Chapter 8

Rethinking Science

As a laboratory investigator, I still held the ill-defined ambition of integrating different kinds of knowing (scientific and hermeneutical), the same goal I had pursued during college. In line with that unmet ambition, I had hoped that beyond the exercise of thinking as a scientist and discovering novel facts, the practice (the actual doing) of biomedical research would also present me with a philosophy of science, perhaps even a metaphysics underpinning scientific insight. While still lodged in laboratory research, I dipped into A. J. Ayer’s *Language, Truth, and Logic* which I found opaque, indeed, unintelligible. The same frustration occurred with Carl Hempel’s *Aspects of Scientific Explanation* and Rudolf Carnap’s *An Introduction to the Philosophy of Science*. I lacked the background, focus, and sustained attention required to comprehend these works. Consequently, my thwarted intent of developing a deepened understanding of the scientific enterprise left me with only the myopic gaze required to design and conduct experiments. Simply doing science absorbed me, although nagging questions occasionally surfaced to remind me of my original aspiration to engage broader intellectual questions.

Unfortunately, I had no one with whom to discuss my unrest. My colleagues, engrossed in laboratory life, seemingly never entertained such thoughts. Maybe they did, but it was not a fashionable topic. And if they had, then the tools available within our limited philosophical expertise could not have approached the deeper issues. For scientists—my mentors and then my peers—the problem of verifying data and examining the foundations of our practice remained confined to the immediate problems of unifying results by
writing papers and grant applications. While vaguely aware of the tectonic shifts occurring in philosophy of science during the 1970s and 1980s, I functioned in an academically insulated world. “Normal science” ensnared me. When I did emerge from the laboratory, I sought a deeper understanding and while writing the critique of immunology, I decided to educate myself about the characterizations of science that seemed to prevail in the various discussions I heard at the Boston Colloquium for the Philosophy of Science.

Stranger in a Strange Land

Few have had the opportunity to switch disciplines within academia. I found the venue at Boston University’s Center for Philosophy and History of Science. The chance to direct the Center was sheer serendipity. I had become interested in the center’s celebrated colloquium, a renowned lecture series that covers all aspects of science studies: history, philosophy and sociology of the natural sciences, mathematics, and the social sciences.1 I first attended its sessions as I began the history of immunology project. I had asked its founding director, Robert (Bob) Cohen, if I might organize a conference around themes resonate with that work. Those proceedings were published, coincident with my first monograph (Tauber 1991d; Tauber and Chernyak 1991). The next year I assumed an informal Associate Director position, and in 1993 Bob retired. The ensuing search for his successor ended in a classic academic deadlock, and I slipped in as the interim director, a position I held for the next seventeen years.2

The primary function and identity of the Center, aside from sponsoring post-doctoral research fellows, consists of its colloquium series. This program had been, and continues to be, a premier forum of science studies that attracts the most prominent scholars in its various sub-disciplines.3 Over the course of

1 Much of the intellectual content, and excellence, of the Colloquium has been captured in Boston Studies in the Philosophy of Science book series that began with Marx Wartovsky (1963). Robert Cohen continued the series, editing more than 150 volumes by the time I assumed Directorship of the Center in 1993. Not all of these books resulted directly from the Colloquium, but a good number did. For a sampling see Cohen and Wartovsky, 1985.
2 While I resigned my hospital positions in 1991 and closed my laboratory in 1995, I continued to practice clinical hematology until 2003. I was tenured in Philosophy in 1998 and retired from Boston University in 2011.
3 For example, among philosophers: Quine, Donald Davidson, Hilary Putnam, David Wiggins, Stanley Cavell, Patrick Suppes, Cora Diamond, David Pears, Arthur Fine, Alasdair MacIntyre, Isabelle Stegners, Abner Shimony, Robert Brandom, Ruth Millikan, Don Howard, Stephen Toulmin, Herbert Dreyfus, Jaakko Hintikka, Alex Rosenberg, Philip Kitcher, Warren Goldfarb, Robert Pippin, John Norton and many others; historians: Janet Browne, Roberts Richards, I. B.
my tenure, I hosted more than 800 speakers, and because I was largely responsible for composing the program each year, I chose the symposia topics and invited individual lecturers. This became a unique education, an opportunity that soon expanded to include having my own views discussed and critiqued by presenting a paper almost every year.

