when Marvin Farber (Freiburg 1923–24) and Dorion Cairns (Freiburg 1924–26 and 1931–32) after their return from Germany wrote American dissertations on Husserl's phenomenology" (Spiegelberg 1982, 663).

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Jonathan Strassfeld

6 Reestablishing Phenomenology in America

Abstract: Many of Husserl's disciples were deeply committed to his vision of phenomenology as a cooperative and foundational enterprise. However, even before his passing in 1937, Husserl's movement, disproportionately composed of Jewish and liberal-Catholic philosophers, faced an existential crisis. Its members were removed from academic posts, forced into exile, and even lost their lives to the Nazi regime. A substantial number of those displaced philosophers found refuge in the United States, aided by connections formed through a pattern of transatlantic exchanges between Harvard and Freiburg. This essay traces the routes émigré phenomenologists followed through the American academy and shows how the networks they established in the early twentieth century shaped the institutions they created on American shores and allowed them to reestablish phenomenology as a philosophical movement in the United States after the death of its founder.

By the 1930s, Edmund Husserl's phenomenological philosophy had become an important philosophical movement in Germany, with its influence quickly spreading across the European continent. However, the rise of Nazism and the death of Husserl upset the foundations of this program. While some adherents, most notable Martin Heidegger, exploited the situation, the group of disproportionately Jewish and liberal-Catholic philosophers who had taken up Husserl's cause were persecuted, enduring professional dismissals, exile, and murder. Like most intellectuals who fled from the Nazis, the phenomenologists who escaped war-torn Europe went West, converging on the United States as a refuge or new home. Although these endangered philosophers took whatever opportunities were available to preserve their lives, families, and careers, the pattern of the phenomenological migration was not entirely a product of chance. Rather, its path was paved by three decades of engagement between American philosophers and their movement, which made possible the reestablishment of phenomenological institutions and intellectual communities on American shores.

The first contact between personal American philosophers and Edmund Husserl was made by William Ernest Hocking, then a graduate student under Josiah Royce at Harvard. Following the advice of the Marburg Kant scholar Paul Natorp, who was then lecturing at Harvard, Hocking sought out Husserl in Göttingen in 1902 under the auspices of a Harvard travel fellowship (Hocking 1959, 5). While

Hocking's visit to Göttingen was cut short at the behest of Hugo Münsterberg, who chaired the committee that granted Hocking's stipend, Husserl and Hocking forged a durable connection.¹ Hocking enrolled in courses on logic and epistemology with Husserl and attended his seminar on Hume. However, "it would be more accurate," Hocking wrote, "to say that this study is under the guidance of Professor Husserl himself, for he has been kind enough to take a personal interest in my work, and has given me every opportunity to converse with him and bring him my questions."² The philosophers remained correspondents and friends for the rest of Husserl's life. Despite its brevity, this first encounter established the trajectory of phenomenology's development in America over the next three decades, as interest in phenomenology continued to grow at Harvard, fostered by Hocking and the neo-realist philosopher Ralph Barton Perry. Facilitated by Harvard's generous travel fellowships, student pilgrimages to study phenomenology in Germany resumed in 1912 and became common in the 1920s. Between 1923 and 1937, at least ten Harvard students, including Paul Weiss, Charles Hartshorne, Dorion Cairns, William Frankena, and Charles Malik, attended lectures by and, in some cases, studied with Husserl or Martin Heidegger directly (Strassfeld 2022, 42–51).

One of these itinerant Harvard students was Marvin Farber, a PhD candidate in Philosophy, who traveled to Germany in 1922 under the auspices of Harvard's prestigious Sheldon Fellowship, carrying a letter of introduction from Hocking.³ Few details of Farber's subjective impressions of Germany have been preserved beyond his account of Husserl "an eminently fine teacher" whose "personal guidance and instruction" he was "fortunate" to have.⁴ However, the diary of the British philosopher W. R. Boyce Gibson, who spent four months of 1928 in Freiburg, provides valuable evidence of the unique conditions that shaped the reception of phenomenology by the foreign philosophers who studied the movement on its native soil. Husserl fostered an ethos devoted to collaboration that called on his students "to take his ideas and work them for all they can." It was like, Gibson wrote, being conscripted as "privates in the great *Wissenschafts-armee*" in which "each of us disinterestedly contributes his quota" (Spiegelberg 1971, 65). From Freiburg, Farber wrote to Hocking that he was so impressed by Husserl that he quickly "formed a resolution to sit at the feet of this sage." Husserl's *Logical Investigations* and *Ideas* became Farber's "daily companions" as he remained at Freiburg for the

1 Hocking to Münsterberg, December 19, 1902, Box 18, Folder "Hugo Münsterberg (1 of 2)," William Ernest Hocking correspondence, 1860–1979. Houghton Library, Cambridge, MA (hereafter WHC).

2 Hocking to Münsterberg, December 3, 1902, Box 18, Folder "Hugo Münsterberg (1 of 2)," WHC.

3 Farber to Hocking, January 8, 1923, Box 8, Folder 19, Marvin Farber Papers. University Archives: The State University of New York at Buffalo, Buffalo, NY (hereafter MFP).

4 Farber to James Woods. April 7, 1923, Box 25, Folder "Woods, James," MFP.

1923–24 university term with a second year's funding as one of Harvard's Parker Fellows.⁵ Farber returned to America in 1924 and received his PhD from Harvard in 1925. His dissertation, "Phenomenology as a Method and as a Philosophical Discipline," was published in 1928, becoming the first major study of Husserl's thought available in English (Farber 1928).

During the subsequent decade, American engagement with phenomenology shifted towards Husserl's apparent heir, Martin Heidegger. Indeed, when asked in 1934 to recommend "the most eminent scholars in [their] field" who might be honored and invited to lecture at Harvard's Tercentenary, its philosophy department named Martin Heidegger as its fourth choice, behind Carnap, who ranked third, but ahead of Bertrand Russell and G. E. Moore.⁶ Explaining their recommendations, R. B. Perry described Heidegger as "the leader of the most vigorous and promising of the more recent movements in German thought," reporting that he "is generally regarded as the most influential contemporary philosopher in Germany, and is beginning to make his influence felt in other countries including America and in particular Harvard."⁷

Despite America's growing interest in phenomenology, when émigré philosophers began seeking refuge from the Nazi regime in the United States, they confronted an academy facing severe financial exigencies due to the Great Depression in which appointments were vanishingly scarce, even for domestic scholars. Because of the dearth of regular positions, an ad hoc system that provided refugees with temporary appointments, funded by outside charities, was improvised by a complex of established charitable organizations and new aid groups. For many scholars it was a vital lifeline—however, because of its tenuous structure, many phenomenologists would spend years teaching variously among universities and departments, drifting between temporary appointments unsuited to their talents, before finding a permanent position that would allow them to work within their field.

The most successful philosopher-émigrés were those whose international reputations made them choice targets for universities. Still, even such eminences

5 Farber to Hocking, July 2, 1923, Box 8, Folder 19, MFP.

6 "Ballots," Box 3, Folder "Greene, J. D. + W. G. Land – Tercentenary 1934–5, 1935–6," Harvard University Department of Philosophy Records (1906–1979): correspondence and other records, ca. 1927–1938 (UAV 687.10). Harvard University Archives, Cambridge, MA (hereafter HDPR); Jerome Greene to R. B. Perry, October 11, 1934, Box 3, Folder "Greene, J. D. + W. G. Land – Tercentenary 1934–5, 1935–6," HDPR.

7 Perry to Greene, October 30, 1934, Box 3, Folder "Greene, J. D. + W. G. Land – Tercentenary 1934–5, 1935–6," HDPR; R. B. Perry to W. B. Land, December 12, 1934, Box 3, Folder "Greene, J. D. + W. G. Land – Tercentenary 1934–5, 1935–6," HDPR.

faced obstacles. For instance, when Harvard's philosophers discussed hiring Carnap in 1934, they concluded that, although he "would be an extremely stimulating person to have," the department's need of him was not "imperative" and it was "improbable" that his appointment would be beneficial to Harvard in the long-term.⁸ At the same time, the department's recommendations for the University's Tercentenary suggest that Husserl or Heidegger might have attained a position at a well-regarded philosophy department had they immigrated to the United States. Indeed, the University of Southern California did attempt to lure Husserl to Los Angeles in 1933 on either a visiting or permanent basis. However, the issue of the appointment of an assistant for Husserl proved to be a sticking point. With both parties still expressing interest in some arrangement negotiations stalled, and Husserl remained in Germany.⁹

For philosophers who could not rely on the strength of their international reputation, the most important American émigré organization was the Emergency Committee in Aid of Displaced Foreign Scholars, which was founded in the summer of 1933. The Committee served both financial and logistical functions. In the former role, it raised money and organized funds from larger organizations to provide grants to cash-strapped and often skeptical American universities. In the latter, it identified American universities with suitable positions for particular foreign scholars. When a match was made, the Committee subsidized the cost for a temporary appointment while the institution determined whether to offer them a permanent position (Krohn 1993, 27–29).

Despite the advantages this program offered American universities, significant resistance to hiring émigrés remained. Much of this was simple xenophobia. American attitudes towards refugees during World War II were deeply mixed, with overt expressions of hostility well within the mainstream. Louisiana State Law School, for instance, explained that they would not hire Gerhart Husserl, the son of Edmund Husserl, because the "combination of Jewish ancestry and undoubtedly broken English would place him at such a handicap with our somewhat provincial student body that it would be almost impossible for him to do the type

8 Perry to Kenneth Murdock, April 11, 1934, Box 4, Folder "Philosophy and Psychology," Records of the Dean of Faculty of Arts and Sciences: Correspondence of Dean K. B. Murdock, 1933–1934. Harvard University Archives, Cambridge, MA.

9 Ralph Tyler Flewelling to Husserl, October 26, 1933, Box 4, Folder "Visiting Professor," University of Southern California School of Philosophy Records. USC Libraries Special Collections, Los Angeles, CA (hereafter USCPR); Flewelling to Husserl, April 4, 1934, Box 4, Folder "Visiting Professor," USCPR; Husserl to Flewelling (Trans. by Dr. Von Koerber), March 11, 1934, Box 4, Folder "Visiting Professor," USCPR.

of work that we want.”¹⁰ Indeed, antisemitism was so commonplace in that era that those expressing overtly anti-Semitic views might deny holding any bias. For instance, Cornell University’s efforts to find employment for one of its Jewish students in 1940 elicited a letter from Dartmouth, stating: “We should not at present, however, appoint a Jew, not because we have any racial prejudice, but because we already have one full Jew and another man whose fractional status is anywhere from 1/8 to 8/8 in our Department.”¹¹ Moreover, the vast majority of European scholars in this diaspora were relatively unknown within the United States. Particularly at elite universities, many discounted the talents of this group, with philosophers facing particular suspicion. As one letter remarked: “there are always questions about these displaced foreign scholars and nowhere more than in philosophy positions.”¹² Despite these obstacles, most refugee phenomenologists found a home in America through the aid of benevolent organizations, family relations, and colleagues encountered through three decades of transatlantic exchanges.

The first phenomenologist to seek refuge in the United States was Moritz Geiger, a Jewish student of Edmund Husserl and Theodor Lipps who lost his position as a professor at Göttingen in 1933. Geiger had visited America twice before: first in 1907 to spend three months at Harvard with Royce, Santayana, Münsterberg; second in 1926 when he was invited to give lectures at Stanford and attend the International Congress of Philosophy, which was being held at Harvard.¹³ Well connected by the time of his third sojourn to America in 1934, Geiger quickly secured an appointment as chair of Vassar’s philosophy department with the help of the Emergency Committee and Columbia’s Frederick James Eugene Woodbridge, who arranged for a grant from the Rockefeller Foundation to cover Geiger’s salary.¹⁴ Geiger understood the incredible fortune of finding such amenable pro-

10 Frederick Beutel to W. E. Hocking, February 19, 1936, Box 4, Folder La-Ly, HDPR.

11 Maurice Picard to Gustavus Watts Cunningham, February 8, 1940, Box 22, Folder “Sage School of Philosophy Correspondence + Recommendations on Teaching Positions 1935–1952 D-I,” Dept. of Philosophy records, 1892–1952. Division of Rare and Manuscript Collections, Cornell University Library, Ithaca, NY.

12 Paul Anderson to Laurens Hickok Seelye, March 11, 1941, Box 32, Folder Herbert Spiegelberg (2 of 2), Emergency Committee in Aid of Displaced Foreign Scholars Records. New York Public Library, New York, NY (hereafter ECFSR).

13 Geiger to Henry Noble MacCracken, November 5, 1933, Box 11, Folder “Moritz Geiger (1 of 2),” ECFSR.

14 Hanna Hafkesbrints to Felix Warburg, September 28, 1933, Box 11, Folder “Geiger, Moritz (1 of 2),” ECFSR; Hafkesbrints to Warburg, October 3, 1933, Box 11, Folder “Geiger, Moritz (1 of 2),” ECFSR; Emergency Committee. 1933. “Status of the Moritz Geiger Case,” October 14,

fessional circumstances and a welcoming community for himself and his wife at Vassar. "These three months at Vassar have been for both of us a time of real happiness," Geiger wrote in May 1934. "All members of the faculty and all students have welcomed us like old friends, and the atmosphere is so full of humanity that we dare to think that the cause of liberty is not entirely lost."¹⁵ Praised for his "unfailing enthusiasm, imagination, and keen perception" as well as "the breadth of humanity and understanding," Geiger was awarded the college's annual "Vassarion" prize in 1937 (MacCracken 1937).¹⁶ His death after a brief illness several months later was both tragic and unexpected. In the wake of his passing, Vassar informed the Committee that "in spite of the very fine impressions that Geiger made" a faction of their faculty was opposed to appointing another German in philosophy. Vassar accepted no more refugee philosophers.¹⁷

In general, scholars whose work could contribute to the war effort had a professional advantage over others.¹⁸ Particularly after America's entry into the war, there was such demand for instructors of mathematics and sciences that proficient émigrés whose expertise lay elsewhere were recruited to teach courses in these fields. This was the case for Aron Gurwitsch, a phenomenologist specializing in the philosophy of psychology and the social sciences who sustained himself through much of the war by working as an instructor in physics. Gurwitsch immigrated to America in 1940, initially serving as a Visiting Lecturer in Philosophy at Johns Hopkins University.¹⁹ Although Gurwitsch was acutely aware of his fortune in escaping Europe, he had difficulty adapting to life in a country in which he remained an outsider. Both sentiments were expressed in his extensive correspondence with his fellow émigré and friend, Alfred Schütz. With war waging in Europe, the everydayness of life in America was an experience akin to a different reality for Gurwitsch. As one letter to Schütz reported:

We are very comfortable in my aunt's house. It is actually a rather fantastic story: we were intended for the concentration camp and then for everything that the Nazi invasion brought with it, and instead of that one finds oneself in this roomy house, one sits on the porch and works on Sartre's theory of the ego. One has to 'realize' all that. And where might Sartre be? At times I have the feeling that I am working on nothing other than obituary notices.²⁰

1933, Box 11, Folder "Geiger, Moritz (1 of 2)," ECF SR; Edward Murrow to Henry Stuart, January 15, 1934, Box 11, Folder "Geiger, Moritz (1 of 2)," ECF SR.

15 Geiger to Mary Waite, May 26, 1934, Box 11, Folder "Geiger, Moritz (1 of 2)," ECF SR.

16 MacCracken to Stephen Duggan, July 16, 1937, Box 11, Folder "Geiger, Moritz (2 of 2)," ECF SR.

17 MacCracken to Duggan, January 3, 1943, Box 14, Folder "Fritz Kaufmann (1 of 3)," ECF SR.

18 Aron Gurwitsch to Betty Drury, September 12, 1943, Box 13, Folder "Gurwitsch, Aron," ECF SR.

19 Gurwitsch to Drury, January 9, 1941, Box 13, Folder "Gurwitsch, Aron," ECF SR.

20 Gurwitsch to Schütz, August 5, 1940, in Grathoff (1989, 15).

In this letter, otherwise written in German, the word “realize” is in English, as though signifying the effort of translation between these two worlds, coexistent but so mutually foreign that one can only be made real within the other by an act of will. As Gurwitsch explained in another letter, “We are quite lonely here. And that not merely in the sense that we see so few people here. But even among those whom we see, we feel very alone, since we are still rooted in another world.”²¹

Gurwitsch had been among the first rank of Husserl’s students and made a considerable impression on his colleagues at Johns Hopkins from 1940–1942. Yet, despite his performance and qualifications the university refused to retain him, citing financial exigencies. Gurwitsch perceived other reasons for his dismissal, as he explained:

I am running the Graduate School practically alone... But what does that, as well as the fact that I use up the publication budget of our department all by myself, have to say when compared to my disqualifications: foreigner, Jew, unbaptized, Jewish wife, etc. *That* is too much. They could tolerate it if someone would buy me a *chair* for \$150,000. Then they would take me in the hope, as one of the professors... told me today, that I wouldn’t live all too long, such that the successor would be more acceptable in the social sense.²²

Gurwitsch was unable to find another position in philosophy in 1942, but sustained himself through a research grant from the American Philosophical Society and a “fellowship by courtesy” at Harvard, secured for him by R. B. Perry, who developed a personal connection to the phenomenological movement after visiting Husserl at Freiburg in 1930. Still without prospects for a permanent position in philosophy, Gurwitsch was able to retain his connection to Harvard by accepting a position as an instructor in Physics the following year. Hired to supplement its faculty for wartime instruction, Gurwitsch remained at Harvard from 1943–1946, teaching courses for army recruits.²³

The most significant migration of phenomenologists to America occurred between 1937–1939, bringing Felix Kaufmann, Fritz Kaufmann, Alfred Schütz, and Herbert Spiegelberg to the United States. In Germany, Fritz Kaufmann had served at Freiburg as *Privatdozent* and Husserl’s assistant. From 1933–1936 Kaufmann also offered *Volkshochschulkurse* to Jewish students denied entry to universities by Nazi race laws. The combination of political and financial pressures on the one hand, and the plight of Germany’s Jewish youth on the other, left Kaufmann

²¹ Gurwitsch to Schütz, August 23, 1940, in Grathoff (1989, 18).

²² Gurwitsch to Schütz, October 9, 1941, in Grathoff (1989, 41).

²³ Gurwitsch to Schütz, July 11, 1942, in Grathoff (1989, 58).

feeling “like a candle lit at both ends” and in search of means with which provide for his family.²⁴ A grant from the Society for the Protection of Science and Learning in London allowed Kaufmann to leave Germany for England in September 1936.²⁵ Kaufmann came to the United States in November of 1937 and, with the help of the Committee for Refugee Scholars, secured a series of temporary appointments in philosophy at Northwestern University. Northwestern’s philosophy department would maintain its association with the phenomenological movement into the postwar era, becoming the first institutional home of SPEP and publishing numerous important texts through its Northwestern Studies in Phenomenology series.²⁶

The Austrian philosopher Felix Kaufmann, unrelated to Fritz Kaufmann, also immigrated to the United States in 1938. Kaufmann was a philosopher of great breadth, whose interests ranged from the foundations of mathematics to the philosophy of law. Visiting Switzerland at the time of his country’s invasion by the Nazis, Kaufmann was warned, as both a Jew and an associate of Austria’s socialist party, against returning home.²⁷ A grant for a \$3,000 was quickly obtained from the Rockefeller Foundation to cover the majority of Kaufmann’s salary on the recently created Graduate Faculty at the New School for Social Research, where Kaufmann remained until his death in 1949.²⁸

The existence of The New School for Social Research dated only to 1919, when it was founded as a center for adult education by a group of progressive intellectuals that included Charles Beard and James Harvey Robinson. In 1933 its director, Alvin Johnson, initiated a campaign to raise funds for the creation of an affiliated graduate school. This new “University in Exile” would fill its faculty with scholars who had lost their positions under the Nazi regime. Given significant autonomy within The New School, refugee scholars were able to make the University a sort of “European scholarly enclave,” while simultaneously gaining a foothold in the American academic world. Indeed, Husserl’s movement was particularly suited to ethos of The New School, whose faculty was committed to a vision of interdis-

24 Kaufmann to Farber, September 14, 1937, Box 11, Folder “Kaufmann, Fritz 1937–39,” MFP.

25 “General Information: Fritz Kaufmann,” Box 14, Folder “Kaufmann, Fritz (1 of 3),” ECFSR; Kaufmann to Bernard Flexner, July 8, 1940, Box 14, Folder “Kaufmann, Fritz (2 of 3),” ECFSR.

26 Dalton Howard to Betty Drury, April 13, 1938, Box 14, Folder “Kaufmann, Fritz (1 of 3),” ECFSR.

27 “Excerpted from confidential memorandum (concerning Austrian scholars) submitted to the Emergency Committee with letter of May 5 from Malcom W. Davis,” Box 14, Folder “Kaufmann, Felix,” ECFSR.

28 Alvin Johnson to Stephen Duggan, May 24, 1938, Box 13, Folder “Kaufmann, Felix,” ECFSR.

ciplinariness that made philosophy the “generative center” of a broad humanistic education that stressed the “essential unity” of the social sciences.²⁹

In 1943, Alfred Schütz joined Kaufmann at the New School. With Kaufman, Schütz, and Husserl’s former student Alexandre Koyré on its faculty, The New School for Social Research had quickly become one of the world’s preeminent centers of phenomenological training expertise—a tradition that continued into the postwar era with the appointment of philosophers such as Dorion Cairns, Aron Gurwitsch, and Hans Jonas and the creation of an American branch of the Husserl archives. Graduates of the New School, including Maurice Natanson, Richard Zaner, Lester Embree, Helmut Wagner, and Thomas Luckmann, would be counted among the leading figures in the postwar generation of American phenomenologists (Krohn 1984, 104; Barber and Embree 2017, 174–384).

In this way, world events had brought a critical mass of phenomenological talent to the United States by the late 1930s. Because of the circumstances in which they arrived, reliant on an informal network of American acquaintances and European peers already established in America, émigré phenomenologists retained some semblance of community through the diaspora. That this cohesion would prove sufficient for a revitalization of the *phenomenological movement* on American shores was, however, far from guaranteed. Indeed, the desire for such a goal could hardly be taken as a given. Many refugee intellectuals, though grateful to their adoptive countries, would never come to think of them as their own. Instead, their eyes were set on a return to Europe and the reconstruction of their homelands following fascism’s defeat. Even those who wished to take root on American shores faced both a challenge—adopting a new language, new professional conventions, and navigating an unknown academic landscape—and a choice of whether to focus on an audience of their expatriated peers, or to attempt to engage a broader American philosophical discourse. Among the group of émigré phenomenologists were scholars particularly committed to the latter course. Still, it is doubtful whether phenomenology would have been restored as cooperative project in the United States if not for two factors: the death of Edmund Husserl in 1938, and the organizational efforts of Marvin Farber.

Despite his continuing work on phenomenology, Farber had maintained only occasional correspondence with his erstwhile teacher in the decade after his encounter with Husserl in Freiburg. However, he was drawn back into Husserl’s orbit in 1936 by entreaties from Edmund and his wife Malvine to assist their son Gerhart, a philosopher and legal theorist, in finding an academic position in

29 “Suggestions of the Coordinating Committee to The Graduate Faculty,” January 20, 1941, Graduate Faculty Minutes, Sept. 1933 – June 1945, Box 1, New School Archives. New York, NY.

the United States. Gerhart's arrival in America was facilitated by Herman Weyl, a mathematician and philosopher of mathematics who was among those recruited by Princeton's Institute for Advanced Study. However, the arrangement to deliver a lecture at Princeton held no long-term prospects. With only the patina of employment necessary to pass customs, Gerhart was safe from Nazi persecution, but still reliant on American acquaintances for assistance and Husserl was grateful for Farber's efforts on his son's behalf.³⁰

In a November 1936 letter, Husserl expressed his happiness at the news that Dorion Cairns and Farber had begun discussing translating his works. Thinking of the phenomenology's future, Husserl urged their cooperation in "συμφιλοσοφειν," which might be translated as "philosophizing together" or, more literally, "sharing a love of knowledge." Indeed, he wrote, "There is nothing more beautiful in this woeful world than a free συμφιλοσοφειν on the same ground, in sharing the same fundamental methodological beliefs!" In the coming years, Farber would take the project of phenomenological συμφιλοσοφειν in America farther than Husserl could have expected (Cho 1990, 30).³¹

Afflicted by pleurisy, Edmund Husserl died in April 1938. In the United States, Husserl's passing would reunite phenomenology's disciples, both American and European, in the project of paying tribute to their former teacher. The idea for a volume dedicated to Husserl seems to have originated with Farber. By mid-May, he had begun to discuss compiling a collection of essays by Husserl's "friends and pupils" with Fritz Kaufmann, who pledged that "for such a work you can count on my contribution as well as on my cooperation in every respect."³² Over the following months, he and Kaufmann compiled a veritable directory of Husserl's former students and acquaintances, phenomenologists, and fellow travelers residing in America (Spiegelberg 1981, 242–246).

Philosophical Essays in Memory of Edmund Husserl was published by Harvard University Press in 1940 and included articles by Dorion Cairns, John Wild, W. E. Hocking, Aron Gurwitsch, Herbert Spiegelberg, Felix Kaufmann, Fritz Kaufmann, and Alfred Schütz, among others (Farber 1940). The long work of commissioning and obtaining manuscripts renewed ties between those in America who had been touched by Husserl, defining the outline of a new Husserl circle with Farber at its center. However, the process of compiling and editing the collection also revealed host of obstacles to importing Husserl to America. For instance, clarity re-

³⁰ Edmund Husserl to Farber, February 18, 1936, Box 9, Folder "Husserl, Edmund and Mrs.," MFP; Malvine Husserl to Farber, February 17, 1936, Box 9, Folder "Husserl, Edmund and Mrs.," MFP; Flexner, Abraham. Letter to John Whyte, May 9, 1936. Box 75, Folder "Husserl, Gerhart," ECFSR.

³¹ Husserl to Farber, November 20, 1936, Box 9, Folder "Husserl, Edmund and Mrs.," MFP.

³² Kaufmann to Farber, May 21, 1937, Box 11, Folder "Kaufman, Fritz 1937–39," MFP.

quired the creation of a common lexicon of English translations for Husserl's technical vocabulary.³³ Further, even in Germany, Husserl's reticence to publish meant that his later philosophy was not widely understood. In America, many of those who had studied with Husserl in the 1920s or earlier remained entirely ignorant of these developments. This problem was highlighted by the first draft of Hartshorne's contribution, "The Social Structure of Immediacy," of which Fritz Kaufmann remarked: "God forgive him as he does not know what he is talking about."³⁴ Reluctant to refuse a paper already written by an invited contributor, Farber wrote to Kaufmann: "If we are to hope to introduce [phenomenology] to the American scene, we cannot afford to alienate friends or potential friends. Unanimity was never achieved in Germany; and it cannot be expected here" (Hartshorne 1940, 223–224).³⁵

As Farber's exchange with Kaufmann indicates, he understood that his editorial decisions could affect the contours of phenomenological development in America, selecting the membership of an informal inner circle, and determining criteria on which the group of philosophers claiming the name "phenomenology" might be organized. A product of the pluralist ethos that characterized Harvard philosophy in that era, Farber opted for an attitude of tolerance towards those who shared Husserl's aims. However, he also gave significant weight to the opinions of his principal collaborators, Kaufmann, Cairns, and Gerhart Husserl. It is impossible to know how different choices might have changed the history of phenomenology in the United States—as if, for instance, Farber had solicited material for the volume from Max Horkheimer or Herbert Marcuse as suggested by Fritz Kaufman, rather than deferring to Gerhart Husserl, who opposed their inclusion, potentially bringing the Frankfurt School in closer contact with American philosophers.³⁶

It was against the backdrop of these deliberations that Farber began to envision the founding of a new phenomenological organization, which would be based in America, far from the reach of Nazi persecution. The first reference to a phenomenological society in Farber's correspondence appears in a July 1939 letter to Fritz Kaufmann, which proposes the foundation of a philosophical organization and publication dedicated to the ideal of philosophy as a rigorous science.³⁷ Five

33 Farber to Kaufmann, February 18, 1939, Box 11, Folder "Kaufmann, Fritz, 1937–39," MFP; Kaufmann to Farber, February 11, 1939, Box 11, Folder "Kaufmann, Fritz 1937–39," MFP.

34 Kaufmann to Farber, October 14, 1939, Box 11, Folder "Kaufmann, Fritz 1937–39," MFP; Kaufmann to Farber, October 15, 1939, Box 11, Folder "Kaufmann, Fritz 1937–39," MFP.

35 Farber to Kaufmann, October 18, 1939, Box 11, Folder "Kaufmann, Fritz 1937–39," MFP.

36 Kaufmann to Farber, October 14, 1939, Box 11, Folder "Kaufmann, Fritz 1937–39," MFP; Farber to Kaufmann, October 24, 1939, Box 11, Folder "Kaufmann, Fritz 1937–39," MFP.

37 Farber to Kaufmann, October 24, 1939, Box 11, Folder "Kaufmann, Fritz 1937–39," MFP.

months later, on December 26, the core members of the nascent International Phenomenological Society met at the New School, adopting a constitution and electing officers, including Marvin Farber as its President (Wagner 1984, 212).³⁸ “If our undertaking succeeds,” Farber would write to Husserl’s former assistant Eugen Fink, “we shall have instated phenomenology as a major philosophical tendency in the United States.”³⁹

Over the next five months Farber and his collaborators undertook the enormous work of organizing a new society, securing it funding, and beginning to plan the first issue of its journal, *Philosophy and Phenomenological Research*. This title (hereafter PPR), as various observers noted, was quite a mouthful.⁴⁰ To phenomenologists, however, the phrase drew a direct line between Farber’s journal and Husserl’s periodical, the *Jahrbuch für Philosophie und phänomenologische Forschung* (Almanac for Philosophy and Phenomenological Research), in which many of the movement’s most significant texts had first appeared. Indeed, a draft announcement found in Alfred Schütz’s papers explicitly refers to PPR as the “successor to Husserl’s *Jahrbuch*.”⁴¹ The journal was also the new phenomenological society’s principal expense. Farber obtained funding for the project in November of 1939, securing a guarantee from the Chancellor of the University at Buffalo of an annual subvention of \$1000.⁴² The other major financier of Farber’s organization was Harvard University’s philosophy department, which gifted PPR \$300 in 1940, \$200 per year from 1941–1943, and subventions of \$100 in 1944, 1946, and 1947.⁴³

The rapid organization of The International Phenomenological Society (hereafter IPS) and PPR was only possible because of the network that had been estab-

38 Farber to Gerhart Husserl, December 7, 1939, Box 9, Folder “Husserl, Gerhart 1936–47,” MFP.

39 Farber to Fink, April 10, 1940, Box 6, Folder “Fink, Eugen,” MFP.

40 Boas, George. 1963. Letter to Farber, August 8, 1963. Box 1, Folder “B,” MFP; Galt, Alexander. 1940. Letter to Richard Hays Williams, October 5, 1940. Box 42, Folder “International Phen. Society D-K,” Marvin Farber PPR Papers. University Archives: The State University of New York at Buffalo, Buffalo, NY.

41 “Journal of Philosophy and Phenomenological Research,” n.d., Box 24, Folder “Marvin Farber Correspondence (1 of 14),” Alfred Schütz Papers. Beinecke Rare Book and Manuscript Library, New Haven, CT (hereafter ASP).

42 Farber to Samuel Capen, April 3, 1946, Box 3, Folder “Capen, Samuel P. 1945–50,” MFP.

43 International Phenomenological Society. 1942. “Statement of Cash Receipts and Expenses February 1, 1942.” Box 27, Folder 618, ASP; Departmental Minutes, February 26, 1942; Departmental Minutes, May 25, 1943; Departmental Minutes, June 8, 1944; Departmental Minutes, May 14, 1946; Departmental Minutes, April 24, 1947, “Department of Philosophy Minutes 1939–40—1949–50,” Harvard University Department of Philosophy Records (1906–1979): minutes of departmental meetings and correspondence files, 1924–1960 (UAV 6871). Harvard University Archives, Cambridge, MA.

lished compiling the Husserl memorial, whose contributors comprised the nucleus of the new group. Farber's principal collaborators were Fritz Kaufmann, Dorian Cairns, Gerhart Husserl, Felix Kaufmann, and Alfred Schütz, on whom he relied for both advice and organizational efforts.⁴⁴ Assembling and editing the Husserl memorial also revealed to Farber and his collaborators many of the challenges their future endeavor would face. "With this experience gained in connection with our memorial volume," Farber noted in a letter to Fritz Kaufmann, "we should know how to avoid errors."⁴⁵ One difficulty suggested by the group's experience was the challenge of cultivating a quantity of high quality material sufficient for a regular publication from the modest group of American phenomenologists. This could be solved, in Farber's estimation, by organizing the phenomenological society internationally and tailoring its publications for a global audience. In early discussions of the journal, Farber advocated accepting works from scholars around the world, publishing abstracts for each translated into three languages (Wagner 1984, 212–213).⁴⁶

Farber never abandoned his aspiration to lead a truly international phenomenological organization. However, his vision changed as he struggled to realize it. The growing worldwide conflagration was a major impediment to international cooperation. Indeed, a note at the end of PPR's first issue explained with a tone of dark understatement that "Due to world conditions the list of foreign members of the editorial staff must be incomplete for the present." The first issue of PPR was published in September 1940, containing articles by Farber, Landgrebe, Felix Kaufmann, John Wild, Fritz Kaufmann, and Edmund Husserl himself. Although each of these men was connected with the phenomenological movement, a note at the issue's conclusion explained that the journal was committed to fostering broad philosophical discourse:

While the philosophy of Edmund Husserl is the point of departure for the publication, it represents no special school or sect. Its aim is to maintain philosophy in the ancient sense, as an exact, descriptive discipline, at the same time bringing it to bear on the modern world. (PPR 1940, 126)

As editor of PPR, Farber continued to advocate this policy of tolerance, at times bringing him into conflict with those who favored a more partisan approach. How-

⁴⁴ Farber to Kaufmann, October 15, 1939, Box 11, Folder "Kaufmann, Fritz 1937–39," MFP; Farber to Kaufmann, November 20, 1939, Box 11, Folder "Kaufmann, Fritz, 1937–39," MFP.

⁴⁵ Farber to Kaufmann, September 25, 1939, Box 11, Folder "Kaufmann, Fritz 1937–39," MFP.

⁴⁶ Farber to Kaufmann, October 15, 1939, Box 11, Folder "Kaufmann, Fritz 1937–39," MFP.

ever, Farber's broad outlook was endorsed by the society's most important collaborators, including the Husserl archives in Louvain.⁴⁷

Harvard's philosophy department also exerted significant influence over PPR's governance and editorial policies, with John Wild serving on the journal's editorial board, and R. B. Perry on its advisory committee. During its first decade, PPR would feature an increasing number of articles by prominent American philosophers such as Ernest Nagel, Richard Brandt, Roy Wood Sellars, and Harold R. Smart, as well as European positivists and logicians including Alfred Tarski, Rudolf Carnap, and Hans Reichenbach. Farber's work succeeded in establishing PPR as a publication of significance that earned plaudits from figures across the broad American philosophical community. Despite the journal's troublesome name, it had, in the words of the New School's Horace Kallen, "taken an important place in the philosophical enterprise of the United States." He continued, "I have found it to be consistently one of the liveliest and one of the best-written and edited of the periodicals in the field."⁴⁸ A 1945 letter from W. V. Quine to Farber simply stated, "I have watched the progress of your journal with much admiration, and heartily congratulate you."⁴⁹ While changes in postwar culture and a broad transformation of American academic institutions would stymie the phenomenological movement's progress in the United States, as discussed in *Inventing Philosophy's Other: Phenomenology in America* (Strassfeld 2022), a solid foothold had been secured by 1940—and in the postwar era, the phenomenological centers established because of this intellectual migration, such as PPR, the New School, Northwestern, and Fordham, would continue to play an important role for phenomenologists in the United States.

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47 Eugen Fink to Marvin Farber, December 1, 1939, Box 6, Folder "Fink, Eugen," MFP.

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Hanne Jacobs

7 Phenomenological Sociology and Standpoint Theory: On the Critical Use of Alfred Schutz's American Writings in the Feminist Sociologies of Dorothy E. Smith and Patricia Hill Collins

Abstract: This chapter provides a historical reconstruction of how Alfred Schutz's American writings were critically engaged by the feminist sociologists Dorothy E. Smith and Patricia Hill Collins. Schutz's articulation of a phenomenological sociology in relation to, among others, the sociology of Talcott Parsons and the philosophies of science of Ernest Nagel and Carl G. Hempel proved fruitful to Smith in the development of her feminist standpoint theory in her 1987 *The Everyday World as Problematic: A Feminist Sociology*. Collins likewise draws on Schutz's writing in the development of her own standpoint theory in her 1986 paper "Learning from the Outsider Within: The Sociological Significance of Black Feminist Thought," but in a way that addresses some of her own concerns with Smith's feminist sociology. As I hope to show with the recovery of this underappreciated history, the critical insights of Smith and Collins with regard to the possible uses and limits of phenomenology for feminist theorizing are still valuable today.

7.1 Introduction

Alfred Schutz's influence on American sociology has been documented quite extensively (see, e.g., Gross 2007; Overgaard and Zahavi 2008; Psathas 2004; and Pula 2022). What has not been considered in the literature is how Schutz's American writings were critically taken up by two prominent feminist sociologists—Dorothy E. Smith and Patricia Hill Collins—in the development of their respective standpoint theories, or epistemologies.¹ This is perhaps not surprising in the case of

¹ Smith expressed her discomfort with using the term "standpoint" to describe her position in a 2010 interview: "In some ways, I wouldn't have minded ditching the notion of standpoint, although I think that it can be useful methodologically. But, it was imposed on us by Sandra Harding (e.g., Harding 2004). I think what she did was something very interesting: she drew together the work of a number of feminist writers and showed that there was this common critical thread. And she describes this as an epistemology, and she called it standpoint epistemology. And that was both useful

Smith, who explicitly states that she is not a phenomenological sociologist, but neither is the American sociologist and founder of ethnomethodology Harold Garfinkel, whose relation to Schutz has received significant attention (see, e.g., Eberle 2012; Hammersley 2019; Psathas 2012; Ruggerone 2013; and Vom Lehn and Dingwall 2014).² With regard to Collins, while it may not be surprising that her critical uptake of Schutz has gone unnoticed by phenomenologists, what is surprising is that it has been suggested that her work could “profit from phenomenology.”³ As I show in what follows, in one of her most referenced and anthologized papers “Learning from the Outsider Within: The Sociological Significance of Black Feminist Thought” (1986), Collins in fact just does that—though without therefore being or becoming a phenomenological sociologist.

This chapter provides a historical reconstruction of how Smith and Collins critically appropriate insights from the writings Schutz produced after his involuntary exile to the United States in 1939 in the development of their feminist sociologies.⁴

and tended to be a bloody nuisance, really” (Carroll 2010, 22). Given that in what follows I focus on texts by Smith that she herself sees as a continuation of the text included in Harding’s volume (“Women’s Perspective as a Radical Critique of Sociology”) and on the text by Collins that was included in Harding’s 2004 anthology, I will use “standpoint theory” and “standpoint epistemology” interchangeably to refer to their respective sociologies, without therefore overlooking important methodological differences, which I will discuss in this paper in relation to their critical appropriation of some insights of Schutz in the development of their respective feminist sociologies.

2 Admittedly, unlike Smith, Garfinkel corresponded with both Schutz and Gurwitsch during his doctoral research (Vom Lehn and Dingwall 2014, 50). Moreover, if one understands Schutz’s influence on Garfinkel along the lines of Psathas (2004) who claims that the “development of ethnomethodology owes its origins to Schutz’s remarkable insights into the world of everyday life, common sense knowledge, and the taken-for-granted” (Psathas 2004, 9, my emphasis), this would not equally apply to Smith. That is, rather than claiming that Smith is in some sense indebted to Schutz for the development of her own standpoint epistemology, I propose we think about Schutz as, in Smith’s own words, one of the “visible and invisible preceptors from whom, in the long course of trying to find a different way of thinking sociologically, I have learned” (Smith 1987, 8).

3 Michael Barber, in a critical exchange with existing critiques of, among others, Patricia Hill Collins’s work states: “Collins’s standpoint theory, as Ferguson describes it, and various forms of intersectionalism, could profit from phenomenology insofar it allows for the fact that we come to experience with sets of intentional activities, conditioned by class, gender and race, and insofar as these sets enable us to apprehend aspects of what is objectively given, though others, differently conditioned, might not even notice them” (Barber 2019, 606).

4 The critical influence of Schutz in feminist sociology extends further than the work of Smith and Collins. Notably the work of Louise Levesque-Lopman (see especially her 1988 work, where she also mentions Smith in the introduction which gives an overview of “Sociological Perspectives on Women’s Experience”). The influence of Schutz on Levesque-Lopman is (according to Psathas) one of indirect personal influence insofar as she self-identifies to be working within a Schutzian-phenomenological perspective without therefore having met or worked with Schutz (Psathas

Schutz's forced emigration required that he adapt to a new intellectual climate and formulate his own views on the methodology of the social sciences in relation to accounts that were prominent in the United States at the time. Specifically, as I discuss in section 7.2, after he arrived in the US, Schutz articulated his position regarding the methodology and fundamental concepts of the social sciences in relation to the sociology of Talcott Parsons and the philosophies of science of Ernest Nagel and Carl G. Hempel, among others. In her early essays collected in *The Everyday World as Problematic: A Feminist Sociology* (1987), Smith then draws on Schutz's discussion of the methodology of the social sciences in his American writings to develop a standpoint theory. Thus, while Smith was only in the US for a short time doing graduate work and then lecturing in the Department of Sociology at UC Berkeley (until 1963, after which she left the US for Canada), the availability of Schutz's writings in English and more significantly Schutz's articulation of how his method differed from sociological and philosophical paradigms then dominant in the US prove fruitful to Smith in the development of her own feminist sociology (section 7.3). For Patricia Hill Collins, in "Learning from the Outsider Within" (1986), Schutz's analyses from his 1944 paper "The Stranger: An Essay in Social Psychology" prove fruitful for articulating a standpoint epistemological approach that is influential to this day, which she in turn differentiates in a critical review from 1990 from Smith's approach in *The Everyday World as Problematic* (among other writings) (section 7.4). Finally, as I hope to show with the recovery of this underappreciated history, the critical insights of Smith and Collins with regard to the possible uses and limits of phenomenology for feminist theorizing are still valuable today (section 7.5). That is, while critically appropriating some of Schutz's methodological postulates, Smith, also formulates a critique of phenomenological sociology that remains relevant. And Collins's productive use of Schutz's phenomenological description of the stranger points toward ways that phenomenological thought may aid in conceptualizing resistant standpoints, albeit within certain limits.