With the directorship, I joined the Department of Philosophy and was tenured five years later. My courses were similarly designed to educate myself and that reflected, as expected, eclectic interests. In fact, I had no specialty. These offerings ranged from the expected philosophies of science, medicine, and biology, but branched out to include American political thought, German idealism, metaphysics, environmentalism, psychoanalysis, philosophies of nature, history, religion, literature, and so on. Of the 32 courses I taught in the College of Arts and Sciences, philosophy of medicine was only offered three times; medical ethics, twice; philosophy of science and history of science, once each, and even if the course name was repeated, each semester was unique in readings and theme. Despite this significant commitment to teaching, by and large, my scholarship dominated the other academic roles I had assumed.

The prospects to learn by teaching and to find guidance for certain technical matters were compelling. However, the most important benefit was to organize the assembled voices of the Colloquium. Hosting those whom I had read with great benefit was a privilege, but few of the presentations provided a direct contribution to my endeavors. Consequently, I often felt that I had enrolled in a tour of intellectual sightseeing. In fact, I stubbornly followed my own interests and made little effort to engage a larger forum. That aloofness provided the independence to pursue my own way. I did so, with a self-confidence originating from I know not where. That I never intended to be a professional philosopher seemed obvious to me. Indeed, I openly declared that I lacked such aspirations. I just wanted a compatible academic home in which to do my work, as I defined it. I am omitting a wide swath of professional development but suffice

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4 I also offered a philosophy of medicine reading course to fourth year medical students, and once a basic immunology course in the Department of Biology. In addition, I supervised several master’s students and two doctoral students, Dinos Meikos and Gal Kober, who wrote dissertations in philosophy of biology under my direction. Also, two doctoral post-doctoral fellows, Eileen Crist and Andrea Grignolio initiated studies of immunology under my guidance.
to note that I secured an understanding of philosophy that allowed me to think about questions bedeviling me since my youth.

I hesitate to describe myself as lonely, however, I found scholarship isolating. I saw others engage socially in ways I did not. They attended conferences, enjoyed regular lunches and dinners together, vigorously corresponded with colleagues, dutifully sat on committees, and participated in university politics. My style and my temperament led me elsewhere. I have come to realize that I represent a type, much like R.C. Collingwood, a well-known and respected Oxford philosopher. He “had a reputation of being a lone wolf. . . . one of those English non-conformists who are fated to create a party of one member” (Toulmin 1974, x). I could identify with Collingwood, who had academic interests in two disciplines, ancient history and philosophy. When I read his acerbic comments about his life among his colleagues, I could only chuckle in amused recognition:

When I took part in these weekly arguments the problems always had to be other people’s problems and the methods of handling them other people’s methods; and that if I tried to raise the problems which I found especially interesting, or to conduct a discussion according to what I thought the right methods, I was met by greater or lesser degree of incomprehension, or by the well-known symptoms of an outraged philosophical conscience. For these experiments very soon taught me what it was important for me to learn: that I must do my own work by myself, and not expect my colleagues in the philosophical profession to give me any help. (Collingwood 1978, 54)

I did not entirely share Collingwood’s experience, for I enjoyed two influential relationships that in many respects oriented my philosophical education. By highlighting them, I am putting aside the numerous stimulating discussions I had with Boston colleagues (Cohen, Sarkar, John Stachel, Abner Shimony, Dan Dahlstrom, and Victor Kestenbaum) and Colloquium speakers, not because they were not useful in developing my own ideas, but few were interested in the issues that focused my own attention.

My most sustained engagement was with Burton Dreben (1927–1999), who joined the Department at the same time I did. Having served Quine (1908–2000) for decades as interlocutor and earnest editor, Dreben was an acknowledged expert of the analytic tradition. Because I was most interested in Wittgenstein, Burt assumed a natural mentorship. I regularly attended his seminars (several devoted to Wittgenstein, as well as others dealing with Frege, Russell,
Austin, Carnap, and Quine) and happily sought his tutelage. These seminars stand out as the highlight of my 17 years in the Department of Philosophy.