7.2 Schutz's Phenomenological Sociology: From Vienna to New York and Beyond

Before being forced into exile from Vienna by way of Paris to New York in 1938 by the rise of antisemitism and National Socialism in Germany, Schutz had already published *Der sinnhafte Aufbau der sozialen Welt* (The Meaningful Structure of the

2004, 4). Also of interest is the paper by Lengermann and Niebrugge (1995), who argue for a "renewed interest in Schutz's work" for a feminist sociology.

Social World), which he presents as a critical development of Max Weber's interpretive (*verstehende*) sociology. Like Weber, Schutz is committed to the value-free character of the social sciences (Schutz 1932, 3), which, according to him, ought merely to describe the subjective meaning (*subjektiver Sinn*) of social (inter-)actions from the perspective of the (inter-)acting subject. Also like for Weber, these descriptions are to perform an idealizing typification of this individual agency in the social world through the generation of so-called ideal types (*Idealtypen*) (Schutz 1932, 4)—such as the police officer, the salesperson, or the traveler. Where Weber's account needs further elaboration, in Schutz's view, is in the development of the methodological foundations of the social scientific investigation of this agency in general and in articulation of the fundamental concept of the sociology: meaningful, and hence interpretable, agency (Schutz 1932, 4–5). In his 1932 work, Schutz develops what he finds lacking in Weber's interpretative sociology by drawing on insights from Edmund Husserl's phenomenology and Henri Bergson's philosophy to provide an account of the (inter-)acting individual as the subjective center of a social world of which the social sciences aim to provide a second-order scientific account. While the *Aufbau* was translated into English in 1967, in what follows I focus on his American writings, which were published in a variety of American philosophy and, less frequently, sociology journals before that translation became available, as these are the writings that Smith and Collins refer to.

After his forced migration, Schutz readily engages with some of the most prominent sociologists of his time, while also aiming to introduce, with Marvin Farber and Aron Gurwitsch, phenomenology to the American philosophical establishment (see, e.g., Embree and Barber 2017; Strassfeld 2022). Particularly relevant for what follows is Schutz's engagement with the sociologist Talcott Parsons from the then-leading sociology department at Harvard, who was, like Schutz, influenced by Max Weber and a translator of his work.⁵ Schutz's first text written upon arrival to the US was a critical review of Talcott Parsons's 1937 *The Structure of Social Action*. This review remained unpublished in its entirety after having been shared with Parsons in 1940.⁶ Schutz had been invited by Friedrich von Hayek to prepare this review (Etzrodt 2013), but when he sent it (titled "Parsons' Theory of Social Action") to Parsons on November 15, 1940, a tense correspondence ensued (Grathoff 1978; see Barber 2004, 93–96; Embree 1980; Endress 2018). This correspondence was preceded by Schutz's participation in the "Seminar on Rationality in the Social Sciences" coorganized by Parsons and the Harvard economist Jo-

5 See Kivisto and Swatos (1990) on the reception of Weber in American sociology including Parsons and Schutz.

6 The review was partially published in 1960 as "The Social World and the Theory of Social Action" in *Social Research* (Schutz 1960; see also Brodersen 1976).

seph A. Schumpeter in 1939–40 at Harvard for their graduate students. On Saturday April 13, Schutz presented the paper “The Problem of Rationality in a Social World” (Barber 2004, 91–93; Staubmann and Lidz 2018, 5), which would be published in 1943 in a slightly abridged version in *Economica* (Schutz 1943).

One of the main points of disagreement between Parsons and Schutz hinges on Schutz’s claim that the individual’s experience of their meaningful action in the social world and its scientific interpretation are not to be conflated—just like one should not conflate finding one’s way in one’s hometown, a town one is familiar with insofar as one has a sense of the distances of relevant places in relation to one’s home, with the activity of the cartographer who has to draw up a map of the same city (Schutz 1943, 131–132). Regarding the methodological importance of this distinction, Schutz writes in the part of the review of Parsons’s book that was published: “The safeguarding of the subjective point of view is the only but sufficient guarantee that social reality will not be replaced by a fictional non-existing world constructed by some scientific observer” (Schutz 1960, 209)—like when a social scientist, such as Parsons, considers the rather rare phenomenon of voluntaristic deliberation between alternatives and a choice between those alternatives to be characteristic of human agency as such (Schutz 1943, 141). Reflecting on their short acquaintance in 1974 in a letter to Richard Grathoff, who edited the correspondence between Parsons and Schutz, Parsons himself is still puzzled by this distinction between meaningful action in everyday life and its scientific interpretation. For Parsons, Schutz’s “special emphasis on phenomenological access to what is called ‘everyday life’ and the insistence that everyday life in this sense is radically distinct from any perspective of the scientific observer” amounts for Parsons—for whom, along Kantian lines, all experience entails categorization—to an “unreal dichotomy” (Staubmann and Lidz 2018, 270). For Schutz, however, this dichotomy or, what is for him in fact a distinction between two kinds of categorizations (Schutz 1932, 157–158), means that the adequacy of the social sciences can by no means be taken for granted and that careful methodological reflection is needed to guarantee that one’s scientific understanding is adequate to the everyday understanding at work in everyday social interactions and life.

Schutz’s views of how the social sciences can achieve adequacy in their scientific account of the subjective phenomenon of human (inter-)action also plays an important role in Schutz’s critical engagement with the then-prominent philosophers of science Ernest Nagel and Carl G. Hempel (another exiled philosopher). In 1952, Schutz attended the American Philosophical Association (see Embree 1997), which organized a symposium on *Problems of Concept and Theory Formation in the Social Sciences* with presentations by Hempel and Nagel. On May 3, 1953, Schutz gave a talk with almost the same title (“Concept and Theory Formation in the Social Sciences”) at the Thirty-Third Semi-Annual Meeting of The Conference

on Methods in Philosophy and the Sciences at Columbia University (Hammersley 2019, 60), where Nagel was then a full professor (Neuber and Tuboly 2022, 6) and which also housed one of the leading departments of sociology in the 1940s and '50s (Psathas 2004, 6). The paper was published in 1954 in *The Journal of Philosophy*, which Nagel was one of the editors of.

In his paper, Schutz addresses point by point what he takes to be Nagel's criticism of Weber's interpretative (*verstehende*) sociology and in doing so articulates his own position on the methodological foundations of the social sciences. Schutz begins his criticism of Nagel by pointing out what they agree on. Unlike some of his contemporaries, Schutz agrees with Nagel that the natural and social sciences do not differ in some fundamental respects. First, Schutz confirms the central role of observation and controlled verification of logically consistent claims for the social sciences: "all empirical knowledge involves discovery through processes of controlled inference, and ... it must be statable in propositional form and capable of being verified by anyone who is prepared to make the effort to do so through observation" (Schutz 1954, 260; my emphasis). Second, the social sciences also aim to articulate "determinate relations between a set of variables in terms of which a fairly extensive class of empirically ascertainable regularities can be explained" (Schutz 1954, 260). Schutz disagrees with Nagel, however, on the nature of the observation and verification through which these regularities are ascertained.

In Schutz's view, Nagel unduly limits observation to sensory observation, which Schutz attributes to his "basic philosophy of sensationalistic empiricism or logical positivism" (Schutz 1954, 261). As Schutz argues, even our everyday understanding (*Verstehen*) cannot be equated to such sensory observation as understanding another person does not coincide with either the observation of overt behavior or with introspection on oneself and the subsequent projection on another person (Schutz 1954, 262). Schutz substantiates this argument by pointing out how the same overt behavior may have different meanings (e.g., a war dance or a barter trade); how negative actions, while not observable in this strict sense, are nevertheless social actions (e.g., refraining from selling something at a given price); and how our experience of the social world is by no means exhausted by face-to-face interactions between social actors (e.g., the anonymous relations involved in one putting a letter in a mailbox) (Schutz 1954, 262–263).

In our everyday dealings, it is not through a narrowly understood form of observation but rather by means of everyday understanding that we always already make sense of the actions of fellow human beings and cultural objects (such as tools but also social institutions), which is a form of understanding through social acculturation that makes possible the experience of a social world (Schutz 1954, 264). How someone understands a given social situation is, for Schutz, moreover something that can in turn be understood by others and even verified (e.g.,

think of the rules of procedure in the context of the court that are based on certain rules of evidence that are used to determine “intent” in the case of a killing). The interpreting (*verstehende*) social scientist ought to do so in a scientific way. But even after asserting that we can in daily life understand how someone else understands a social situation, the question remains how it is possible “to deal *scientifically* ... with subjective phenomena” (Schutz 1960, 218, my emphasis).

While Schutz considers the epistemological worry regarding the knowledge of other minds as already solved by our understanding of others’ actions within everyday life, the relation between understanding as a method in the social sciences and understanding as we practice it in our everyday social relations and actions does require for him careful methodological consideration. Failing to provide an account of this results, in Schutz’s view, in overlooking an essential difference between the natural and the social sciences: “the world of nature ... does not ‘mean’ anything to the molecules, atoms, and electrons therein. The observational field of the social scientist, however, namely the social reality has a specific meaning and relevance structure for the human beings living, acting, and thinking therein” (Schutz 1954, 266–267). Social science is, hence, “founded on” (in the Husserlian sense of being dependent on) the commonsense thinking of everyday life in a particular way: “the constructs of the social sciences are, so to speak, constructs of the second degree, namely constructs of the constructs made by the actors on the social scene, whose behavior the social scientist has to observe and explain in accordance with the procedural rules of his science” (Schutz 1954, 267). This means that, unlike the natural sciences, the social sciences cannot but include a reference to the individual agent as a subjective point of view or the acting subject as a center of meaning—which Schutz terms the “postulate of subjective interpretation.” At the same time, the scientificity and objectivity of the social sciences is exactly guaranteed by their not being on a par with our everyday understanding but by radically breaking with this first-order understanding in what they deem relevant (this is also in line with the value-free character of sociology). Concretely, the social sciences generate idealizing typifications of actions and agents that are logically consistent (postulate of logical consistency). What guarantees their validity or objectivity is, however, their adequacy. As Schutz articulates this postulate of adequacy: the social scientists’ typifications are to be “constructed in such a way that a human act performed within the real world by an individual actor as indicated by the typical construct would be understandable to the actor himself as well as to his fellow-men in terms of common-sense interpretation of life” (Schutz 1954, 271; see also Schutz 1943, 147). Schutz’s insistence on the necessary reference to the subjective point of view and the demand that any sociological construct be adequate to that subjective point of view are two postulates that are productively put

to work by Smith in her feminist reform of sociology—all the while pointing to some serious limitations of Schutz’s phenomenological sociology.

7.3 Smith on the Everyday World as Problematic

In her 1987 *The Everyday World as Problematic: A Feminist Sociology*, which collects writings from the 1970s up to its publication, Smith explicitly draws on Schutz in the development of a feminist critique of the sociological discourse of her time. I focus on the chapter “A Sociology for Women” (1987, 49–104), which was written between 1977 and 1981 (1987, 44) and which Smith considers a development of a paper (“Women’s Perspective as a Radical Critique of Sociology”) she circulated in the early 1970s and which was written for the meeting of the American Academy for the Advancement of Science (Pacific Division) at Eugene, Oregon, in June 1972. In this earlier paper, which was anthologized in *The Feminist Standpoint Theory Reader* (Harding 2004), Smith develops “how a sociology may look if it began from the point of view of women’s traditional place in it and what happens to a sociology which attempts to deal seriously with that” (Smith 2004, 21). Smith characterizes the sociology of her time as a conceptualization and transposition of people’s lives and experiences into a theoretical framework that is beholden to and participates in certain relations of governing or ruling. That is, in a Foucauldian vein, Smith points out how “mental illness, crimes, riots, violence, work satisfaction, neighbors and neighborhoods, motivation, etc. ... are the constructs of a practice of government” (Smith 2004, 23) or facts that are given shape by this practice. Sociology, as the theoretical investigation of these constructed facts, participates in this practice of governing. Since society and sociology in Smith’s time were governed by interests particular to men, sociology inevitably ends up reproducing these interests as well and, in this way, contributes, according to Smith, to the alienation of women from their own experience (Smith 2004, 22). As a result, women joining the profession of sociology may be confronted with “a disjunction between how women find and experience the world beginning (though not necessarily ending up in) from their place and the concepts and theoretical schemes available to think about it” (Smith 2004, 22).

To articulate this disjunction, “bifurcation” (Smith 1987, 82, 86, 89; 2004, 25, 27, 28, 32), or “rupture” (Smith 1987, 51, 59), Smith draws on Schutz’s articulation of the relation between the natural attitude of everyday life and the scientific attitude as Schutz articulated these attitudes in his 1945 paper “On Multiple Realities.”⁷ In this

7 In her earlier 1972 paper, Smith does not refer to Schutz. However, her use of terms like “rele-

paper, Schutz describes the relation between science and the world of everyday life that Parsons had such a hard time understanding.⁸ The world of everyday life is characterized as a “world of working” and as “paramount” over against other realms (such as the world of dreams and imagination) we may engage in (Schutz 1945, 549, 553). The different realities we spend our lives in are characterized by Schutz as “finite provinces of meaning” that correlate to particular interests and parts of our personalities and are structured by what Schutz calls different “systems of relevances” (Schutz 1945, 550), due to which our experience is by necessity selective. Given the pragmatic character of our relation to the paramount world of everyday life, this world is experienced in a way that is dictated by our current projects (e.g., a wire in one’s toolbox stands out in the context of trying to hang a bird house on a tree). It is these everyday systems of relevances that the social scientist abandons or brackets while inquiring into the social interactions in this everyday world (Schutz 1945, 565). In doing so, the scientist also brackets his “physical existence and therewith also his body and the system of orientation of which his body is the center” or, in short “his subjective point of view” (Schutz 1945, 566–567). At the same time, by engaging in the scientific attitude, the scientist takes over the system of relevances or problems particular to a given science, which means that “the decision of the scientist in stating the problem is in fact a very small one” (Schutz 1945, 568). What is more, like in his later critical exchange with Nagel, Schutz already here underlines the importance of the postulate of consistency as well as the postulate that scientific claims are to be derived from tested observation common to all sciences all the while emphasizing the distinction between the natural and social sciences insofar the latter’s generalizations

vances” and “natural attitude” (Smith 2004, 26), as well as the characterization of the bifurcation between our embodied experience of the everyday world and its conceptualization in this text are on a par with what she states in the chapter in the 1987 volume, which does reference Schutz in these respects. In this chapter in the 1987 collection, Smith references Schutz’s American writings as they are published in the first volume of the *Collected Papers* (1962)—which includes the 1954 paper in which Schutz engages Nagel—and his work on relevance that was edited by Maurice Natanson (2011).

8 Unlike the paper Schutz presented at Parsons’s seminar in 1942 (Schutz 1943), the paper on multiple realities (1945) was first published in a philosophy journal (*Philosophy and Phenomenological Research*) that Schutz co-founded with Marvin Farber. In it, Schutz heavily references William James, G. H. Mead, Edmund Husserl, and Henri Bergson in the development of his position, which is not unsurprising given the venue of publication. In the section that discusses the scientific attitude (“The World of Scientific Theory”) and from which Smith draws, Schutz refers the reader to his account of the relation between the natural attitude and scientific attitude as presented at Parsons’s seminar.

are to be “compatible with all the pre-experiences of the world of daily life”—the aforementioned postulate of adequacy (Schutz 1945, 569, 572).

It is Schutz’s distinction between different provinces of meaning, the distinction between the natural and scientific attitude, and the postulate of adequacy that will prove fruitful for Smith in the development of her feminist sociology. As she writes, “to help us analyze further the problem of women’s relation as subjects or knowers to the sociological discourse, I shall draw on Alfred Schutz’s description of the finite provinces of meaning and of the changes in the organization of consciousness associated with shifts from one province to another” (Smith 1987, 69). That is, Schutz’s distinctions between our paramount reality and other provinces of meaning with their different and distinct systems of relevance (including those of scientific research) allow for Smith to localize a standpoint with its own sets of relevances that are outside of and different from the relevances that are characteristic of both the dominant common sense and its scientific articulation in sociology (Smith 1987, 78). While the sociologists of Smith’s time overlook this experience, for the feminist sociologist, this experience or distinctive standpoint—in the case of Smith herself, the experience of a divorced woman having to care for children while lecturing on and researching a different world than her own—becomes the starting point of a sociology *for* women.

Hence, Schutz’s idea of different provinces of meaning and the distinction between the natural attitude and the scientific attitude allows for Smith to make visible the lifeworld as experienced by women. Moreover, it is in and through the shift in focus on the everyday world of women, that the inadequacy of prevailing sociological paradigms becomes apparent. As Smith writes: “The theories, concepts, and methods of our discipline claim to be capable of accounting for and analyzing the same world as that which we experience directly” (Smith 1987, 85). However, Smith concludes that they fail to account for this world and hence violate the postulate of adequacy. Perhaps unsurprisingly, in this respect Schutz’s own phenomenological sociology appears to be no exception, as some of his own claims about the everyday world betray his position in society. What is more, building on her critique of some of Schutz’s claims, Smith also articulates a methodological criticism of Schutz’s phenomenological sociology that has not lost its relevance today.

A first point of criticism that Smith formulates concerns Schutz’s own generalizing typification of the relevance of housework in one’s daily life.⁹ As Schutz writes in his work on relevance that was published in 1970: “only very superficial

⁹ This is not Smith’s only criticism of Schutz’s characterizations of everyday life. She also takes issue with Schutz’s Heideggerian claim that our fundamental anxiety in relation to our own mortality is fundamental to the organization of our relevances and projects (see Smith 1987, 64) pointing out that projects and relevances are organized for women by others.

levels of our personality are involved in such performances as our habitual and even quasi-automatic ‘household chores’ ... requiring and receiving our full attention if only momentarily” (Schutz 2011, 98–99). Smith discerns here the distorting influence of Schutz’s own position, which is presumably bracketed, on his second-order scientific understanding of the everyday world. As Smith sees it, Schutz’s description is in fact a second-order articulation of the practices of governing that invisibilize women’s labor and their everyday life.

Without challenging Schutz’s general picture of these various levels of personality and their organization in relation to project in the world of working, we can also recognize what is presupposed in just that organization, namely, that the routine matter, the household chores, *are not problematic*, do not become a central focus of man’s work. ... The place of women, then, in relation to this mode of action is where the work is done to facilitate men’s occupation of the conceptual mode of action. ... At almost every point women mediate for men the relation between the conceptual mode of action and the actual concrete forms on which it depends. (Smith 1987, 83, my emphasis)

Taking the point of view of women thus, in this instance, discredits “sociology’s claim to constitute an objective knowledge independent of the sociologists’ situation” (Smith 2004, 28). Importantly, however, for Smith, a feminist sociology consists in more than pointing out what has been “left out, overlooked” by established sociology (Smith 2004, 21) and for her critique is also always an “attempt to define an alternative” (Smith 1987, 78). This alternative entails two further points of criticism of Schutz’s phenomenological sociology, that are of a methodological kind.

While Smith productively makes use of Schutz’s distinction between the natural attitude of our paramount reality in which we are localized as embodied subjects in an everyday world of working and the scientific attitude which brackets this bodily existence and its particular interests or relevances, she also takes issue with how Schutz characterizes their relation:

Beginning from the standpoint of women locates a subject who begins in a material and local world. It shows the different cognitive domains structuring our realities, *not, as Schutz describes, as alternatives*—a paramount reality on the one hand and the scientific domain on the other—but rather as a bifurcation of consciousness, with a world directly experienced from oneself as center (in the body) on the one hand and a world organized in the abstracted conceptual mode, external to the local and particular places of one’s bodily existence. (Smith 1987, 84–85, my emphasis)

While Schutz’s distinction between our everyday understanding and our second-order scientific understanding of this understanding is taken over by Smith, it is a critical appropriation insofar as she rejects Schutz’s characterization of their relation. Schutz’s own characterization of the relation between our everyday

world and its scientific understanding betrays his specific location in society in Smith's view: "The specific character of the sociological mode of reflecting upon society ... in suspending the actual and particular position of the knower must be understood as itself located" (Smith 1987, 74–75). That is, as already implied by Smith's critique of Schutz's characterization of housework: "Participation in the 'head' world is accomplished in concrete settings making use of definite material means" (Smith 1987, 85). And while Schutz could overlook this because women's work conceals itself typically for scientific men losing themselves in the "head" world, "to those who do this work, the local and concrete conditions of the abstracted mode are thematic" (Smith 1987, 85). And for those women like Smith who engage in this work as well as scientific research, the former can never be left behind—hence the bifurcation of her consciousness.

Insofar as Schutz's phenomenological sociology aims for a second-order understanding of our first-order understanding that is value-free and generates typifying characterizations of our everyday life and social interactions, the very project of such a phenomenological sociology seems to become questionable once one takes women's point of view seriously in sociology—as it betrays sociology's inadequacy to this point of view all the while being unable to bracket or leave behind the systems of relevances of a dominant male position in society. But if so, this raises the question how a feminist sociology that starts in women's everyday experience and their bifurcated consciousness can fare any better. Or, to ask this question in Smith's own words, would "proposing a sociology grounded in the sociologist's own experience" not just amount to "the self-indulgence of inner exploration or any other enterprise with self as sole focus and object" (Smith 2004, 29)?

Smith's answer to this question consists in a second, more radical, Marxist criticism of Schutz's phenomenological sociology. That is, for Smith, feminist sociological research ought to focus on the scientific investigation of the relations of ruling, which are the social and historical-material relations that govern the everyday world. Research into these relations ought to take as its starting point the everyday experience to which our sociological theorizing is beholden: "The movement of research is from a woman's account of her everyday experience to exploring *from that perspective* the generalizing and generalized relations in which each individual's everyday world is embedded" (Smith 1987, 185). Though to understand these generalized relations it by no means suffices to provide the kind typifying descriptions Schutz was after with his phenomenological sociology. That is, to understand, for example, the relations that obtain between mothers and schools (and constructs such as "homework," "progress," etc.; see Smith 1987, 168–175) or relations between people and cities (and the difference between "rental-units," "single family dwellings," and "respectable neighborhoods"; see Smith 1987, 155–156), it is by no means sufficient to restrict oneself to a generalized description of how these

relations and constructs are experienced, because these social relations are “not produced by the mysterious ‘idealizations of intersubjectivity’ which Schutz’s cognitively based conception of social reality requires” (Smith 1987, 126). Instead, as a Marxist analysis of these relations would reveal, these meanings are actually constructed in and through concrete material relations of ruling particular to a capitalistic society, which remains invisible when one just describes social (inter-)action. Or as Smith states it already in 1972:

No amount of observation of face-to-face relations, no amounts of analysis of common-sense knowledge of everyday life, will take us beyond our essential ignorance of how it is put together. Our direct experience of it constitutes it (if we will) as a problem, but it does not offer any answers. The matrix of direct experience as that from which sociology might begin discloses that beginning of an “appearance” the determinations of which lie beyond it. (Smith 2004, 32)

And this is why Smith denies what others have suggested to her: “that a phenomenological sociology is a feminist sociology merely because it begins with the consciousness of the knower and is hence ‘subjective’” (Smith 1987, 86). That is, as Smith is all too well aware, this phenomenological perspective takes for granted or leaves unproblematized “the material and social organization of the bifurcated consciousness, and does not render its organization and conditions examinable” (Smith 1987, 86). Once we realize this, “we are no longer stuck with shared meanings or intersubjectivity as the guarantor and ground of the social” (Smith 1987, 123) as a phenomenological sociology would be—and the possibility of a different kind of inquiry becomes visible that *does* examine the organization and conditions of our lifeworld by inquiring into the material and productive relations that bring about this lifeworld.

What this criticism of Smith on Schutz makes clear is how a phenomenological sociology that restricts itself to a second-order descriptive understanding of our everyday dealings is at best quite limited in its explanatory value and at worst ends up with a distortive description of our actual experience because of its replication of the systems of relevance of a dominant position in society (failing the postulate of adequacy). At the same time, Smith also shows how our experience of the everyday world could constitute the starting point of the investigation that would be “capable of explicating for members of the society the social organization of their experienced world” (Smith 1987, 89) by looking at material and productive relations, which is a project that aligns with contemporary critical phenomenology (see Ahmed 2006, ch. 1), even if Smith’s contribution seems to have been forgotten. Insofar as this sociological investigation begins from women’s experience of the world, and given that Smith recognizes that “women are variously located in society” (Smith 1987, 85), this raises the question of which experience is to furnish the

critical starting point of such an investigation—and it is this question that is at the heart of Collins’s critical appraisal of Smith’s standpoint theory. Of interest for this chapter is, moreover, that Collins, in her own methodological articulation of the role of experience in the development of a critical standpoint vis-à-vis prevailing sociological conceptualizations, draws on Schutz’s description of a particular ideal type—specifically, “the stranger”—and through this different critical appropriation of Schutz, Collins addresses some of the issues she discerns in Smith’s version of standpoint theory.

7.4 Collins on the Stranger and the Outsider Within

In her 1992 “Transforming the Inner Circle: Dorothy Smith’s Challenge to Sociological Theory,” Collins critically reviews three collections of Smith’s essays, including the 1987 volume. In this review, Collins explicitly aligns herself with Smith’s project when she likens Smith’s “critical position on sociological knowledge” to her own work: “In my own work I refer to this position as that of an ‘outsider within’” (Collins 1992, 74). At the same time, Collins wonders: “but will her [Smith’s] thinking take us where we want to go?” (Collins 1992, 77). Collins goes on to articulate a two-fold criticism of Smith’s feminist challenge to existing sociological theory and her standpoint theory. Both criticisms can be understood as following from the different standpoint theory Collins herself developed in her 1986 paper “Learning from the Outsider Within” and her 1990 *Black Feminist Thought: Knowledge, Consciousness, and the Politics of Empowerment*.

The first criticism that Collins articulates is of Smith’s characterization of the point of view from which dominant sociological theories can be challenged. That is, the standpoint of women’s experience of the lifeworld that is to be the starting point for the inquiry of the material organization of this lifeworld and its relations of ruling appears, for Smith, to be an individual experience or an individual’s bifurcated consciousness. As Collins articulates it: “Smith describes how her *individual* experiences as a woman provide her with a unique perspective, but she does not develop this insight to invoke traditions of local knowledges produced by historically marginalized *groups*” (Collins 1992, 77). This is in part due to Smith’s reliance on texts at the expense of oral traditions as well as her own positionality as a white woman. So, for example, in her development of a sociology for women, there is a rich tradition of Black feminist theorizing that Smith overlooks. In her 1986 article, and more elaborately in *Black Feminist Thought: Knowledge, Consciousness, and the Politics of Empowerment* (1990), Collins documented this tradition,

thereby challenging ignorance in feminist theorizing about this tradition by identifying feminist critiques by Black women going back to the nineteenth century and their shared themes (see Dotson 2015 on this feat). As Collins points out, “academic intellectuals’ failure to investigate these traditions does not mean that alternative traditions do not exist” (Collins 1992, 78). That Smith overlooks such traditions, including the Black feminist tradition, is in Collins’s view because she “underemphasizes diversity created by race, gender, class, sexual orientation, and age” as well as their intersecting, due to which she overlooks “the knowledges produced by these groups as they actively resist objective knowledge that justifies their subordination” (Collins 1992, 78) which makes Smith’s approach in this respect not different from other existing sociological approaches. Collins, however, considers it possible that Smith’s account be “complemented” with an “equally well-developed theoretical and empirical attention to the social construction of resistance as organized through local knowledges” (Collins 1992, 79) which is exemplified by Collins’s own work.

A second point of criticism is of a more methodological nature. Collins articulates the critique by invoking Audre Lorde’s warning that “the master’s tools will never dismantle the master’s house” (Collins 1992, 79). More concretely, Collins concludes that despite the value of Smith’s challenge to the prevailing sociologies of their time, she did end up participating in the relations of ruling: “Smith chose to adhere to the rules, to do theoretical sociology in a way that makes sense to members of the inner circle. ... her discourse never can be truly transformative because it is organized in the language of the inner circle, essentially addressed to its members” (Collins 1992, 79). While Collins herself has been critical of the way ideas central to the Black feminist tradition (such as intersectionality) have been operationalized in academic theorizing (see e.g., Collins 1999, 2017, and 2019), it is her embeddedness in a Black feminist tradition that transcends her own perspective that may put her in a better position to continue to challenge sociology. In her 1986 article, Collins methodologically articulates this position as one of an “outsider within” and her characterization of this position shows some important differences in relation to how Smith’s articulates her bifurcated consciousness. What is more, and relevant in the context of this paper, in the articulation of Black women sociologists’ standpoint as that of an outsider within, Collins draws on Schutz’s 1944 paper “The Stranger: An Essay in Social Psychology,” which was published in the *American Journal of Sociology*.

Schutz’s essay on the stranger provides a concrete illustration of what it would mean to provide a second-order typification of a social relation or an idealizing description of a personality type in a given social context. That is, while Schutz’s descriptions of the stranger could be understood in the context of his own position as a Jew in Vienna and then in the US (Tada 2023), the descriptions that Schutz pro-

vides of the stranger aim to apply to any kind of stranger—be it someone who voluntarily relocated to another country, the farmer's son who enrolls in college, someone who is born into a segregated society, or someone who finds themselves in exile due to persecution in their homeland. What Schutz wants to do in this paper is “to study in terms of a general theory of interpretation the typical situation in which a stranger finds himself in his attempt to interpret the cultural pattern of a social group which he approaches and to orient himself within it” (Schutz 1944, 499). The topic of the stranger is by no means unprecedented in sociology (Schutz himself refers to numerous American and German authors), though Schutz brings his own phenomenological sociological approach to bear on the topic.

What is, in Schutz's view, characteristic of any stranger is that they experience the thinking as usual of the social in-group to which they do not belong, but with whom they are confronted, in a different way than any of the members of this in-group. For Schutz, thinking as usual for members of an in-group is determined by their inherited and shared cultural pattern or their “peculiar valuations, institutions, and systems of orientation and guidance (such as the folkways, mores, laws, habits, customs, etiquette, fashions)” (Schutz 1944, 499) that define their fields of possible and actual actions in their everyday world. While in-group members' understanding of social situations is in fact incoherent, inconsistent, and only partially clear as any such understanding, for the in-group member thinking as usual has “the appearance of a *sufficient* coherence, clarity, and consistency” (Schutz 1944, 501). The stranger, unlike those who are part of the in-group, does not have this kind of access to this thinking as usual, which according to Schutz entails “the stranger's objectivity” (Schutz 1944, 506). This objectivity does not only consist in the stranger's critical attitude that follows from his “vivid feeling for the incoherence and inconsistency of the approached cultural pattern” but, more profoundly, comes from this awareness of the limits of “thinking as usual” in general due to his experience of being a stranger (Schutz 1944, 506–507).

In her 1986 paper, Collins characterizes sociology's system of knowledge in terms of “thinking as usual” and claims that Black women who enter this discipline of sociology “become, to use Simmel's (1921) and Schutz's terminology, penultimate ‘strangers’” (Collins 1986, 26) and continues to quote Schutz's characterization of the stranger:

The stranger ... does not share the basic assumptions of the group. He becomes essentially the man who has to place in question nearly everything that seems to be unquestionable to the members of the approached group. ... To him the cultural patterns of the approached group do not have the authority of a tested system of recipes ... because he does not partake in the vivid historical tradition by which is has been formed. (Schutz 1944, 502 as quoted by Collins 1986, 26–27)

To describe the particular way that Black women sociologists are strangers, she uses, however, her term “outsiders within.” What is more, Collins also provides a concrete sense of the way in which the outsider within—in this case, Black women who enter a “white male insider-influenced” sociology—can afford a critical point of view that illuminates sociology’s anomalies (Collins 1986, 27). More concretely, Collins points to two different kinds of anomalies that may become visible from the perspective of the outsider within. First, there are omissions of facts or observations about these outsiders themselves that have resulted in the invisibility of Black women in the sociological research of Collins’s time (Collins 1986, 27). Second, there are the actual “distortions of facts and observations about Black women” in and through the generation of misogynoir stereotypes (Collins 1986, 28). Both anomalies are addressed by Collins throughout her work.

Collins’s critical use of Schutz’s articulation of the experience of the stranger can serve as an indicator of an important distinction between Smith’s and Collins’s respective standpoint theories. That is, for Smith, women’s standpoint is the possible site of a bifurcated consciousness that holds together the everyday world and how it is conceptualized in dominant discourses. While this bifurcated consciousness or disjuncture may yield a subsequent critique of such discourses, Collins points out that “remaining on the line of fault leaves the inner circle unchanged” and that Smith “eventually chose to adhere to the rules, to do sociology in a way that makes sense to members of the inner circle” (Collins 1992, 79). However, the outsider within does not in the same way experience bifurcation—rather as a stranger they are confronted with an alien common sense—and instead (will continue to) experience a tension—in the case of Black women entering sociology, a tension between one’s own personal and cultural experiences and sociological theory. This tension, however, also affords a critical standpoint because due to being a stranger to another common sense, the latter’s anomalies become more easily detectable. It is Collins’s view that sociology can only be really transformed by trying to “conserve the creative tension of outsider within status by encouraging and institutionalizing outsider within ways of seeing” (Collins 1986, 29). So rather than leaving the standpoint from which the critique on dominant sociological theories is formulated in favor of a Marxist style analysis, Collins suggests that outsiders within linger with that experience. That they can do so, is because their experienced reality is a collective one (see also Collins 1999, 85), with its own history and (oral) traditions and as such always already provides an alternative to existing discourses. And what Collins hopes for is that this experienced reality “is used as a valid source of knowledge for critiquing sociological facts and theories, while sociological thought offers new ways of seeing that experienced reality” (Collins 1986, 30).

7.5 On the Uses and Limits of Phenomenology

I have already shown in passing how Smith's critical use of Schutz's phenomenological sociology remains relevant. That is, Smith's feminist sociology both indicates the use and limits of a focus on the experience of everyday life. If we take Smith's critique of Schutz's phenomenological sociology seriously, it would seem that a phenomenology that aims to understand the relations of ruling cannot restrict itself to providing phenomenological descriptions but must also inquire into the actual mechanisms that go into forming and maintaining these relations of ruling. And the inquiry that Smith proposes into the actual relations of ruling and the ways in which the everyday world is organized calls for a multitude of inquiries from different standpoints. That is, as Smith herself acknowledges "from different standpoints different aspects of the ruling apparatuses and of class come into view" (Smith 1987, 107).

This is not to say, however, that phenomenological descriptions could not also do another kind of work. That is, as is exemplified by Collins, phenomenological concepts and types like the one of the stranger may provide "new ways of seeing that [Black women's] experienced reality" (Collins 1986, 30). This, however, only applies insofar as these concepts are used by those with outsider within status in the articulation of their experienced reality and this articulation will inevitably introduce new concepts. After all, the outsider within, while a stranger, is not just any stranger—or, as Collins points out in retrospect about her own paper: "Nothing in the literature that I consulted in the 1980s really fit. Talks of insiders, outsiders, and marginal men came close *but something was missing*" (Collins 1999, 85, my emphasis) and it is only with the concept of the outsider within that Collins thought she had found an apt description of her own position and other Black women like her in sociology. What this means, however, is that existing phenomenological concepts, as they are all too often developed from the perspective of a dominant position, may not "really fit" (e.g., as Fanon [1965] has shown when introducing his notion of a historico-racial schema). Given both Smith's and Collins's poignant insights into the uses and limits of, in this case, Schutz's phenomenological sociology, their works can be taken as a prolegomenon to any future phenomenological work committed to understanding the relations of ruling (Smith) while avoiding omissions and distortions in its theorizing (Collins).

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III Logical Empiricism

George A. Reisch and Adam Tamas Tuboly

8 Reviving the Unity of Science Movement: Philipp Frank's Journey to Harvard

Abstract: Two unanswered questions in the history of philosophy of science are how Philipp Frank became a well-known philosopher in America and how he fell quickly into obscurity and disfavor within professional philosophy. In this paper we shall concentrate on a few preparatory issues: we will sketch the way Frank arrived at the States through the lens of his background in Prague to show that he was not a traditional refugee, as he saw a chance to establish logical empiricism in the States on a bigger scale than before. Recognizing opportunities and openings in the world of American education, he immersed himself to create an accessible brand of positivism and teach others how to propagate it in American schools and universities. This is a so-far underappreciated project for advancing the fortunes of scientific philosophy in America, one that Frank—more than any of his colleagues—would embrace and make his own.

8.1 Introduction

On September 19, 1938, Bertrand Russell and his family reached New York to take up his temporary professorship at the University of Chicago. Upon his arrival, the famous philosopher and logician was awaited by a couple of reporters, asking him about peace, war, and the world-situation. But Russell was not the only newly arrived philosopher with a special interest in these topics.

A few weeks later another philosopher arrived in New York, but without attention from the press. One of his friends saw him near Riverside Drive Park, “sitting on a bench, lonely, staring at the river, a refugee from Hitler’s advance into Czechoslovakia” (Feuer 1988, 68). No one expected that within a few years, he would be almost as prominent in the United States as Bertrand Russell and his friend Albert Einstein. He would be interviewed as an authority by Boston newspapers about atomic scientists and atomic geopolitics, he would be one of the original members of the long-running *Conference on Science, Philosophy, and Religion*, he would appear on the cover of the *Saturday Review of Literature*, and J. Edgar Hoover’s FBI would take note of his prominence as Einstein’s biographer and investigate him as a possible communist. He was Philipp Frank, a physicist-turned-

philosopher, prominent logical positivist, whose personal history and philosophical overview promises important lessons for historians of philosophy of science.¹

Two unanswered questions in the history of philosophy of science are how Philipp Frank became so quickly such a well-known philosopher in America and how, shortly before his death in 1966, he fell just as quickly into obscurity and disfavor within professional philosophy. These questions require a detailed examination of Frank's popular, cultural, scientific, and educational trajectories from Vienna to Cambridge via Prague.² In this paper we shall concentrate on a few smaller, preparatory issues with two aims in mind. First, we will sketch the way Frank arrived at the States through the lens of his background in Prague to show that he was not a traditional refugee. Though he saw the recent and upcoming troubles, Frank aimed to go back home as a loyal citizen of the Czechoslovakian Republic. But once he was deprived of that possibility due to the war, he saw a chance to establish logical empiricism in the States on a bigger scale than before. Recognizing opportunities and openings in the world of American education, he immersed himself in creating an accessible brand of positivism and teaching others how to propagate it in American schools and universities. This is a so-far underappreciated project for advancing the fortunes of scientific philosophy in America, one that Frank—more than any of his colleagues—would embrace and make his own.

8.2 Preparing for America

In 1938, Frank began his twenty-sixth year in Prague, researching, teaching, and leading the Theoretical Physics Institute at the German University of Prague. Some of his previous colleagues had passed away, while others—young and old alike—emigrated in response to the growing daily threats. As Frank described the situation, “our university is such that one feels being transferred to the Third Reich.”³ Just a few years before, however, because of its liberal atmosphere

1 “Lise Meitner Termed Pacifist by Dr. Frank,” *Boston Globe*, August 11, 1945, p. 6.; “Nazi Refugee Physicist Working in Argentina,” *Boston Globe*, March 26, 1951, p. 28. On the FBI, see the declassified files on Einstein, and Reisch (2005, ch. 13). Eventually, Frank and Russell would meet in Chicago, the former commenting it as “a lot of famous philosophers are now concentrated in Chicago ... you can speak almost of a concentration campus” (Frank to Ernest Nagel, October 28, 1938; from the personal collection of Yvonne Nagel).

2 On Frank, see Reisch and Tuboly (2021), cf. with George Reisch and Adam Tamas Tuboly's forthcoming biography of Frank (Reisch and Tuboly ms.).

3 Philipp Frank to Josef Frank, December 1936, Otto Neurath Nachlass, Wiener Kreis Archiv, Rijksarchief in Noord-Holland, Haarlem, The Netherlands, 327 (hereafter, ONN). All translations from the archive's German correspondence are our own unless indicated otherwise.

and its German-speaking university, Prague had become a center for German emigrants including many Jewish students unable to enroll at universities in the Third Reich. As Peter Bergmann—student of Frank, collaborator with Einstein, and theoretician of general relativity—recalled later, “among all the foreign universities offering instruction in German ... Prague was least expensive by a considerable margin” (Bergmann 1966, 4). The very same reason turned out to be persuasive for many students from the Balkans, the Soviet Union, and even Baltic nations. The mathematician Lipman Bers, one of the refugee students from Livonia, recalled later that Czechoslovakia “was [in the 1930s] a haven for all political refugees” (Albers and Reid 1987, 274).

For those living permanently in the Republic, having no other perspective, it became obvious day after day in late 1938 that the German Reich was trying to get its hand on the last island of Eastern European democracy. As early as 1934, this paradise of diverging opinions and people had been challenged by street fights over nationalistic issues at the university. Anticipating further violence, Carnap decided to leave Prague where he had been teaching since 1932. In early 1935, he was invited to Harvard's 300th-anniversary conference to lecture and receive an honorary doctoral degree. “A first step towards America,” he commented in his diaries in February. For the big event, Frank was nominated to be the official representative of the German University of Prague but owing to lack of time he handed over that task to Carnap who soon took a permanent position at the University of Chicago.⁴

Jeremy Bernstein's recollections help show how devastating the situation was in Prague:

Frank once told me that in the 1930s, there were three factions at the university. There were the Nazis, who were afraid the Russians would invade. There were the Communists, who were afraid the Germans would invade. And there were the Jews, who knew that whoever invaded, it would be bad for them. There was only one thing the three groups could agree to. They hired a tutor to teach them English so they could emigrate to America. (Bernstein 2020, 145)

Frank, a Jew, belonged to the third group. He knew that independently of who the invaders were going to be, things would turn out badly for them.

But like others living through these uncertain times, especially those within the walls of the university which provided at least *some form* of normalcy and reg-

⁴ Philipp Frank to Institute of International Education, February 24, 1937, Emergency Committee in Aid of Displaced Foreign Scholars Papers, 1927–1949, Folders 15 and 16, “Frank, Philipp.” The New York Public Library, Archives & Manuscripts (hereafter, ECFSR). About the situation in Prague that time, and for further references, see Reisch and Tuboly (ms.).

ular routine, Frank wavered for a long time between optimism, pessimism, and lethargy. This is well documented in his correspondence during these difficult years. At first, in March 1938, he wrote about the “Scylla and Charybdis” of the situation, owing partly to “threatening influences from our neighbors.” The two options were to “undertake a shift to the right” and live in a semi-dictatorial regime “which will hand over all the cultural institutions such as theatres, schools, etc. in German territory to the Nazis,” or to resist and “risk a war” that may lead to a major European incident. Neither option was feasible for Czechoslovakian citizens. Within two months, he recognized that the “the conditions are very critical” and all the problems will be resolved soon as the situation was unbearable for a long time. No one knew, however, whether it would be “done peacefully or violently. In any case, our university is likely to change in such a way that it will be practically unbearable.” As the “acute fright has gradually turned into a chronic one” in the next month, people became aware of the “tactic of attrition” that will turn slowly, and at the right moment, “into a directly aggressive one.” As he wrote to Otto Neurath, “there won’t be many in our faculty who want to wage a decisive fight against swastika influences. Defensively, of course, they will do it, but few will want to create a new difficulty.”⁵

Despite all difficulties and the continuously worsening situation of the university, Frank and his wife Hania were “doing well personally” and they even “started going to dance halls and dancing together.” During such fearful times and crises, it could have been mentally lifesaving to find refuge in familiar routines or in taking up new ones. That did not mean, however, that *all* was well, and Frank did notice the strangeness of the situation. He called the situation “Ssanin 1905!”⁶

Ssanin was an Austrian-Polish silent film from 1924, based on a novel with the same title by the Russian Michail Petrowitsch Arzybaschew in 1907. It was written after the unsuccessful 1905 Revolution that was directed against the Russian Tsar and his system. The main figure of the novel and the film was Wladimir Petrowitsch Ssanin, a true nihilist of the time, a representative of a broken society that had failed to stand up for change. He suggested abortion and suicide to friends and relatives. He committed rape and spread his immorality and nihilism throughout the country. The story was designed to spark a revolution for sexual freedom, but the book was banned as pornographic. Ssanin and his followers had turned their attention from change and revolution to self-centered nihilism and sexuality without limits. Frank obviously exaggerated when he compared all this to his and

5 Frank to Otto Neurath, March 4 and May 5, 1936 (ONN/236).

6 Frank to Otto Neurath, November 13, 1936 (ONN/236).

Hania's dancing. But turning inward to such pleasures instead of changing the socio-political circumstances he nonetheless saw through the lens of Ssanin.

Frank's humor and sarcastic optimism did not last long. Pessimism and despair came when, as Frank put it, "all free-thinking people" became accustomed to the new military attitude of the Third Reich. While he saw the practicality of adopting such an attitude, Frank was afraid and existentially confused. He told Neurath, "everything is turned upside down and it is difficult to find your way around."⁷

Hans Kelsen, an internationally renowned leader in law and jurisprudence, helps illustrate the changes Frank observed. As early as 1933, when Kelsen was first considered for a post at the University of Prague, Nazi students protested with slogans like, "Out with Jewish professors!" Three years later, when Kelsen was appointed, the situation got worse. Once, when he was about to lecture, a "raucous crowd of nationalists," many of them not even students, occupied the lecture hall, making Kelsen's way to the podium almost impossible. When he started to lecture, one of them shouted, "Down with the Jews, all non-Jews must leave the hall," after which Kelsen was left alone in the lecture room. When he reported the incident to the Dean, he noted that his own students were "beaten up badly, thrown down the stairs and the like" (Schuett 2021, 123–124).

Though Kelsen was basically "prevented by force" to lecture, Frank took the worst part of the incident to be the fact that "academic authorities did everything they could to protect the rioters."⁸ Kelsen received anonymous letters with swastikas, in which he was threatened with the fate of Theodore Lessing, a Zionist German philosopher, who had fled Nazi Germany to Czechoslovakia but was shot in his country house in August 1933 by Sudeten Nazi sympathizers. While Kelsen was protected for a while by detectives because of these threats, the rector of the university told him that the letters were written by communists and that he had no pressing need to worry about these issues since "such things do happen." Frank sarcastically agreed, telling the rector, "definitely, for example, during the Reichstag fire,"⁹ when the burning of the German parliament was attributed to communists and used to rationalize curtailing civil rights as the Third Reich began its reign.

Frank wrote to Ernest Nagel that the future was "very dark," and added, "'Dark' is perhaps an optimistic expression. Because according to some, the future is 'clear', but unpleasant."¹⁰ This note of resignation conveys the fact that Frank

7 Frank to Neurath, March 4, May, June 7, and June 1938 (ONN/237).

8 Frank to Otto Neurath, November 13, 1936 (ONN/236).

9 Philipp Frank to Josef Frank, December 1936 (ONN/327).

10 Frank to Ernest Nagel, May 28, 1938; from Yvonne Nagel's personal collection.

fundamentally believed—despite the gloomy observations of others and himself, obviously—that his planned life in the Czechoslovakian Republic could continue in the long run. Nonetheless, soon Frank would be outside of Czechoslovakia in the United States. His optimism and pessimism continued to vacillate until war broke out in September 1939. At that point, finally, Frank abandoned his loyal hope to remain living and working in Prague (Hofer 2021).

8.3 A Life-Saving Lecture Tour

In 1938, Frank turned fifty-four years old. Under normal conditions, he would have had a decade more to direct his institute, finish his official assignments, and to start think about his well-deserved pension time, presumably filled with travel, speeches, conferences, and holidays in the mountains. But at the age of fifty-four, Frank asked for an unpaid sabbatical from the University to give lectures in the United States that were arranged by the Institute of International Education. At that point, war seemed avoidable. Frank probably had in mind just a trip to explore the States, in case he might be later forced to follow his friends and colleagues who had emigrated.

The Institute, still existing today, was established in 1919, by Nicholas Murray Butler (President of Columbia University), Elihu Root (former Secretary of State), and Stephen Duggan (professor at City College of New York)—the latter being its director until 1946. In the wake of the Great War, they hoped that their new institute could foster mutual understanding between nations. In his 1938 annual report, Duggan stated that “international misunderstanding exists today to an unusual extent.” But Duggan and his Institute were optimistic nonetheless: “it is safe to say that cultural relations have probably done more to draw us towards the desired goal than any other activity that has yet been attempted” (IIE 1938, 29).

For the Institute, the most important way to achieve, or at least to contribute to the goal of building understanding and acceptance was to develop a student-exchange program—first between the United States and Europe, and later including Asia and South America. American students, upon finishing their college education in the U.S., would then enroll for postgraduate work in a foreign university. Through 1929, at least 5000 American students went abroad, and the Institute helped close to 10,000 foreign students attend American colleges and universities. Eventually the program was extended and a new category of “visiting professor” was established. Visiting professors were recommended by the Institute’s representatives abroad, based on their scholarship and ability to lecture in English. The terms were that universities and colleges at which visiting professors lectured

paid the lecturer's fee, while the Institute would "draw up his [sic] itinerary and program" (IIE 1938, 35).

Through 1938, the Institute organized lectures and lecture tours for 245 foreign scholars and "men of affairs" who visited 1782 colleges, universities, and other institutions. The visiting professors would spend half a week at the campus, "lecture to students on problems of international affairs and discuss with faculty members and advanced students details of these problems," often inviting the public as well (IIE 1938, 35).

Frank's lecture tour was conceived by the Institute in early 1937 after Carnap contacted the Institute to inquire about a possible tour for his former colleague. Frank wanted to speak generally on behalf of logical empiricism as a new movement, known in America through logicians and pragmatists, but not yet applied to the understanding of modern physics, his area of expertise. "Being equally familiar with the modern physical theories and the new logic of science, I think I should be able to explain this new conception of exact science to the American public,"¹¹ he wrote to the Institute in his application.

The Institute first investigated Frank's background and reputation (they contacted, for example, the émigré Viennese physicist and longtime friend of Frank, Arthur Haas), and reached out to various scholars for formal recommendation. Three letters of recommendation have survived. The first described Frank as an "eminent scholar" with one of the "keenest critical minds" at present in the field of philosophy of physics and science. Frank was of "high philosophical culture" and possessed "literary talents." He was so talented, the letter emphasized, that he was even recommended as Albert Einstein's successor in Prague after Einstein left in 1912.

The letter writer would know because he was Albert Einstein. Although Frank had asked Einstein to write a few words to the Institute in his favor, he may not have anticipated how important—at this juncture, and again in the future—his friendship with Einstein would turn out to be. By helping Frank obtain his lecture tour and leave Prague when he could, Einstein's letter was arguably lifesaving.¹²

11 Rudolf Carnap to the IIE, December 20, 1936 (ECFSR); IIE to Frank, December 30, 1936; and Frank to the IIE, February 24, 1937 (ECFSR).

12 Frank to Albert Einstein, March 1937; Frank to Albert Einstein, March 1937, Albert Einstein Archive, The Hebrew University of Jerusalem; and Albert Einstein to the IIE, March 10, 1937, Harvard University Archives, Papers of Harlow Shapley, 1906–1966, HUG 4773 (hereafter, HSP). The other two very positive recommendations came from Frank's colleague and friend, the chemist Jaroslav Heyrovský, professor at the German University of Prague in the Department for Physical Chemistry, and from Brackett Lewis, secretary of the American Institute in Prague. Jaroslav Heyrovský to

Even after the recommendations, the Institute's director, Stephen Duggan had reservations about Frank's topics and feared that the tour "will not produce great results." He claimed that the Institute *could* put a note in their Bulletin about Frank's availability to lecture on the philosophical problems of modern physics, "but that is not the kind of subject for which there is a demand for lectures."¹³ The Institute nonetheless published a flyer advertising Frank's planned lecture tour in the States during the autumn of 1937.

The flyer elicited inquiries immediately. This showed interest in Frank and his proposed topics and helped disprove Duggan's fears. One came from North Carolina, noting that in 1936, at the American Physical Society meeting the principal speaker was Niels Bohr. This year, they wanted Philipp Frank as they saw the advertisement in the Bulletin. The letter was signed by the young John A. Wheeler, then an associate professor of physics and later an important American physicist who collaborated with Bohr and Einstein.¹⁴ Unfortunately, Frank was unable to deliver the lecture that Wheeler requested since his tour was delayed. In the end, he left Prague in the summer of 1938, and departed from England to America at the end of September, after the Cambridge Congress for the Unity of Science.¹⁵

On his way, he visited the London-office of the Society for the Protection of Science and Learning, founded in 1933 as the Academic Assistance Council and renamed in 1936 to help refugee academicians from the continent. The Society was established by the economist-politician William Beveridge who learned about the German situation during a tour in Vienna and immediately set up a plan to gather British politicians and philanthropists to organize a rescue committee. There Frank was asked about the political situation in Prague and the crisis in Czechoslovakia. A few weeks after his visit to this office, Frank sailed to New York. It was a long journey, with sufficient time to prepare lectures, and to conduct discus-

Stephen Duggan, May 14, 1937 (HSP); Brackett Lewis to Stephen Duggan, May 15, 1937 (HSP). On Haas and Frank, having very similar scholarly and life paths until their emigration, see Wiescher (2021).

¹³ Stephen Duggan to Brackett Lewis, May 28, 1937 (ECFSR) and Mary Waite to Rudolf Carnap, May 7, 1937 (ECFSR). Further positive letters were sent to the Institute by Charles Morris (May 1, 1937), Carnap (April 17, 1937), and Arthur Haas (April 13, 1937), all kept in ECFSR.

¹⁴ John A. Wheeler to Stephen Duggan, October 21, 1937 (ECFSR). For further letters, inquiring about Frank's availability as a lecturer, see S. B. Arenson from the University of Cincinnati Section of Sigma Xi (October 14, 1937); Edward S. Allen from Iowa State College (October 27, 1937); all kept in ECFSR. But Frank's advertisement note was sent to Virginia, Michigan, Harvard, Cornell, Columbia, Northwestern University, Ohio State University, Wellesley College, Syracuse University, Indiana University, University of Missouri, and Drexel Institute in Philadelphia.

¹⁵ Frank to Ernest Nagel, May 28, 1938, Archives of Scientific Philosophy, Ernest Nagel Papers, 1925–1982, ASP.2020.01, Archives & Special Collections, University of Pittsburgh Library (hereafter, ASP-ENP).

sions with various people on board. One topic of discussion was ominous and unforgettable. In a letter to his close friend Albína Dratvová, Frank described how news of the “Munich agreement” was broadcast on the ship. As part of a Western strategy to appease Hitler, the agreement allowed Germany to annex the Sudetenland, part of Czechoslovakia. As Frank described the fallout: “It was a French ship. At first, the French and the English people did not understand the news. They were rather happy that war has been avoided. But there were also Czech people on the ship, and they immediately knew what it was about and were very unhappy.”¹⁶

Aiming to get back to Prague no later than early January 1939, Frank obtained a “visitor’s visa” that was valid only for a few months. Perhaps naively, given this diplomatic success for Hitler’s expansive ambitions, Frank still hoped that he could return to Prague in the winter of 1939 and continue his career. He was mistaken about this—perhaps distracted from the gravity of the situation by the busy, exciting lecture tour ahead of him. According to his letter to Dratvová, he was scheduled for talks at twelve universities, often jointly sponsored by Philosophy and Physics Departments, mainly about the philosophy of physics, of relativity, quantum mechanics and metaphysics.¹⁷ As the tour began an officer at the IIE sent a circular letter to all the colleges and universities where Frank lectured to inquire about their impressions, the reception of the lectures, and their opinion of Frank’s talks. Some of the replies survive to provide a picture of Frank’s initial American reception. Charles Morris, who was his close friend and a sympathizer of logical empiricism and the Unity of Science Movement, claimed that in Chicago they were “highly pleased with Frank as a person and a lecturer.” Frank was able to attract many people from various departments with “different standpoints” and faculty and students were “uniformly appreciative.”¹⁸

Frank lectured at the nearby University of Illinois as well. Glenn R. Morrow, professor of philosophy, wrote that Frank was competent but his topic, “the logic of science,” did not attract large audiences after his initial lecture. Morrow’s view was not universal, however. Some of the university’s best undergraduates “were much interested” in Frank’s topics, according to P. Gerald Kruger, professor of physics, who reported that the lectures were “decidedly successful.”¹⁹

Frank gave two lectures at the University of Notre Dame, where he spoke to the Departments of Physics, Mathematics, and Philosophy. All participants agreed that Frank’s talks “were very much worthwhile” and stimulated a great deal of dis-

16 Frank’s letter to Albína Dratvová, October 25, 1938, in Podaný (1995, 136).

17 “Philipp Frank (Prague), Lecture Tour” (HSP); brochure advertising Frank’s American lecture series, October–November 1938 (ASP-ENP).

18 Charles Morris to Edgar J. Fisher November 20, 1938 (HSP).

19 M. T. McClure to Edgar J. Fisher, December 14, 1938 (HSP).

cussion, according to the head of the Physics Department. A famous attendee of the lectures was the above-mentioned friend of Haas who was by that time a renowned professor of physics at Notre Dame.²⁰

At the Graduate School of the University of Oklahoma, Frank's appearance "was a very great success." The audience was bigger than expected and the university was "more than pleased" since Frank's talk attracted professors from other schools and universities.²¹ The *Oklahoma Teacher* reported that Frank discussed popularized accounts of science and "pointed out incorrect popular attitudes which result from attempts to read metaphysical meanings into statements of scientific principles and suggested possible ill effects which might follow the spread of such attitudes." Frank's lecture was so successful, evidently, that he believed he might be offered a one-year position at the flagship campus in Norman.²²

Finally, Frank lectured at the California Institute of Technology, Pasadena, after which Robert A. Millikan, the 1923 Nobel Prize Winner in physicist who knew Frank personally from Prague, reported that Frank drew "relatively large audiences from all our scientific departments." He "really understands both modern physics and philosophy and is therefore more competent to treat these two fields jointly than anyone else I know" Millikan added. Indeed, Frank's dual-expertise was becoming central to his reputation. Among the first courses Frank taught at Harvard was "philosophy of physics"—"the first systematic course" in this hybrid subject, the *New York Times* reported—one that would surely clarify and bring scholarly credibility to popular speculations and misconceptions, the announcement implied.²³

20 Henry J. Bolger to Edgar J. Fisher, November 23, 1938 (HSP). Later, when Frank was about to negotiate a place for himself in the country, he did not consider Notre Dame. He wrote to Richard von Mises, when he was also looking for a job in America, that he does not recommend the University of Notre Dame, "because there are already some Viennese who always do you more harm than good." He surely did not have Haas in mind, who just helped him with the IIE. In fact, he named Karl Menger "in particular [who] fights any hiring of foreigners." Frank to Richard von Mises, April 16, 1939, Harvard University Archives, Papers of Richard von Mises, HUG 4574.2 (hereafter, RVMP). Quoted from and translated by Siegmund-Schultze (2024). This claim of Frank among the competition between émigrés requires a substantive interpretation, which we cannot give here, though it already shows the even more complicated situation at the universities that Frank had to face as well.

21 Homer L. Doge to Edgar J. Fisher, November 28, 1938 (HSP).

22 Frank to Shapley, March 17, 1939 (HSP) As far as we know, Frank never went back to lecture or teach in Oklahoma.

23 Robert A. Millikan to Edgar J. Fisher, December 7, 1938 (HSP). Cf. "Reported From the Fields of Research," *The New York Times*, April 21, 1940. The announcement of Frank's course at Harvard appeared without a byline. It was probably written, or at least edited by Waldemar Kaempffert

In addition to the twelve universities listed on Frank's IIE Itinerary, he also lectured at the University of California at Berkeley in November on "modern physics and common sense" and again on the same topic in December to the Philosophy and Physics Departments at Columbia University.²⁴ These lectures and an additional invitation from Brown University suggest that Frank's reputation and appeal were growing and spreading as his lecture tour continued.²⁵ In a letter to Edith Nagel, Frank joked that he felt "like a wandering actor who gives in every town his performance."²⁶ But Frank surely understood that his tour was a success that boded well for a future in the United States. This doesn't mean that Frank did not want to go back—he considered Prague to be his home, noting in a letter, "I would gladly stay in Prague and my wife sticks to Prague very much." Nor does it mean that Frank looked forward to rebuilding his career after being established in Prague. "Our university, where I was active for 25 years will hardly exist and I really do not know with what I will go further in my life,"²⁷ he wrote to a friend. But Frank ultimately did not have much of a choice in these matters.

8.4 Stuck in the Land of Promises

Soon after his tour concluded, Frank's visitor's visa was about to expire, and in February 1939 he asked for an extension of his unpaid leave from the German University of Prague. He received the extension along with a trickle of news from Prague that painted a bleak, "sinister" picture that Frank described to the Society for the Protection of Science and Learning in London, whose office he had visited on his way to the States. In a letter, Frank wrote,

The pressure of the Nazi government has forced the Czech ministry to nazify the German university. Though the Sudetenland is now separated from Czechoslovakia and belongs to Germany, the German authorities force the Students who are of *Sudetengerman* origin to study at the German university in Prague, in order to form a center of Nazi propaganda within Prague. As

(see below), and therefore shows once more Kaempffert's journalistic support of Frank and the unity of science movement, more broadly.

²⁴ "Campus Note," *The Columbia Daily Spectator*, December 9, 1938; Carnap's diary entry, November 22, 1938 (Carnap's Diaries 1936–1970). "Physics Lecture," *The Daily Californian*, November 18, 1938, p. 11.

²⁵ Frank to Albina Dratvová, October 25, 1938, in Podaný (1995, 137); R. B. Lindsay to Edgar J. Fisher, October 12, 1938 (ECFSR).

²⁶ Frank to Edith Nagel, October 28, 1938; from Yvonne Nagel's personal collection.

²⁷ First letter to Jaroslav Heyrovský, second to Albina Dratvová, quoted from Podaný (1995, 137 and 138).

I understood from Vienna, all students, who have their home in *Sudetenland*, are excluded from the Austrian and German universities, in order to force them to fill up the German university in Prague and to serve willy-nilly as Nazi agents among the Czechs.²⁸

Within three months, German forces took control of the Czech lands of Czechoslovakia as the “Protectorate of Bohemia and Moravia.” More than seventy professors were removed from their positions, and those few who were permitted to remain were closely supervised and inspected by the Nazis. “The situation in Prague becomes more and more sinister,” Frank wrote. Bits of information that came to him, Frank implied, were cloaked by his old friends and colleagues’ correspondents in coded or “hidden” text, presumably to circumvent Nazi censors:

I received a letter today from Prague from a friend. He wrote in a hidden language that some professors of the German university as well as the Czech one were arrested; e.g. Oskar Kraus ... a man without any political color, Privatdozent: Kurt Sitte ... a Sudeten German, who dared to propagate democracy among the Sudeten German students. Prof. Zd. Nejedlý of the Czech University, a professor of the history of musics, author of a large biography of Masaryk and of the composer, Smetana.²⁹

With reports like this, by early 1939 Frank finally gave up on his plan to shortly return to Prague. “It is impossible for me to continue my teaching job,” he told the Harvard physicist Edwin C. Kemble. In a letter sent from the address of the Institute of International Education, Frank declared that there is “widespread interest” in the States for the problems he has been working on in the last few years. “It would be a great advantage for my scientific work,” he wrote, “if I succeeded to remain in this country.”³⁰

One immediate and enormous problem was that remaining in the States would mean that Frank would lose his salary and savings. As Frank commented to the Society for the Protection of Science and Learning, he now “belong[ed] to the people who have lost their position” and he asked whether the “organization would be able to give me a certain support during the few months I have to wait here.”³¹ But the Society could not help refugees in the United States and suggested that Frank turn to the local Emergency Committee for help.³²

28 Frank to the Society for the Protection of Science and Learning, January 20, 1939, Archive of the Society for the Protection of Science and Learning, Oxford University, Bodleian Archives & Manuscripts, Philipp Frank File, MS. S.P.S.: 327/6.

29 Frank to Edgar J. Fisher, May 8, 1939 (ECFSR).

30 Frank to Edwin C. Kemble, February 9, 1939 (HSP).

31 Frank to the Society for the Protection of Science and Learning, January 20, 1939 (Archive of the Society for the Protection of Science and Learning, Oxford University, Bodleian Archives & Manuscripts, Philipp Frank File, MS. S.P.S.: 327/6; hereafter “SPSL”).

Frank was in contact also with Cecilia Razovsky at the National Coordinating Committee in New York that was established in 1934 and became known after 1939 as National Refugee Service. He explained that his loss of salary was just one problem to be solved. In addition, his visa-situation was dire. He urgently needed help because he had obtained only a temporary, visitor visa to the United States. As Frank's problem became known, his American colleagues went into action to find Frank a position at a college or university that would allow his visa to become permanent. Kemble approached Percy Bridgman, who happily supported Frank's case and wrote immediately to Frank that his chances were "very good" to obtain a permanent position and American citizenship after two years of lecturing. His field of research is "one of great importance, one of increasing interest at this present one, and one with which very few, if any, competent persons in this country are at present occupied." Bridgman also noted that he always read Frank's papers with pleasure and found his "clarity and sanity of thought almost unique," leaving his best supportive remark to the end: "Personally I would be glad to do what I can to help him to a permanent position."³³

While Bridgman reassured Frank that there was demand for his expertise within the American intellectual scene, Frank's Viennese friends counseled him to tailor his philosophical lectures, and even his specific terminology, carefully for American audiences. Arthur Haas advised Frank not to use such words as "idealism" and "materialism" as a foreigner in America because it would be considered "tactless and an interference in American relationships." Neurath agreed with Haas and told Frank not to prepare his lectures in detail, but instead to read the room and "adapt himself" to the "respective forum as far as possible" by talking about "specialties that hardly anyone but [he] knows so well, at least within a radius of 100 miles." During the hiring process of Frank at Harvard, the astronomer Harlow Shapley argued that Frank "has something that no one else in America provides—a special relation to the fields of physics and philosophy."³⁴ This is indeed what Frank did after all, he stood out from the crowd as the major representative in America of the growing unified science movement as a physicist-turned-philosopher with an insider's understanding of professional physics who

³² Esther Simpson to Frank, January 30, 1939 (SPSL).

³³ Percy W. Bridgman to Frank, February 15, 1939; and Bridgman to Cecilia Razovsky, February 15, 1939 (HSP).

³⁴ Harlow Shapley to the Emergency Committee in Aid of Displaced Foreign Scholars, April 10, 1939 (HSP).

could also enlighten the public about the philosophical significance and interpretation of modern achievements.³⁵

Neurath had in fact prepared the ground for Frank's arrival as a public intellectual in the United States. A few years before he had urged Frank to write an article for the American audience that would introduce himself. An ideal place to do that would have been the *New York Times*, where Neurath's cousin, Waldemar Kaempffert was the science-editor. Frank sent to Kaempffert a translation of his talk, "Modern efforts towards the 'Unity of Science'," that Frank delivered over the radio in Prague in March 1936.³⁶

Kaempffert did not publish the paper, but certainly agreed to help Frank become better known to educated Americans. In one of several pieces that Kaempffert wrote about Neurath and the unity of science movement and its new *International Encyclopedia of Unified Science* (IEUS), Kaempffert wrote that "to most Americans the name of Professor Philipp Frank means little, to Europeans much." He drew a lively picture of the Vienna Circle, especially in terms of where they came from (Mach, James, Russell), and borrowed from Frank's manuscript to describe the movement's current plans and ambitions.³⁷

Frank would utilize Kaempffert's introduction by sending a copy to the Institute of International Education when he first inquired about a lecture tour. It helped kickstart Frank's reputation as an expert in both physics and philosophy of science

8.5 Getting a Job

However promising Kaempffert's publicity, the success of Frank's lecture tour and his emerging profile as a public intellectual did not provide Frank with what he needed most: a permanent position that would provide income and allow him to remain legally in the United States.

A series of opportunities appeared, but many were unattractive or impossible. One was the University of Istanbul in Turkey, where Frank would replace Reichembach who went to UCLA in 1938. But Frank had "certain aversions to the pros-

35 Frank to Neurath, September 1937; Neurath to Frank, September 16 and November 18, 1937 (ONN/237).

36 Frank to Otto Neurath, March 1936; Neurath to Frank, March 27, 1936; Frank to Neurath, April 1936 (ONN/236). Frank's radio lecture was called "Neue Bestrebungen für die Einheit der Wissenschaft," *Der Hausarzt*, March 26, 1936, p. 1.; cf. Frank to Neurath, May 5, 1936 (ONN/236).

37 Frank, "Modern efforts towards the 'Unity of Science'" (ONN/382/R.20–2); and Kaempffert, "Unifying the Sciences," *The New York Times*, January 10, 1937.

pect of living permanently in a country," where he has "no opportunity of any scientific contact and collaboration," he noted to Kemble. Frank seemed to realize, however, that he may not be in a position to choose where he would go. In correspondence with Richard von Mises—then at Istanbul—he indicated that he would have been willing to be interviewed by the rector at the University of Istanbul had he not been already in New York that time.³⁸ Frank was desperate enough to tell different people what they needed to hear in order that he keep his options open, even in Turkey.

Another possibility was the New School for Social Research in New York City. In March 1939, Neurath was in active contact with Alvin Johnson and Horace Kallen, the former being the director, the latter a founding professor there. They arranged a deal: if Frank could secure funding in advance, he would be granted a position. Neurath put a lot of effort into this mission, and he quickly secured replies and a positive decision from the leaders of the New School. He even seemed to be proud of what he arranged for his old friend. But Frank, apparently enamored with other possibilities and rumors, let Neurath down by failing to follow up and write to the New School.³⁹ Frank did not burn any bridges, however, for he would later lecture there.⁴⁰

The wait and the uncertainty weighted heavily on Frank and Hania. Financial pressures mounted as well. Frank was not able to retrieve his savings and he had spent the money he had brought with him on costs from his lecture tour.⁴¹ Royalties from Frank's books were now in question, too, because there were non-Aryan authors among the contributors to the famous Frank-Mises (an edited textbook of mathematical physics from the mid-1920s) and sales declined precipitously in Ger-

38 Frank to Edwin C. Kemble, February 9, 1939 (HSP); Frank to Richard von Mises, February 7, 1939 (RVMP).

39 Neurath to Frank, March 7, 1939, and Frank to Neurath, April 15, 1939 (ONN/237); Neurath to Carnap, April 14, 1939, RCP, 102–53–29. Frank commented on his invitations to the New School via Neurath and Kallen also to Richard von Mises, see his letter of April 30, 1939 (RVMP).

40 Neurath to Frank, March 7, 1939, and Frank to Neurath, April 15, 1939 (ONN/237); Max Horkheimer to Frank, November 15, 1939, Max Horkheimer Archiv, Frankfurt am Main, "Briefwechsel Horkheimer-Frank" (I, 7). Frank had held an introductory lecture in Horkheimer's circle in November, due to the extensive interest in positivism. As Karl Korsch—who replied to the talk—wrote in a letter, Frank's lecture was "not uninteresting," although he and the members of the Horkheimer circle talked at cross purposes. See Dahms (1990, 61). Even later, in the mid-1950s, after his retirement, Frank became a lecturer there, having his own full courses on "philosophical interpretations of science," "the liberation of atomic energy," and "the fundamental laws of physical science." See the *New School Bulletin*, September 5, 1955 (vol. 13, no. 1); January 21, 1957 (vol. 14, no. 21), and September 3, 1956 (vol. 14, no. 1).

41 Frank to Edwin C. Kemble, April 7, 1939 (HSP); Frank to Society for the Protection of Science and Learning in London, January 20, 1939 (SPSL).

many.⁴² Indirectly, Frank's financial savior turned out to be Einstein. With the assistance of Kaempffert, Frank negotiated a contract with the publisher Alfred Knopf to write a biography of Einstein. A generous advanced royalty payment took some weight from Frank's shoulders, but only for a while.⁴³

Frank's hopes for a university position were raised in late 1939 when Charles Morris and Rudolf Carnap at the University of Chicago arranged a one-year position for him. But that fell through—largely, Frank believed, because of the contentious academic politics that pitted the university's president, Robert Maynard Hutchins, against positivism and the unity of science movement. Hania was so desperate about the situation that she wrote a letter to an aid committee, suggesting that someone from the university put pressure on the administration.⁴⁴ Upon hearing this news, Frank's acquaintances at Harvard—namely Bridgman, Shapley, and the university president James Bryant Conant—eventually managed to provide Frank with a one-year position that would take care of his visa requirements. After a subsequent one-year contract and sustained support from these colleagues, Frank obtained in 1941 a tenured, half-time position in Harvard's physics department.

8.6 Americanization and Education

Besides his help and support from Bridgman and Shapley, Frank was presumably attractive to Harvard's physics department. During the 1920s and early 1930s, American Physics Departments significantly oriented themselves towards experimental work. They had achieved many important results, but did not care much about theoretical work, which was partially relegated to the mathematicians. Physics teachers who tried to introduce American students to the quantum revolution and relativity theory often failed in their efforts. As I. I. Rabi (who would later befriend Frank) once put it, students of his generation were taught by second-rate scholars—though “to call them second-rate would be high praise” (Hoch 1983, 232).

Frank was among those few European physicists who learned from and came to know the most important European physicists, such as Ludwig Boltzmann, Ehrenfest, Schrödinger, Einstein, Born, and Ernst Mach himself. In Europe, he was also an accomplished theoretical, mathematical physicist, dealing with the biggest names, co-writing an influential handbook, publishing textbook articles and edi-

⁴² Frank to Edwin C. Kemble, February 25, 1939 (HSP).

⁴³ Frank to Edwin C. Kemble, February 25, 1939 (HSP).

⁴⁴ Carnap's diary entry, May 3, 1939 (Carnap's Diaries 1936–1970). For more details, see Reisch and Tuboly (2021, 18–24).

tions, and knowing his way around the most advanced and sophisticated theories. He was not an original, creative physicist, but certainly a good one who was known and respected in the community.

On the other hand, in coming to America Frank was late: he arrived years after the first big refugee boom that included many physicists and mathematicians. And when he arrived, he was welcomed not as a first-rate physicist but as an interdisciplinary figure representing philosophy of science and the unity of science movement.⁴⁵ But even among philosophers of science, including Feigl, Hempel, Carnap, Tarski, and others who had arrived from Europe before him, and among American philosophers such as Charles Morris, Sidney Hook, W. V. O. Quine, and Albert Blumberg, who brought logical positivism to the States after their European tours in the 1930s, Frank was—again—late.

Even one of Frank's most important supporters at Harvard, Percy Bridgman, had doubts that Frank's best days were ahead of him. In a letter to Frank's other prominent supporter Shapley, Bridgman wondered whether Frank could do anything new and novel as the "Vienna School" has shot its bolt."⁴⁶ But once Frank settled into his position at Harvard, he quickly began to distinguish himself and build upon his reputation as the logical positivist who was the most trained and knowledgeable in theoretical physics. While most of his European colleagues pursued more and more refined intellectual agendas and helped to create philosophy of science as a recognized subdiscipline within philosophy apart from the social and cultural ambitions of the unity of science movement, Frank's agenda expanded along with his American audiences. He participated regularly, for example, in the *Conference on Science, Philosophy, and Religion* where he established himself as an expert in science education and, in particular, relations between science education and political culture. At the controversial first meeting of the Conference in 1940, where Mortimer Adler made headlines by declaring that the philosophers' current infatuation with "positivism" was more corrosive to culture than Hitler, Frank offered a thoughtful defense of positivistic science education as a bulwark against Nazism and totalitarianism—the very specters that the Conference had dedicated itself to fighting.

Without an appointment in Harvard's philosophy department, Frank was not equipped to train many future professors of philosophy. Instead he gravitated naturally to teaching undergraduates of different kinds and in different fields. In the United States, where graduate assistants and nontenured faculty taught elementa-

45 On the emigration of mathematicians, many of whom were relevant even within the context of theoretical physics as well, see Siegmund-Schultze (2009).

46 Percy W. Bridgman to Harlow Shapley, March 28, 1940 (HSP).

ry courses, Frank was at first not allowed to teach the introductory and elementary courses he wanted to teach, but the war changed that. After the attack on Pearl Harbor in December 1941, Conant had made the university available for military training and Frank appears to have thrived teaching introductory physics to military officers in training. Frank helped fill a gap, because Harvard's physics faculty shrank during the war. Only "eight of 44 prewar lecturers and instructors were still on hand" by late 1943 (more than 20,000 Harvard men and women were enlisted and "under arms" by the beginning of 1944). Newspapers reported on the strain and noted that even undergraduates and a female teacher were working alongside Frank, who, instead of his usual theory of relativity, now taught elementary thermodynamics. "In a certain way it is a new and interesting adventure for me," he wrote to Neurath. "I come into much closer contact with the students and learn much more of the human side of the American student."⁴⁷ By teaching undergraduate thermodynamics, Frank first came into contact with Thomas Kuhn.⁴⁸

Frank's growing interest and familiarity with "the human side of the American student" set the stage for his involvement in President Conant's postwar educational initiative in General Education. Announced in 1943 and up-and-running by the late 1940s, the program introduced curricular reforms at Harvard that Conant hoped would be adopted by colleges as well as secondary schools across the nation. Having lived and traveled in the United States for four years, and now teaching introductory physics to young soldiers-in-training, Frank was growing familiar with habits, customs, values, and sensibilities of Americans. This allowed him to see a place for the unity of science movement and logical empiricism to find a home, and to grow, in American culture. He excitedly wrote to Neurath in late 1943 that the time was ripe for a "revival" of the unity of science movement.⁴⁹

There were two elements in the nation's character that Frank mentioned to Neurath as he described his ambition. One was America's penchant for practical utility, for invention, for finding ways to quickly and effectively get any job done. As one of his supervisors had once put it, Frank reported to Neurath, he should be sure to give students a 'whipping' and "pack into them as much 'material' as possible and to control as strictly as possible that they have assimilated it."⁵⁰ The other

47 Frank to Otto Neurath, February 12, 1942 (ONN/237); information on Harvard taken from John T. Bethell (1998, 160–161). Bethell named Frank as "one prominent refugee" and Charlotte Houermans from Radcliffe as the woman instructor.

48 Kuhn noted that if he had an undergraduate thermodynamics course, it must have been with Frank. Kuhn graduated in 1943, thus he must have been in one of the first classes of Frank. See Baltas, Gavroglu, and Kindi (2000, 268).

49 Frank to Otto Neurath, December 10, 1943 (ONN/237).

50 Frank to Otto Neurath, February 12, 1942 (ONN/237).

element was the foundation of Conant's new program: the nation's faith in education. During the war, education was key to preparing the military officers who would lead the nation (hopefully) to victory. During peacetime, it was the path by which citizens pursued wealth, happiness, and success. Now was the time for a revival of the unity of science movement, Frank explained to Neurath because "As in this country education has been regarded as a very important and serious job, it seems to me very helpful to make use of this interest as a starting point."⁵¹ Joined to and established within the kind of science education that Conant's new project called for, Conant's reforms could function as "the thin end of a wedge" that could propagate logical empiricism's theory of science and the unity of science movement throughout North American culture. Scientific philosophy could spread and thrive, that is, not only as an esoteric, academic theory of science, but also alongside American pragmatism as a tool for 'getting the job done'—for understanding the world scientifically, as the Vienna Circle had long advocated, and for sustaining modern, democratic culture against the threat of totalitarianism. This was the existential goal behind the annual *Conferences on Science, Philosophy of Religion*, and it was the geopolitical aim of Conant's educational reforms, as well. An effective curriculum for all educated Americans—one that included basic familiarity with modern science, its methods, and its history—would help preserve the essential, democratic spirit of the United States in the face of threats from communism and socialism that Conant—soon to become one of America's leading intellectual cold warriors—glimpsed on the nation's horizon (Reisch 2019; Conant 2017).

Frank became an enthusiastic teacher in the General Education Program, offering courses on "The Philosophical Interpretation of Twentieth Century Physics" (*Natural Sciences* 113) and "Introduction to the Philosophy of Science" (*Natural Sciences* 112). He gave lectures at Harvard (often in connection with its many summer courses) and across the country about general education and the roles of philosophy of science within it. In these lectures and in several publications, both for academics and the public, he articulated his now-familiar picture of scientific theories as logical and mathematical structures that absorbed empirical meaning through "operational" connections to observations statements and empirical measurements. By capitalizing on Bridgman's terminology, Frank took care to present logical empiricism not as a European import, but a natural complement to American pragmatism—a theme he would return to frequently in his postwar writings. He also cultivated a linguistic focus on how scientific theories are interpreted, often metaphysically, by scientists as well as popularizers and political leaders—

51 Frank to Otto Neurath, December 10, 1943 (ONN/237).

a focus that joined Frank's American philosophy of science to the popular general semantics movement, to which his colleagues Roman Jakobson (at Harvard, and previously a friend from Prague) and Charles Morris (at Chicago), himself a champion of the pragmatic tradition, were important contributors (Frank 2021).

In the eyes of many of his colleagues, Frank's writings and his projects were sometimes seen as unprofessional and bordering on 'mere' popularization. Against the backdrop of technical philosophy of science practiced by Carnap, Reichenbach, Feigl, Hempel and others, Frank's writings could seem introductory, even literally childish. As Stephen Toulmin would put it in a 1951 review, Frank's book *Relativity—A Richer Truth* came off as children's literature: "It's sub-title would perhaps be 'Logical Empiricism Told to the Children'" (Toulmin 1951, 181). Hilary Putnam would extend this line of criticism in his 1958 review of Frank's book *Philosophy of Science: The Link between Science and the Humanities*. The profession and its interests had grown up, Putnam suggested, but Frank had not. "Anyone who still thinks that the issue in philosophy of science is between 'operational definition' and 'metaphysical interpretation' might enjoy reading Frank's book. Afterward, he should learn some *real* philosophy of science" (Putnam 1958, 750).

Toulmin and Putnam were right. Frank *did* want to teach logical empiricism to children. But to Frank, such an observation was hardly an insult. It instead acknowledged his ambition to teach the basic ideas and sensibilities of logical empiricism to everyone—to children, to their teachers within the General Education movement, to educated adults interested in semantics, and even to practicing professional scientists whose textbooks and papers, Frank observed, betrayed mistaken understandings of logic, evidence, confirmation, and truth. Only by so promoting and popularizing scientific philosophy, Frank realized—probably correctly, in our view—could scientific philosophy's original enlightenment ambitions have a chance of being realized in the postwar world.⁵²

⁵² For that, see the relevant upcoming chapters of Reisch and Tuboly. Both authors were supported by the MTA Lendület Values and Science Research Group. We are indebted to Sander Verhaegh for his invitation to this project, and to Reinhard Siegmund-Schultze for discussions and help. For more personal acknowledgments, see Reisch and Tuboly (ms.).

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9 Herbert Feigl on the Idea of a “Scientific Humanism”

Abstract: Herbert Feigl played a crucial role in the spread of logical empiricism in the United States. Not only did his and Albert E. Blumberg’s famous article “Logical Positivism: A New Movement in European Philosophy” (1931) help disseminate the ideas of the Vienna Circle in the American context, but his work also proved influential and lasting at the institutional level. Thus, he founded the Minnesota Center for Philosophy of Science in 1953 and co-founded the journal *Philosophical Studies* in 1949 as well as the series *Minnesota Studies in the Philosophy of Science* in 1956. Less well known is that he was affiliated to the American Humanist Association in which several of the emigrated logical empiricists found “an ideology that seemed very similar to our basic philosophical attitude” (Feigl 1981, 78–79). In the present paper, I will focus on Feigl’s plea for a “scientific humanism” as a secularized form of religion and give some information about the context surrounding it.

9.1 Introduction

“Religion is deeper than God” (Dworkin 2013, 1)—this assertion of the late Ronald Dworkin stands paradigmatically, as it were, for the core idea of the view of Herbert Feigl to be discussed in this paper. As is well known, Feigl came to the United States, more precisely to Harvard University, in 1930 financed by a Rockefeller fellowship. In 1931, he published together with Albert E. Blumberg the influential article “Logical Empiricism: A New Movement in European Philosophy.” The same year he was hired as a lecturer at the University of Iowa. In 1937, he received the U.S. citizenship. Three years later he became a full professor at the University of Minnesota in Minneapolis, where he founded the Minnesota Center for Philosophy of Science in 1953. Feigl remained in Minneapolis until his death in 1988.¹

In the present paper, I wish to examine Feigl’s little-researched affiliation with the American Humanist Association, his corresponding plea for a “scientific” humanism, and the historical context surrounding this particular point of view. Feigl’s affiliation with the American humanists, in particular Roy Wood Sellars, might help us better understand the reception of logical empiricism in the United States. More concretely, Feigl and Sellars, despite their disagreements about the de-

1 More information about Feigl’s life can be found in Neuber (2022, sect. 1).

tails, shared a humanistic outlook that might have proved instructive to bridge some of their cultural and philosophical differences. In other words: humanism was presumably the strongest intellectual link in the emigration episode under review here. I will proceed as follows. In section 9.2, I shall provide some information regarding Feigl's emigration to the United States. In section 9.3, his affiliation with the American Humanist Association will be discussed. Section 9.4 deals with Feigl's particular version of a "scientific" humanism. Section 9.5 uncovers some references to related, historically antecedent, views of Roy Wood Sellars. Section 9.6 concludes the paper with a few comments on Feigl's and Sellars' commitment to an overarching *realistic* point of view.