Dreben had been described as “a Socratic gadfly” because of his iconoclastic views of philosophy that I found appealing, and for whatever reasons he saw me as worthy of his mentorship (Kurzman 1984). He confirmed my own skepticism about philosophy and by exploring the limits of thought in the analytic tradition he appealed to my sense of philosophical rigor and doubts that “results” as I understood such endpoints were even possible in philosophy. Based on an empirical survey, not some winsome observation, I concurred that I would have to settle for the process of philosophizing, for philosophers seemingly cannot agree whether truth is absolute or relative, whether knowledge is possible or skepticism is correct, whether universals exist or only particulars, whether we have free will or not, whether God exists or not, whether the morally right thing to do is maximize the good or to act in a way that respects non-consequential constraints on action, whether meaning is a matter of representational content or inferential role in discourse, whether to know something you must be aware you know it or not, and so on. In short, they agree on nothing of philosophical moment. Every field of inquiry disputes, but at least in the fields that serve as our paradigms of knowledge, such as the natural sciences and mathematics, one finds nothing like the preposterous proliferation of incompatible positions that is the hallmark of over two thousand years of philosophy (Leiter 2018, 197–78).

Indeed, I found myself engaged in an activity radically different from biomedicine. “Activity” is a rather pale way of describing the utterly strange discourse I hoped to master. After all,

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5 Surveys were taken in 2009 and repeated in 2020 (Bourget and Chalmers 2009; 2014; 2020). How bias might be ascertained is not clear, but those identifying themselves by discipline showed a majority in philosophy of mind, metaphysics, and epistemology among the 32 subjects represented. The results show some consensus (>70% is my cut-off) on a priori knowledge, non-skeptical realism, atheism, and scientific realism (Bourget and Chalmers 2009; 2014). While the 2020 survey showed some changes in alignment with various philosophical positions, I found the most interesting findings concerned which philosophers the respondents “identified with.” The top four were Aristotle, Hume, Kant, and Wittgenstein, followed by my other favorites scattered among the 56 remaining candidates: James (#17), Dewey (#19), and Putnam (#20) were bunched together, while Nietzsche (#24) and Kierkegaard (#36) were surprisingly low in the popularity contest, and then Rorty (#43) had a relatively poor showing. Heidegger at #26 was lower than I expected, but no higher than he deserved (Table 7, Bourget and Chalmers 2020). Again, epistemology, metaphysics, normative ethics, and philosophy of mind were the leading areas of specialization, while philosophy of science ranked #9 and philosophy of biology #22 of the 33 subjects listed (Figure 4, Bourget and Chalmers 2020).
philosophy is the strangest of subjects: it aims at rigor and yet is unable to establish any results; it attempts to deal with the most profound questions and yet constantly finds itself preoccupied with the trivialities of language; and it claims to be of great relevance to rational enquiry and the conduct of life and yet is almost completely ignored. But perhaps what is strangest of all is the passion and intensity with which it is pursued by those who have fallen in its grip. (Pyke 2011, 98; cited by Leiter 2018)

Although receptive to Dreben’s general point of view, I sought someone who would balance his slash and burn attitude with a more constructive approach. That individual was Hilary Putnam (1926–2016), with whom I developed a deep friendship.

Hilary was a towering figure at Harvard, possessing an extraordinary, eclectically intellectual intelligence coupled to a vast philosophical scholarship. He exhibited the essential character of philosophy’s ceaseless movement and assumed that no resting place of inquiry would be found. When accused of too often switching positions, he countered that because intellectual commitments are open to constant self-criticism, philosophy, by its very character, cannot provide final answers. He embraced that caveat about analysis, not as a weakness of philosophy, but embedding its strength. Later we both taught at Tel Aviv University and once when we discussed our respective seminars, he told me how he would frequently amend, and often significantly change, a position he had argued the week before! The futility of finding an end point struck me as obvious, and he seemed so comfortable with his own fluctuating positions that I was reassured that my own vacillations were not necessarily the sign of confusion, but rather constitutive to the process of doing philosophy.