9.2 Feigl's Emigration to the United States

After a short stay as a student of mathematics, physics, and philosophy at the University of Munich, Feigl transferred to the University of Vienna in 1922. Already during his time in Munich, he was worried by the anti-Semitic atmosphere. Thus, in one place he retrospectively reports: "My social and political interests were most dramatically aroused by my experiences (1921–1922) at the University of Munich. Twelve years before Hitler's rise to power, the anti-Semitism in Germany was already quite noticeable" (Feigl 1981, 5). In Vienna, Feigl became a co-founder, together with Friedrich Waismann, of the famous Vienna Circle around Moritz Schlick, under whose supervision he completed his dissertation on chance and law ("Zufall und Gesetz") in 1927. In 1929, he published his monograph *Theorie und Erfahrung in der Physik*. Still two years later, he realized that his prospects in Austria as well as in Germany tended towards zero. In his own words:

During the spring of 1931 ... it became clear to me that my chances for a teaching position in an Austrian or German University were extremely slim. True, the ever so optimistic and kindly Schlick was convinced that I would obtain a *Privatdozentur* (position as a lecturer) at the University of Vienna. But though I was Austrian by birth, I had become a Czechoslovakian citizen after the revolution in 1918. My home was then in Reichenberg (Liberec), in the Sudetenland, where I was born and grew up, and had attended primary and secondary schools. My parents, though thoroughly "assimilated," were of Jewish decent. More realistic than Schlick, I abandoned the idea of a teaching career in Europe, and began applying in a number of American universities. (Feigl 1981, 73–74)

As already indicated, Feigl was hired at the University of Iowa in 1931. Immediately before his emigration, he had received letters of recommendation from Albert Einstein, Percy W. Bridgman, C. I. Lewis, and Alfred North Whitehead. Lewis, at that time in Harvard, wrote in his letter dated April 14, 1931: "Dr. Feigl is one of the

group—with Carnap, Reichenbach, and Schlick—who represents the newly formulated ‘neo-positivism,’ which represents what we in America are sure to regard as the most promising of present movements in Continental philosophy” (cited in Limbeck-Lilienau 2010, 102). In fact, it was Lewis in particular who helped Feigl get the job in Iowa. Interestingly, even in this context, Feigl’s Jewish ancestry apparently played a role. Again, in his own words:

Three universities, Rutgers, New York University, and the State University of Iowa, were the only ones that wanted to “look me over”, and toward the end of May 1931 I visited all three places. The late Dean George Kay, a prominent geologist of Canadian origin, telephoned Professor Lewis long distance. As Lewis later related to me, Dean Kay asked him in detail about my qualification, character, and personality. At the end of that (about twenty minutes!) telephone conversation, Kay finally asked: “Is he a Jew?” To this, Lewis, the noble New Englander, gave the—to me unforgettable—reply: “I am sure I don’t know, but if he is, there is nothing disturbing about it.” (Feigl 1981, 74)

Eventually Feigl and his spouse Maria Kasper settled in Iowa City and sometime later received the U.S. citizenship.

It is interesting to note that Feigl himself supported emigrant scholars from Europe to find employment or at least financial funding in the United States. For example, already based in Minneapolis, he engaged in assisting Rose Rand, a former member of the Vienna Circle, to obtain a grant from the Bollingen Foundation in New York City. After Feigl received a letter requesting his help from Else Staudinger,² the Executive Director of the American Committee for Emigré scholars (March 7, 1955, HFP, 04–122–127), he immediately sent a strong letter of recommendation to the director of the Foundation, ensuring him that there was “no question in [his] mind that Dr. Rand is very well equipped for research in her chosen field and that she holds high promise for a successful completion of her work” (Feigl to Ernest Brooks, March 10, 1955, HFP, 04–122–129).³ Five years later, Feigl wrote a similar letter of recommendation for Maria Reichenbach, the widow of his fellow logical empiricist Hans Reichenbach (see HFP, 04–122–131). By and large, then, the logical empiricists’ network was maintained as far as possible in American exile, as also evidenced by Frank’s involvement in the case of Rose Rand’s support.

2 For biographical information about Else Staudinger, see the 1966 obituary in the New York Times: <https://www.nytimes.com/1966/03/13/archives/dr-else-staudinger-dies-at-76-helped-thousands-of-refugees.html> (accessed July 5, 2024).

3 Quoted with friendly permission of the Philosophical Archive (Philosophisches Archiv) of the University of Konstanz. The signature in brackets is the Archive’s signature (like the following ones too). Many thanks to Dr. Daniel Wilhelm from the Konstanz Archive for the support in the localization of the relevant material.

Feigl himself was, it seems, very grateful for his personal emigration fate. In one place he points out:

As I reflect on my motivations in connection with the Minnesota Center for Philosophy of Science, it seems fairly clear to me that my formative experiences in the Vienna Circle ... have encouraged me to endeavor collaborative teamwork in philosophical research. In this regard, I have found the intellectual atmosphere in American philosophy and science even more favorable than that of the Continent in the twenties and early thirties. On the whole, I have found that American scholars are remarkably open-minded, willing to accept criticism as much as to proffer it. (Feigl 1981, 89–90)

In short, Feigl identified himself and his Center with the American style of doing philosophy. Without his emigration, things would surely have developed worse for him. Due to his emigration, American philosophy gained an important promoter.⁴

9.3 Feigl and the American Humanist Association

The American Humanist Association (AHA) was founded in 1941, emerging from the Humanist Fellowship, which was established in 1927. The AHA's overarching credo—in its current form—is that “humanism is a progressive philosophy of life that, without theism or other supernatural beliefs, affirms our ability and responsibility to lead ethical lives of personal fulfillment that aspire to the greater good” (AHA n.d.). According to Feigl,

our [the logical empiricists'] attitude toward theology and religion was that of the naturalist or scientific humanists. Indeed, several of us found in the general position of the American Humanist Association an ideology that seemed very similar to our basic philosophical attitude. If, as most humanists prefer, ‘religion’ is not connected with any theology whatever, then a deep commitment to such human values as basic and equal rights, the civil liberties, the ideal of a peaceful and harmonious world community, may well be said to be the religion of the humanists—and of the positivists. (Feigl 1981, 78–79)

Among the “positivists” or, better, logical empiricists it was Philipp Frank who, along with Feigl, engaged most forcefully in the American humanist movement (see especially Frank 2021 which, according to its editors, was finished around 1960). Yet, in the case of Feigl, this involvement started already in the late 1930s. In 1939, for example, he gave a talk at a meeting of one of the AHA's regional predecessor organizations, the Humanist Society of Iowa. Moreover, Feigl became in-

⁴ Further information about Feigl's emigration to the United States can be found in Neuber (2018).

volved with the First Unitarian Society of Minneapolis, whose principles were quite similar to those of the humanists. Eventually, in 1944, Feigl was appointed an assistant secretary of the (then already established) AHA.

Remarkably, Feigl repeatedly received requests from representatives of Christian organizations to give talks. For example, on November 3, 1961, C. Theodore Molen, Chairman of the Passavant Lecture Committee of the Chicago Lutheran Theological Seminar, wrote to Feigl:

Dear Professor Feigl:

I trust that Mr. Oscar Schmiede has been in touch with you following his recent conversation with Dean Donald Heiges of Chicago Lutheran Seminary. As Chairman of the Passavant Lecture Committee I am writing to confirm our desire to have you lecture on the campus on March 22 und 23, 1962. ...

The topic of the lecture series this year is “The Post-Christian Era.” ...

The other speaker in this series of dialogical lectures will be Professor Julian N. Hartt of Yale University [Department of Religious Studies; M. N.]. ...

Since we are in the midst of preparing for quarter examinations, more information will be forthcoming later in the month.

Sincerely,

C. Theodore Molen, Jr. ... (HFP, 04–119–93)

Feigl agreed in principle to give a lecture but expressed reluctance to do so. Thus, in his reply to Molen, he explained:

When Mr. Schmiede spoke to me first, he did not mention the title of your series “The Post-Christian Era” (what does this mean?) nor that my engagement would involve discussions with Professor Julian Hartt. While this is quite acceptable to me, I regret I shall not have the time for reading any of his publications; – nor am I at all competent to speak on any of the internal issues of modern theology. As you probably know, my own outlook is that of scientific humanism,—clarified, I hope, by some logical analysis. If, what I have just told you, speaks from your point of view against my lecture engagement, I shall not at all be disappointed if you cared to invite someone else in my stead. May I recommend (in this case) that you think of Abraham Kaplan (UCLA); Sidney Hook (New York University); Walter Kaufmann (Princeton); Charles Frankel (Columbia University); M. S. Everett (Oklahoma A. & M. College, Stillwater, Oklahoma). Each of these scholars knows much more about religion than I do; and each of them is at least close to a humanist outlook. (HFP, 04–119–94)

In 1963, Feigl received a similar request from the Young Men’s Christian Association at Davidson College in Davidson, North Carolina. In his response, as in the previous case, he agreed in principle but added by way of explanation:

I should also say that I possess no thorough, scholarly knowledge of either Judaism or Christianity and therefore do not like to engage in public discussion about religion or theology. It is true I am an outspoken humanist and scientific empiricist, and would be glad to present that

point of view—not as a “substitute” for religion, but in its philosophical distinction from, and relation to, religion and/or theology. (HFP, 04–119–111)

All in all, then, it appears that Feigl purposefully used, or ‘instrumentalized,’ the invitations of Christian organizations to propagate his scientific-humanist position. But what exactly did this position entail? It is this question to which we turn next.⁵

9.4 Feigl on “Scientific” Humanism

Regarding Feigl’s main contributions to the issue of a scientific humanism, the following three papers are worth mentioning: “Naturalism *and* Humanism” from 1949; “Is Science Relevant to Theology?” from 1966; and “Ethics, Religion, and Scientific Humanism” from 1969 (all reprinted in Feigl 1981). In what follows, I will review these three papers and attempt to draw out Feigl’s central claims and arguments. In doing so, I hope to make clear that Feigl’s scientific humanism includes a *moderate ethical non-cognitivism*.

Let us begin with “Naturalism *and* Humanism.” As the title already indicates, Feigl sees no contradiction between these two stances. The subtitle is also revealing: “An Essay on Some Issues of General Education and a Critique of Current Misconceptions Regarding Scientific Method and the Scientific Outlook in Philosophy.” The reference to the issue of education makes it clear that, according to Feigl, modern—naturalistic, science-oriented—humanism and classical humanism in the vein of Erasmus of Rotterdam stand in continuity with each other. However, Feigl also notes close connections between modern humanism and “the Enlightenment of the eighteenth century” (Feigl 1981, 367). What is more, he identifies representative groups of modern humanism, namely American pragmatism, logical empiricism, and American (“liberal”) Unitarianism. On the other hand, he identifies as competing—anti-naturalistic—groups positions such as neo-Thomism, literary humanism, and dialectical materialism, all of which, in his view, are characterized by “reliance on theological or metaphysical presuppositions” (Feigl 1981, 367).

Based on these preliminary clarifications, Feigl specifies his particular understanding of *religion*. He points out:

If by religion one refers to an explanation of the universe and a derivation of moral norms from theological premises, then indeed there is logical incompatibility with the results, meth-

⁵ Let it be noted that, in 1980, Feigl (like other philosophers such as Willard van Orman Quine, A. J. Ayer, and Sidney Hook) signed the “Secular Humanist Declaration” of the Council for Democratic and Secular Humanism (see Kurtz 1980).

ods, and general outlook of science. But if religion means an attitude of sincere devotion to human values, such as justice, peace, relief from suffering, there is not only no conflict between religion and science but rather a need for mutual supplementation. (Feigl 1981, 374)

This aligns well with the AHA’s core message about religion, which is to focus on ethics, values, and living without theistic constraints. The distinctive feature of Feigl’s fusion of religion and humanism lies in its emphasis on *science*. Feigl writes:

[A] mature humanism requires no longer a theological or metaphysical frame Human nature and human history become progressively understood in the light of advancing science. It is therefore no longer justifiable to speak of science *versus* the humanities. Naturalism *and* humanism should be our maxim in philosophy and in education. A Scientific Humanism emerges as a philosophy holding considerable promise for mankind—if mankind will at all succeed in growing up. (Feigl 1981, 377)

Thus, naturalism and the program of a scientific philosophy are seen as the saviors in the dawning age of a modern humanism after the immediately preceding horrors of the Nazi era. “Scientific Humanism” stands for this particular perspective which, it should be noted, contrasts most clearly with Theodor W. Adorno and Max Horkheimer’s *Dialectic of Enlightenment* from 1947, the view of which—especially regarding science—is far less optimistic.⁶

In “Is Science Relevant to Theology?” Feigl’s central question revolves around the idea of a *demythologized* theology. Specifically, the paper addresses the issue of transempirical faith in the sense of the modernists in contemporary theology, that is, thinkers such as Paul Tillich, Rudolf Bultmann, and Dietrich Bonhoeffer.⁷ Feigl’s central thesis is clear. It reads: “Partly demythologized theology is a questionable halfway house, unclear in content, intent, or truth-claim. Theology completely demythologized is no longer a theology at all” (Feigl 1981, 406). In this con-

6 For example, in one place of the *Dialectic of Enlightenment* Horkheimer and Adorno categorically declare: “[E]nlightenment is totalitarian as only a system can be. Its untruth does not lie in the analytical method, the reduction to elements, the decomposition through reflection, as its Romantic enemies had maintained from the first, but in its assumption that the trial is prejudged. When in mathematics the unknown becomes the unknown quantity in any equation, it is made into something long familiar before any value has been assigned. Nature, before and after quantum theory, is what can be registered mathematically; even what cannot be assimilated, the insoluble and irrational, is fenced in by mathematical theorems Thought is reified as an autonomous, automatic process, so that it can finally be replaced by the machine” (Adorno and Horkheimer 2002, 18–19). It is further worth mentioning that Horkheimer explicitly attacked the logical empiricists’ focus on logic and the exact (mathematized) sciences as early as 1937 in his article “Der neueste Angriff auf die Metaphysik.” For details, see Dahms (1994, 97–143).

7 The term ‘transempirical faith’ is Feigl’s and not that of the modernist theologians’. It is meant to refer to the belief in orthodox theological dogmas.

text, it should be seen that Bultmann's view in particular was quite close to Feigl's own conception of a scientific humanism. For example, in his 1941 *New Testament and Mythology*, Bultmann writes:

Can the Christian proclamation today expect men and women to acknowledge the mythical world picture as true? To do so would be both pointless and impossible. It would be pointless because there is nothing specifically Christian about the mythical world picture, which is simply the world picture of a time now past which was not yet formed by scientific thinking. It would be impossible because no one can appropriate a world picture by sheer resolve, since it is already given with one's historical situation. (Bultmann 1984, 3)

In short, modern man is incapable to understand the thought and language forms of the antique, pre-scientific past. Bultmann therefore argues for the uncovering of the non-mythical core of the faith of Christianity through what he calls an existential interpretation.⁸ This should make it possible to understand the New Testament, written from the mythical worldview, in a form appropriate to modern man in the context of today's prevailing scientific worldview. Accordingly, in another passage, Bultmann claims: "We cannot use electric lights and radios and, in the event of illness, avail ourselves of modern medical and clinical means and at the same time believe in the spirit and wonder world of the New Testament" (Bultmann 1984, 14). For Feigl, passages like these amount to an *ethicization* of theology (just as in Kant). He points out:

If we go to the extreme of demythologization (and must we not, in all consistency, do just that?), what else is left but the *moral message* of religion? ... Jesus, along with Moses, the Prophets, and Mohammed, is then viewed as an—indeed exceptional—but still entirely human and highly progressive teacher of morality. (Feigl 1981, 404)

In short, theology without myth is no longer theology. Rather, it becomes a secularized religion being compatible with both a worldly morality and a corresponding scientific attitude.

Feigl's "Ethics, Religion, and Scientific Humanism" is no doubt the most interesting piece in the given context. In this paper, Feigl sets out to defend scientific humanism as an "ethical outlook" (Feigl 1981, 408). Proceeding from the assumption that there are different kinds of belief, he raises two questions, namely:

⁸ It should be noted that Bultmann's notion of an existential interpretation is quite complex both in terms of its origins and in terms of its critique. Suffice it to mention that this notion has its philosophical roots in certain conceptions to be found in the works of Wilhelm Dilthey and especially Martin Heidegger, and that it was vehemently criticized by catholic theologians in particular. For further details, see Hübner (2003) and Jaspert (2014).

a) “What do you mean?” and b) “How do you know?” (see Feigl 1981, 409). As Feigl is eager to emphasize, these two questions lie *beyond* the “so-called warfare between science and theology” (Feigl 1981, 409) invoked by the late nineteenth-century historian, diplomat, and Cornell University co-founder Andrew D. White (see White 1896). Nor is it about a *rapprochement* between theology and science. Feigl states:

We are no longer primarily concerned with discrepancies between the modernists and theologians, nor to revise theological doctrines in such a fashion as to make them compatible with science.... Rather, the question, ‘What do you mean?’ really is the central issue; namely, the meaning of the word ‘belief’ or of any cognate terms. (Feigl 1981, 409)

Equipped with this analytic methodological strategy, Feigl goes on to distinguish between three different meanings of the word ‘belief’ (see Feigl 1981, 410–414). This first meaning is *empirical* and illustrated by sentences such as ‘I believe that there might be a rainstorm tonight.’ Such sentences are capable of observational test (which, in turn, gets optimized in the context of science). The second meaning of the word ‘belief’ is *transempirical* and illustrated by sentences such as ‘I believe in resurrection.’ Such sentences are a matter of faith and as such belong to the realm of theology. The third meaning of ‘belief’ is associated with what Feigl programmatically calls *commitment* and illustrated by sentences such as ‘I believe in human equality.’ Such sentences involve “taking a firm attitude” (Feigl 1981, 413); but, according to Feigl, they do *not* express a knowledge claim.

Now, in a next step, Feigl confronts us with two theses. *Thesis One*: “you don’t get any place with ethical justification unless you start with certain commitments” (Feigl 1981, 416). Feigl is drawing here on Kant’s distinction between hypothetical and categorical imperatives and proposes to make the same distinction “in plainer language” by dividing “conditional” from “unconditional” imperatives (see Feigl 1981, 416), equating the latter with the belief type of commitment. *Thesis Two*: “there are ethical ideals that seem fairly basic in human concerns” (Feigl 1981, 417). Feigl is thinking here, as an example, of the ideals of the Jewish prophets, according to which the principles of goodness, fairness and equality are crucial to morality.

All of this fits well with humanism. But what about the naturalist component of Feigl’s stance? Matters can be clarified if we consider the following passage from Feigl’s 1969 approach:

There is a golden mean that combines the valid element of monism—i.e., that ethical principles are universally applicable—with the empiricism of relativism which teaches that human values are related to human nature. If you want a label for this call it ‘scientific humanism.’ (Feigl 1981, 418)

Thus, the scientific-humanist approach entails a naturalized conception of universally valid ethical principles. However, Feigl's naturalist approach is *not* an eliminative one, as can be seen from the following passage:

There are certain activities and abilities of the human animal that are essential for his survival. But when civilization takes over, something else supervenes in addition to what was a purely biological function in the first place.... The original functions ... do not fade out but are supervened by the further functions that represent our higher cultural activities.... Sexuality may become love. I don't recommend that sexuality fade out, but love is something more than mere sexuality. (Feigl 1981, 418–419)

Passages like this have led Wulf Kellerwessel to assign Feigl's stance to *ethical cognitivism* and thus to distinguish it from Rudolf Carnap's corresponding views (see Kellerwessel 2010, 178–179). I dare say that this is not correct. In point of fact, Feigl, just as Carnap, defends a *moderate ethical non-cognitivism* (for Carnap's particular approach, see Damböck 2022). Remember that, according to Feigl, commitments are not knowledge claims. If they were, then his position would indeed belong to the cognitivist crowd in ethics. However, just as with Carnap, Feigl maintains that a distinction should be made between 'internal' and 'external' questions regarding the choice of a relevant 'framework.' Thus, in his 1952 "Validation and Vindication: An Analysis of the Nature and the Limits of Ethical Arguments," Feigl refers the reader to Carnap's 1950 "Empiricism, Semantics, and Ontology" and emphasizes that Carnap's paper contains an "extremely important and clarifying discussion of the distinction between questions within a presupposed frame and questions concerning the frame itself" (Feigl 1981, 391, fn. 3).⁹ Already in his essay "Existential Hypotheses: Realistic versus Phenomenalistic Interpretations" from 1950, Feigl had taken up this distinction from Carnap, albeit in the context of establishing a certain form of scientific realism (for details, see Neuber 2011). In "Validation and Vindication," Feigl applies the distinction to ethics, intending to suggest a moderate non-cognitivist approach.

So, what is this approach all about? The following passage from "Validation and Vindication" will help answer this question:

⁹ The corresponding passage in Carnap's original paper reads as follows: "And now we must distinguish two kinds of questions of existence: first, questions of the existence of certain entities of the new kind *within the framework*; we call *internal questions*, and second, questions concerning the existence or reality *of the system of entities as a whole*, called *external questions*" (Carnap 1950, 21–22). As can be easily seen, Carnap is talking about ontological and not ethical issues in his 1950 paper.

[T]he supreme norms of a given ethical system provide the ultimate ground for the validation of moral judgments. No matter how long or short the chain of validating inferences, the final court of appeal will consist in one or the other type of justifying principles. Rational argument presupposes reference to a set of such principles at least implicitly agreed upon. Disagreement with respect to basic principles can thus only be removed if the very frame of validation is changed. This can occur either through the disclosure and explication of a hitherto unrecognized common set of standards, i.e., still more fundamental validating principles to which implicit appeal is made in argument, or it can be achieved through the pragmatic justification of the adoption of an alternative frame, or finally, through sheer persuasion by means of emotive appeals. (Feigl 1981, 386)

Accordingly, it is important for Feigl to distinguish between the supreme norms or basic principles of an ethical system on the one hand—that would be the *commitments* from before—and moral judgments on the other. While moral judgments are *validated* by basic principles, the basic principles themselves receive their justification through what Feigl calls *vindication*. In the context of vindication, knowledge claims are out of place. Rather, it is (in the last analysis) the emotive appeals that guide us in justifying our commitments. However, Feigl argues, “[t]here is a great deal of validation in ethical arguments which is only too easily lost sight of, if attention is primarily fixed upon persuasion or vindication” (Feigl 1981, 388). Therefore, his non-cognitivism is a *moderate* one: Although our fundamental commitments can only be vindicated by pragmatic means, moral statements are capable of validation and thus cognitively relevant. Feigl explains:

In analogy to the analysis of justification in the cognitive domain I suggest that moral judgments are to be reconstructed as knowledge-claims and as subject of validation (or invalidation) by virtue of their accordance (or non-accordance) with the supreme norms of a given ethical system. In order to carry out this reconstruction, judgments of right and wrong, and likewise statements of obligation and of rights, must be construed as empirical propositions. (Feigl 1981, 388)

Whereas the radical non-cognitivist would regard fundamental commitments (basic principles) and moral statements as equivalent and, consequently, both as purely emotional, Feigl is concerned to ‘rescue’ moral statements as knowledge claims. For this reason, he considers his own approach “in more than one way closer to the Kantian” (Feigl 1981, 390) than to the emotivist view as advocated by Charles L. Stevenson (cf. Stevenson 1944). On the other hand, with respect to fundamental commitments, he explicitly admits that “the relativism implicit in the emotivist analyses (of Stevenson, for example) may prove insuperable” (Feigl 1981, 388).

On the whole, then, Feigl’s scientific humanism, being based on fundamental commitments such as ‘I believe in human equality,’ can only be justified pragmati-

cally and this (in the last analysis) by emotive appeals. The moral statements *contained* in this particular frame, e.g. ‘Being racist is wrong’ or ‘Behaving fair is good,’ have the status of empirical propositions, just as the answers to internal questions in Carnap’s sense (see Carnap 1950, 22).¹⁰ To be more concrete, in the given context, moral statements like these are part of a liberal (undogmatic) form of *education*. In Feigl’s own words:

I think that all we can do in human society is to avoid the preaching of morality. Instead we should educate by example, and especially in regard to our children. ... If I had any reason to believe that orthodox religions would promote peace and justice in this world I wouldn’t criticize them at all. The empirical evidence seems to speak against them. ... The flame and sword of Islam is one example, the Crusades another. (Feigl 1981, 420)

In light of this empirical evidence against theistic religion, one might plausibly conclude that scientific humanism needs to take over. Or so it is argued.

9.5 Feigl’s Humanist Approach in Relation to Sellars’

Regarding the idea of a scientific humanism, Roy Wood Sellars can be considered an important precursor of Feigl. His main contributions to the humanism issue include *The Next Step in Religion* from 1918 and *Religion Coming of Age* from 1928.¹¹ Moreover, Sellars co-authored the famous “Humanist Manifesto” from 1933. However, although Sellars’ approach is quite close to Feigl’s in its plea for the replacement of the theistic with the humanistic point of view, his interpretation of the sta-

¹⁰ As a reviewer of this paper has plausibly suggested, there is some relation here to Moritz Schlick’s quite similar account of moral statements in his *Problems of Ethics* from 1930. However, it has been argued by others (for example Siegetsleitner 2014, 313–317) that Schlick actually *rejected* the non-cognitivist approach to ethics. At any rate, Schlick’s method in ethics was explicitly psychological and thus implied the *subordination* of a supposedly autonomous ethics to empirical psychology (see Schlick 1939, ch. I, sect. 12). This in turn, I submit, is a strong motive for a non-cognitivist interpretation, even if I cannot elaborate on this point here (for reasons of space).

¹¹ Interestingly, in his retrospective *Reflections on American Philosophy from Within*, Sellars claims that it was he himself who, with his *The Next Step in Religion*, “introduced humanism in a systematic way to the English-speaking world. This was before John Dewey, Sir Julian Huxley and others became spokesman for a similar outlook” (Sellars 1969, 153). Indeed, Huxley’s seminal *Religion without Revelation* did not appear until 1927 and Dewey’s “What Humanism Means to Me” as late as 1930.

tus of *moral values* differs significantly from Feigl’s non-cognitivist approach. It is this point on which I shall briefly focus in the following.

To begin with, Sellars starts his *The Next Step in Religion* with the thesis that “the deepest spiritual life has always concerned itself with the appreciation and maintenance of values. He who acknowledges, and wishes to further, human values cannot be said to be irreligious or unspiritual” (Sellars 1918, Foreword). Nonetheless, Sellars argues, being not irreligious does not entail being a theist. He points out:

Such attitudes and expectations as prayer, ritual, worship, immortality, providence, are expressions of the pre-scientific view of the world. But as man partly outgrows, partly learns to reject the primitive thought of the world, this perspective and these elements will drop from religion. (Sellars 1918, 6)

From here it is only a small step to the following re-definition of religion: “*Religion is loyalty to the values of life*” (Sellars 1918, 7; emphasis in the original).

Now, chapter XVI of *The Next Step in Religion* is entitled “The Humanist’s Religion.” The context of this chapter is what Sellars calls “the shadow of the Great War” (Sellars 1918, 215), which in 1918 had just ended. Its central thesis is that the “religion of human values” must be the “leader” regarding political and economic affairs and that there is no need for a “rabid anti-theism” (Sellars 1918, 215). Instead, Sellars argues for a fusion of spirituality and reason, implying that “reason by itself is not enough” (Sellars 1918, 218–219). He explicitly demarcates his position from the one defended by August Comte, claiming that the latter “was unable to cut himself loose from his association with organized Christianity” (Sellars 1918, 219).¹² Last but not least, Sellars proposes a “marriage of naturalism and humanism” (Sellars 1918, 219). What is meant by this becomes desirably clear when one consults the 1928 *Religion Coming to Age*. There, Sellars argues for a “new” kind of naturalism which is characterized by the “enlarging” of the conception of the natural (see Sellars 1928, 237–238). Specifically, Sellars believes that a *social* level must be added to the biological level if naturalism is to be truly convincing. At the same time, he assumes that the social level is itself rooted in man’s nature, so that humanism remains within a framework accessible by scientific means. In his *The*

¹² It was mainly Comte’s *followers* who upheld quasi-Christian forms of organization and cult all over the world. One thinks of the Chapelle de l’Humanité in Paris, the Church of Humanity in Liverpool or the Templo da Humanidade in Rio de Janeiro. But also in Comte himself one can find what he called the “New Supreme Great Being” (*Nouveau Grand-Être Suprême*) which he understood as a comprehensive system of faith and ritual, including priesthood, liturgy and sacraments. For further details, see Davies (1997, 28–29).

Philosophy of Physical Realism, published in 1932, Sellars summarizes this point even more pointedly as follows:

I speak of naturalism's enlarged span. I mean by this that the naturalistic outlook has spread effectively to human life and social affairs. Gone now is the clumsiness in these matters of the older naturalism. Science and philosophy have marched hand in hand into this territory, science gathering facts and building up new concepts, philosophy analyzing these concepts, suggesting modifications and relating them by means of its principles to the categories of antecedent levels. (Sellars 1932, 19)

Through this “spread of the naturalistic outlook to the human fields,” Sellars continues, the new naturalism—or, as he alternatively calls it, the “new materialism”—“flowers into humanism” (Sellars 1932, 19).

All this is quite in line with Feigl's corresponding views. But there are also significant differences that have to do with the fact that Sellars' position, unlike Feigl's, points in the direction of an *ethical cognitivism*. Thus, for example, in chapter XVII of *The Philosophy of Physical Realism*, Sellars proclaims: “I shall myself take as objective a view of values as possible” (Sellars 1932, 445). Interestingly, Sellars combines this orientation with a clear commitment to *communism* as an “ideal of social organization” (Sellars 1932, 445). Characterizing himself as a “religious humanist” (Sellars 1932, 448), he explicitly rejects relativism (or subjectivism), since, in his view, relativism entails what he calls *factualism*, i.e., the reliance on “brute facts without possibility of revision through discussion and investigation” (Sellars 1932, 453). Sellars explains:

[F]actualism is much like external authoritarianism so far as values are concerned. It discounts development, increased insight, creative understanding. It is abrupt, limited in its time-reference, unaware of the proper approach to questions of value, negligent of possibilities. It is for this reason that the word “should” has no meaning for it. (Sellars 1932, 455)

Accordingly, in contrast to Feigl, who in the context of vindication advocates the relativist perspective and its quasi-authoritarian recourse to emotive appeals, Sellars commits himself to the objectivity and thus the cognitive status of basic commitments, i.e., values.¹³ What is more, Sellars, again in contrast to Feigl, considers

¹³ W. Preston Warren, in his commentary on Sellars' philosophy, correctly recaps the latter's central claim in ethics as follows: “The philosopher indeed has the task of standing back to get perspective on morality and its demands and of undertaking to show which ethical claims are most adequate, and hence defensible. His job is not, therefore, just the disclosure of principles but the determination of the *principle* of moral principles: the meaning and function of morality and its categories, and the how of its most effective formulation” (Warren 1975, 92).

valuing an *autonomous form of cognition* rather than a purely pragmatic matter. In his own words:

What I am trying to do is to make explicit the mechanism of valuing, much as I sought in epistemology to make explicit the mechanism of perceiving. The point is that valuing is a supplementary process which presupposes some measure of cognition and which regards as interpretatively relevant data which are irrelevant to pure cognition. The reason is evident. It is that appraisal is something different from pure knowing. It is a viewing of the object in the light of data which have the capacity to reveal how the object enters the economy of our lives. And it is evident that only subjective data intimately connected with the drama of the self or of the social group ... could have the capacity to disclose this power of the object. (Sellars 1932, 467)

Consequently, non-cognitivism is not an option for Sellars, the reason being that appraisal as the one component of valuing always *presupposes* knowledge as the other.¹⁴

It is important to note, however, that Sellars does *not* embrace value realism in the sense of a thorough (consistent) Platonism, i. e., the view that values exist independently of the valuing subjects. Rather, his perspective focuses on the *process* of valuing and thus on the interplay between the valuing subject and his or her respective object of interest and appraisal. It is against this backdrop that Sellars, in his essay “Can a Reformed Materialism do Justice to Values?” (1944), aims to develop “a *via media* between Platonism, on the one hand, and merely affective subjectivism, on the other” (Sellars 1970, 241). To be sure, the Platonist line in his thought, implies that “axiology cannot be separated from ontology” (Sellars 1970, 240).¹⁵ But the actually important point for him amounts to his “thesis that value judgments—including moral ones—are as genuinely interpretative and referential as cognitional ones” (Sellars 1970, 241) and that “valuation involves a peculiar reflexive story added to the cognitional framework *and having its own kind of*

14 Warren rightly states in this connection: “Comparably, with the non-cognitivists and their view that values are in essence the enforcements of feelings, the problem of value [according to Sellars] is one of definition in relationship to knowledge” (Warren 1975, 82–83).

15 But notice, again, that he explicitly argues against “value reification” (Sellars 1932, 242) in the thorough Platonist sense. The objects of valuing are, in his view, the ordinary everyday objects and persons that surround us. See, in this connection, also the *Fifth Thesis* of the 1933 “Humanist Manifesto,” which reads: “Humanism asserts that the nature of the universe depicted by modern science makes unacceptable any supernatural or cosmic guarantees of human values. Obviously humanism does not deny the possibility of realities as yet undiscovered, but it does insist that the way to determine the existence and value of any and all realities is by means of intelligent inquiry and by the assessment of their relation to human needs. Religion must formulate its hopes and plans in the light of the scientific spirit and method” (quoted from Sellars 1970, 333).

objective significance” (Sellars 1970, 242; emphasis in the original). From this, his critique of *logical empiricism* seems to follow immediately. Sellars writes:

[T]he shortcomings of the logical positivists in their theory of values parallel their sensationism in their theory of knowledge. Since they really have only sensations instead of physical continuants, in the one case, we should expect them to have only feelings and their objective import in the other. (Sellars 1970, 243)

This assessment is undoubtedly distorting since the logical empiricist movement was much more multifaceted than Sellars insinuates here. As we have seen with Feigl, it is *not* only feelings that have “objective import” in the field of ethics. The latter’s moderate non-cognitivism does *by all means* allow for cognitively objective moral statements. It is only the fundamental commitments, or what Sellars considers values, that are transferred to the pragmatic or emotive level of choosing an overarching linguistic framework. Indeed, exactly at this point Sellars and Feigl—albeit both defenders of humanism—part ways.

9.6 Feigl, Sellars, and the Vienna Circle in America

Let us return to the context of Feigl’s scientific humanism, a context that was undoubtedly determined by Feigl’s emigration to the United States. We have already seen that the former members of the Vienna Circle supported each other in the new American context. Moreover, they were supported by American philosophers such as C. I. Lewis, Charles Morris, Ralph Barton Perry, and Willard van Orman Quine. In Feigl’s case, it is interesting to see that *his philosophical position in general began to solidify first and foremost in the American context*.¹⁶ Thus, in his retrospective essay “The *Wiener Kreis* in America,” published in 1969, he reports at one point: “Among the American philosophers who have left me with a lasting and deep impression are—in the early years—John Dewey, Ralph Barton Perry, C. I. Lewis, C. H. Langford, R. W. Sellars, and Morris Cohen” (Feigl 1981, 90). As is well known, Feigl attempted to establish a *realistic variant* of logical empiricism in the course of the 1940s and early 1950s (for details, see Neuber 2011). Roy Wood Sellars proved to be an important source of inspiration in this connection. Feigl looking back:

16 Notice that he was as young as 29 when he came to America.

My own emancipation began in the middle thirties and was stabilized in the forties. Studies and teaching in the field of the philosophy of science helped me regain, refine, and buttress my earlier realistic position. I was also greatly encouraged by the scientific realism of Hans Reichenbach and the realistic epistemologies of my steadfast dear friends Roy W. Sellars and Wilfrid Sellars. (Feigl 1981, 39)

Feigl, being a personal friend of Roy Wood Sellars, was also in correspondence with him. As far as I can gather from the available archival material, there was no exchange between the two on the humanism question. However, there is a highly interesting letter from Sellars to Feigl regarding the realism issue. In this letter—dated March 3, 1945—Sellars again reduces logical empiricism to a rather rude variety of sensationalism. He writes:

Dear Feigl:

I have been writing a critique of positivism—or what[e]ver alias is preferable—in an endeavor to locate just where the movement is opposed to physical realism and a critical type of materialism. In other words, I am trying to find some definite differentia. ... Now I may be wrong about it but it has been my belief that the European positivists knew little about the development of Anglo-American realism at the time they were incubating their position. I gathered as much from a conversation with Franck [sic!]. I gather from your references in your paper [Feigl 1943?] that the movement is very hospitable and inclusive. But I do feel that there must be some principles which are basic. Otherwise it is more like a crowd movement than a philosophy. I hope you will help me to locate the differentia. My suspicion is that you reduce perception to sense-data and do not recognize the factors of denotative reference and symbolism and characterization. In other words, the difference is epistemological. (HFP, 03–221-D)

It is very likely that these lines helped Feigl to sharpen his realistic instincts, especially since what Sellars calls *denotative reference* played a crucial role in Feigl's mature account of (scientific) realism (see Feigl 1950 and Neuber 2011).

Be that as it may, with respect to humanism, Feigl and Sellars represent two different approaches associated with different conceptions of basic values. While Sellars argued in terms of cognitivism, Feigl defended a moderate form of non-cognitivism close to that of Carnap. All in all, the idea of a scientific humanism dominated Feigl's thinking from the late 1930s until the end of his life and it documents that the emigrated logical empiricists had more to offer than merely a logico-theoretical perspective.

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Fons Dewulf

10 The Failed Reception of Voluntarism in Logical Empiricism

Abstract: The migration of logical empiricist philosophy has mostly been evaluated as a success. In this paper, I argue that a central element of logical empiricist philosophy failed to make an impact on post-war US philosophy, namely its voluntarist conception of rationality. I show that Carnap and Reichenbach from the 1930s onward shared a voluntarist conception of scientific philosophy. In their works both highlighted the ineliminable role that matters of volition play in scientific and ethical inquiry. These matters of volition cannot be constrained by reason. This voluntarist conception of rationality entails on the one hand that philosophy's clarifications are not universal, but relativized to the inquiry of its own time. On the other hand, voluntarism also entails that philosophy itself is not a descriptive enterprise, since reflections on aims, intentions and self-interpretations are involved. I argue that the first-generation American students of Carnap and Reichenbach largely missed this voluntarist aspect due to the intellectual and institutional migration of the logical empiricist movement.

10.1 To Be or Not to Be?

What is *Hamlet* about? No play in the oeuvre of Shakespeare, or perhaps in the history of literature, has been the object of more interpretations. Hamlet, prince of Denmark and heir to the throne, returns home. Unexpectedly, he finds his father deceased, his uncle on the throne and his mother remarried to the new king. Unable to make sense of what has happened in his absence, he meets the ghost of his father who reveals that his own brother murdered him. The ghost urges Hamlet to take vengeance, but throughout the play the prince fails to take action. Although this play is typically associated with existential philosophy, I will argue that *Hamlet* can also prove enlightening to understand logical empiricist philosophy, and especially its significance (or the absence thereof) in the development of professional U.S. philosophy in the decades after the Second World War.

There is no doubt that Rudolf Carnap and Hans Reichenbach were intellectually the most important migrants of the logical empiricist movement, given the influence of their work and teaching on scholars such as W. V. Quine (1970), Ernest Nagel (1978), Hilary Putnam (2008), Wesley Salmon (1997) and Adolf Grünbaum (1978), who all played important roles in the professional and intellectual evolution

of American post-war philosophy in general and philosophy of science in particular. In the literature, the migration of logical empiricist philosophy has mostly been evaluated as a success (Hardcastle and Richardson 2003, Tuboly 2021, Verhaegh 2020). In this paper, I revisit Reichenbach and Carnap's shared philosophical project, which formed the background for their work in post-war US philosophy, and I will argue that this shared philosophical project found almost no fertile soil to flourish.

10.2 The Rise of Scientific Philosophy in the West

The end of the Second World War was an important moment in the history of the logical empiricist movement. By the end of 1945, many members had found positions in U.S. academia: Reichenbach at UCLA, Carnap in Chicago, Feigl in Minnesota, Hempel at Queens College, Richard von Mises and Philipp Frank at Harvard. Crucially, however, Otto Neurath had stayed behind on the old continent, in Oxford. Before the war, Neurath had been the central organizer of all the activities of the logical empiricist network, most notably five international conferences on the Unity of Science and the foundation of the monograph series *Encyclopedia for Unified Science*. Just when Neurath was planning to rekindle his organizational zeal and turn his gaze on the reconstruction of Europe, he died at the end of 1945. "We are all quite shocked at the news of Neurath's death," Hempel quickly reported to Carnap. He and Ernest Nagel "felt that this was the time when things ought to be started again." They "had counted on Neurath's contagious zeal and optimism to start the new development."¹

Although the continued publication of the *Encyclopedia* was an obvious part of this "new development," Hempel and Nagel believed a meeting was necessary of the most important members of the logical empiricist network, to talk about the future of their intellectual and practical efforts. Only in July 1947, Carnap, Frank, Morris and Reichenbach founded the *Institute for the Unity of Science* as the official body to negotiate the continued publication of the *Encyclopedia for Unified Science* with Chicago University Press. However, the Institute was created with a greater ambition in mind, to "encourage the integration of knowledge by scientific methods and to conduct research in the psychological and sociological background of science."² These larger plans never materialized: the grand scale of Neu-

¹ Hempel to Carnap, January 1, 1946, Rudolf Carnap Papers, Box 102, folder 46, item 04, Archives of Scientific Philosophy, University of Pittsburgh (hereafter, RCP).

² Institute for the Unity of Science, July 31, 1947, Hans Reichenbach Papers, Box 18, folder 34, item 11, Archives of Scientific Philosophy, University of Pittsburgh (hereafter, HRP).

rath's envisaged never-ending *Encyclopedia* was abandoned, the institute failed to create its own research journal and it never succeeded in continuing the conference series of the pre-war period.

By 1947, it was clear that Neurath's zeal had been an essential part of the success of the logical empiricist network. The momentum that the network had built up in the 1930s through its organizational successes was fading away. At the same time, the necessity of the network itself was also less felt, since most of the members were getting entrenched in professional American philosophy. Still, in order to rekindle the lost momentum of their movement, Carnap envisaged an introductory book about logical empiricism with Carl Hempel. Carnap even wondered whether Hempel should work on his planned monograph for the *Encyclopedia*, *Fundamentals of Concept Formation in Empirical Science*.

Perhaps as a coeditor of the *Encyclopedia* I should not say such a thing, but frankly speaking, I think your planned monograph would by far not be as important as the planned book. I believe that this book or generally a survey and introduction to empiricism is at present a very urgent need. I believe the lack of greater influence of empiricism in this country is chiefly due to the lack of such a book. And in the European countries the soil is just now ready to receive new seed, and it depends upon us whether it will be the rotten old seed of obsolete ideologies or something better that can develop in the future. I believe, any philosophical movement, in order to be successful, needs a bible, a book appealing to a wider public. It cannot live only by the meagre nourishment from specialized articles and books, good though they may be.³

Due to unknown circumstances, the ambition to sow the seed of logical empiricism in the fertile soil of the post-war period never materialized.⁴ Carnap's bible remained an idle ambition. At the same time as Carnap's above plea, Hans Reichenbach pleaded—in a polemical style—before his colleagues at the APA to abandon traditional philosophy whose “individualistic creations” lacked the “absence of general agreement.” The only path open to twentieth-century philosophy was “the adoption of the scientific method” (Reichenbach 1948, 346). By that time, both the rejection of tradition and the call to make philosophy scientific were tropes in logical empiricist philosophy, e.g. in the preface to the *Aufbau* (Carnap 1928, xiii–xiv), the Manifesto of the Vienna Circle (Carnap, Hahn, and Neurath 2010 [1929], sec. 2), in the opening statement of *Erkenntnis* (Reichenbach 1930), or in advertisement of the *Encyclopedia* (Neurath 1937, 270). If logical empiricism was to succeed in North America at the end of the 1940s, it had to succeed in de-

³ Carnap to Hempel, November 18, 1947, Carl Hempel Papers, Box 28, folder 5, Archives of Scientific Philosophy, University of Pittsburgh (hereafter, CHP).

⁴ Hempel's reply to Carnap, if there was one, is not available in the archives.

veloping these tropes into a fruitful philosophical project in which a younger generation could invest.

10.3 Was There a Shared Philosophical Project between Carnap and Reichenbach?

The various migrated members of the logical empiricist network did not have a shared set of philosophical ideas, and publicly only claimed to be united by a shared attitude (Tuboly 2021). In the case of Carnap and Reichenbach as well, there is a substantial difference in their philosophical methods. Below, these substantial differences will be ignored—despite their interpretive importance—in order to highlight the shared project that allowed them to understand each other as common advocates of a scientific philosophy.

The natural place to begin the search for the philosophical vision that Reichenbach and Carnap shared is the first international conference of the logical empiricist movement in 1935 which took place in Paris. There was so much confusion within the network of scholars gathered at the conference that there was no agreed name for their joint activity, let alone a shared understanding of that activity (Dewulf and Simons 2021). However, both Reichenbach and Carnap took the opportunity to clarify their visions of a “scientific philosophy” in front of an international audience—in French and German respectively. Both addresses were never translated into English, but were important programmatic statements.

Carnap and Reichenbach clarified their project by embedding it within a historical narrative. Although Reichenbach thought that history could not play a role in solving philosophical questions, it was important to see “the historical current of ideas that has brought us here, and that determines our place in the history of human thought” (Reichenbach 1936, 28).⁵ That current began, as Reichenbach’s story went, with the birth of the science of mechanics in the late renaissance and the remarkable research program that led to a consensus around Newtonian physics. This great intellectual innovation of the new science went hand in hand with an important reorientation of philosophical thought: instead of finding rational truths, philosophy would aim “to offer an interpretation of this new science” (Reichenbach 1936, 29).

Philosophers sought to understand how the mathematical geometry central to the new physics could be justified, if not by experience. Kant’s solution was the category of synthetic a priori knowledge and the concomitant idea that reason

5 Translations, unless otherwise indicated, are my own.

could have “pretensions to prescribe rules to experience” (Reichenbach 1936, 30). With the advent of relativity theory and quantum mechanics in Reichenbach’s own times, Kant’s solution had to be replaced. This not only entailed that the proposed rules of reason turned out false, but also that philosophy’s self-conception as a rule-giver perhaps had to be altered. “To understand the problems of our own times” (1936, 32), Reichenbach proposed to develop a new conception of the boundaries of man’s rational powers. Logical empiricism was “the culmination of this development that extended through the centuries” (Reichenbach 1936, 35): a novel conception of rationality that no longer pretended to find fundamental principles about nature from pure reason alone. Philosophy had to become “scientific,” not in virtue of being part of empirical research, but in virtue of its ability to offer an improved understanding of the bounds of reason in the face of the revolutionary developments in the contemporary, scientific enterprise.

In a similar vein, Carnap discussed the historical process that led to the scientific philosophy he was advertising at the conference. In the early-modern period, philosophy had abandoned metaphysical speculation of nature for an epistemological investigation of the conditions of possibility of empirical knowledge. Again, reason functioned as a law-giver for science (Carnap 1936, 36). However, in the first decades of the twentieth century, the Kantian solution for the interpretation of non-experiential content in physical theories had to be abandoned. If reason could not guarantee the validity of non-empirical principles in science, traditional epistemology was defunct. At the conference, Carnap called on his colleagues to abandon the problems of such epistemology and replace them purely with questions about the choice of language systems for scientific theories (Carnap 1936, 39). Just like Reichenbach, Carnap proposed a new conception of the boundaries of man’s rational powers. Rational investigations could pertain to the forms of the language of science, or to the objects, processes and facts in a given language. “The investigation of the facts is the aim of the sciences [*Realwissenschaften*] empirical research; the investigation of the forms of language is the aim of logical, syntactical analysis. We cannot find a third realm of objects next to empirical and formal” (Carnap 1936, 40). These two investigations encompass the entire domain of human reason.

The central theme in both of these Paris addresses is the defense of new boundaries that Carnap and Reichenbach envisage for philosophy, quite explicitly in relation to Kant’s conception of those boundaries. To some in the audience (and to some philosophers today), those boundaries were most likely puzzling. Philosophy could no longer investigate the a priori conditions for knowledge, nor could it function as a law-giver to experience in any way (no epistemological rules to guide research, no epistemological foundation). As a result of these boundaries, Reichenbach and Carnap both charted a novel path for philosophy that did not fit

anything that had come before: it was not metaphysics, not empirical research and not rationalist, empiricist or transcendental epistemology. They called it “scientific philosophy,” “logical empiricism,” “logical analysis” or “logic of science.” But what is it?

Two traditional interpretations of the logical empiricism of Carnap and Reichenbach are problematic. First, one of the most common and influential early interpretations of Carnap, by W. V. Quine, took the project to be an update of traditional empiricism, crudely the idea that a theory of knowledge aims to create a warrant for knowledge by reducing it to sense-perception through logical operations (Quine 1969, 78).⁶ However, Reichenbach and Carnap, in the 1930s are both clear that a traditional empiricism is not tenable. In his 1935 address, Carnap refutes the idea that knowledge can be reduced to sense-perception. “The structure, that we assign to an object in any description is not only dependent on the object itself, but also essentially dependent on the form of the language that we use to describe it” (Carnap 1936, 39). Since the form of the language is *fundamentally* “a matter of decision” (Carnap 1936, 40), it cannot be empirically warranted. In the same vein, Reichenbach sees Humean empiricism as a flawed project which sought to provide induction with a justification that was on a par with mathematical knowledge, whereas no such justification can be provided (Reichenbach 1936, 32).

An alternative interpretation of logical empiricism takes it to be some form of naturalism. Several early academic papers for a U.S. audience highlighted the affinity with American pragmatism in this regard (Morris 1935; Somerville 1936; see Tuboly 2021). There are three aspects of Carnap and Reichenbach’s ideas in the 1930s that make such an interpretation plausible. First, both vehemently exclude from philosophical discourse any form of metaphysics that defends some type of a priori knowledge. Second, both claim that philosophy cannot offer any a priori guide for the scientific enterprise: philosophy cannot function as a law-giver for rational investigations. These two aspects align with the general characteristic of naturalism to reject a special status for a priori knowledge, and to deny any higher realm of knowledge over and above scientific knowledge (Friedman 1995, 7; Verhaegh 2018, 2; Kocsic and Tuboly 2021, 9753). A third aspect is their joint call to make the philosophical community scientific. Reichenbach and Carnap frequently liken traditional philosophers to rhetoricians who persuade others to follow their invented system, which led to inevitable systems-wars between philo-

⁶ Ever since the work of Michael Friedman and Alan Richardson, Quine’s interpretation might seem like a non-starter to begin with (Friedman 1999; Richardson 1998). But, the caricature of their philosophy as an extension of Humean empiricism with modern logic was endorsed by many early expositions, like Feigl and Blumberg’s summary of logical positivism (Feigl and Blumberg 1931; see Verhaegh 2024).

sophical schools. Their scientific philosophy was supposed to offer a different model that would make philosophy into a unified field, just like the sciences. This again aligns well with the overall characteristic of naturalism to place philosophy and science in continuity with each other, especially methodologically.

Clearly, the scientific philosophy of Carnap and Reichenbach in the 1930s had many aspects which we now call naturalist. However, there is also a problem with such interpretation. Although Reichenbach and Carnap obviously think that philosophical inquiry isn't autonomous from empirical science, they do not place philosophical inquiry on a par with empirical, scientific inquiry. Reichenbach is clear that philosophy is aimed at offering an *interpretation* of scientific knowledge. Similarly, Carnap understands philosophy as the logical analysis of the scientific enterprise and takes great pain to distinguish the investigation of facts by empirical science from the investigation of language systems—two distinct domains of rational inquiry. Such a sharp distinction between the empirical and philosophical level of investigation problematizes the continuity between philosophy and science that is a hallmark of naturalism.

10.4 Voluntarism Unbound: Decisions Are Not Facts

The above-mentioned schism between philosophy and empirical research is related to an important theme in Carnap and Reichenbach's writings: the distinction between facts and decisions (Richardson 2007, 299). Ever since *Logische Syntax der Sprache*, Carnap claimed that "in logic there is no moral theory. Anyone is allowed to construct their logic, the form of their speech" (Carnap 1934a, 45). "A proposal for a new syntactical construction of a specific element in the language of science is, when viewed on principle, a freely decidable fixation [*eine frei wählbare Festsetzung*]" (Carnap 1934a, 260). Decisions are not right or wrong, but fruitful or unfruitful. In a summary paper for an American audience, "On the Character of Philosophic Problems," Carnap explains that a philosophical position can only have the character of an *assertation* about a specific scientific language in use, or of a *proposal* to construct a scientific language in a particular way. If philosophers discuss the latter type of position, "one is not dealing with a discussion about true or false, but with a discussion as to whether this or that mode of expression is simpler or more pertinent [*zweckmäßiger*]" (Carnap 1934b, 15).

The distinction between matters of fact and matters of decision also features prominently in Reichenbach's introduction to *Experience and Prediction*. The critical task of philosophy, according to Reichenbach, is the analysis of science. To per-

form such analysis properly, one must make a distinction “without which the process of scientific knowledge cannot be understood.” “Scientific method is not, in every step of its procedure, directed by the principle of validity; there are other steps which have the character of volitional decisions” (Reichenbach 1938, 8–9). Some decisions, e.g. a choice of the unit of measurement, yield equivalent scientific theories, while other decisions, e.g. the choice what scientific inquiry aims at, radically change the knowledge enterprise in which one is engaged. Reichenbach called these bifurcational decisions. All these decisions are radically different from statements: “the character of being true or false belongs to statements only, not to decisions” (Reichenbach 1938, 11). One can make a factual statement that a certain decision was made, one can make a logical statement about the fact that something is a decision, but decisions themselves still have a character of their own. “Logical analysis shows us that within the system of science there are certain points regarding which no question as to truth can be raised, but where a decision is to be made” (Reichenbach 1938, 12). Philosophy cannot dictate which decisions to take. From a rational point of view, one is free to choose. Philosophy can only investigate the consequences of decisions for the knowledge enterprise in which one is engaged, and, by doing that, philosophy offers clarification. This clarification exhausts what philosophy can do.

We may point out the advantages of our proposed decision, and we may use it in our own expositions of related subjects; but never can we demand agreement to our proposal in the sense that we can demand it for statements which we have proven to be true. (Reichenbach 1938, 13)

Typical examples of such matters of decision are the units of measurement. In *An Introduction to the Philosophy of Science*, Carnap states that the choice of measuring standard is conventional. Reason does not guide one in that decision. To measure length and time, one is rationally free to choose “a rubber rod and a pulse beat and then paying the price by developing a fantastically complex physics to deal with a world of enormous irregularity” (Carnap 1966, 94). Similarly, the geometry of physical space cannot be decided by experiment and reason alone:

Physicists could have kept Euclidean geometry and could have explained the new observations by introducing new correction factors into mechanical and optical laws. Instead, they chose to follow Einstein in his abandonment of Euclidean geometry. (Carnap 1966, 162)

Given these statements, the scientific philosophy of Reichenbach and Carnap can be characterized by these four basic points:

- 4.1. There is a fundamental distinction between matters of facts and matters of volitions/decisions.

- 4.2. Certain elements of a scientific theory are matters of volition.
- 4.3. Reason leaves matters of volition open to choice.
- 4.4. Evaluating decisions is related to practical ends that an actor takes to be engaged in.

Because these points entail that rational principles alone do not determine specific parts of scientific reasoning, the position can be labeled as a voluntarist conception of rationality. Our knowledge enterprise cannot be understood without taking practical ends into account—ends which result from free choices, unbound by reason. Accordingly, our knowledge enterprise cannot be the object of a theory aimed purely at describing how things are. There is no theory of science possible without taking intentions and aims into account and these cannot be grounded or justified by pure reason alone. This understanding of voluntarism has, more recently, been elaborated and defended by Bas van Fraassen, quite explicitly as an extension of Reichenbach's reconfiguration of empiricism (van Fraassen 2002, 223). Similarly to Carnap and Reichenbach's pleas for a scientific philosophy, van Fraassen's position also entails a non-traditional understanding of what a philosophical position is, namely not a truth-apt belief or statement, but a "stance" (van Fraassen 2002, 47).⁷

Carnap and Reichenbach's voluntarism also results in anti-foundationalism. The logic of science cannot justify some of the necessary building blocks of scientific theories, like its geometry. Since the late 1980s, through the work of Michael Friedman (1999), Alan Richardson (1998), Thomas Uebel (2007) and André Carus (2007), such an anti-foundationalist reading of logical empiricism has been defended against the Quinean interpretation of its philosophy as a modern revival of Humean foundationalist empiricism. In all these contemporary interpretations, the voluntarism is mostly discussed in the context of the non-empirical, constitutive principles in scientific theories, e.g. concerning space and time.

However, ineliminable choices and a role for the will can be found in the entire make-up of our cognitive enterprise. For Carnap, the question whether to conceive physical objects in quantitative terms, like Galilean physics, or in qualitative terms, like Aristotelian physics, is a matter of choice (Carnap 1966, 59). There is no abstract reason that rationally compels one to use a quantitative approach. Also, there is "no general rule how to choose which parts of scientific theories or observations to put into question in the face of contradiction—this is a free choice" (Carnap 1934, 245). So, there is no rationally binding framework to guide theory-

⁷ Richard Jeffrey was the first to dub Carnap's meta-philosophy a form of voluntarism (Jeffrey 1994, 847). André Carus has also investigated Carnap's philosophical development within a voluntarist conception of rationality (Carus 2007, 45).

change. For Reichenbach, the aims of science themselves “are, logically speaking, a question not of truth-character but of volitional decision, and the decision determined by the answer to this question belongs to the bifurcation type” (Reichenbach 1938, 10). This more radical voluntarism makes the historical transition from Aristotelian to Newtonian physics not rationally compulsive, but the result of different choices in the make-up of our epistemic world view, more akin to a conversion than to an argument.

Reichenbach and Carnap also applied their voluntarism to the moral world. Already in the 1930s, Carnap was a well-known ethical non-cognitivist: just like decisions about how to handle contradictions between experiment and theory, decisions about what action one should perform, are volitional, a matter of practical stance [*praktische Stellungnahme*] and do not have a truth value (Carnap 1934c, 177). Such practical stances are often the result of upbringing and external influence from others, but they are not set in stone. One’s stance can be informed by factual information, but it cannot be justified in any theoretical way.⁸ Reichenbach’s 1948 APA address also features an attack on a philosophical ethics: “fundamental ethical principles can be accounted for by the philosopher as little as the axioms of physical space” (Reichenbach 1948, 340). All ethical imperatives are fundamentally “volitional decisions” (Reichenbach 1948, 345). Earlier philosophers, such as the rationalists or traditional empiricists sought to find a proof for the validity of inductive reasoning, just as they sought to establish ethics on an unquestionable basis. This philosophical aim was mistaken: reason cannot function as a law-giver, neither in the cognitive, nor in the moral domain (Reichenbach 1948, 334).

In *The Rise of Scientific Philosophy* (henceforth, *Rise*), Reichenbach develops this voluntarist ethics extensively: “logic does not compel me to do anything” (Reichenbach 1951, 294). Because reason cannot direct decisions, Reichenbach introduces the will, as the subjective source of all volition, that we find in ourselves.

Acts of volition may concern various objects; we want food, shelter, friends, pleasure, and so forth. That we find in ourselves acts of volition, is a matter of fact; they are distinguished from perceptions or logical laws in that they appear as products of our own in a situation leaving us choice. I may go to the theater or I may not; it is my will to go. I may help another man or I may not; it is my will to help him. (Reichenbach 1951, 282)

Distorted forms of morality, that early-modern philosophers like Kant expounded, had concluded that our will, in order to be good, had to be in accord with some

⁸ For further discussion of Carnap’s voluntarist ethics and its historical context, see Damböck (2022).

abstract, rational principle. Reichenbach's voluntarism was a call to "stand on our own feet and trust our volitions, not because they are secondary ones, but because they are our own volitions" (Reichenbach 1951, 291). The ultimate source of volitions had to be purely subjective, originating from the individual itself. Just like Carnap, Reichenbach believes that the will, even though it is a subjective driving force for one's actions, can be influenced by one's surroundings (Reichenbach 1951, 299). The malleability of our volitions is a good thing: individuals harmonize their actions by taking examples from others, discussing their actions with others, and by investigating the consequences of their decisions for their environment. The only role that the philosopher can play in the social harmonization of volitions is the same role as in the domain of science: to clarify which elements in our practical questions are fundamentally related to decisions, and which elements are related to empirical investigations.

Reichenbach's application of his voluntarism to both the cognitive and moral domain is a conscious reinterpretation of Kant's philosophical project. In the APA address, Reichenbach refers to this famous passage from the conclusion of the *Critique of Practical Reason* (Reichenbach 1948, 335):

Two things fill the mind with ever new and increasing admiration and awe, the oftener and the more steadily we reflect on them: the starry heavens above and the moral law within. I have not to search for them and conjecture them as though they were veiled in darkness or were in the transcendent region beyond my horizon; I see them before me and connect them directly with the consciousness of my existence. (Kant 1996 [1788] 5:161)

For Kant, the source of our knowledge of the movement of the starry heavens and of the moral principles for action is autonomous, directly related to the eternal, necessary and constitutive principles for all cognitive and moral beliefs, which can be uncovered through rational reflection on ourselves as thinking subjects. Reichenbach's voluntarism also defends an autonomous source of knowledge in the epistemic and moral domain, but strips that autonomy of its universal and foundationalist pretensions. No universal guiding principles for science or ethics can be found through rational reflection. However, philosophy remains for Reichenbach a reflection on ourselves as cognitive and ethical agents, and this reflection shows the necessity of ineliminable choices that guide the formation of our beliefs in the epistemic and moral domain, but cannot be grounded in a proof or argument.

Carnap and Reichenbach's shared voluntarism can be summarized in the following four elements. These are not limited to the conventional aspects of scientific theories, like the previous four, but are now generalized to the role of reason in any possible human inquiry.

- 4 A. There is a fundamental distinction between matters of facts and matters of volition/decision.
- 4B. Reason leaves matters of volition open to choice.
- 4C. The aims of our scientific and ethical enterprise, which guide our inquiry in those domains, are matters of volition.
- 4D. In clarifying and interpreting scientific and ethical inquiry, philosophy cannot provide rules or principles that determine its aims and evaluate its progress.

Once one takes on this voluntarist conception of what rational reflection can achieve, one must also accept that this voluntarism applies to itself. Consequently, there is no argument that can prove this voluntarist conception of rationality. To take on such voluntarism, to reorient what you take the aim of philosophy to be, is itself more akin to a conversion, a repositioning of the will. There is no argument which proves that traditional philosophical goals are unattainable. There is no compelling proof to abandon the search for some epistemic or moral rules given by reason. Traditional philosophy has to be *overcome, renounced or rejected*:

To those who cannot give up rationalist philosophy, scientific philosophy has not much to say. Rationalism is an emotional bias... To abandon the desire for the unattainable requires a revision of emotional weights. (Reichenbach 1951, 255)

The philosopher, who had to renounce setting up the principles of physics, will be ready also to renounce setting up the fundamental ethical imperatives. (Reichenbach 1948, 345)

What is required for a scientific philosophy is a reorientation of philosophic desires. (Reichenbach 1951, 305)

We reject the use of the Narcotic [of metaphysical philosophy to solve ethical problems]. If others want to enjoy its pleasures, then I cannot refute them on a theoretical basis. But that doesn't mean I am neutral as to how people conceive their decisions. (Carnap 1935, 179)

The discussion of skepticism by Reichenbach is a good example of his defense of voluntarism. The empiricism of Hume was laudable in its rejection of a priori metaphysics, but ultimately failed and ended up in skepticism concerning all empirical knowledge, because Hume upheld the same philosophical desire as rationalist philosophers, to prove or ground our empirical knowledge on fundamental rational principles. These were “unattainable aims” (Reichenbach 1948, 343). The empiricist who turns skeptic does not draw the right moral from Hume's argument, which would be to give up the “rationalist creed” that the truth of knowledge-statements must be guaranteed through arguments (Reichenbach 1948, 338). Reichenbach's history of modern philosophy and science, which he constantly repeats from 1935 to 1951, is an attempt to nudge the reader in drawing the right moral from

the history of philosophy, i.e. giving up the rationalist creed. Reichenbach's history is not an argument: it does not provide a compelling reason to give up traditional philosophical projects.

According to Reichenbach's robust voluntarist meta-perspective, philosophical reflection is itself not factual. This meta-philosophy is perhaps most evident in one of his early popularizing texts, "Die Weltanschauung der Exakten Wissenschaften," written in 1928 for the non-academic journal *Die Böttcherstrasse*.⁹ In this article, Reichenbach described the world view which had developed from modern science as the result of "a gradual redirection of emotions"—a strong voluntarist statement which he repeated in *Rise*. This worldview was itself not "another explicit conceptual structure." Reichenbach categorizes it as a "basic attitude, an intellectual stance, a habit of mind" that envelops the scientific enterprise (Reichenbach 1978 [1928], 241).

Rational knowledge in our sense is not tantamount to categorization within the pre-established cubby-holes of a reason that governs a priori, but simply amounts to unconditional faith in the power of the human capacity for knowledge—within the framework of a critique of its own goals. Thus the rational element is itself subject to change; and it emerges with increasing clarity that the basic stance of science is a faith more akin to an instinct than to rational insight, to will than to knowledge. Thus the will, the tenacious, malleable, indefatigable, and yet eternally modifiable will, is probably the basic element that truly represents the world view standing behind the scientific investigation of nature. (Reichenbach 1978 [1928], 244)

In order for rational inquiry to avoid skeptic inactivity, given the absence of universal rules to guide its trajectory, one needs the courage to decide what one aims at and to accept that this aim itself cannot be grounded in any way. As an example of the necessity of ungrounded courage required in our knowledge enterprise, let us return to the plot of Shakespeare's *Hamlet*. Close to the end of *Rise*, Reichenbach added an "interlude" discussing the significance of Hamlet's soliloquy *to be or not to be*, to avenge his father or not. Reichenbach dissects Hamlet's inability to act as the result of a lack of epistemic courage. Hamlet only has indirect evidence that his uncle killed his father and so decides to procure more evidence by setting up a play for his uncle. The plot of this play tells the story of a king who is murdered. By probing the response of his uncle to the devised plot, Hamlet hopes to find evidence of his uncle's guilt. Even though his uncle is obviously disturbed by the play, Hamlet still has difficulties deciding whether his uncle killed his father. Although the new evidence increases the reasons to believe that his uncle is guilty, Hamlet fails to accept it. Reichenbach has his logical Hamlet complain: "I want certainty, but all the logician has for me is advice to make posits" (Reichenbach 1951, 251).

9 Richardson (2005, 84) discusses this text in relation to Reichenbach's transcendental heritage.

Reason alone cannot guide Hamlet: in order to act, courage is required. Because Hamlet desires a ground for his belief, guided by traditional epistemic desires of certainty and ultimate truth, just like early-modern philosophers, he ends up as a skeptic unable to do anything.¹⁰ Reichenbach's moral is clear: reason or logic becomes useful in life only in combination with the courage to accept that belief and action ultimately spring from the will.

10.5 Voluntarism's Modern Anti-Naturalist Revival

In contemporary philosophy, Bas van Fraassen has explicitly taken up Reichenbach's voluntarist conception of rationality. In close parallel to Reichenbach, van Fraassen defends that traditional epistemology, either in rationalist or empiricist guise, is a defunct project that reached for the unattainable, namely a rational foundation of our knowledge claims. Instead of searching for such a foundation, van Fraassen offers a voluntarist picture of our rational capacities: "rationality is in some good sense empty; any truly coherent opinion is truly rational" (van Fraassen 2000, 273). Much more explicit than Reichenbach and Carnap ever did, van Fraassen identified that the challenge for such a voluntarism lies in showing how skepticism and debilitating relativism are avoided. To that end, van Fraassen invokes a non-naturalist version of Neurath's boat: as a thinking subject one never finds oneself without prior epistemic (and moral) commitments. Although rationality cannot constrain how to deal with one's beliefs beyond the avoidance of bare incoherence, previous commitments and our understanding of those commitments limit what we believe and how we approach those beliefs.

For at any given time, an individual responds to his experience in terms of and on the basis of his prior opinion and understanding (much of which is built into the language in which he frames that response). This means that although there may be leeway and options, they are highly constrained by the way in which we come to the new experience or new evidence. (van Fraassen 2000, 278)

¹⁰ Alisdair MacIntyre uses the plot of *Hamlet* as an example to discuss the transitions between theoretical frameworks in the history of science. His moral is strikingly close to Reichenbach: no logical reason can compel such transition, and ultimately, no logical set of rules is sufficient to understand scientific progress (MacIntyre 1977). MacIntyre believes that the moral from *Hamlet* is the failure of logical empiricist philosophy of science, and he is seemingly unaware that Reichenbach already used the same play for a similar message.

By relativizing rationality in this way, voluntarism charts a course between *Nothing goes* and *Anything goes*. As thinking subjects, we cannot but understand ourselves from within our given cultural and scientific time-frame. Decisions are not arbitrary, because “they express both the history and the project which constitute the subject’s current stance on the matter” (van Fraassen 2011, 160).

A similar relativization of philosophy is also present in Carnap and Reichenbach’s writings. When they situated philosophy on the same level as science, they meant that the clarification offered by the philosopher cannot but start from the contemporary scientific practice for which such clarification was meant to be significant. At the end of *Syntax*, Carnap emphasized that “all work in the logic of science, in philosophy in general, is condemned to be fruitless, if it is not performed in close contact with the sciences [*Fachwissenschaften*]” (Carnap 1934, 260). Therefore, philosophers work on “the same field” [*auf demselben Feld*] as the scientist, even though there is a different division of attention [*Aufmerksamkeitsverteilung*]. Philosophers have the language of science as their objects of study, and in order to perform relevant interpretive work for the scientific enterprise, the philosopher’s clarification must start from the contemporary understanding of science and its history. In his APA address, Reichenbach makes this clear as well: the analysis of knowledge can no longer start from the abstract, instead it starts from the scientific practice which we find ourselves in and which we value. “In so doing, the philosopher will know that all he can strive for is a philosophy of the knowledge of his time” (Reichenbach 1948, 345). In the German context of Carnap and Reichenbach’s early work, such relativization of philosophy’s aim had already been part of the Marburg Neo-Kantian tradition which took the contemporary sciences as facts whose preconditions it is the task of philosophy to study (Heis 2013, 73).

Van Fraassen explicitly discusses voluntarism in opposition to Quinean naturalism.¹¹ The latter is a type of objectifying epistemology, a factual theory writing project about cognitive functioning in which neither value judgments, nor an intentional subject play any role (van Fraassen 2002, 76). Given the constant emphasis on the non-factual nature of philosophy in Reichenbach and Carnap’s writings, the opposition with objectifying epistemology is a good characteristic to distinguish their meta-philosophy from naturalism as well. Alan Richardson has already discussed the similarity between van Fraassen’s voluntarist reinterpretation of empiricism and the anti-naturalism inherent in Reichenbach and Carnap (Richardson

¹¹ Michael Friedman’s historical work on Reichenbach and Carnap was also aimed to oppose such naturalism—see Friedman’s own APA address (Friedman 1995). Whether Friedman or van Fraassen are correct in their understanding of Quinean naturalism as a factual theory about our cognition, is a separate matter.

2005; 2011). There is one question that Richardson addressed and was so far ignored: “exactly who is making these decisions?” (Richardson 2005, 79) As Richardson pointed out, Carnap after 1935 mostly refused to use vocabulary about the thinking subject or the source of decision/volition, and largely sticks to some extra-systematic ‘we’ that cannot be spoken of in philosophy (Richardson 2005, 86). In contrast, Reichenbach had no scruples talking about a willing subject, but he had no philosophical details to add. Fundamentally, philosophy according to Carnap and Reichenbach only has the resource to reflect on those aspects of our moral and epistemic enterprise which are decisions, and to point out *that* there always is a responsibility concerning the aims of inquiry which cannot be removed by some process of reasoning. In the same way, Carnap and Reichenbach do not have an argument for their voluntarist conception of human reason, and its concomitant reorientation of the aim of philosophy. They can only show their commitment to make sense of science in its historical process, express their conviction of the importance to reorient the task of philosophy, and narrate how they place their commitment as a response to the history of philosophy and science as they understand it—which is the central historical message in their Paris addresses.

10.6 Voluntarism Neglected: The American Students

If Reichenbach and Carnap shared an overall voluntarist conception of rationality that redefined the boundaries of philosophical discourse and put central emphasis on the ineliminable element of volition involved in our ethical and cognitive undertakings as thinking subjects, then why was this not at the heart of further development of the logical empiricist project after the Second World War? One issue, that Carnap identified early on, was the lack of any “bible” that could have guided the reception of the overall project of logical empiricist philosophy. Only Reichenbach’s *Rise* was a candidate for such a bible, but the book suffered from some fatal flaws. As Carnap expressed to Reichenbach, he had enjoyed reading the book, but “in its present form it is of course chiefly meant for laymen.”¹² Such format limited its value and its potential.

Despite Reichenbach’s emphasis on the precision of his scientific philosophy, his book was a rhetorical exercise filled with ambiguous statements. In the conclusion, Reichenbach lamented that the glorification of philosophy’s past had undermined the philosophical potency of the present generation, and consequently

12 Carnap to Reichenbach, September 19, 1951, HRP, Box 37, folder 17.

many students of philosophy had adopted a philosophic relativism and believed there to be no philosophical truth. Scientific philosophy was praised as the antidote: it arrived at conclusions “as precise, as elaborate, and as reliable as the results of the science of our time” (Reichenbach 1951, 325). The implication of this lament would be not only that there is philosophical truth, but that scientific philosophy is the best avenue to reach it. However, this is a very misleading way to put things. Reichenbach had just argued throughout the entire book that philosophers mistakenly through the centuries had attempted to derive truths or provide foundations for truth. The whole point of scientific philosophy’s reconfiguration of what philosophy aims at was to abandon the search for such foundational truth.

The reception of *Rise*, in the academic literature, was mostly negative. In the *Scientific American*, Ernest Nagel complained that Reichenbach’s history of early-modern philosophy was misleading (Nagel 1951, 70). Nagel could not find a consistent understanding of scientific philosophy, other than that it rejects traditional approaches to philosophy and accepts Reichenbach’s own theories. He also lamented Reichenbach’s swooping claims about the superiority of his own frequency interpretation of probability as an aggressive overestimation of his own intellectual capacity (Nagel 1951, 71). Nagel never came close to a voluntarist interpretation of *Rise* as described above. Similarly, in *Philosophical Review*, Norman Malcolm could not wrap his head around the status of scientific philosophy. The supposed solution to philosophical problems on offer in the book were neither the result of empirical methods, nor did the solutions achieve a similar consensus as solutions to scientific problems (Malcolm 1951). Malcolm believed that Reichenbach’s scientific philosophy understood the knowledge of physics and symbolic logic as a necessary condition to discuss *traditional* philosophical problems, which, according to Malcolm, was obviously false. Again, the review failed to bring out the voluntarist project and its concomitant novel boundaries of rationality. Overall, professional philosophers had a hard time taking Reichenbach’s book seriously: important journals like *The Journal of Philosophy* and *Philosophy of Science* did not even publish a review. It is safe to conclude that the *outward* reception of the voluntarist project of Carnap and Reichenbach after the Second World War was non-existent. However, the younger generation that was trained or heavily influenced by Carnap and Reichenbach also did not pick it up.

A good example of the complete absence of voluntarism in the *inner* reception of Carnap and Reichenbach is Carl Hempel. In his *Philosophy of Natural Science*—probably the most influential textbook of philosophy of science in the post-war period—there is no trace of any voluntarism. Whereas Reichenbach gave the element of decision a central place in his critical analysis of knowledge in *Experience and Prediction*, Hempel makes no mention of it. He presents the philosophy of science more or less as a theoretical representation of scientific inquiry, some kind of fac-

tual theory of science (Hempel 1966, 2). He also presents the descriptive and explanatory aims of science as matters of fact that can be taken for granted (Hempel 1966, 47).

Another example of a student of Reichenbach who completely ignored the voluntarism, is Wesley Salmon. In an overview of the intellectual inheritance of Reichenbach and Carnap, Salmon summarized the “spirit of logical empiricism” as a philosophical analysis of a particular scientific concept or theory that is grounded in the latest state-of-the-art developments and uses the best available logic and mathematics to deal with the issues at hand. There is no mention of an overarching non-foundationalist conception of rationality (Salmon 1997, 346). Hilary Putnam, who studied under Reichenbach alongside Salmon, believed that philosophy, in Reichenbach’s view, aims at conceptually clarifying scientific theories. However, the clarification serves a fairly traditional philosophical purpose, to enlighten metaphysical issues concerning the nature of space, time and causality by uncovering what scientific theories say about them (Putnam 2008, 104). Putnam generally interpreted both Reichenbach and Carnap as philosophers who offered a metaphysical picture of the world, but employed science to create that picture, unlike earlier philosophers (Putnam 1991). Putnam understood Carnap’s work in inductive logic as an attempt to provide an algorithm for proving all valid formulas in inductive logic and so formalize the one and only scientific method (Putnam 1983, 198).

10.7 The Eclipse of Scientific Philosophy

The first generation of U.S. philosophers like Hempel, Salmon or Putnam missed the voluntarist conception of reason that was part of the scientific philosophy as advertised by their teachers. Only the second generation of students inspired by logical empiricism, like Bas van Fraassen and Michael Friedman, returned to the voluntarist aspects and elaborated on them. The failure of Carnap and Reichenbach to adequately convey the overarching meta-philosophical project to the American students who followed in their wake should be understood in the context of migration. To survive as a project in the period of migration, scientific philosophy had to make the new boundaries for philosophy in its relation to science both institutionally and intellectually significant. Migration created many intellectual and institutional pressures that prevented success.

From an intellectual point of view, the project of Carnap and Reichenbach, as characterized by 4.A–D, was originally developed within the landscape of interbellum German philosophy. Neo-Kantian philosophers, like Herman Cohen and Ernst Cassirer, had already reconceived the status of synthetic a priori knowledge by

reorienting philosophy's critical task directly in relation to scientific developments of their time. The voluntarism of Carnap and Reichenbach maintained not only this relativization of philosophy's task, but also the non-factual status of philosophy's critical clarification.¹³ Whereas the former characteristic suited well with the American naturalist and pragmatist tradition in general, the latter was closely tied to a post-Kantian, anti-naturalist conception of philosophy as non-metaphysical and non-factual. American students of the post-war period were never familiarized with such a meta-philosophy and did not read the German work of Carnap and Reichenbach.

Moreover, Carnap and Reichenbach themselves in the 1950s were unsuccessful in conveying it. Reichenbach's attempt at meta-philosophy, both in his APA address and in *Rise*, was filled with polemical remarks to such a degree that almost no reader engaged with the voluntarist conception of reason. This failed reception could have turned out different. In the summer of 1945, Reichenbach was making provisions for Ernst Cassirer to stay at UCLA, and he was looking forward to introduce his students to Cassirer's seminar.¹⁴ If students like Salmon and Putnam could have become familiar to the Neo-Kantian background of Reichenbach's ideas and its non-naturalist conception of reason, American philosophy might have taken a different intellectual turn. Similarly, if Carnap had decided to drop his technical work on inductive logic and had instead produced an accessible introduction to his meta-philosophy, the technical interpretation of his work also could have taken different routes.

At the institutional level, the 1930s had shown successes for the logical empiricist network, in the international and interdisciplinary conferences and in the Encyclopedia project. Neither of these two early successes were continued after the war. The American *Institute for the Unity of Science* proved unsuccessful in promoting the novel type of intellectual work that scientific philosophy stood for: to provide clarification and integration of the scientific enterprise. If Reichenbach had not died in 1953, perhaps the *Institute* would not have gone quietly into the night during the 1950s.

Even though the voluntarism inherent in scientific philosophy was not taken up in the post-war period, the logical and science-oriented nature of Carnap and Reichenbach's philosophy proved a lasting resource for the analytic tradition. Through the more recent historical and interpretative work, the voluntarist conception of reason even returned to play its role within that tradition. Ultimately,

¹³ I have not discussed the intellectual origins of Carnap and Reichenbach's voluntarism in detail. For a discussion of voluntarism in the context of early twentieth-century German philosophy and its representation in the work Moritz Schlick, see Textor (2021).

¹⁴ Cassirer to Reichenbach, April 10, 1945, HRP Box 37, folder 5, item 5.

“what matters is that we for ourselves create the consciousness of the transition which is already taking place, in order to execute it with clarity and method” (Carnap 1936, 41).

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IV Critical Theory and Political Philosophy

Thomas Wheatland

11 Philosophical *Flaschenpost*: Critical Theory and the Transatlantic History of Postwar Philosophy

Abstract: This essay examines Max Horkheimer's provocative engagements with U.S. philosophers, such as Sidney Hook and Ernest Nagel. These efforts spectacularly failed to achieve their desired goals—a fruitful relationship between the Frankfurt School and prominent American philosophers in New York—and instead resulted in permanent animosity. In retrospect, Horkheimer's strategy for winning the support of his new American contacts may seem misguided and doomed, but he was correct to see the common ground between Critical Theory and Marxian pragmatism—even if his rhetorical tactics were risky and misguided. The essay attempts to make sense of Horkheimer's failed gambit, and also seeks to demonstrate how other, more peripheral members of the Frankfurt School, such as Herbert Marcuse, Franz L. Neumann and Erich Fromm, had greater success in gaining traction among U.S. philosophers and social scientists. Their efforts would prove to be more significant in establishing a foundation for Critical Theory's American philosophical reception in the 1960s.

In a letter of June 29, 1940, Max Horkheimer eloquently developed one of the metaphors that became central to the history of Critical Theory in the United States. Writing to the actress and screenwriter Salka Viertel, Horkheimer despaired: “In view of everything that is engulfing Europe and perhaps the whole world our present work is of course essentially destined to being passed on through the night that is approaching: a kind of message in a bottle” (Horkheimer 1995b, 726, translated by the author). In my book, *The Frankfurt School in Exile*, I advised readers to abandon this metaphor—to smash the bottle so to speak. In light of the archival research that I had completed, Horkheimer's statement seemed a poetic exaggeration—largely, because there had been an attempted engagement with U.S. thought and culture in the 1930s and 1940s, some of which was quite successful. And yet, there may be something to the Flaschenpost metaphor when describing Critical Theory's encounter with U.S. philosophy. Before examining Horkheimer's failed engagements with his U.S. counterparts, it is important to elucidate the opportunity that did exist and that motivated his continued efforts.

Horkheimer and his colleagues were right in seeing some shared affinities between the Marxian Pragmatism of Sidney Hook and their own work.¹ Operating in the shadows of the Second International, the revisionism of Eduard Bernstein, and the Russian Revolution, another mediated position needed to be found by Hook and the Frankfurt School—navigating between the dialectical materialism subscribed to by orthodox revolutionaries, the accommodation politics of mainstream socialist parties, and the vanguard strategies of the Bolsheviks. The Frankfurt School and Sidney Hook returned to the writings of Marx and the Hegelian legacy that inspired him in order to locate alternatives to the forms of Marxism that were popular in the 1930s. Following in the footsteps of Karl Korsch and Georg Lukács, the Frankfurt School and Hook tried to re-orient Marxism as a social scientific and philosophical method rather than as a doctrine or as a mere political tool for political reform (or coup d'état). Both Hook and the Institute for Social Research, thus, understood Marxism as a socio-philosophical methodology that united philosophy together with social scientific analysis for the purpose of promoting rational and progressive social change through action.

In light of this shared affinity and Horkheimer's repeated attempts to engage with U.S. philosophers in the late 1930s and early 1940s, one must view the early transatlantic, philosophical history of Critical Theory as both a failure, as well as a missed opportunity. There were possibilities for collaboration and the reception of Critical Theory among the NY Intellectuals and among the more left-leaning members of the Vienna Circle, and yet Horkheimer and the philosophers of the Frankfurt School chose a path that in retrospect seems to have doomed their engagements. Methodologically, the Frankfurt School had a markedly different vision of Marxism than Sidney Hook and his friends, and thus the efforts at philosophical engagement led to antagonisms rather than collaboration.

Horkheimer and the Frankfurt School's encounters with Sidney Hook, Ernest Nagel, Meyer Shapiro and Otto Neurath, both in person and in print, need to be seen within the context of the Institut's larger ambitions during the mid-1930s—as part of a quest to refine and clarify its methodology. While the Frankfurt School continued to pursue the goal of a comprehensive theory of contemporary society, which Horkheimer had announced as the group's primary goal in his inaugural address as director of the Institut für Sozialforschung in January 1931, he saw his individual contribution to the larger work of the Frankfurt School as an effort

¹ In addition to being an early participant in the circle around *Partisan Review*, Hook also served as an intellectual and political mentor to the New York Intellectuals. During the 1930s, Hook distinguished himself as perhaps the foremost American authority on the writings and legacy of Karl Marx. As such, Hook helped the New York intellectual community grapple with the intricacies of Marxian theory and the dialectic.

at clarifying dialectical logic.² Because Hegel's philosophy, together with its reinterpretation by Karl Marx, lay at the heart of the Frankfurt School's approach to social research, a clarification of dialectical logic held the promise of elucidating the complicated relationship between social philosophy and sociology that formed the basis of their methodology.

From the inception of this work on dialectical logic, Horkheimer began with an effort to sort out the differences between Hegel and Marx. As Horkheimer explained in a letter to his primary sociological collaborator, Erich Fromm, written in 1934,

Currently all my interest is focused... on the conceptual foundations of dialectical logic... I have been preoccupied primarily with the difference between the idealist and materialist dialectic. A particularly important aspect of this difference is the role of the ego... When the materialist turns his attention to the ego, he poses questions that can only be answered scientifically, particularly those relating to the social determinants at a given historical moment. The idealist, on the other hand, rejects this additional complication of the inquiry and attempts to determine the ego more or less speculatively. The transformations, which the ego undergoes, are viewed as *necessary steps of knowledge*. In this way individual insights can be integrated into a theory of the ego as independent and uninfluenced by external factors, and knowledge gains the sacred meaning that it possesses in idealism... The idealist is constantly guided in this thought process by the ideal of attaining a standpoint that is no longer particular and he measures individual insights according to a state, in which human consciousness is *no longer conditioned*, insofar as his knowledge—not necessarily quantitatively, but in terms of what is essential—grasps “the totality.” Thus, idealist philosophy and thought in general is dominated, right down to the smallest steps of its arguments, by an illusion: the spurious concept of an absolute ego... The materialist, on the other hand, negates particular insights by consciously placing them in relation to the given historical situation, which must be practically overcome. (Horkheimer 1995a, 177–188; and see Abromeit 2011, 306 for the English translation)

At the moment that Horkheimer conveyed these insights to Fromm, his conception of Idealism and the flaws with the Idealist conception of the ego were limited to Kantian and Hegelian Idealism. Without the insights of historical materialism, the concepts created by the Kantian and Hegelian egos were self-alienated. Kant's and Hegel's idealist egos distorted conceptions of themselves resulting in similar misrepresentations of their perceptions and historical understandings of the world. Marxism corrected these flaws by making the materialist ego self-reflexively aware of the material conditions shaping itself and its awareness of the world.

2 See Wiggershaus (1994, 177–191) and Abromeit (2011, 301–335).

This letter to Fromm is useful for two reasons. First, it demonstrates that Horkheimer was more critical of Hegel and Hegelian dialectics than many of his contemporaries, especially Hook, recognized. Second, it sets the stage for Horkheimer's subsequent efforts to refine his dialectical logic throughout the 1930s and 1940s. Beginning, as we see with Kant and Hegel, Horkheimer systematically subjected all of the other philosophical schools of the late nineteenth and early twentieth centuries to both historical materialist and immanent critiques. While he rejected the various manifestations of *Lebensphilosophie*, phenomenology and irrationalism out of hand as retreats into metaphysics with ominous implications for the emergence of Fascism, Nazism and all other modes of contemporary authoritarianism, his analyses of Neo-Kantianism, Pragmatism, and Logical Positivism uncovered forms of self-alienation as problematic as what he had identified in this critique of Kant and Hegel. Horkheimer, thus, placed himself on a path toward telling American Marxists and other left-leaning intellectuals that they were victims of false consciousness. It was a scorched-earth approach to philosophical debate—by seeking to undermine the foundations of their philosophical and social scientific positions, Horkheimer sought to gain converts to “Critical Theory.”

The first publication that articulated a sustained critique of Logical Positivism, as well as the influence that it was beginning to exert over Americans, was “The Latest Attack on Metaphysics.” In an effort to situate his critique of Logical Positivism and its influence on American Pragmatism within the present moment, Horkheimer began with a historical materialist analysis. While he noted the incredible capacity of contemporary science to “offer the most advanced knowledge of nature,” he noted that many of its findings ironically sustained the appeal of the reactionary metaphysics that had given rise to Fascism and Nazism. As Horkheimer explained,

With science alone, mirroring as it does the chaotic reality in nature and society, the dissatisfied masses and thinking individuals would be left in a dangerous and desperate state. Neither their private nor their public store of ideas can do without a covering-over ideology. For this reason, it was necessary to maintain science and metaphysical ideology side by side. (Horkheimer 1989a, 133–134)

The new authoritarianism of the 1920s and 1930s and the metaphysics that sustained it, in Horkheimer's view, was thus a symptom of larger crises within advanced, monopoly capitalism, which scientific inquiry made abundantly evident—but which the public was not willing to see or admit.

Despite the fact that Logical Positivism and the other manifestations of neopositivism were situated as opponents of this reactionary metaphysical revival (Horkheimer 1989a, 139–141), Horkheimer's immanent critique leveled the accusation that the Logical Positivists only partly and half-heartedly combated the present cir-

cumstances, because they did not fully challenge the contemporary moment, as his own philosophical approach did. As Horkheimer explained,

With respect to the future, the characteristic activity of science is not construction, but induction. The more often something has occurred in the past, the more certain that it will in all the future. Knowledge relates solely to what is and to its recurrence. New forms of being, especially those arising from the historical activity of man, lie beyond empiricist theory. Thoughts which are not simply carried over from the prevailing pattern of consciousness, but arise from the aims and resolves of the individual, in short, all historical tendencies that reach beyond what is present and recurrent, do not belong to the domain of science. (Horkheimer 1989a, 144)

Logical Positivism, with its devotion to contemporary applications of the scientific method, could not adequately challenge the new authoritarianism, despite its intentions, because it failed to fully confront the crises afflicting the present economic, social, political, and cultural status quo. These deficiencies not only made Logical Positivism insufficient for accomplishing many of its stated political aims, but they also doomed it to present distorted observations and analyses of present society. As Horkheimer explained,

The conception that science establishes and classifies given data with a view to predicting future facts and that such a function exhausts the task of science, isolates knowledge and fails to remedy that isolation. The consequence is a ghostlike and distorted picture of the world. The empiricists, however, fail to see that this is the case. (Horkheimer 1989a, 154–155)

The conception and application of science touted by the Logical Positivists was thereby self-alienated and represented a further manifestation of the pervasive false consciousness that suffused contemporary society and culture.

If Horkheimer advocated neither metaphysics nor Positivism, then what did he present as an alternative that would accomplish the aims that he articulated throughout “The Latest Attack on Metaphysics?” While his language lacked the precision that was later evident in “Traditional and Critical Theory,” Horkheimer made a preliminary attempt to explain his alternative. He wrote:

Since the development of a higher spontaneity hinges on the creation of a rational community, it is impossible for the individual to simply decree it... The prerequisite of this goal is that the individual abandon the mere recording and prediction of facts, that is, mere calculation; that he learn to look behind the facts; that he distinguish the superficial from the essential without minimizing the importance of either; that he formulate conceptions that are not simple classifications of the given; and that he continually orient his experiences to definite goals without falsifying them; in short, that he learn to think dialectically. (Horkheimer 1989a, 181)

Contemporary defenders of Otto Neurath and his vision of Logical Positivism, like Andreas Vrahimis, fail to appreciate the self-reflective nature of the methodology articulated by Horkheimer. By concentrating on the substance of Horkheimer's critique of Logical Positivism and not the (albeit vague) formulations of an alternative methodology, contemporary commentators, like Vrahimis, falsely accuse Horkheimer of mistakenly thinking that he could fashion a "standpoint above science" from which he could critique it (Vrahimis 2022, 135–160). Such a critique of Horkheimer echoes those formulated by Sidney Hook back in the 1930s and 1940s and similarly relies on a common strategy that had been used to dismiss Hegelian philosophy and its metaphysical residues. The problem with this critique (as articulated by Hook and by Vrahimis) is that it fails to recognize the self-reflexive dimension of Horkheimer's methodology. Rather than formulating a transcendent "God's-eye view" *above* the present situation, Horkheimer saw his methodology as a self-conscious burrowing from *within* contemporary society—a kind of *immanent* "mole's-eye view."³

Part of what hindered Horkheimer's effort in "The Latest Attack on Metaphysics" was his lack of precision in establishing the basis for his alternate philosophical and social scientific methodology. This same weakness was also evident in the face-to-face encounters that took place between members of the Frankfurt School and Sidney Hook, Ernest Nagel, Meyer Shapiro and Otto Neurath.⁴ Looking back on these meetings with Horkheimer and the other members of the Institute, Hook recalled his insistence that "either the dialectical method was synonymous with what passed ordinarily as scientific method or that it was a kind of hocus pocus" (Hook 1980, 177). Hook and his American colleagues demanded clarification and concrete examples, which Horkheimer and the other members of the Institute struggled to provide.

To more clearly articulate his position, as well as to broaden his critique to include the adoption of Positivist and Pragmatist methods within the social sciences, Horkheimer attempted to refine and expand his argument with the publication of a true, programmatic statement of the Frankfurt School's practice of dialectical logic and its social scientific applications. The result was Horkheimer's famous article "Traditional and Critical Theory."

While the new article drew an explicit connection between Positivism and Pragmatism, Horkheimer repeated essentially the same epistemological criticisms

³ The formulation of self-reflexive critical social science as a "mole's-eye view" has been inspired by Geo Maher's reflections on post-colonial theory. See Maher (2022).

⁴ See Wheatland (2022, 110–122).

that had appeared in “The Latest Attack on Metaphysics”—just in more provocative terms. As Horkheimer wrote,

Among the various philosophical schools it is the Positivists and the Pragmatists who apparently pay most attention to the connections between theoretical work and the social life-process. These schools consider the provision and usefulness of results to be a scientific task. But in reality this sense of practical purpose, this belief in the social value of his calling is a purely private conviction of the scholar... The scholar and his science are incorporated into the apparatus of society; his achievements are a factor in the conservation and continuous renewal of the existing state of affairs, no matter what fine names he gives to what he does. (Horkheimer 1989b, 196)

Despite the fact that Logical Positivists, like Neurath, and Pragmatists, like Hook, shared similar views regarding the constitution of contemporary society, their focus on utility disclosed the fact that both philosophical schools were un-self-reflexive products of a monopoly capitalist society and thereby contaminated by its ideology. As Horkheimer stated more explicitly than he had previously, “The false consciousness of the bourgeois savant in the liberal era comes to light in very diverse philosophical systems” (Horkheimer 1989b, 198). The truth claims made by Positivism and Pragmatism were epistemologically flawed (self-alienated), because these claims were distorted by the ideology of monopoly capitalist society.

While his critique was not particularly new, he did break new ground in seeking to provide the specificity that had been absent in his prior attempts to articulate an alternative philosophical and social scientific methodology that would address the concerns expressed by his American interlocutors. While “traditional theory” uncritically accepted the ideology and social realities of monopoly capitalism, Horkheimer insisted that “critical theory” could begin with the questioning of the autonomous ego at the core of Positivist and Pragmatic epistemologies. As Horkheimer explained,

Bourgeois thought is so constituted that in reflection on the subject which exercises such thought a logical necessity forces it to recognize an ego which imagines itself to be autonomous. Bourgeois thought is essentially abstract, and its principle is an individuality which inflatedly believes itself to be the ground of the world or even to be the world without qualification, an individuality separated off from events. The direct contrary of such an outlook is the attitude which holds the individual to be the unproblematic expression of an already constituted society... Critical thinking is the function neither of the isolated individual nor of a sum-total of individuals. Its subject is rather a definite individual in his real relation to other individuals and groups, in his conflict with a particular class, and, finally, in the resultant web of relationships with the social totality and with nature. The subject is no mathematical point like the ego of bourgeois philosophy; his activity is the construction of the social present. Furthermore, the thinking subject is not the place where knowledge and object co-

incide, nor consequently the starting point for attaining absolute knowledge. (Horkheimer 1989b, 210–211)

By adopting a critical stance toward subjectivity itself, Horkheimer articulated the first step in his negativistic self-reflexive, self-critical methodology that held the promise of eventually overcoming the false consciousness afflicting both Positivism and Pragmatism. In his view, the Positivists and the Pragmatists, with their naïve belief in the autonomous subject, created a false narrative of scientific objectivity and imagined that they studied the world from the “God’s-eye view” that they falsely attributed to him.

Unlike Karl Korsch and Georg Lukács, who were important influences on both Sidney Hook and the Frankfurt School, Horkheimer drew a distinction between his Critical Theory and their Western Marxist approaches to philosophy. While Hook, like Korsch and Lukács, continued to see the subject position of the proletariat as crucial for the social, political, economic, and intellectual liberation from the Bourgeoisie, Horkheimer rejected the subject-object identity theory inherent in this hope (Horkheimer 1989b, 213–214). Rather, Horkheimer saw the liberation from false consciousness as an activity to be pursued by the critical intellectual. As Horkheimer explained,

The social sciences take human and nonhuman nature in its entirety as given and are concerned only with how relationships are established between man and nature and between man and man. However, an awareness of this relativity, immanent in bourgeois science, in the relationship between theoretical thought and facts, is not enough to bring the concept of theory to a new stage of development. What is needed is a radical reconsideration, not of the scientist alone, but of the knowing individual as such... The classificatory thinking of each individual is one of those social reactions by which men try to adapt to reality in a way that best meets their needs. But there is at this point an essential difference between the individual and society. The world which is given to the individual and which he must accept and take into account is, in its present and continuing form, a product of the activity of society as a whole. The objects we perceive in our surroundings—cities, villages, fields, and woods—bear the mark of having been worked on by man. It is not only in clothing and appearance, in outward form and emotional make-up that men are the product of history. Even the way they see and hear is inseparable from the social life-process as it has evolved over the millennia. The facts which our senses present to us are socially performed in two ways: through the historical character of the object perceived and through the historical character of the perceiving organ. Both are not simply natural; they are shaped by human activity, and yet the individual perceives himself as receptive and passive in the act of perception. (Horkheimer 1989b, 199–200)

Only a “critical theory,” through its thoroughgoing negativity toward the status quo and all of its complex consequences, possessed the potential for liberating human

beings from the pervasive forms of reification reinforcing the false consciousness of monopoly capitalist ideology.

The obvious question is why did Horkheimer select such a high-risk strategy in these two crucial articles and in his personal dealings with American philosophers? Horkheimer could have taken a more diplomatic approach in presenting the work of the Frankfurt School to US colleagues. He could have sought to build more cordial and constructive relations by emphasizing the common ground that the Institute shared with American scholars and academic institutions. The Frankfurt School had already practiced such diplomacy when pursuing the successful affiliation to Columbia University's Sociology Department that was finalized in 1934. In fact, their new Columbia colleagues appear to have mistaken them for a cutting-edge center for empirical sociology. Rather than presenting their work as a challenge to prevailing sociological trends and methodologies in the United States, the Frankfurt School endeavored to demonstrate how much their work shared in common with American social research.⁵

Such diplomacy was notably absent in the Frankfurt School's approach to American philosophers. By contrast, Horkheimer sought a winner-take-all debate with his philosophical colleagues. The obvious down-side to this more confrontational approach to U.S. philosophers seems obvious. By potentially offending guests, such as Hook, Nagel, Shapiro and Neurath, through subjecting their philosophical positions to immanent and historical materialist critiques, Horkheimer risked transforming the Frankfurt School's most likely U.S. philosophical sympathizers into enemies. Not only did he accuse them of epistemological self-delusion, but he also accused them of ideological blindness and political naivety. If the Frankfurt School understood how to successfully navigate U.S. academic and American intellectual networks, why did they not replicate their successful strategy that had been deployed to American sociologists?

While Horkheimer largely outsourced the responsibility for the Institute's initial networking efforts with American sociologists, Horkheimer took personal responsibility for the Frankfurt School's approach to U.S. philosophers. The Institute for Social Research was a multi-disciplinary organization dedicated to the analysis of contemporary society. As such, each member had areas of specialization. For example, while scholars such as Erich Fromm and Franz L. Neumann spearheaded work within the social sciences, colleagues such as Leo Löwenthal, Walter Benjamin and Theodor W. Adorno concentrated on contemporary culture. Although all members of the Institute had training in philosophy, the expert within the field was initially Horkheimer, who was later supported by Herbert Marcuse

⁵ See Wheatland (2009, 35–60).

and Theodor W. Adorno. Thus, Horkheimer personally shaped the strategy used in the Frankfurt School's approaches to American philosophers. While Horkheimer's handling of the Frankfurt School's interactions with US philosophy in the 1930s and 1940s seems bewildering when taken at face value, his approach makes sense when considered in light of his own development as a philosopher.

As John Abromeit detailed in his superb intellectual biography of the young Horkheimer, his philosophical orientation, which remained strikingly consistent once formed, arose from his confrontations with Schopenhauer, Neo-Kantian Positivism, Phenomenology, and Marxism.⁶ His views developed in critical opposition to the major philosophical trends that he encountered as a student.

Like so many other Germans of his generation, Horkheimer's intellectual formation was shaped by the First World War. It helped to illuminate the authoritarianism and brutality that lay beneath the surface of Wilhelmine society. In response, Horkheimer developed the aesthetic sensibilities of a radical Bohemian and developed a life-long fascination with the philosophy of Arthur Schopenhauer.

As Horkheimer commenced his university studies, his identification with aesthetic radicalism diminished and his interest in Marxism flourished. Although Horkheimer stayed clear of organized political movements, he supplemented his formal education in philosophy and Gestalt psychology with participation in Marxian student reading groups. Thus, Horkheimer eschewed the revisionist Socialism of the SPD (*Sozialdemokratische Partei Deutschlands*), as well as the Leninism of the emergent KPD (*Kommunistische Partei Deutschlands*). At the same time that his extra-curricular reading and discussions provided him with a capacious knowledge of Marxian theory, his academic studies familiarized him with both Positivism and phenomenology. Horkheimer's philosophical mentor, Hans Cornelius, was a specialist in Neo-Kantianism and its intersection with Gestalt psychology, but he was also a keen student of mathematics, physics, biology and the arts. In fact, his philosophical commitment to Positivism was deeply enriched by his knowledge of these other fields, and he encouraged Horkheimer to follow in his own footsteps. Cornelius, also, encouraged Horkheimer to briefly study with Edmund Husserl to gain exposure to phenomenology.

Horkheimer did, indeed, accept the guidance of Cornelius. He accepted Cornelius's training in Positivism within philosophy and the social sciences and traveled to audit lecture courses with Edmund Husserl and Martin Heidegger. Privately, however, Horkheimer developed a critique of this formal, philosophical education that was shaped by the lingering influence of Schopenhauer combined with his developing knowledge of Marx and Hegel. As Alfred Schmidt has observed,

⁶ See Abromeit (2011, 27–184).

Horkheimer's thinking maneuvers from the outset between two fronts: against meaning-constitutive, dogmatically proclaimed metaphysics, and against anti-conceptual positivism, which abstractly denies any meaning whatsoever that extends beyond the here and now. For Horkheimer, just as thought cannot be restricted to immediate utility, so there can be no pure desire for knowledge, removed from material reality. Those who pretend to pursue a 'disinterested striving for truth' are laboring under an ideologically loaded 'philosophical delusion.' Thought pursued 'for thought's sake' has 'lost its sense, which is to be a means of improving the human condition.' Moreover, such a 'pursuit of truth' contradicts its own claims inasmuch as 'it necessarily replaces truth with a phantom: the absolute, i.e. transcendental truth.' (Schmidt 1993, 27)

The conception of philosophy that Horkheimer developed during his years as a university student took shape as an immanent, historical materialist critique of the Positivism, Phenomenology and metaphysics that he had studied with his formal mentors, and this critique was inspired by the extra-curricular reading from the same period. It would appear that Horkheimer sought to guide his American colleagues through a similar philosophical conversion experience.

Horkheimer's correspondence from the late 1930s and 1940s makes clear that he understood the magnitude of his miscalculation and failure with American philosophers. At least part of the problem, he attributed to his lack of ability with English and American academic language. As Horkheimer explained in a letter to Friedrich Pollock,

... in addition to the usual dullness and naivety of such documents, our presentations express a certain perplexity due to our inability to master the English style of thinking and writing... With regard to myself, I become more and more aware of my utter inability to do such things. This language has developed into a tool by which you can point to things which you already know. It does not enter into an interaction with the object. Never is the word understood as reflecting the nature of the object, nor is the experience of the object shaped by the intellectual potentialities inherent in the particular word expressing the object. The relation of the word and thing, of the sentence and the subject matter, is a purely mechanical one. Speech must be to the point. In our case this means that we must show how far we have revealed new facts or organized old ones in such a way that the knowledge gained by them can be put to a new use. Wherever the tool of language and style has been adapted so well to reality, the scientific and business methods of speaking and writing are so highly developed that each empty promise to deliver new facts or new uses or to show new pragmatic possibilities is immediately noticed in how you express yourself... Industrial monopolism which has superseded the poor old, half natural techniques of buying and selling in all fields, has changed the character of language altogether. It has eliminated the last metaphysical elements. A man who would take ideological statements at their surface value, whether he would do so in

order to believe them honestly once and for all, or to refute them as lies, would not be an idealist, but would simply misunderstand the meaning of the words.⁷

Self-censorship was a related, but somewhat different, source of linguistic troubles in the United States. Members of the Frankfurt School famously wrote and spoke in “Aesopian language.” Fearing political persecution in America, just as they had suffered in Germany, Horkheimer forbid members of the Frankfurt School from involving themselves in political activity and subjected their work to careful censorship of overtly Marxist terminology (Marcuse and Habermas 1978–1979, 129–130). Although it is easy to mistake such concerns as paranoid, one must remember the political circumstances facing a group of German-Jewish émigrés during the 1930s and 1940s. The Institute was a frequent target of government investigations, and its members were aware of them. During the early years at Columbia, the offices on 117th Street were visited by German-speaking, New York police detectives searching for Nazi sympathizers within the German community (Löwenthal 1995, 747–752). Later the Institute was frequently monitored by the FBI resulting from anonymous accusations regarding Communist sympathies among the group’s members.⁸ The sheer bulk of the FBI files suggests that Horkheimer had good reason to forbid political activity. Harsher repercussions likely would have resulted had Horkheimer’s warnings been ignored. Nevertheless, Horkheimer limited the Frankfurt School’s ability to express itself and its approach to philosophy by limiting the manner in which its members articulated and deployed the Marxian concepts underlying their thought.

While Horkheimer’s efforts with American philosophers never recovered from his initial missteps and a true intellectual migration of Critical Theory to the world of American philosophy did not commence until the rise of the Frankfurt School’s fame in the 1960s, the ground for this later U.S. philosophical reception of Critical Theory was indirectly laid by more marginal members of the Institute, like Herbert Marcuse, Franz L. Neumann, and Erich Fromm. In one way, the work that they did to prepare the way for the eventual American reception of Critical Theory was entirely consistent with the methodological visions promoted by Horkheimer in the 1930s. Although the work of Neumann and Fromm did not fit

7 Horkheimer to Friedrich Pollock, November 28, 1943, Max Horkheimer Archiv, series IX, box 86, document 10b. Frankfurt am Main.

8 Heavily redacted evidence of these investigations is contained in the files of the Federal Bureau of Investigation, Washington D.C. See FBI subject files on Theodore Adorno (file numbers 100–106126–12; 100–106126–24; and 100–106126–30), Erich Fromm (file number 105–112622), Max Horkheimer (file number 61–7421), and Herbert Marcuse (file number 121–24128). These materials were obtained by a request from the FBI through the Freedom of Information Act.

comfortably within the field of philosophy and was better suited to political theorists, sociologists, and psychologists, it remained aligned with the methodological vision that Horkheimer developed. In articulating his formal articulation of a “Critical Theory,” Horkheimer made an important contribution to the invention of social theory. In this sense, one can make the claim that Horkheimer, by expanding the domain of philosophy in the 1930s, did make room for his colleagues, Neumann and Fromm to more diplomatically and successfully find an audience and to begin to develop the types of collaborations that had been originally sought in the 1930s.

Herbert Marcuse’s *Reason and Revolution* merits its own reception history. Perhaps no work by a member of the Frankfurt School accomplished more philosophical heavy-lifting on behalf of the promotion of Critical Theory in America. In part, the book was inspired by the frustrations that Horkheimer experienced in the late 1930s. Part of the struggle that Horkheimer encountered in articulating the Frankfurt School’s methodology resulted from American perception of the work of Hegel. When the Institute arrived in America, many American scholars, like John Dewey, George Santayana and Carl Mayer, had grown to see the philosophy of Hegel as hopelessly reactionary and proto-Nazi.⁹ Because so much of the Frankfurt School’s methodology traced its roots back to Hegel, this perception was an obstacle for the case that they made on behalf of Critical Theory.¹⁰ In the aftermath of their failed encounters with Sidney Hook, it became apparent to Marcuse that this image of Hegel needed to be combated by carefully explaining Hegelian philosophy to a U.S. audience. *Reason and Revolution* was dedicated to this cause, and it succeeded in accomplishing its author’s ambitions. Because of its novelty, density, and complexity, however, its effects were not seen immediately. Instead, *Reason and Revolution* slowly gained an audience of Americans curious about Hegel, Marx and the connections between the two—and this audience was ready to examine Critical Theory on its own terms when the global student movement brought the Frankfurt School to international, popular attention. By rehabilitating Hegel and his methodology, Marcuse was able to refashion the foundation on which Critical Theory had been built thereby altering the way it was received in the 1960s and 1970s by American philosophers and intellectual historians.

Franz L. Neumann, by contrast, produced work in the 1940s that served as an object lesson for American scholars in the social sciences and humanities who sought to take issues like ideology and power more seriously in a monopoly capitalist world. He avoided the epistemological controversies that Horkheimer pro-

⁹ See Dewey (1915), Santayana (1916), and Mayer (1942). Also see Harrelson (2012–2013) and Kaag and Jensen (2017).

¹⁰ The challenge was acknowledged by contemporary US reviewers of *Reason and Revolution*. See Herz (1942).

voked by diplomatically articulating the methodology for his works of the 1940s in a manner that antagonized no one. Instead, he emphasized how much his thought shared in common with trends within American academia enabling his work to stand on its own merits and to function as an inspiration for American readers.

In a rare transcript of a discussion protocol from 1941, Neumann explicitly pondered the questions surrounding the Frankfurt School's methodology and the challenges to its American acceptance while still a member of the Institute.¹¹ Horkheimer set the terms of the discussion by characterizing the small and uncertain opening he thought available for securing support from American foundations. They were evidently no longer satisfied with "empiricism" alone, he noted, but increasingly recognized the importance of "theoretical viewpoints." Yet American social science in general—and presumably the evaluators for the foundations—also insisted that theoretical claims were "hypotheses" that required verification by empirical research, a methodological conception that was antithetical to that of the Institute. The primary question of the consultation was whether the group could explain its method so as to overcome the obstacles and to seize the opportunity that Horkheimer foresaw.

Strikingly, Horkheimer's opening statement at the 1941 discussion was interrupted by Neumann twice. As soon as Horkheimer said that they were expected to supply an explanation of their method, Neumann interjected that the explanation must not appear Marxist. Moments later when Horkheimer referred to the empirical testing of hypotheses expected of them, Neumann moved the discussion towards an examination of this conception. Making his own assessment of American social scientific trends, Neumann stated that "[T]he general consensus is that it is necessary to have a working hypothesis, but it is not known how this can be discovered" (Horkheimer et. al. 2012). Julian Gumperz, Felix Weil, Herbert Marcuse, and T. W. Adorno all disputed Neumann's characterization. Each of Neumann's challengers fell back on the Empiricist, Positivist and Pragmatist stereotypes that had been common in the Institute since Horkheimer's "Traditional and Critical Theory"—in an effort to be unbiased, Americans avoided hypotheses altogether or they developed functional hypotheses aimed at achieving limited but instrumentalist goals. Citing the examples of Thorsten Veblen, Robert Lynd, and Max Lerner, Neumann made the case that things were changing and that the Institute's old stereotypes of "traditional theory" had to be reconsidered—and thus a clearer statement of the Institute's methodology was necessary (Horkheimer

11 See Horkheimer et. al. (2012).

et al. 2012).¹² Following Neumann's lead, Horkheimer declared that "[I]t would never occur to us to construct a hypothesis because we find a quite specific state of the question [*Fragestellung*] already given... We would rather revert to certain conceptions of society that we already possess" (Horkheimer et al. 2012). In an effort to anticipate the likely American skepticism to Horkheimer's impulse to simply defend Critical Theory, Neumann tried to imagine the objections of US social scientists: "what is correct about the theory on which you base yourselves? To come to such an understanding with the American who does not accept the theory is very difficult" (Horkheimer et al. 2012).

Neumann imagined a statement of methodology less strident and less philosophical than Horkheimer and the other members of the Institute were advocating. As Neumann suggested,

This is not about working out our own method but about the question, "How do I tell it to the children?" Until now we have been satisfied to say that we seek to integrate all the social sciences. That does not suffice. The question is whether we can present our method so as to attack the hypothesis-fact problem. We distinguish ourselves from sociology in that we view phenomena as historical phenomena, which Americans do not do. We must emphasize that we are not engaged in sociological but in social-scientific work, and we must explain this. The difference is enormous, and we must show this. (Horkheimer et. al. 2012)

History, as Neumann noted years later in his autobiographical contribution to a collection of essays dedicated to reflecting on the intellectual migration of European thought to the United States,¹³ was the key to allowing the European social theorist to gain traction in America.¹⁴ It avoided the thorny question of Marx, and yet developed an epistemological vocabulary true to the Institute's theory that simultaneously could be readily explained and defended to U.S. scholars. Neumann contended that persons like himself, trained in the German tradition, were able to achieve two things. First, they brought skepticism about the ability of social science to engineer change. But more important, they attempted to "put social sciences research into a theoretical framework" (Neumann 1953, 24). By overstressing the significance of empirical data collection and ignoring theory, Neumann argued that Americans made themselves vulnerable to the following criticisms:

¹² It is important to note that Neumann had reviewed some of these new social scientific trendsetters that he directly referenced in the "Methods Discussion." See Neumann (1939a) and Neumann (1939b).

¹³ See Neumann (1953).

¹⁴ Historians of sociology confirm Neumann's insight about the value and appeal of historicism in 1940s America, especially as publicized by the Institute's influential ally, Robert S. Lynd. See Steinmetz (2007).

... that the predominance of empirical research makes it difficult to see problems in their historical significance; that the insistence upon mastery of a tremendous amount of data tends to transform the scholar into a functionary; that the need for large sums to finance such enterprises tends to create a situation of dependence which may ultimately jeopardize the role of the intellectual as I see it. (Neumann 1953, 24)

Despite these reservations, Neumann did see some merits in the American approach—"the demand that scholarship must not be purely theoretical and historical, that the role of the social scientist is the reconciliation of theory and practice, and that such reconciliation demands concern with and analysis of the brutal facts of life" (Neumann 1953, 24–25). For Neumann, the full understanding of the "brutal facts" required that they be put in a context of a historical theory that comprehended both past developments and present potential for the future, and the knowledge of the "brutal facts" precluded an illusory projection of that future. Even theoretically unsatisfactory empirical inquiry could be adapted for purposes of the more critical view, as long as the "facts" it examined did in fact bear on the course of development.

Neumann's classic book, *Behemoth*, is an object lesson in the approach to methodology that Neumann envisioned and for which he advocated. *Behemoth* analyzed the "brute facts" of the contemporary moment by accumulating empirical data that was interpreted in relation to its theoretical and historical working "theses" about late capitalist society. While *Behemoth* relied on a combination of American and European research techniques, it avoided the challenge of "telling it to the children" by way of avoiding any discussion of its own methodology. The closest that Neumann came to disclosing the book's methodology arose when he told his readers that it was essential to examine the ideology of National Socialism in relation to its underlying factual realities. As Neumann wrote,

When we read Plato and Aristotle, Thomas Aquinas and Marsilius of Padua, Hobbes and Rousseau, Kant and Hegel, we are fascinated as much by the inner beauty of their thinking, by their consistency and elegance, as by the way their doctrines fit in with sociopolitical realities. The philosophical and sociological analysis go hand in hand. National Socialist ideology is devoid of any inner beauty... The immediate and opportunistic connection between National Socialist doctrine and reality makes a detailed study of the ideology essential. Ordinarily, we must reject the notion that sociology can determine the truth or falsity of a system of ideas by examining its social origin or by associating it with a certain class in society. But in the case of national socialist ideology, we must rely on sociological methods. There is no other way of getting at the truth, least of all from the explicit statements of the national socialist leaders. (Neumann 1944, 37)

Neumann articulated a social theoretical methodology in harmony with Critical Theory. While avoiding the provocative attacks against Positivism and Pragmatism

that Horkheimer believed were necessary, Neumann elected to not antagonize his readers. Rather he amassed a wealth of social facts to demonstrate that National Socialism had no coherent political theory and to show that the Third Reich could not be called a state. *Behemoth* was an immanent critique of Nazi ideology that was painstakingly supported by a historical materialist analysis of German society from the Wilhelmine Empire up through the Second World War. Its novel approach to social research was deeply influential to important mid-century social researchers, like C. Wright Mills, who shaped the mindsets and attitudes of young readers of the 1960s who would re-discover the Critical Theory of the Frankfurt School.¹⁵ Through the inspiration of Critical Theory that came through the example of *Behemoth*, Mills taught a young generation of Americans that American society was not what it appeared and that the status quo of the 1960s needed to be more critically interrogated if a better society was to be possible. The readers of C. Wright Mills became some of the first U.S. readers and practitioners of Critical Theory during its second American wave in the 1960s and 1970s. It was this new generation of students of Critical Theory who rediscovered it and began applying it to American philosophy.

Although Erich Fromm is often forgotten by most students and scholars of Critical Theory, he was perhaps the most important figure within the Frankfurt School during the 1930s and had the largest reception by a U.S. audience. Like Franz L. Neumann, who only overlapped briefly with Fromm at the Institute, Fromm was a more astute reader and observer of trends within American academia. As a practicing psychoanalyst and sociological researcher, Fromm developed relationships with the emerging Neo-Freudian network in the United States and closely monitored the work of American social scientists grappling with issues comparable to the Institute's studies of authoritarianism and political attitudes among working class people during the Great Depression. Thus, Fromm was positioned to become a key strategist in finding a U.S. home for the Institute for Social Research and was integral in preparing the written materials that were shared with American colleges and universities. Unlike Horkheimer, Fromm did not deride empiricism and, in fact, relied on empirical social science to support his integration of psychoanalysis and sociology.¹⁶

Once the Frankfurt School relocated to Columbia University, the entire staff of the Institute taught courses within the extension school, participated in the sociology department's faculty seminars, and hosted their own seminars that were open to interested faculty and graduate students throughout New York, but

¹⁵ See Geary (2009) and Miller (1994).

¹⁶ See Wheatland (2009, 43–60).

Erich Fromm was the only figure from the Institute who welcomed American collaborators to join in its ongoing research. Thus, Columbia faculty, like Robert Lynd, Robert MacIver, Theodor Abel, Charles Beard, and Samuel Lindsay, became closely involved with advising and assisting the Frankfurt School, while graduate students, like Mirra Komarovsky, Moses Finkelstein, and Benjamin Nelson, joined Fromm's research teams and helped translate Institute writings into English. Fromm even initiated collaborations between the Institute and scholars at other social research institutions in the New York area (such as Paul Lazarsfeld at the University of Newark and Ruth Monroe at Sarah Lawrence). As a result, Fromm rapidly emerged in the eyes of most Americans as the public face of the Frankfurt School in the 1930s.¹⁷

A complicated set of intertwined factors led to Fromm's acrimonious split with the Frankfurt School in 1939. Correspondences suggest that part of the problem arose from Horkheimer's jealousy, money problems, and growing mistrust of Fromm, but this was deepened by significant intellectual differences over the Institute's adherence to psychoanalysis. Fromm's integration of psychoanalysis and sociology, as well as his contacts with Neo-Freudians, such as Karen Horney and Harry Stack Sullivan, helped to precipitate Fromm's questioning of psychoanalytic drive theories. While his colleagues within the Frankfurt School remained convinced by the biological determinism and universality of human psycho-sexual development, Fromm grew to emphasize the mediating and decisive role played by society and social experiences. Theodor W. Adorno's arrival in New York and his presence within the Institute intensified these tensions.¹⁸

Following Fromm's break with the Frankfurt School, his relationships with American scholars and public intellectuals deepened. No longer constrained by Horkheimer's cautiousness and jealousies, Fromm grew closer to Neo-Freudians, like Horney and Sullivan, and became a regular presence within the "Culture and Personality Movement," which enabled him to share his ideas and gain inspiration from cultural anthropologists such as Margaret Mead, Ruth Benedict and Edward Sapir.¹⁹ These relationships proved to be crucial as Fromm continued work on a project begun in 1936. Fromm sought to elaborate and develop many of the ideas that formed the core of his articles and studies undertaken with the Institute for Social Research—his conception of social character and the role that the family and other societal institutions played in its formation, his reflections on matriarchal and patriarchal societies, and particularly his explorations

¹⁷ See Wheatland (2009, 43–81, 142–158).

¹⁸ See Friedman (2013, 56–62), McLaughlin (2021, 70–71), and Wheatland (2009, 81–87).

¹⁹ See Friedman (2013, 76–78).

of authoritarianism and sado-masochism in the era of monopoly capitalism. The result was his book, *Escape from Freedom* (1941). With the assistance and inspiration of his American friends, *Escape from Freedom* resonated with a wide reading audience in the United States and has sold more than five million copies since the date of its publication. It made possible not only Fromm's widespread introduction to the American public, but also may represent the first accessible and widely-read articulation of Critical Theory in the United States. While his former colleagues from the Frankfurt School privately differed with him on some of the book's central findings relating to psychoanalysis, as well as his nostalgic position toward the medieval, pre-modern world that Fromm derived from Jacob Burckhardt and Johan Huizinga, Fromm's focus on modern consciousness, alienation, reification, authoritarianism, and the transformation of classical bourgeois society precipitated by the rise of monopoly capitalism were unmistakably aligned with the early Critical Theory of both Horkheimer and the Institute. This should come as no surprise, of course, because Fromm had been crucial in helping the Frankfurt School develop these early positions in what came to be called "Critical Theory."

Like Neumann, Fromm was careful to avoid fully articulating the role that Marxism played within his thought. A careful reader familiar with the writings of the Young Marx and Western Marxism would easily detect it, but it would be hard for other readers to discern. Fromm also chose a more diplomatic strategy in presenting the relationship between his own approach to social research and those that he was encountering in the United States. In a manner similar to Neumann's *Behemoth*, Fromm's *Escape from Freedom* represented an integration of European social theory and the American empirical approach to the social sciences that was justified and explained through another vague evocation of history. In a passage in which Fromm succinctly articulated his central methodology and its connection to his conception of the social character, Fromm wrote:

But man is not only made by history—history is made by man. The solution of this seeming contradiction constitutes the field of social psychology. Its task is to show not only how passions, desires, anxieties change and develop as a *result* of the social process, but also how man's energies thus shaped into specific forms in their turn become *productive forces, molding the social process*. (Fromm 1969, 28)

For Fromm, the self and society functioned in a symbiotic relationship. While he could have disclosed his debt to Marx and the conception of historical materialism, Fromm recognized that he might reach more readers by foregoing such explicit references and instead by describing his approach to social research in simpler, albeit less precise, language.

Until the meteoric rise of Herbert Marcuse in the mass media during the Spring of 1968, Erich Fromm was the best-known figure from the Frankfurt School

in the United States. *Escape from Freedom* launched his career as a public intellectual and enabled him to influence scholars, political leaders, and the general public in the United States. Perhaps most significant for Fromm's widespread reception was his relationship with David Riesman. Riesman openly expressed his intellectual and personal debts to Fromm, and his book, *The Lonely Crowd*, built on Fromm's concerns regarding the conformist nature of contemporary American mass culture and recast it within a more distinctively American Liberal idiom. In addition to helping to transmit Fromm's ideas to an even larger U.S. audience, Riesman also introduced Fromm to political insiders within the Liberal Establishment. Thus, Fromm became an informal advisor to Adlai Stephenson and was able to share his opinions directly with the Kennedy Administration.²⁰ Partly as a result of his proximity to the Liberal Establishment, Fromm had a sizable impact on the intellectual formation of the New Left, but he was rapidly abandoned for voices more critical of that Liberal Establishment, as SDS (Students for a Democratic Society) grew into a mass political movement after it emerged as the leading voice within the anti-war movement.²¹

Although Horkheimer's attempts to successfully recruit American philosophers failed, there were several other messages in bottles sent by the Frankfurt School. Some of these messages, like *Reason and Revolution*, took decades to have their intended impact. Other messages, like those cast into the sea by Franz L. Neumann and Erich Fromm, were enthusiastically read, pondered and absorbed—but by scholars in other fields outside of philosophy. In this manner, Critical Theory grew to gain an audience within disciplines such as political science, sociology and psychology. It also attracted important allies, like C. Wright Mills and David Riesman, who subsequently incorporated Critical Theory into their own thought and methodologies. Although Critical Theory did not find a direct way into the field of philosophy, American philosophers who were attuned to the social sciences began to encounter some of the basic ideas of Critical Theory. This established a hospitable environment for the more formal philosophical encounter with Critical Theory that began in the 1960s after the rise and reception of Herbert Marcuse among the New Left and in the press of that era.

²⁰ See Friedman (2013, 184–185, 199–200, 215–218); see also McLaughlin (2021, 81–110).

²¹ See Jamison and Eyerman (1995).

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Eric Schliesser

12 Ernest Nagel and Felix Oppenheim Respond to Leo Strauss, and the Road Not Taken

Abstract: This chapter presents the reception of Leo Strauss by analytic philosophers after Strauss's emigration to the United States. It gives a brief survey of the polemics against Strauss and his school by analytic philosophers, which aided in the self-constitution of analytic philosophy as a rival school of thought in philosophy. But most of the chapter is devoted to recovering the significance and influence of a criticism of Strauss by Ernest Nagel. The chapter argues that this response is of intrinsic interest because it involves the relationship between science and values. Nagel's response was foundational to Felix Oppenheim's political philosophy or what Oppenheim called his project of 'conceptual reconstruction.' Lurking in Oppenheim's response to Strauss are questions about the role of the Principle of Sufficient Reason in philosophy. The chapter argues that an opportunity was missed to clarify questions pertaining to the inductive risk of philosophy more than a half century before these issues became fashionable again. This chapter, thereby, contributes to the history of non-Rawlsian analytic political philosophy.

12.1 Introduction

The *Proceedings of the American Philosophical Association of 1948–1949* reports that, on the recommendations of the Executive Committee, Leo Strauss (1899–1973) was elected to full membership in that year (APA 1949). There is no sign this was anything but routine. At that point Strauss had been stateside for close to a decade.

Educated by elite German philosophers, including Cassirer (who was his supervisor), Husserl, and Heidegger (whom Strauss admired), Strauss arrived stateside alongside the great exile of the Vienna and Berlin schools, who with the scientific wing of pragmatism (under Ernest Nagel's leadership) helped form the

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backbone of American analytic philosophy which flourished simultaneously as the school named after him did. (For biographical information on Strauss I have relied on Tanguay 2007 and Sheppard 2007.) While the present chapter does not ignore the polemics between the schools, it primarily reconstructs how Strauss' *philosophy* was criticized by analytic philosophers, and this reconstruction sheds light on recent debates pertaining to the inductive risk of philosophy and the fact-value distinction as well as a forgotten episode in the reception of the Principle of Sufficient Reason.

After a few years in England, where in 1936 he published a major book on Hobbes, *The Political Philosophy of Hobbes: Its Basis and Its Genesis* (Strauss 2014), Strauss had come to the United States in 1937, where he received an appointment at the New School. The book on Hobbes had been reviewed respectfully in *The Journal of Philosophy* and *The Philosophical Review* (Lamprecht 1937; Sabine 1938).¹ In these reviews, Strauss was understood as offering a developmental and contextual account of Hobbes. If Strauss had published nothing after the Hobbes book, he could have been understood as an early anticipation of the so-called Cambridge school's 'contextual' turn in the history of thought.²

In an otherwise critical review of Strauss' 1948 work *On Tyranny* (Strauss 1991 [1948]), Vlastos (then at Cornell), who in his prime was "one of the most influential" scholars in ancient philosophy (Annas 2004, 30, 42), writes, "Those who have read and admired Professor Strauss's earlier book on Hobbes will be disappointed in this monograph." In fact, Vlastos (1951, 593) goes on to *praise* Strauss' "learning" and "agility of mind" before complaining that "the weakness of this [new] work can be traced directly to his present addiction to the strange notion that a historical understanding of a historical thinker is somehow a philosophical liability."³

Once stateside, Strauss continued publishing numerous book reviews and articles, often in *Social Research*, then recognized as a "philosophical periodical"

1 Sabine was an important historian of philosophy then, who helped build up the Cornell department. The *Journal of Philosophy* review is signed S. P. L. which probably stands for Sterling P. Lamprecht (a historian of philosophy). See also Oakeshott (1937), which is very positive and insightful, where critical (and very much worth re-reading) and Watkins (1955; 1957).

2 While Quentin Skinner has been polemical toward Strauss in other work, he cites this book approvingly in *Reason and Rhetoric in the Philosophy of Hobbes* (1996). I do not mean to suggest that founding contextualism would have been Strauss' self-understanding; he clearly was also grappling with and responding to Carl Schmitt. On this see McCormick (1994).

3 In 1985, in his famous take-down of Strauss in the NYRB, Burnyeat also remarks "The range of his learning is indeed formidable; his command of ancient and medieval languages cannot fail to impress; his minute scrutiny of each text establishes an aura of reverence for its author." <https://www.nybooks.com/articles/1985/05/30/sphinx-without-a-secret/> (last accessed July 9, 2024).

(Strauss 1939; 1941; 1947).⁴ While American scholars of that age still regularly read scholarly works in German I have found no trace of the impact of Strauss' earlier *Die Religionskritik Spinozas als Grundlage seiner Bibelwissenschaft* (1930) in the 1940s or 50s.

At the dawn of the cold war, in October 1949, Leo Strauss gave a number of Walgreen lectures at the University of Chicago that were published in 1953 as *Natural Right and History* (hereafter NRH).⁵ In 1949 he also became a professor of political science at The University of Chicago. The appointment and the book propelled him into prominence (Burnham 1954, 24).

Before long Strauss was known in his own right and for the school he had founded. This 'school' received notoriety with the 1962 publication of *Essays on the Scientific Study of Politics*, edited by Herbert J. Storing—all the authors, in addition to Strauss (who contributed an "epilogue") were students of Strauss or had spent considerable time with Strauss. *Essays on the Scientific Study of Politics* is a fierce (and often uncharitable) polemic against then mainstream empirical social science.⁶

That Straussianism came to be seen as a school can be inferred from a 1962 review by the prominent game and social choice theorist, William Riker, of Buchanan and Tullock's *Calculus of Consent*. Riker mentions, in passing, that "Some, like Leo Strauss, have urged a return to the great tradition; but unfortunately the process of return, as it appears in the work of both master and students, has turned out to be an even more sterile historicism than that against which they revolted" (Riker 1962: 408). In the flagship journal of the American Political Science Association, Stanley Rothman starts his anti-Straussian polemic with the following two sentences:

Perhaps no single individual has had as much impact on the discipline of political science during the past several years as has Leo Strauss of the University of Chicago. Both he and his disciples (and they are disciples in the "classical" sense) have engaged in a full scale attack upon the premises underlying the contemporary study of politics. (Rothman 1962, 341)⁷

4 See: "Notes," *The Philosophical Review* 49 (2), Mar. 1940: 281 and "Notes," *The Philosophical Review* 51 (2), Mar. 1942: 242. During his time at The New School, Strauss published more work in *Social Research*, then edited and published by the Graduate Faculty of Political and Social Science of the New School for Social Research.

5 For a review that anticipates many of the subsequent, recurring criticisms lodged at Strauss, see Plamenatz (1955).

6 For then contemporary (and critical) reaction, see Schaar and Wolin (1963).

7 The accompanying footnote reads: "The most articulate of Strauss' students include Walter Berns, Joseph Cropsey, Harry V. Jaffa and Allan Bloom."

Compared to the fire-works that Straussianism generated within political science, the response by philosophers was relatively muted. The full-scale polemics against Strauss start with John Yolton (1921–2005), who was a PhD student of Ryle's (1950–52) that resulted in a D.Phil, thesis, "John Locke and the Way of Ideas; a Study of the impact Locke's Epistemology and Metaphysics upon his Contemporaries" (Buickerood and Wright 2006). This dissertation and the subsequent book (Yolton 1956) are important mile-stones within the development of so-called 'contextual' historiography *within* analytic philosophy. After the book was published, in a subsequent article, Yolton contested the "violently distorted interpretation recently advanced by Leo Strauss" of Locke in NRH (Yolton 1958, 478).⁸ He used the occasion to argue that "Strauss's general esotericist thesis suffers a severe blow when we consult the techniques he employs" (Yolton 1958, 478).⁹ Strauss' reliance on diagnosing and practicing esotericism was a subsequent recurring source of polemics among analytic philosophers.¹⁰

In particular, those working in ancient analytic philosophy objected to Straussian esotericism from the start (Vlastos 1951) and returned to it regularly, which is especially notable in a very famous (1985) NYRB essay, "Sphinx without a Secret," by Myles Burnyeat, then Laurence Professor of Ancient Philosophy. Later Malcolm Schofield called this a "devastating critique" of "Leo Strauss's treatment of Plato and its development into a cult and a political ideology" (Schofield 2019, 62).¹¹ I do not mean to suggest that Strauss' reliance on esotericism was the only source of disagreement. Scholars working on ancient philosophy, especially, also objected to Strauss' reliance on and handling of the evidence derived from Xenophon's testimony of Socrates.¹² Outside of analytic philosophy, political theorists and historians of political thought also objected to Strauss primarily on methodological grounds.¹³ These polemics, and the boundary policing involved, have much to teach us about the sociology and discipline/identity formation of post-WWII analytic philosophy.

⁸ Cf. Plamenatz (1955).

⁹ Yolton's case is not as tight as one might wish for; see <https://digressionsnimpresions.typepad.com/digressionsnimpresions/2022/06/on-esoteric-locke-and-cicero-yoltons-polemics-with-strauss-pt-2.html> (last accessed July 9, 2024).

¹⁰ Outside of analytic philosophy, the best and most sober criticism of Strauss' esotericism is Blau (2012). For a thoughtful defense see Melzer (2020).

¹¹ Schofield is a Fellow of St John's College, Cambridge, and Emeritus Professor of Ancient Philosophy in the University of Cambridge.

¹² In addition to Vlastos (1951), see especially Irwin (1974) to get a sense of the polemics.

¹³ See Rothman (1962) and, especially, Skinner (1969).

Even so, this paper focuses on less well remembered and more subdued interactions. For I show that Strauss' work also elicited a more substantive philosophical response, especially from Ernest Nagel. This response is of intrinsic interest because it involves the relationship between science and values. I show that Nagel's response was foundational to Felix Oppenheim's political philosophy or what Oppenheim called his project of 'conceptual reconstruction.' In addition, I show that lurking in Oppenheim's response to Strauss are questions about the role of the Principle of Sufficient Reason (hereafter PSR) in philosophy. My paper suggests that an opportunity was missed to clarify questions pertaining to the inductive risk of philosophy more than a half century before these issues became fashionable (as 'public philosophy' and 'responsible speech').

In the first two sections (12.2 and 12.3), I show how in his classic *The Structure of Science* (1961), Ernest Nagel engaged quite seriously with Strauss on the fact-value distinction. I also show that Nagel's argument is essential to Felix Oppenheim's method of political philosophizing.

In the last section (12.4), I analyze Oppenheim's criticism of Strauss in the 1950s.¹⁴ Oppenheim represents a different way of doing political philosophy from both Strauss and the subsequent dominant strain of Rawlsianism, and so this provides a useful glimpse at a largely forgotten episode in the history of analytic philosophy.

12.2 Ernest Nagel vs Strauss

The discussion in Nagel's *Structure* of Strauss occurs in chapter 13 on the "methodological problems of the social sciences" in section V on "The Value-Oriented Bias of Social Inquiry." This is a rather long, ambitious chapter (over 50 pages). The chapter made Nagel a recognized authority in the philosophy of social science, and got him invited to debates internal to (say) economics (Schliesser 2022). The chapter also includes, for example, Nagel's ferocious criticism of Hayek's *The Counter-Revolution of Science*, which Nagel partially recycles from a polemical 1952 review in *Journal of Philosophy*.

Nagel could be a very fierce polemicist, including a public, political polemicist (Schliesser 2022). But the response to Strauss in *Structure*, does not fit that mold.

¹⁴ I do not mean to suggest it is the first substantive criticism of Strauss among American political philosophers. In 1954, Arnold S. Kaufman published "The Nature and Function of Political Theory" in *Journal of Philosophy* with very interesting criticisms of Strauss. But it is not obvious Kaufman ought to be understood as an analytic philosopher then, although he certainly was educated in a NYC pragmatist environment.

Rather the section—it's about five pages—attributes to Strauss a family of arguments, which Nagel calls “sophisticated” (Nagel 1961, 490), that [A] “social sciences cannot be value free.” Nagel treats this as entailing or being nearly identical to the thesis [B] that there are no “compelling reasons” that “an ethically neutral social science is inherently impossible” (Nagel 1961, 495). It may seem a bit surprising that [A] is thought to entail [B] because ethical neutrality may well be a significant ‘value’! To be sure, Nagel himself derives this formulation from Strauss, who attributes the identity of “value-free” or “ethically neutral social science” to Weber (Strauss 1965 [1953], 40).

Before I get to the details of Nagel's arguments two observations: first, Nagel responds to a version of Strauss' argument published in 1951 in *Measure*. To the best of my knowledge this journal did not have a long life or wide circulation. But Strauss reprinted a version of the article as chapter II in NRH. The passage(s) from Strauss that Nagel quotes can be found there on pp. 50–53. The omissions in Nagel are basically all the places where Strauss is criticizing Weber directly. (I return to that below.) NRH was published in 1953, and famous, so it is a bit odd that Nagel does not cite it.

Second, a few years ago, Anna Alexandrova published a paper, “Can the Science of Well-Being Be Objective?” in *The British Journal for the Philosophy of Science* (2018) that has quickly become a classic in its own right. This paper engages with the material I am about to discuss (and mentions that Strauss was Nagel's target), and expresses my own substantive disagreements with Nagel better than I could. So, it is by no means obvious that Nagel's position still represents the consensus view. Alexandrova has indirectly helped rehabilitate a position akin to Strauss (but decoupled from Straussianism). So, I am primarily focused on describing the significance of the debate rather than evaluating it.

Section V of Nagel's chapter is devoted to the following issues:

Since social scientists generally differ in their value commitments, the ‘value neutrality’ that seems to be so pervasive in the natural sciences is therefore often held to be impossible in social inquiry. In the judgment of many thinkers, it is accordingly absurd to expect the social sciences to exhibit the unanimity so common among natural scientists concerning what are the established facts and satisfactory explanations for them. Let us examine some of the reasons that have been advanced for these contentions. It will be convenient to distinguish four groups of such reasons, so that our discussion will deal in turn with the alleged role of value judgments in (1) the selection of problems, (2) the determination of the contents of conclusions, (3) the identification of fact, and (4) the assessment of evidence. (Nagel 1961, 485)

I quote it for two reasons: first, because it shows that the way Nagel operationalizes ‘value neutrality’ is in terms of unanimity, or consensus. To readers of Kuhn's *Structure* this should be familiar. The broader context of Nagel's argument shows

that the relevant class participating in or constituting the relevant consensus is “competent workers in the ... sciences” (Nagel 1961, 448). In addition, the question for social science is articulated in terms of the purported “unanimity so common among natural scientists concerning what are the established facts and satisfactory explanations for them.” Nagel is aware, of course, that natural scientists do not always agree, but this occurs for him either at the research “frontier” (Nagel 1961, 448) or in contexts where the disagreement is an effect of “alternative formulations” that are “mathematically equivalent” (Nagel 1961, 158). So, value neutrality is constituted by expert agreement over facts and the theories or (equivalent) models that explain them.

Second, the response to Strauss falls under what Nagel calls (3) “the identification of fact.” Nagel identifies Max Weber’s position regarding the role of value judgments with (1). He does not identify a single unique author with (2), but in it he treats S. F. Nadel (an anthropologist, who was “a pioneer of multi-sited ethnography,”), his own teacher Morris R. Cohen, and A. E. Burt (whose work on Newton is still important) as typical representatives of it (Nagel’s own discussion draws on work by the economist and later Nobel laureate Gunnar Myrdal). Finally, Karl Mannheim is treated as the exemplar of someone who holds (4). What is neat about this list is that it is both politically diverse and reflects the practices of different kinds of social science(s).

Nagel attributes to Strauss two distinct criticisms: the first is that “the distinction between fact and value ... is untenable when purposive human behavior is being analyzed, since in this context value judgments enter inextricably into what appear to be ‘purely descriptive’ (or factual) statements” (Nagel 1961, 490). The second is that purported “factual claims about means-ends statements” are themselves infected with values (Nagel 1961, 491). It is not hard to see that they are similar in kind, because in both contexts we are dealing with the possible presence of a kind of teleology or opaque context.

Nagel elucidates the second criticism he attributes to Strauss with an argument he derives from Myrdal’s (1958) *Value in Social Theory*:

the character of the means one employs to secure some goal affects the nature of the total outcome; and the choice men make between alternative means for obtaining a given end depends on the values they ascribe to those alternatives. In consequence, commitments to specific valuations are said to be involved even in what appear to be purely factual statements about means-ends relations. (Nagel 1961, 491)

In Myrdal an argument like this is offered in the context of criticizing what Myrdal takes to be standard separation of an economics process into three: an initial situation, a means, and a given end, with the intention of only treating ends as sub-

ject to value judgments. And Myrdal claims that the means are also subject to such value judgments (as are combinations of means and ends).

In his response to the views he attributes to Strauss, Nagel emphasizes that Strauss is *right* about three features:

(a) that a large number of characterizations sometimes assumed to be purely factual descriptions of social phenomena do indeed formulate a type of value judgment; (b) that it is often difficult, and in any case usually inconvenient in practice, to distinguish between the purely factual and the 'evaluative' contents of many terms employed in the social sciences; and that (c) values are commonly attached to means and not only to ends. (Nagel 1961, 491 [letters added to facilitate discussion])

Nagel insists that Strauss equivocates on two notions of 'value judgment': first,

the sense in which a value judgment expresses approval or disapproval either of some moral (or social) ideal, or of some action (or institution) because of a commitment to such an ideal; and the sense in which a value judgment expresses an estimate of the degree to which some commonly recognized (and more or less clearly defined) type of action, object, or institution is embodied in a given instance. (Nagel 1961, 492)

Nagel adds that these notions are often conflated in the social sciences and that sometimes it is not so easy to distinguish them. But he concludes his argument that "there are no good reasons for thinking that it is inherently impossible to distinguish between the characterizing and the appraising judgments implicit in many statements, whether the statements are asserted by students of human affairs or by natural scientists" (Nagel 1961, 494).

Now, rhetorically, it is important that Nagel's argument—and this is characteristic of his argument in the whole chapter—generally has the following form:

'an apparent problem X in social science occurs *also* in natural sciences and when X occurs in natural science, X does not prevent the development of consensus in the natural sciences (and, thus has been tamed in the natural sciences) and so X does not pose an in principle obstacle to consensus in social science.'

So, for example, Nagel illustrates the two senses of "value judgment" with an example from biology.¹⁵ And the effect of this move is to turn the critic of value neutrality in social science into a critic of the purported value neutrality of natural science. Nagel assumes nobody (not even the sociologist of knowledge he discusses

15 To what degree Nagel can presuppose the value neutrality of biology is actually an interesting topic connected to his life-long effort to provide an analysis of function statements.

under (4)) will go that far. Of course, somebody may well concede Nagel's position for general relativity, but worry about conceding it in the life sciences.

It is worth saying something about what Strauss is up to. In its original context, Strauss' argument is directed against Weber. In particular because Weber is taken to be a spokesperson for the following position:

Natural right is then rejected today not only because (i) all human thought is held to be historical but likewise because it is thought that (ii) there is a variety of unchangeable principles of right or of goodness which conflict with one another, and none of which can be proved to be superior to the others (Strauss 1965 [1953], 36)

Strauss associates Weber with features of what he calls 'historicism' (viz, i) and with value pluralism (viz, ii). To be sure, Strauss recognizes that Weber is not a pure historicist, because Weber recognizes the historical situatedness of the sciences alongside the "trans-historical" nature of its "findings regarding the facts and their causes. More precisely, what is trans-historical is the validity of these findings" (Strauss 1965 [1953], 39). And because Strauss wants to argue for natural right, or at least, the inherent possibility of the recovery of natural right, he is critical of Weber.

So, Strauss is not especially interested in doing social science (although I qualify that below). But he is interested in a kind of self-limitation Weber puts on social science: "the absolute heterogeneity of facts and values necessitates [for Weber] the ethically neutral character of social science: social science can answer questions of facts and their causes; it is not competent to answer questions of value" (Strauss 1965 [1953], 40). Unless values are self-contradictory or help generate incoherence, the social scientist must be silent about them. Now this position is coherent, and it is not obvious why Strauss cares about social science method at all.

The answer to that question is that Strauss (correctly) discerns that Weber is a kind of skeptic about value or normativity as such: "his belief that there cannot be any genuine knowledge of the Ought [sic]" (Strauss 1965 [1953], 41). And so observed, value pluralism is an *effect* of this Weberian skepticism (which according to Strauss naturally leads to a nihilism that Weber obscures from himself). To be sure, Strauss attributes to Weber a kind of formal possession of the norm, "Thou shalt have ideals," (Strauss 1965 [1953], 44); so on this view Weber is not a radical (second-order) skeptic about value. For Weber the content of this norm is, as Strauss notes, "Follow thy god or demon," which Strauss reinterprets as "devotion to a cause" (Strauss 1965 [1953], 46).

As an aside, the really significant observation on Weber's philosophy for political theory by Strauss is that

Weber's thesis that there is no solution to the conflict between values was then a part, or a consequence, of the comprehensive view according to which human life is essentially an inescapable conflict. For this reason, 'peace and universal happiness' appeared to him to be an illegitimate or fantastic goal. Even if that goal could be reached, he thought, it would not be desirable; it would be the condition of 'the last men who have invented happiness,' against whom Nietzsche had directed his 'devastating criticism.' (Strauss 1965 [1953], 65)

That is to say, Strauss diagnoses how he takes Weber's Nietzscheanism to anticipate Carl Schmitt's position (even if Schmitt is not mentioned by Strauss in *this* context.) Now, the objection to Weber by Strauss that Nagel cites occurs in the discussion of Strauss' analysis of Weber's sociology of religion, which "presupposes a fundamental distinction between 'ethos' and 'techniques of living' (or 'prudential' rules)." And Strauss suggests that "the sociologist must then be able to recognize an 'ethos' in its distinctive character; he must have a feel for it, an appreciation of it, as Weber admitted" (Strauss 1965 [1953], 50). Then occurs the passage Nagel (selectively) quotes on 490–491. In Nagel's hands it is completely unclear that Strauss is offering an immanent critique of Weber.

If one only reads Nagel (without knowledge of Strauss' original) then it is natural to think that Strauss's rejection of ethical neutrality of social science is itself an obstacle to consensus or unanimity in social science; so that lurking in Strauss' argument is a kind of denial of social scientific objectivity. But this is a misrepresentation by Nagel; for Strauss the whole point of recognizing values in social science is to make social scientific and historical objectivity possible. (Because Strauss is often so critical of then existing social scientific practice this is easy to miss.) This is completely explicit in Strauss. For, Strauss concludes his own argument against Weber (at least the present one that Nagel is focused on) as follows:

The rejection of value judgments endangers historical objectivity. In the first place, it prevents one from calling a spade a spade. In the second place, it endangers that kind of objectivity which legitimately requires the forgoing of evaluations, namely, the objectivity of interpretation. The historian who takes it for granted that objective value judgments are impossible cannot take very seriously that thought of the past which was based on the assumption that objective value judgments are possible, i.e., practically all thought of earlier generations. Knowing beforehand that that thought was based on a fundamental delusion, he lacks the necessary incentive for trying to understand the past as it understood itself. (Strauss 1965 [1953], 60–61)

That is to say, for Strauss establishing the possibility of establishing natural right just is the possibility of establishing a trans-historical consensus/agreement or unanimity in social science. (To avoid confusion: all parties to the debate then agree that history, as a scholarly discipline, is at least partially part of social science—in German, history is a *Wissenschaft*.)

So, somewhat ironically Nagel and Strauss *agree* about what we might call the formal aims of social science, that it involves a species of objectivity that makes a scholarly, fundamental consensus possible. This fundamental *agreement* is obscured by Nagel's presentation. So a reader of Nagel who is unfamiliar with Strauss may well come away thinking that Strauss is a critic of the sciences or anti-scientific. It cannot be ruled out that this rhetorical effect is an intentional pay-off of Nagel's treatment of Strauss.

As an aside, recently a transcription was published of Strauss' 1965 lecture course, "Introduction to Political Philosophy," at The University of Chicago (Strauss 2018). He had assigned Nagel's *Structure* in the course, and he prepared a response to some of Nagel's criticism that is very much worth exploring.¹⁶ The existence and nature of this material suggests that Strauss also made an explicit decision not to engage directly publicly with some of his analytic critics.

As presented here, Nagel's criticism of Strauss may be thought to belong to the philosophy of social science. However, as I show in the next section, it also had an influential afterlife within the somewhat forgotten pre-Rawlsian era of analytic political philosophy.

12.3 Felix Oppenheim's Program of Conceptual Reconstruction

As Daniel Dennett informed me, Felix Oppenheim was the son of Paul Oppenheim of Hempel and Oppenheim (1948) fame.¹⁷ While there was not much distinctly analytic political philosophy before Rawls, Oppenheim (1913–2011) would have to figure in any such a story.¹⁸

In the "post-script" to a 2001 festschrift, *Freedom, Power and Political Morality*, devoted to him, Felix Oppenheim (1913–2011) rhetorically asks if he is the "lone survivor of a movement by now of merely historical interest" and distances himself from the suggestion that he was a logical positivist because he rejects "operationalism and radical empiricism" (Carter and Ricciardi 2001, 218). Rather, he prefers to understand himself as committed to *conceptual reconstruction*, which provides concepts "with descriptive definitions in order to make them available

¹⁶ Strauss (2018). The discussion is wide-ranging: Strauss treats Nagel as a kind of 'positivist' who he compares to Reichenbach (Strauss 2018, 110).

¹⁷ One might add "Oppenheim and Putnam (1958)" fame and (inter alia) "Bedau and Oppenheim" (1961) fame, too.

¹⁸ So what follows is meant, in part, as a supplement to Wolff (2013).

for fruitful communication even among persons or groups with different normative views” (Carter and Ricciardi 2001, 218).

Conceptual reconstruction is, thus, a species of conceptual engineering or conceptual articulation and has two goals: first, it is in the service of fruitful communication in the context of substantive normative and political pluralism. (As such, the project is not far removed from the spirit of the Rawlsian enterprise of conceptually constituting an overlapping consensus.) Of course, one may do conceptual reconstruction in a homogeneous society, but there it would be less needed.

What is distinctive about Oppenheim’s program is that he thinks that a ‘descriptive’ definition does “without the use of valuational notions” (Carter and Ricciardi 2001, 219). This also makes clear that facilitating fruitful communication is not the only possible use of conceptual reconstruction: second, Oppenheim also intends to provide social scientists—he explicitly mentions political scientists—with concepts that can be used in their descriptive or empirical social science (Carter and Ricciardi 2001, 221). The absence of valuational notions facilitates in the aim of avoiding the imposition of normative views on the social phenomena studied.¹⁹

Assuming that such conceptual reconstruction without valuational notions is possible, Oppenheim offers explicative definition “which might diverge from ordinary language” (Carter and Ricciardi 2001, 219). To what degree such divergence makes the output of concept reconstruction less amenable to political life is ignored, but it seems safe to predict that conceptual reconstruction is more useful to those committed to value-free social science than to political agents in the middle of public contestation. Descriptive definitions so conceived “are not true or false, since they are linguistic stipulations” (Carter and Ricciardi 2001, 219). They are, rather, more or less useful. And this consequentialist understanding of success has a Carnapian flavor to it (Stein 1992).

It is to be allowed, however, that in the context of mutual trust or elite bargaining, “political actors sometimes make some effort at ‘resolving those differences through argument and persuasion’” and then the presence of descriptively defined concepts may be very useful (Carter and Ricciardi 2001, 221).²⁰ Oppenheim’s program can work in a political culture in which technocratic tools (say a lexicon) are welcomed to reduce social friction or are used to reduce conflict over the terms of a bargain. What this requires is that “a lexicon be uncontestable, in the sense that its vocabulary be made up of definitions that are not valuationally tinted” (Carter and Ricciardi 2001, 221). Such a lexicon can be contested and con-

¹⁹ See also Deutsch and Rieselbach (1965, 153–154), where Oppenheim is mentioned.

²⁰ Oppenheim is partially quoting Ball’s contribution to the *festschrift*.

testable in the sense that an improved vocabulary might facilitate more efficient or consensual (etc.) bargaining. No lexicon is final.

Oppenheim allows that his conception of descriptive definition is dependent on an “idea” he “did take over from the logical positivists: the separability of ‘facts’ and ‘values’ on the *conceptual* level” (Carter and Ricciardi 2001, 219, original emphasis). In context, he does not argue for the claim. However, Oppenheim had defended the claim in a 1973 paper. There he acknowledges that in the speech acts of *ordinary or everyday life* there is no such separability (Oppenheim 1973, 58). So, there is another sense in which Oppenheim echoes Carnap’s philosophy: it relies on a relatively sharp distinction between a natural language of ordinary folk and a specialist language (which is often formal). In the 1973 paper, it is explicit that Oppenheim is more focused on designing “the language of social and political inquiry” (Oppenheim 1973, 59).

Unfortunately, the 1973 paper argues by way of criticizing those who had either *de facto* or in practice denied such separability. So it is not really helpful in reconstructing Oppenheim’s positive argument *in favor* of the conceptual separability of facts and values thesis. But for Oppenheim the positive argument is embedded in his earlier account of descriptive definitions. For example, near the start of his 1970 “Egalitarianism as a Descriptive Concept,” Oppenheim writes:

Equality and inequality of characteristics are no doubt descriptive concepts. Indeed, whether A and B have the same age or nationality or income can be empirically ascertained. So can assertions that A has greater ability or aptitude than B. These are characterizing value judgments: such statements are descriptive, not normative. (Oppenheim 1970, 143)

This terminology is clearly indebted to the material of Ernest Nagel’s 1961 *The Structure of Science* discussed in section 12.2 above. Unsurprisingly Oppenheim cites the relevant page-numbers of Nagel (in which he is critical of Strauss) in the accompanying footnote. So, the first main pay-off of the present investigation is that Oppenheim’s program of conceptual reconstruction *presupposes* that Nagel’s criticism of Strauss is substantially correct, and that, thus, characterizing value judgments can be kept sufficiently distinct from the normativity we find in appraising judging.

As I show in the next section, there is very good evidence that Oppenheim was familiar with Strauss’ *Natural Right and History*. So, he clearly must have made up his own mind about the adequacy of Nagel’s response. However, if the reader agrees with me that Alexandrova’s (2018) criticism of Nagel on this score is apt, then Strauss’ unwillingness to pursue the exchange with Nagel in public also gave Oppenheim false confidence in the robustness of his program.

12.4 Felix Oppenheim's Criticisms of Leo Strauss

In a 1955 essay, Oppenheim defended a form of epistemological relativism. In particular, Oppenheim is committed to the claim that “words such as ‘good,’ ‘desirable,’ and ‘valuable’ do not designate properties of things or events or actions, but express the speaker’s subjective preferences” (Oppenheim 1955, 411). Oppenheim is explicit that he is echoing Charles L. Stevenson’s *Ethics and Language*. To be sure Oppenheim’s “relativism is opposed to value-objectivism, not to objectivism in science. If ‘objectivity’ means possibility of objective, i. e., intersubjective, verification, relativism denies the objectivity of intrinsic value-judgments, but not the objectivity of empirical statements” (Oppenheim 1955, 411). Oppenheim goes on to claim: “Relativism does not question the possibility of explaining or giving reasons for people’s valuations; relativism denies the possibility of validating or giving grounds for them” (Oppenheim 1955, 412).

An auxiliary claim that matters a lot to Oppenheim’s argument is that “logically, there is no necessary connection between any particular value-judgment and either absolutism or relativism” (Oppenheim 1955, 412). This will play an important role in Oppenheim’s argument against Strauss. Oppenheim identifies Strauss twice as one of his targets. First, in a footnote attached to the following passage:

[A]bsolutists tend to consider relativism incompatible with whatever goals they adjudge demonstrably valuable. Thus value-absolutists who are also political absolutists (i.e., who claim that the values underlying political absolutism are the ‘true’ values) maintain that relativism is dangerous because it promotes democratic values. Absolutists who believe that the democratic way of life can be proved to be the best often hold that a subjectivist be a ‘true’ democrat.

The latter thesis is based on arguments such as the following: if good and evil is not a matter of objective knowledge, then everything is a matter of indifference. The relativist must therefore be tolerant of every ethical viewpoint, even of doctrines which command the overthrow of democracy. Or still worse: if there is no justice, it does not matter how one acts; so one may just as well climb on the bandwagon. Thus relativism becomes akin to nihilism, cynicism, and opportunism. (Oppenheim 1955: 414–415)

In the accompanying footnote, Oppenheim quotes the following passage from Strauss, NRH: [II] “If our principles have no other support than our blind preferences, everything a man is willing to dare will be permissible. The contemporary rejection of natural right leads to nihilism—nay, it is identical with nihilism” (Strauss 1965 [1953], 5, Roman numeral added to facilitate discussion).²¹

²¹ In the 1965 edition it is on p. 4.

Second, Oppenheim goes on to claim, shortly thereafter, “absolutists are likely to shift the argument from the logical to the psychological level and maintain that it is psychologically impossible to commit oneself whole-heartedly to any set of values unless one is convinced that it corresponds to the objectively valuable.” And in the accompanying footnote, Oppenheim quotes Strauss from NRH as follows: **[II]** “Once we realize that the principles of our actions have no other support than our blind choice, we really do not believe in them any more. We cannot wholeheartedly act upon them any more. We cannot live any more as responsible beings” (Oppenheim 1955, 415).²²

Both of Strauss’ claims can be found in the “Introduction” of NRH. In immediate context Strauss does not offer detailed arguments for these claims so they will seem rather ungrounded. It is a bit peculiar that Oppenheim ignores here the rest of Strauss’ arguments developed later in the book. Even so, there is something interesting to be said about the state of the debate between Strauss and Oppenheim.

In the first Strauss passage **[I]**, Strauss does not claim here that relativism leads to “cynicism or opportunism” as Oppenheim suggests. Even so, it is not obvious why according to Strauss nihilism follows from the rejection of natural right or from thinking that our principles are derived from our preferences.

The second **[II]** passage helps start to explain why Strauss thinks nihilism follows from the kind of relativism somebody like Oppenheim defends. It is not obvious why Strauss seems to be committed to such a dramatic slippery slope argument. But it becomes intelligible if we recognize that, fundamentally, there is a suppressed premise in Strauss’ argument: that something needs to ground the principles of our actions or these grounds (and so on) to halt the slide down the slope (toward nihilism). So, lurking in Strauss’ position is a kind of appeal to some version of the PSR to halt a regress argument.

That Strauss is committed to something like the PSR is not wholly obvious in context of the introduction to NRH. But later, in the long chapter 2 on his critique of Weber’s account of the distinction between fact and values (which is the chapter that Ernest Nagel criticizes), in the context of ascribing to Weber the idea that “science or philosophy rests, in the last analysis, not on evident premises that are at the disposal of man as man but on faith,” Strauss goes on to claim: “By regarding the quest for truth as valuable in itself, one admits that one is making a preference which no longer has a good or sufficient reason. One recognizes therewith the principle that preferences do not need good or sufficient reasons” (Strauss 1965 [1953], 72).

22 Oppenheim is quoting Strauss (1953, 6). It is also p. 6 in the 1965 edition of NRH.

There is much to be said about Strauss' account as a reconstruction of Weber, and as a diagnosis of the effects of Weber's position. But what does seem clear is that Strauss associated the rejection of what Oppenheim calls 'absolutism' with the claim that preferences are ungrounded or at least not grounded in sufficient reason.

In fact, being alert to this feature of Strauss' position also helps explain the recurring use of "blind" in modifying "preferences" and "choice" in the passages [I]&[II] Oppenheim quotes in the notes. This use of 'blind' echoes the kind of language early-modern critics of epicureanism would use when drawing on the PSR (see Schliesser 2021, 81, 85–86, 89, 107–112, 118, 127–128). Such preferences and choices are treated as unguided, and so *de facto* random. So, lurking in Strauss is a defense of, or at least an acceptance of (the utility of), the PSR,²³ and metaphysics more generally.

Interestingly enough Oppenheim *agrees* about the stakes (but not on the PSR). Oppenheim claims that his flavor of "Relativism does not question the possibility of *explaining* or giving reasons for people's valuations; relativism denies the possibility of *validating* or giving *grounds* for them" (Oppenheim 1955, 412, original emphasis). So, not unlike Russell and much of the mainstream of analytic philosophy since, Oppenheim rejects the PSR (at least in the context of values).²⁴

I do not mean to suggest that the disagreement between Oppenheim and Strauss is only, ultimately, about the status of the PSR. Oppenheim's explicit criticisms of Strauss are also framed by an *implicit* disagreement over the status of inductive risk of relativism. Oppenheim does not name the critic who holds that "relativism is subversive": "they brand relativism as socially harmful if not outright subversive" (Oppenheim 1955, 414). But that philosophy should be mindful of harming society is a commitment that runs through Strauss' philosophy.²⁵ Concern over the inductive risk of philosophical activity has not been fashionable within analytic philosophy. For, as Oppenheim retorts, such concern (and here he anticipates a move familiar from Nagel) does not undermine the "validity of the thesis: To argue this way would be like questioning the validity of nuclear physics by pointing to the H-bomb" (Oppenheim 1955, 414).

In 1955, Oppenheim had understood Strauss's criticism as a kind of psychological thesis. He is unconcerned by Strauss's argument because he understands his

23 On this point, see Bruell (2011) which cites more passages in NRH in support of this claim. I thank Thomas Cleveland for calling my attention to this.

24 See Della Rocca (2021), who cites Russell (2010). There is also a connection to the voluntarism of Carnap and Reichenbach described in Dewulf's chapter (this volume). I thank a referee for alerting me to this.

25 For evidence of this claim see, especially, the (very critical) chapter on Strauss in Holmes (1993).

own moral relativism as an epistemological thesis. Even if one is utterly unmoved by Strauss's position (which is expressed rather concisely and ultimately rooted in the PSR), it is pretty clear that Oppenheim has misjudged it. Strauss's claim is not a contribution to psychology, but existential. The fact that Strauss thinks our preferences lack ground means that for him they are fundamentally unexplained from the perspective of rationality. So, Strauss's position is much more related to epistemology than Oppenheim allows.

Oppenheim must have felt something akin to the problem I have just diagnosed because Oppenheim returns to his disagreement with Strauss in a 1957 APSR article. He quotes (in redacted form) the same passages from Strauss [I]&[II] as he did in 1955. But he offers a new response.

Before I get to that, one aside. In his article Oppenheim first mentions Strauss in the following way,

Non-cognitivism does not maintain that value-words, even in the intrinsic sense, are meaningless, but only that they have normative, evaluative, directive, rather than cognitive meaning. Natural law theorists are therefore mistaken when they claim, as e.g., Leo Strauss does, that [III] the denial of natural law implies 'the prohibition against value judgments in social science.' (Oppenheim 1957, 50)²⁶

Oppenheim quotes Strauss out of context. In context Strauss is criticizing Weber's account of the separation of facts and values! But this does not affect the rest of Oppenheim's or my own argument.

The new 1957 response is basically Humean in character, although Oppenheim's terminology is ultimately derived from Carnap (1947).²⁷ In responding to [I]&[II] Oppenheim draws on his own account of rational choice and Carnap's idea of total evidence:

To make a rational choice, the decision-maker must predict the consequences of each of the alternative courses of action open to him in a given situation. His decision will be rational provided these anticipations are based on the total evidence available to him. Such predictions are, of course, true only with a certain degree of probability, and a decision may be rational, yet unsuccessful, and vice versa. (Oppenheim 1957, 52)²⁸

²⁶ In an accompanying footnote he cites Strauss (1965 [1953], 52).

²⁷ In his 1957 APSR paper, Oppenheim cites his own (1953) and in it Carnap (1950). This incorporates material from Carnap (1947). (See p. 343, note 3 in Oppenheim 1953.) For discussion of underlying philosophical issues, see Good (1967).

²⁸ Cf. Oppenheim (1953, 343): "To arrive at a rational decision, it is sufficient to make warranted predictions about the significant effects of one's significant alternative potential actions."

Let us leave aside how compelling Oppenheim's view of rational choice is. Although it's notable how thin the view is; there is no requirement here of internal consistency at a time or over time. It is not even obvious we are dealing with a species of instrumental rationality (because the predictions are in no way connected to the arbitrary preferences or principles on which choices are founded). Having said that, and in light of his 1953 article ("rational choice"), I am assuming that Oppenheim is assuming a kind of instrumental rationality here, and that he intends to be describing how preferences (he uses "goals") can be satisfied in light of evidence and knowledge.

What is important for present purposes about Oppenheim's new response to Strauss is that he blocks the claim that choices founded on ungrounded preferences are arbitrary altogether. They are now constrained by evidence and how it relates to genuine possibilities and one's knowledge of these (since they are based on predictions). In addition, these actions can be intelligible in an important way because a spectator (who becomes privy to the evidence and knowledge used) may well predict and perhaps even understand the choices made. In fact, this is, on my view, really Oppenheim's underlying point. He wants (recall the discussion above of his program of conceptual reconstruction and looking ahead to his book *Dimensions of Freedom*) to provide conceptual tools for empirical science. He understands his position as "an extrinsic value-judgment, namely the empirical hypothesis: whenever you want to bring about the state of affairs which, under the circumstances, will be most valuable (or least disvaluable) to you, apply the rules of rational decision-making" (Oppenheim 1957, 53).

Even if the Straussian would accept Oppenheim's account of rational choice, the Straussian rejoinder is not hard to guess: such predictable choices or instrumental rationality in the serves of ungrounded or arbitrary ends may well exhibit a species of madness/sociopathy or evil. (That is to say the nihilism charge has not been blocked.) So, I doubt that Strauss would be very impressed by Oppenheim's follow up even if he were to grant that given Oppenheim's aims Oppenheim has succeeded on his own terms. If we step back from their debate, the philosophically important point lurking here is that prediction by social science and the rationality of what is predicted may come apart. That is of course a truism, even if it is often forgotten. The problem (of the inductive risk of philosophy itself) with the truism arises when predictions by social science (grounded in certain philosophical conceptions of rational choice) become action-guiding and so, say, contribute to the irrationality of the outcomes (think of game theory and mutually assured destruction).

That is, lurking in the Strauss-Oppenheim debate is a debate over the relationship between philosophical theorizing and its instrumental role in social science, or other forms of social activism. With the benefit of hindsight, it is a shame that

Kaufman's earlier (1954) criticism of Strauss did not play a role in this debate because Kaufman (who cautiously defends an instrumental, social role for political philosophy) notes that Strauss's epistemology of philosophical or eternal truths is itself underspecified and that it is by no means obvious there are truths separate or separable from sciences, including formal ones.²⁹ Oppenheim echoes the point (without citation of Kaufman), when he notes in a footnote, while reviewing Popper's *Conjectures and Refutations* that "it is, however, not always easy to determine whether the anti-empiricists here referred to would claim that their method for apprehending 'truth' is scientific, but non-empirical, or non-scientific—i. e., supra-rational. This ambiguity may be found, e. g., in Herbert J. Storing, ed., *Essays on the Scientific Study of Politics* (New York 1962)" (Oppenheim 1964, 351, fn. 14).

During the revival of recent interest in questions over inductive risk of philosophy, the fact-value distinction, and science & values more generally, only Alexandrova has noticed that some of the received, standard positions in analytic philosophy on these matters were sometimes sharpened in response to some elements of Leo Strauss's philosophy. And while it would be wholly misleading to suggest that the increasing rejection of these received views vindicate Strauss's arguments or his mode of philosophy, the dispassionate historian will note that the pull of some of the challenges Strauss articulated to the philosophical defense of mid-century social science was re-discovered independently from Strauss's writings. While this should not make us overlook the ways Straussianism can still provoke and annoy, it would be a mistake to reduce its significance wholly to a fork in the road worth forgetting altogether. For, as I have noted, lurking in Strauss' writing is (recall his apparent embrace of the PSR) a defense, or at least a conditional defense, of metaphysics—a seed of which blossomed, most unexpectedly, in Syracuse.³⁰

²⁹ Kaufman comes very close to grasping Strauss' skepticism. On the complex relationship between the PSR and skepticism, see Della Rocca (2017). Only Quentin Skinner seems to have realized Kaufman's significance to the debate between Straussians and their critics. See Skinner (1969, 12–13, fn. 49). When Yolton confronted Strauss' purported position that "either we are able to ground moral criticism in natural rights, or we are faced with moral nihilism," Yolton does not criticize Strauss' epistemology, but argues (in a proto-Gadamarian vein) that forms of circularity cannot be avoided in dealing with "accepted" social values but that such circularity need not be vicious. Yolton misses Strauss' skepticism and fideism altogether.

³⁰ See Bernadete (1964), especially p. viii.

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David Dyzenhaus

13 ‘Immanentizing the Eschaton’: Eric Voegelin, Hans Kelsen, and the Debate over Secular Religion

Abstract: Within the philosophical migration in the 1930s from Austria and Germany to the US there was a small minority of scholars who laid the basis for the toxic blend of militant Christian conservatism, libertarianism, and anti-liberalism that drives the Republican Party in the Trump era. I focus on Eric Voegelin and his debate in the 1950s with Hans Kelsen, the supervisor of Voegelin’s PhD thesis in 1920s Vienna. Voegelin claimed that liberalism is an authoritarian ideology that seeks an “immanentization of the eschaton,” a realization of paradise on earth. Kelsen’s “Pure Theory of Law” is constructed in the same scientific spirit of inquiry as that of the Vienna Circle and his response to Voegelin was devastating. But he languished in obscurity in the Berkeley Political Science Department and his Pure Theory hardly figures in Anglophone philosophy of law, while Voegelin remains a significant and pernicious influence today.

13.1 Introduction

Philosophy in the English-speaking world is profoundly shaped by the influx of European academics who in the 1930s sought refuge in the United States from persecution in their home countries, principally Germany and Austria. For many philosophers in the analytic tradition, the transformation of the discipline wrought by these scholars—particularly the logical empiricists of the Vienna Circle—is cause for celebration. And almost all would probably welcome the fact that the scholars of the Vienna Circle, as well as as of other philosophical dispositions, for example, the Frankfurt School, happened to be of the right sort politically, at least liberal, or social democratic, or further to the left.

Little notice is taken by analytic philosophers of the small minority within this scholarly migration whose way of thought is both antithetical to empiricism and who came from the other end of the political spectrum, for example, Eric Voegelin,

Note: I thank Coel Kirkby, Martin Pickavé, Sander Verhaegh, and Lars Vinx for comments on a draft, and the participants at the Tilburg University conference “Euro-American Migration and the Development of Postwar Philosophy” for discussion. For a much shorter and different account of the debate, see Dyzenhaus 2022c. For an attempt at a response, see Henningsen 2022.

my focus here, but also Leo Strauss and Gerhart Niemeyer. This is a mistake, if only because the influence of this minority is immense, in departments of political science and in prominent law schools, more significantly in the real world of politics. But there is another reason to take them seriously for the challenge they pose—that it is not happenstance that the majority came from somewhere within the liberal-left end of the political spectrum since their empiricist methodology conceals an ideological agenda.

To bring out this challenge, I will focus on a debate in the US in the 1950s that took place between Hans Kelsen, one of the leading philosophers of law of the last century, and Erich Voegelin, whose PhD thesis Kelsen had supervised in Vienna in the 1920s. Both were part of the same migratory wave to the US.

Kelsen was an Austrian Jew, a social democrat. He was one of the authors of the 1920 Austrian constitution, still in force today. He designed in the process the world's first dedicated constitutional court, and sat as a judge on its first bench. He played a prominent role in debates about the new international order that was put in place after the First World War and, as a law professor in Cologne, in the existentially important debates in late Weimar with his reactionary colleague Carl Schmitt about which institution should be considered the 'guardian of the constitution' (Vinx 2015). Fired from his position under the Nazi 1933 *Law for the Restoration of the Civil Service*, he made his way via Geneva and Prague to the US, and ended up an obscure and lonely figure in the Berkeley Political Science Department. In Europe and in Latin America his stature is preserved. But he hardly has a presence in Anglophone legal scholarship and almost none in the world outside the academy.

Though not part of the Vienna Circle, Kelsen had a close relationship with its main figures and with them was on the social democratic and liberal side of the political divide (Jabloner 1998). In addition, his project of developing a value-free science of law—a "Pure Theory"—had much in common with their project of scientific philosophy, with one important difference in that Kelsen argued that a scientific theory of law had to account for the normative dimension of legal order. Philosophy of law, that is, is concerned with a world of oughts which cannot be reduced to facts about the empirical world. Still Kelsen believed that there could be a value-free science of such norms, one that did not presuppose any moral or political commitments.

Voegelin was German, and studied at the University of Vienna, under the direction of Kelsen and Othmar Spann, a conservative social theorist. He was appointed to the Faculty of Law in 1929 as a *Privatdozent* and then in 1936 as an assistant professor. He was neither Jewish nor socialist. Historian of political thought Mark Lilla claims that Voegelin fled Austria because in two books published in the early thirties he had attacked "pseudoscientific works supporting the Nazis' biolog-

ical racism.” That made him “a choice target of Austrian Nazis, who authorized his arrest immediately after the *Anschluss* in 1938” (Lilla 2016, 28).

Voegelin spent much of his career in the Department of Government of Louisiana State University, later the University of Munich and the Hoover Institution of Stanford University. The website of the Eric Voegelin Institute at Louisiana State claims him as “one of the most original and influential philosophers of our time.”¹ I agree about the extent of his influence, but it is not that of a philosopher. Rather, it is the result of views that should deeply trouble liberals, democrats and anyone who values the rule of law. Since no snapshot of his views is available, they will emerge here through placing them in context and tracing their evolution.²

Kelsen’s debate with Voegelin poses sharply the question whether a value neutral science is possible. On Voegelin’s side of the divide, the claim is that once one leaves the project of inquiry into facts about the empirical world all one finds is substantive political positions that jostle with each other for power. But the claim does not amount to a relativism about value. Indeed, he and the others in the minority, for example, Leo Strauss and Gerhart Niemeyer, regarded relativism as the sin of the empiricist outlook, one which left the West defenseless against its enemies, in the 1930s first Communism and then Nazism, which they found too radical and godless a solution to the malaise of the West.

In the late twentieth and into this century, the sense of the enemy has changed. It is now liberalism against which their disciples, for example, John Finnis in Oxford and Adrian Vermeule at Harvard, mainly contend (Dyzenhaus 2022c). They understand Western societies to be dominated by liberal elites engaged in a cosmopolitan project of imposing their value system on the world, all under the guise of value neutrality. In place of what they think of as the secular religion of liberalism, they advocate an aggressive assertion of value—the values of the one true position, the Christian heritage of the West, what US Senator Joshua Hawley, in a Voegelin-inspired essay, called “A Christian Vision for Kingdom Politics” (Hawley 2012).

1 <https://faculty.lsu.edu/voegelin/voegelins-works/about-dr-voegelin.php> (last accessed July 9, 2024).

2 The closest Voegelin came to setting out a positive view in the works I have consulted is his monograph, *The Nature of the Law* (Voegelin 1991 [1957]). The editors say (xiii) that the book is “Voegelin’s only comprehensive and systematic text on law,” a “product of the mature Voegelin.” But this essay of 69 pages is for the most part a critique of Kelsen’s Pure Theory for lacking substance and is neither original nor well done, compared, for example, to the critiques set out in the 1920s and 1930s by Carl Schmitt, his fellow reactionary, and Hermann Heller, who was on Kelsen’s side of the political divide.

In this light one can reframe the challenge to empiricism and positivism as follows. It is not only happenstance that the majority of émigré philosophers came from somewhere within the liberal-left end of the spectrum since their empiricist and positivist methodology conceals a political agenda. But, or so these rightwing thinkers suppose, political argument cannot take place on the basis of experience of the empirical world, since it requires the very metaphysical contest over transcendent moral value that the logical empiricists and positivists such as Kelsen reject. And as what they regard as Kelsen's failed attempt to set out a pure science of norms shows, there is no resting place—no neutral space—between godless empiricism and religious commitment, even if one is agnostic, as Voegelin perhaps was, on what content that commitment should have.

Section 13.2 sketches Voegelin's theoretical position, while Section 13.3 sets out the debate by situating it within that sketch. Section 13.4 unpacks the implications of his position and Section 13.5 suggests how Kelsen's theory provides the resources to respond to Voegelin and others on the extreme right of the spectrum.

13.2 Voegelin's Views

Lilla's claim about Voegelin's exit from Austria is deeply misleading because he fails to situate it in the context in which Voegelin wrote these works and he says nothing about Voegelin's own position at this time. He thus perpetuates the official view of Voegelin's many admirers, one which Voegelin himself was careful to encourage. In his memoir, he points out that his critique of biological theory as a basis for racism was "not quite compatible with National Socialism" and that one of his books on this topic "was withdrawn from circulation by the publisher and the remainder of the edition was destroyed" (Voegelin 2011 [1973], 52–53). He even laments that this book, "one of my better efforts, has remained practically unknown, though it would be of considerable help in the contemporary, rather dilettantish [sic], debates between evolutionists and anti-evolutionists" (Voegelin 2011 [1973], 52–53).

This is a remarkable display of hubris given the turgid content of his books on race which range widely in an attempt to show that various candidates for supporting the claim that the "race idea" is the key to modern politics fail: biological explanations, theories that define themselves against an ill-understood Jewish other, and theories based on the "Nordic idea" because they are too cosmopolitan and internationalist in nature and so inadequate to the political reality of the nation state (Voegelin 1997 [1933]). But these critiques are far from preparing the way for a dismissal of the race idea. Indeed, Voegelin made clear that the first book was

just a ground-clearing exercise for the second (Voegelin 1997 [1933], 120–121). And the second book comes to the following conclusion:

Now we see how the race idea becomes effective in the construction of the community—effective in the two intimately connected ways of objectively constructing the community through the idea of race and of subjectively convincing the people involved in the community that the race is essential for their connectedness as community. Race is no longer merely the object of scrutiny, seen at a distance; but a body-soul-spirit reality that includes the scholar himself, and the concept of race that is formed in the concrete situation is no longer a scientific concept but a tool for interpreting the meaning of one's own life and the broader life of the community. It is not merely the creation of a passive attempt at "understanding," but an instrument in the service of the future shaping of the community; it is the idea of the community as a bodily context as it is projected into the future by its members. (Voegelin 1999 [1933], 179–180)

Voegelin thus differed from the Nazis only in that, as Aurel Kolnai put in 1938 in *The War Against the West*, perhaps the first comprehensive study of Nazi and fascist ideology, he "always stays at a certain refined distance from passionate partisanship." Kolnai concluded that "if this condemns Voegelin in Hitler's eyes, it does not by any means acquit him in ours.... In so far as he is on bad terms with official Naziism, he is not the only man to incur such misfortune through stating Nazi *Weltanschauung* too intelligently" (Kolnai 1938, 458–459).

In other words, the issue between Voegelin and Nazi racists was not racism, but the basis for it, as he himself made clear in two responses to critics in 1934 (Voegelin 2001a and 2001b).³ Moreover, there was enough support for his more academically refined kind of racism in Germany for him to expend much energy in twice trying to get a position there after Hitler's seizure of power by sending his books on race along with supplicating letters to Nazi academics teaching at German institutions (Faye 2015). He got a sympathetic hearing, but was rejected the first time because of his relationship with Kelsen, which prompted him the second time to write a long letter proving his "Aryan" purity and proclaiming his distance, both academically and personally, from Kelsen. Indeed, it is not clear on what basis Lilla's claim about Voegelin's fleeing ahead of imminent arrest rests. In his autobiography, Voegelin says only that he wanted to emigrate because he had been fired from his academic position, feared that his passport would be confiscated, so left in a hurry while he could still do so legally (Voegelin 2011 [1973], 70–71).

Voegelin's disciples together with Lilla also ignore the fact that in the 1930s Voegelin was a prominent member of "Black Vienna," the motley collection of con-

³ See especially Voegelin (2001b, 22), where he states: "The vital sources of the German nationalist movement are to be found, not in science, but in an experience that gives rise to myth."

servatives, fascists and Nazi sympathizers who made up the opposition to the social democrats, Marxists and communists of “Red Vienna.” More specifically, he was part of a group of intellectuals who embraced a fascist anti-scientific view of the world against the left-leaning Vienna Circle sorts (Wasserman 2014). The work which followed his two books extolling the race idea as the key to politics was his 1936 work on the authoritarian state (Voegelin 1989 [1936]). His claim there is not that authoritarianism is the problem of the Austrian state. Rather it is the cure for the political problems of the pluralistic state order in Austria, a cure which began when, in the midst of the political crisis of 1934, fascist politician and Federal Chancellor Engelbert Dollfuss dissolved parliament and assumed dictatorial powers. Moreover, the point of the book is how to make the Austrian state into as efficient an instrument of fascist rule as possible.

On this issue, Voegelin is less reticent than his disciples in acknowledging what he was up to. In his memoir, he remarks scornfully that “[t]he Austrian veering toward Mussolini as a protection against the worst evil of Hitler apparently was beyond the comprehension of ardent Marxists, who could do nothing but yell ‘Fascism’” (Voegelin 2011 [1973], 68). He also says that the book was his “first major attempt to understand that an authoritarian state that would keep radical ideologists in check was the best possible defence of democracy” (Voegelin 2011 [1973], 9).

This astonishing claim—that if you want to defend democracy you should embrace fascism—is highly significant for our current political predicaments, a point I come back to later. For the moment, I want to note that Voegelin sought to sanitize his unapologetic fascism by claiming that “his theoretical attitude in these matters at this time was not very different from that of Robert H Jackson’s “formula” in the *Terminiello* case of 1949 that democracy is ‘not a suicide pact’” (Voegelin 2011 [1973], 9). His claim was utterly disingenuous. All that Justice Jackson in his dissent in that US Supreme Court case held was that the constitutional right to free speech did not extend to speech inciting a mob to violence. The full quotation reads:

The choice is not between order and liberty. It is between liberty with order and anarchy without either. There is danger that, if the Court does not temper its doctrinaire logic with a little practical wisdom, it will convert the constitutional Bill of Rights into a suicide pact.⁴

That is a far cry from Voegelin’s full-throated defense of a fascist regime in which “liberty” does not figure once as a constitutional principle in his effort to show how the Austrian authoritarian state can be streamlined and in fact is mentioned only once on the very last page of the book:

⁴ *Terminiello v. City of Chicago* 337 U.S. 1, 37.

Our analysis of constitutional mechanisms was made from the viewpoint of the order of the principal political powers. In doing so, we have omitted one essential element of these arrangements—"the liberal element". By "liberal" we mean a metaphysics of the human being and the state according to which the individual, as a metaphysical substance, must also be a power in the state structure, a power that sets absolute limits to the authority of the state. The problems that are raised by the metaphysics of the person and are reflected in the ideas of liberty and fundamental rights affect the organization of the authoritarian state just as they affect every organization. Their discussion, however, would open up basic questions of philosophy of the state that we do not want to address in a study of the particular problems of the Austrian state. (Voegelin 1989 [1936], 362)

One could try to interpret this opaque paragraph as suggesting that once the authoritarian state had succeeded in stabilizing the situation by eliminating Nazis, communists, and socialists from the political arena, one could think again of making a place for liberalism and its commitment to civil liberties in the Austrian constitution. But since such liberties would open up the possibility of the kind of political pluralism that Voegelin regarded as having created the problem the authoritarian state was supposed to cure, this interpretation is wholly implausible.

The only plausible interpretation is that, in the same way as the project of forging a kind of fascist state suited to the Austrian political context had begun only two years prior to Voegelin's book, so had the project of forging the race idea of the person best suited to anchoring the project. The "liberty and fundamental rights" of such a person would be very different from anything associated with the liberal idea since it would be at the least antithetical to it. But one cannot say much about it until certain obstacles have been surmounted, the relics of the old liberal pluralist order. Moreover, Voegelin did not confine his activity to giving intellectual succor to the fascist regime. He was a member of a secret committee of the community college Ottakring, which recommended and achieved the "political cleansing" of its staff, that is, getting fired from their positions academics whose views were opposed to the authoritarian state (Limbeck-Lilienau and Städler 2015, 331–332).⁵

This stance cannot be written off as a youthful indiscretion. The idea of curing the ills of the modern world by re-establishing a culturally and racially homogeneous basis for Western societies is the persistent theme of Voegelin's life's work. Implicit within that theme is that reaching this goal requires maintaining the domination of the white Christian race. It is this theme that explains Voegelin's influence. For he is not influential as a philosopher, despite the Voegelin Institute's claim. Rather, as Lilla correctly says, he was an "amateur historian" of great range, so great that Lilla says he may seem like George Eliot's Mr. Casaubon, the "obses-

5 For Voegelin's view of education in which this cleansing was undertaken, see Voegelin (2001c).

sive polymath” of *Middlemarch* “whose search for the ‘key to all mythologies’ left him only torsos of unfinished works.” Lilla rejects this conclusion, finding that Voegelin’s trawls through the history of Western thought did come up with a key: “guiding all Voegelin’s writings was a basic intuition about the relation between religion and politics, and how transformations in that relation could explain the cataclysms of history” (Lilla 2016, 29).

That intuition turns out to be a claim about ‘immanentizing the eschaton,’ advanced in Voegelin’s most influential work *The New Science of Politics*: that the history of ideas in the West should be read as a clash between various “Gnostic” ideologies, including liberalism which is a secularized religion (Voegelin 1987 [1952], 119, 166). Voegelin takes Gnosticism to be the creed that the earthly world is created by an evil lesser god and that only a select few have access to the divine, which they can facilitate bringing to earth through some apocalypse. He applies the label promiscuously to any position that seeks to change the nature of man. On this view, Stalinism, Nazism, and liberalism are equally Gnostic. As Voegelin’s debate with Kelsen reveals, one is drawn to this vague idea, rooted in a dubious account of the history of thought, only if one is oneself interested in promoting a cataclysm that, far from saving liberal democracy, will bring about its implosion.

13.3 The Kelsen-Voegelin Debate

Narrowly conceived, their debate consists of *The New Science of Politics* and Kelsen’s response, which he wrote in the mid-1950s, and then extensively rewrote, for unknown reasons not letting the manuscript go to publication, despite its having been accepted twice by different presses. (The second time around he withdrew the manuscript at the galley proof stage and had to reimburse the press a large sum to cover its costs.) Both the original response and the much extended version were published posthumously. (Kelsen 2004 [1954]; 2017 [1962]) But the debate in a sense goes back to the early 1930s when Voegelin began publishing work critical of his former supervisor.

For example, early in *Race and State*, the second of his books on the race idea, Voegelin identifies Kelsen’s Pure Theory as a prime example of the main problem with the German tradition of *Staatslehre* which takes the nature of the modern state as the main task for academic inquiry in politics and law. Because Kelsen thinks that the task involves understanding the state as it manifests itself in any social or political context, he deliberately excludes moral, political, and social considerations from his attempt to construct a scientific account of legal order as a system or norms, a theory of the authority of the modern legal state. For Voegelin, this attempt must fail since it “purges” from inquiry consideration of the role of

the “person and the community.” The racial theories that he lists in the book are, he thinks, on the right track at least in that they seek to ground their theories of the state in social and anthropological reality, where “anthropology” means not the study of actual groups, so much as a philosophical theory of the person as a social being (Voegelin 1997 [1933], 6–7).

Voegelin then elaborated this critique in a fifty-page discussion in *The Authoritarian State*, in which his principal objection is that a legal order can only function if it is “materially uniform,” which requires that certain preconditions are put in place (Voegelin 1989 [1936], ch. 6). He is rather vague about these preconditions, but one can infer what he has in mind given his endorsement earlier in the book of Schmitt’s idea of the “total state” (Voegelin 1989 [1936], 58–63). There Voegelin’s principal point of reference is Schmitt’s claim in 1931 in “The Guardian of the Constitution” that only the head of the executive is capable of guaranteeing the substantive homogeneity of the people, which requires getting rid of parliamentary democracy and social pluralism, and that consequently all legal authority should be located in the head (Schmitt 1931).

Voegelin and Kelsen had no contact from the late 1920s until the early 1950s when, on learning that Kelsen was engaged in a review of *The New Science of Politics*, Voegelin wrote to him. A testy correspondence ensued in which he attempted to assure Kelsen, who is not mentioned in the book, that he did not consider his former supervisor to be a target of his attack on Gnosticism. Kelsen was rightly unconvinced.⁶ But the question remains why he put so much effort into responding to Voegelin, a question only sharpened by the fact that he could not bring himself to publish his reflections.

The Hans-Kelsen Institut in Vienna, which oversaw the publication of Kelsen’s extended response, suggests that the answer may lie in the fact that Kelsen defended Marx’s critique of religion against Voegelin’s charge that Marxism is a secular religion and that it was politically risky to publish such a defense in the McCarthy era (Kelsen 2017 [1962], xii). In a detailed account of this saga, Bjørn Thomassen rejects this hypothesis. He diagnoses Kelsen’s bid to take down Voegelin as due to the offense of the *Doktorvater*—the evocative German term for supervisor—at the revolt of a former student. And he argues that Kelsen’s reluctance to publish

6 For an account of their exchange, see Olechowski (2020, 848–852). Olechowski also provides a summary of Kelsen’s critique of Voegelin on pp. 840–848. On p. 842 he makes the lapidary claim that Voegelin “can only with difficulty be called Kelsen’s ‘student’” [“nur schwerlich als sein ‘Schüler’ bezeichnet werden kann”] which gives the lie to Voegelin’s claim that for him Kelsen was the “more attractive teacher” than the co-supervisor of his thesis, Othmar Spann, whose social theory supported fascism, though of the more radical Nazi version, and who with Voegelin was prominent in Black Vienna. See Wasserman (2014).

was evidence of his inability to confront fully Voegelin's critique of Gnosticism (Thomassen 2014). I doubt that the first claim can be right since Kelsen had ignored Voegelin for many years. But the second claim raises the puzzle stated by Lilla. Was Voegelin an obsessive amateur collector of myths to no point or is there something valuable to be gleaned from his writings about the problems of modernity?

Kelsen thought that there is a point. But he saw in it exactly the danger posed in the glimpses of it that appear from time to time in Voegelin's Casaubon-like lists of his snapshots of positions, a methodology he adopted throughout his career. These glimpses amount to dog whistles that his disciples can then make audible for their audiences. Indeed, one reason why Kelsen may have not seen an urgent need to publish his initial response to Voegelin, or his rather inflated revision of it, is that he had deftly set out his main observations in 1955 in "Foundations of Democracy," a 101-page article which took up the whole of one issue of *Ethics*, a leading philosophy journal (Kelsen 1955). Perhaps because Voegelin's name is to be found only in the footnotes, this article seems to have escaped the attention of many commentators despite the fact that Kelsen devotes several pages close to the beginning to a sustained critique of "the new science of politics" (Kelsen 1955, 6–14).

Kelsen begins by identifying the central problem of politics after the war as the threat posed by Soviet communism, because it fights "the democratic idea under the guise of democracy" in presenting the dictatorship of the Party as the rule of the proletariat. Soon into the article, however, he turns to an analysis of a "quite similar pattern of thought," using as the exemplar *The New Science of Politics*. He notes that the book contains the distinction between an "elemental" and an "existential" type of representation. He then sets out the point of the distinction. The elemental type, i.e., the institutions of representative democracy, is merely formal, lacking any "substance." "Substance" can only be delivered by the existential type, which Kelsen disinters from Voegelin's obfuscations as amounting to the equivalent of a one-party state in which the leader represents the "whole society," and which is identified with the state to the extent that the 'state' as a concept is not present in Voegelin's reflections. It is this bond between leader and the people that Voegelin designates as "existential," one which is corrupted if it is mediated by the institutions of representative democracy. Kelsen argues that the bond is "fascistic" in nature, though he recognizes that this is an implication rather than anything Voegelin directly states (Kelsen 1955, 6–14). And he makes it clear in a footnote that in his view there is no distance between Schmitt's explicitly fascist theory of democracy advanced in late Weimar and Voegelin's "science of politics" (Kelsen 1955, 32, fn. 49). Such a theory holds that only a strong leader unfettered by institutional constraints, i.e., a dictator, can existentially represent the group that counts authentically as "the people."

Given this devastating analysis of Voegelin's political position in a leading journal, there was little need to burden the English world with more, other than Kelsen's sense that Voegelin's line of thought resonated with wider trends in scholarship which he wished to highlight. But there is something to be said for Kelsen's relentless focus in the extended critiques on some of the thinkers who figure in Voegelin's cast of characters. For Kelsen exposes not so much the amateurish nature of Voegelin's ventures into the history of political thought as that they are distortions, contrived to fit his cast of characters into the procrustean bed of his tendentious understanding of Gnosticism.

Kelsen concluded the first draft of his response to Voegelin by noting the apparent irony in the fact that his closing observation in *The New Science of Politics* is that "there is a glimmer of hope" in that "the American and English democracies which most solidly in their institutions represent the truth of the soul, are at the same time existentially the strongest powers" (Kelsen 2004 [1954], 108; Voegelin 1987 [1952], 189). Kelsen failed to quote Voegelin's next line: "But it will require all our efforts to kindle this glimmer into a flame by repressing Gnostic corruption and restoring the forces of civilization" (Voegelin 1987 [1952], 189). He did, however, comment on the implicit claim in it, saying that "[t]his is the—quite contradictory—truth of gnosticism as to the nature of modernity. It is in the end Voegelin's gnostic dream" (Kelsen 2004 [1954], 108). But given this contradiction, what was Voegelin really up to?

13.4 By Their Fruits Ye Shall Know Them

Voegelin makes a brief appearance in a book by someone who, unlike Voegelin, has a credible claim to be "one of the most original and influential philosophers of our time," Harvard political theorist Judith Shklar's *After Utopia: The Decline of Political Faith* (2020 [1957]). As the title suggests, this pessimistic work argued that political theory was at an impasse. On the one hand, it had to contend with "two enemies of the Enlightenment"—romanticism and what she called "social theology"; a contention made difficult by the fact that these ideologies are "not primarily political philosophies" (Shklar 2020 [1957], 270). On the other hand, because they are forces within politics, "they must be dealt with by some more comprehensive ... political philosophy." But the diagnosis of her book was that "no such theoretical renovation seems at all possible" because of the "absence of a satisfactory secular social philosophy" (Shklar 2020 [1957], 270).

Among Shklar's examples of social theology was Voegelin's claim in *The New Science of Politics* that "all modern political thought, liberal, socialist, and totalitarian, is descended from a religious heresy, the 'Gnosticism' of Joachim of Floris, and

that in their general character *all* political theories since Hobbes are the same, since all are secular religions” (Shklar 2020 [1957], 210; her emphasis). She pointed out that “this view is peculiar to Erich Voegelin, and represents an extreme version of the theory of social theology.”⁷

Shklar’s scare quotes around “Gnosticism” and her use of “peculiar” signal to the reader that Voegelin’s idea of Gnosticism is an invention that he has projected back onto a figure to give some historical heft to his sweeping conjectures. But while Shklar clearly had little respect for the intellectual credentials of his position, she mentioned it because it was an example of her more general claim that it is “clear to all social theologians that the real conflict is a war between totalitarianism and Christianity, with no alternatives of a purely secular kind” (Shklar 2020 [1957], 210).⁸

The point about “no alternatives” is important, as by default liberalism is not an alternative. On this view, liberalism is the real enemy for it has established itself as the secular religion of the West in the late twentieth century, just as Nazism established itself in Germany in 1933. Moreover, liberalism is seen as in a way even more pernicious than explicitly authoritarian ideologies. It claims not only to be secular, but also neutral in that the only good it promotes is the good of a stable political order in which individuals can pursue their own conceptions of the good on peaceful terms. But, the rightwing critics of liberalism argue, liberalism’s relegation of the pursuit of the good to the private sphere of individual life is deeply corrosive of many conceptions of the good, especially the authentically religious ones, and through such corrosion it promotes its own version of the millennium at the expense of all others.

There is a curious trajectory to this view. The central intellectual figure in *The New Science of Politics* is Joachim of Fiore, as he is better known, a twelfth-century theologian and Calabrian abbot who is central to a stream of scholarship on the relationship between religion and politics driven by a sense that modernity is

7 “Erich” is the original spelling, which Voegelin changed when he moved to the US. Kelsen gets only a bare mention in this work (Shklar 2020 [1957], 239–240). Shklar points out that he with Bertrand Russell argued that a “belief in absolutes leads to authoritarianism,” but she denies that any such link can be established.

8 Voegelin of course would have denied all of this. He says that he has in his files documents labeling him “a Communist, Fascist, a National Socialist, an old liberal, a new liberal, a Jew, a Catholic, a Protestant, a Platonist, a neo-Augustinian, a Thomist, and of course a Hegelian—not to forget that I was supposedly strongly influenced by Huey Long” (Voegelin 2011 [1973], 74). (Long was the American politician who was either revered as a populist hero who took up leftwing causes or despised as a ruthless demagogue.) But he regards these labels as just proof of the fact that he was toiling away as a “scientist”—apparently of the same stature as Max Weber—who “honestly wants to explore the structure of social reality” (Voegelin 2011 [1973], 73–74).

not characterized by a secularization of politics, but by a translation of theological themes into secular ones.⁹ Joachim is important to this stream because his theology postulated a meaning to history, an end state in which some apocalyptic event would usher in an age in which God's absence from the world would be remedied on earth in a triumph of righteousness (McGinn 2018).

The triumphs of Communism, Fascism, and then Nazism spurred a wave of interest in the interwar period and thereafter in Joachim on the basis that these are different secularized or pseudo-secularized versions of his Gnosticism, his understanding of the consequences of God's absence from and hence the presence of evil in material worldly existence. Matters are complicated by the fact that there are two different interpretations of Joachim's thought, as stated clearly in one of the most important works from this period, Karl Löwith's 1949 *Meaning in History*:

Joachim's expectation of a new age of 'plenitude' could have two opposite effects: it could strengthen the austerity of a spiritual life over against the worldliness of the church, and this was, of course his intention; but it could also encourage the striving for new historical realizations, and this was the remote result of his prophecy of a new revelation. (Löwith 1949, 159)

The second effect, Löwith said, "reappeared as a Third International and a third *Reich*, inaugurated by a *dux* or a *Führer* who was acclaimed as a saviour and greeted by millions with *Heil!*" (Löwith 1949, 159).

Now what Gnosticism is, whether Joachim was a gnostic, if so what his Gnosticism amounted to, and what actual influence his ideas had when they were re-discovered by political movements in the twentieth century, are all matters of scholarly debate. And as Kelsen pointed out at some length in his longer monograph response to *The New Science of Politics*, Voegelin's historical claims about Joachim are both inaccurate and question-begging (2017 [1962], ch. 3) Be this as it may, my concern here is with which interpretation, however sound as a matter of historical accuracy, is taken as action-guiding in our political condition—the political condition of the second half of the twentieth century until the present.

In this regard, Voegelin deliberately downplayed the first interpretation according to which the mystical elements of Joachim's doctrine, the insistence on an openness to transcendence, is meant to guide a retreat from politics and other worldly concerns. That raises the question what Voegelin was advocating. Was he simply, like Löwith, diagnosing the theological roots of the second effect

⁹ For an excellent account of the debate in the twentieth century on this relationship, see the biography of Jacob Taubes (Muller 2022). Taubes engaged with Voegelin, as he did with many others, but his view was that the apocalypse would deliver a secular millennium of a leftist hue.

whereby Christian mysticism transubstantiates, as it were, into totalitarianisms of various kinds? Here we can note that Voegelin had no formal church affiliation and did not ever proclaim his faith, which requires his disciples to trawl through his life and work for indications that he was a true believer (Sandoz 2012).¹⁰ In addition, there is the factor we saw Kelsen recognize—that Voegelin ends *The New Science of Politics* by asserting that “the American and English democracies which most solidly in their institutions represent the truth of the soul, are at the same time existentially the strongest powers” (Kelsen 2004 [1954], 108; Voegelin 1987 [1952], 189). As Kelsen noted, this assertion contradicted much else in Voegelin’s diagnosis of the problems of modernity. Above all that raises both the question of the content of what Kelsen called Voegelin’s “gnostic dream” and what resources he himself had to counter it (Kelsen 2004 [1954], 108).

Voegelin’s disciples have an answer to both questions. They offer what I have called “liberalism with a minus sign” to highlight that such positions, while they do not advocate a substantive political theory, do rule out certain candidates, in particular, the family of normative political and legal theories that argue that there are intrinsic qualities of legal order that make government under the rule of law tend to serve the values associated with liberal democracy (Dyzenhaus 2016, 503–504). They permit any substance as long as it built on the friend-enemy distinction that Schmitt urged in *The Concept of the Political* is the distinction to which “political actions and motives can be reduced” (Schmitt 1996 [1932], 26).¹¹

Those who put forward such theories need not be theists. And even most of the theists among them are realistic enough to know that the return of theocratic rule according to the dictates of their own militant version of Christianity is not

¹⁰ See also for a rather desperate attempt to show that Voegelin was a true believer, Niemeyer (1995).

¹¹ Among the most prominent Voegelians are John Finnis, Oxford Professor of Philosophy of Law, Adrian Vermeule, Law Professor at Harvard, and Patrick Deneen, Professor of Government at Notre Dame. While there are differences between these thinkers as to the extent to which they wish to abolish the distinction between church and state, and as to how authoritarian they think the state’s imposition of their preferred ideology—a rightwing Catholic political theology—should be, the fact that they embrace liberalism with a minus sign leaves each with no principled basis for contesting any more authoritarian version than the one he happens to hold. Finnis’s disciples in the UK, Canada, Australia and elsewhere and Vermeule in the US have forged a transatlantic “common good” alliance which seeks to appropriate rights talk and constitutionalism in a project to design the state so that the undefined “one who is in charge of the community” can “promulgate” the common good. See the website of “The Common Good Project,” <https://www.law.ox.ac.uk/common-good-project> (last accessed July 9, 2024). Orbán’s Hungary appears to be their model. See Dyzenhaus (2022a).

realizable in practice. So they make a pact with whatever anti-liberal forces share enough of the tenets of their ideology to make for some common ground; and they disguise their hatred of the achievements of liberal democracy under the pretext of saving us from the control of cosmopolitan, rootless elites, a rhetoric with a frightful past.

Their theories thus shape the contours of the political space in which arguments can take place by excluding liberalism. As Schmitt argued in *The Concept of the Political*, only theories can be admitted that make a claim to legitimacy on the basis of a thick conception of the homogeneity of “the people,” one which establishes a bond between the ruler and that proportion of the population, the ruler’s “base” we would these days say, who are considered existentially appropriate members of the political community (Schmitt 1996 [1932]). But here the observation of an eminent scholar of church history is appropriate. “We must always remember that it was his followers, not Joachim himself, who realized the radical potential implied in his thought” and the “abbot himself would have been horrified” (McGinn 2018, 7).

Would Voegelin have been horrified? There is good reason to doubt this. As we have seen, his memoir shows no sign that he thought that there was any rupture in his thought from the period of his participation in Black Vienna to the 1970s. If anything, he asserts the value of his earlier works as a basis for the later ones. In addition, he was one of the inspirations for the intellectual circle around William Buckley and his magazine *National Review*, a circle which laid the basis for the toxic and complex blend of militant Christian conservatism, libertarianism, anti-liberalism, and anti-science ideology that drives the Republican Party in the Trump era (Bogus 2011).

In this light, we can solve the puzzle of how Voegelin could claim that “the American and English democracies which most solidly in their institutions represent the truth of the soul, are at the same time existentially the strongest powers.” Recall that the next sentence reads: “But it will require all our efforts to kindle this glimmer into a flame by repressing Gnostic corruption and restoring the forces of civilization” (Voegelin 1987 [1952], 189). Written in 1951 for the Walgreen Lectures, then revised and published in 1952, these concluding observations of *The New Science of Politics* are made at a time when one could view these two democracies as not only having triumphed over the Nazis, but also as superior in force to the Communist bloc.

More significantly, within the couple of pages that precede the observations and, which appear at first sight oddly tacked onto the body of the book, Voegelin suggests that what makes English democracy distinctive is that it “preserved the institutional culture of aristocratic parliamentism as well as the mores of a Christian commonwealth, now sanctioned as national institutions.” Similarly, while the American Revolution was “strongly affected by the psychology of the enlighten-

ment,” it also “had the good fortune of coming to its close within the institutional and Christian climate of the *ancien régime*” (Voegelin 1987 [1952], 188). These two democracies had not, as the French and German Revolutions had done, brought about “modernity without restraint,” that is, liberalism run amok (Voegelin 1987 [1952], 189).

But these pages are not as oddly tacked on as they might at first appear. As Kelsen discerned, the content of the dream is a have-your-cake-and-eat-it-too one. The substantive homogeneity of a majority Christian and white political community is in place, ruled over by an aristocratic elite which will remain in power because of the long-standing institutional arrangements in which people trust. The pages are then a somewhat disguised wink to the cognoscenti. The majority of the population think that they have—and they trust in—rule by the stable democratic institutions that make up the body politic of “we, the people.” In reality, they are living in the Christian climate of the *ancien régime*—the common good promulgated by their rulers under the guise of democracy.

But since the 1950s, political climate change has been severe. It has become “modernity without restraint,” in the US, the era ushered in by the Civil Rights Movement, the Warren Court, and feminism; in our century, the election of a black President, the Me Too and Defund the Police movements, trans rights, etc. Voegelin’s disciples do not, however, think that they are compelled to “veer towards Mussolini” since they have learned a lesson about democracy that was unavailable to the likes of Voegelin and Schmitt and to him in the 1930s.

Their main teacher is Victor Orbán and he has taught that the political space need not exclude democracy as long as the version of democracy is “illiberal democracy.” This is a democracy whose institutions have been hollowed out or captured so that, first, the return of the ruler in periodic elections is guaranteed to the extent possible and, second, institutions such as parliaments and the judiciaries are disabled from mediating the promulgation of the common good. There is then, they suppose, no need to put in place fascism because illiberal democracy suffices (Dyzenhaus 2022a).

That leaves my final question: Was Shklar right when she claimed that “no such theoretical renovation seems at all possible” because of the “absence of a satisfactory secular social philosophy?” (Shklar 2020 [1957], 271).

13.5 Kelsen’s Answer

In my view, renovation is possible if one returns to some classics of political philosophy in, broadly speaking, the empiricist tradition, as I have tried to show in a book that traces “the long arc of legality”—an arc of thought about the legitimate

authority of the modern legal state—that stretches from Hobbes to Kelsen (Dyzenhaus 2022b). Now Kelsen did not consider his intellectual roots to lie in Hobbes's legal theory. But in the debate with Voegelin he came to Hobbes's defense in an intriguing way.

Voegelin closes *The New Science of Politics* with an analysis of Hobbes's *Leviathan*. He says, accurately, that Hobbes saw the need to found the state on the basis of a civil faith, but claims that he did so by “throwing out anthropological and soteriological truth,” that is, the truth of the correct philosophical conception of the social individual and the truth offered by religious doctrines of salvation. This leads, Voegelin asserts, to the “destruction of the soul” and the “fallacious immanentization of the Christian eschaton” (Voegelin 1987 [1952], 166).

In one of his book-length responses, Kelsen points out that this is a highly partial, one might say dishonest, interpretation of Hobbes (Kelsen 2017 [1962], 85–89).¹² Hobbes explicitly rejected the idea that there could be a final stage of humankind. Rather, he wished to design a modern legal state in which people could live peacefully together despite their very different conceptions of the good and with rulers disabused of the idea that they are able to force belief in any idea of the soul or salvation. This, he realized, would be a fragile achievement, subject to all sorts of perils, and its design had to take into account and remain open to human experience and nothing more in seeking to build a stable and decent society for any given political community.

Hobbes, then, had no aim of creating a world without religion, nor indeed a civil faith beyond one that supposed that careful attention to human capacities to craft institutional solutions to the problem of how to live peacefully together suffices will bear fruit, as long as we remain open to our collective experience as human beings in this world. When it comes to our common lives, all that transcends human experience is more such experience, not anything that is claimed by some dreamer of the absolute to have been revealed to them.

On the account I offer in my recent book, Kelsen updates Hobbes's theory of the modern legal state for the twentieth century and our own (Dyzenhaus 2022b). But that updating requires accepting that one's scientific theory cannot be value neutral. My guess is that Kelsen's commitment to value neutrality is what ultimate-

¹² Voegelin fully shared the view of his disciples that he was a great philosopher. His nauseatingly hubristic *Autobiographical Reflections* is replete with expressions of extreme disdain for the liberal or further left members of the US academy who lacked the knowledge he claimed of the history of thought and could not measure up to his sense of his own brilliance. So great is his disdain that he does not bother to name them. He does at one point accuse Marx of being “an intellectual swindler for the purpose of maintaining an ideology” (Voegelin 2011 [1973], 76). One could well retort with Marx in the Preface to *Capital*: “*De te fabula narratur*”; “of you the tale is told.”

ly held him back from publishing his monograph-length responses to Voegelin. That commitment left him unable fully to explain why his theory rules out from the normative domain of politics and law claims that appeal to anything that transcends shared human experience. By itself that exclusion does not give one a commitment to democracy or to substantive theories of the rule of law or to civil rights. But it opens up the space for debating such commitments, a space which is hard to imagine without democracy, the rule of law, and civil rights. In contrast, Voegelin, Schmitt, and their disciples today are all about giving the authority to some undefined ruler to fill the space of politics with anti-liberal content.

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