I had first-hand experience of his style. Close to my retirement, I published Science and the Quest for Meaning (Baylor 2009a) that Hilary had read and then critiqued. While generally enthusiastic (he was a most generous man), he criticized me for presenting a position that he had held a decade earlier and not the more recent one he currently preferred (Tauber 2009a, 203–5).6 I rebutted that

6 The matter concerned realist/anti-realist arguments, a discussion that I placed in a lengthy endnote. My “modest” constructivist position was based on an anti-realist stance Hilary disputed. That he found my argument tucked away in small print I find noteworthy. He later published his complaint in Putnam 2012; the specific criticism, 103–08. More generally, I found his notion of “inner realism” particularly conducive to my own views. He had abruptly rejected metaphysical realism in 1976 and while opposing the radical constructivist notion that the world is ontologically dependent on human concepts and values, Putnam argued that the mind imposes a functional conceptual framework upon the world (a position akin to Kant’s):
I was not using *him*, but rather his delineation of a *problem*, one he had discarded but I chose to keep. He seemed somewhat mollified but not quite satisfied. Of course, most do not share Hilary’s flexibility. For instance, I vividly remember how Alasdair MacIntyre stormed off, outraged, when I quoted Levinas, “the best thing about philosophy is that it fails” (Levinas 1986, 22). MacIntyre thought philosophy “advanced” (e.g., Grundman 2018). I do not; it evolves and never achieves closure in the sense of “solving” a problem. I came to this position by witnessing the debates about scientific knowledge and its interpretation and easily applied that lesson to philosophical arguments. The ways science has been characterized over the past century exemplifies this point, as discussed below.

Characterizing Science

During the twentieth century, general characterizations of science fell into three groupings. The first cluster placed science within a general philosophical context, which meant interpreting the methods, products, and intellectual structure of science as part of a comprehensive epistemology. Critiques by Husserl, Whitehead, Heidegger, and John Dewey sought to demonstrate how science framed the modern world in every aspect of human experience and how that presentation distorted (or imperiallyistically trumped) other forms of knowing. These diverse characterizations collectively sought to reclaim an agent-centered understanding of the scientific venture. This was a form of subjectivism conceived within a humanistic framework that may be understood, in the context of the previous discussion, as a reactivated romantic sensibility.

The second set, largely dominated by the logical positivists of the Vienna Circle (but including earlier critics, like Pierre Duhem and Henri Poincaré), developed when philosophical idealism finally sputtered to its end during World War I. Whereas their nineteenth-century forebearers assumed empirical conditions of knowing that were applied from the physical and life sciences to the social sciences as well, these Viennese anti-metaphysicians extended a rigorous scientistic method to traditional philosophical problems writ large (Reichenbach 1951; Hylton 1990; Giere and Richardson 1996; Tait 1997; Rock-
They thus regarded science analytically and attempted to formalize a philosophy of science based on principles of verification and falsification, the nature of observation, theory construction, and the basis of truth claims without consideration of the knowing/interpreting subject. They promoted the scientific enterprise by building on foundations established by August Comte, William Whewell, and John Stuart Mill, and concluded that subjectivity contaminated the pursuit of “positive” or “true” knowledge. Indeed, the distinction of scientific “facts” and corrupting subjective “values” represents the crucial positivist distinction. Shortly before positivism was dismantled in the 1950s and ’60s, Horkheimer, opined, I think fairly,

Today there is almost general agreement that society has lost nothing by the decline of philosophical thinking, for a much more powerful instrument of knowledge has taken its place, namely, modern scientific thought. It is often said that all the problems that philosophy has tried to solve are either meaningless or can be solved by modern experimental methods. In fact, one of the dominant trends in modern philosophy is to hand over to science the work left undone by traditional speculation. Such a trend toward the hypostatization of science characterizes all the schools today called positivist. (Horkheimer 2004, 40)

Those who resisted positivism’s advance (including Horkheimer) or doubted its promises were characterized as suffering from “a failure of nerve” (Husserl 1970, 9). That hubris proved vulnerable (Zammito 2004).

Nineteenth-century positivism was based on four major tenets: 1) nature might be observed without distortion of human cognition that depends on a notion of objectivity that requires a radical separation of observer from observed, so that no subjective values are allowed to play in the gathering and analyzing of data; 2) from data, facts emerge and those facts may be assembled into models and theories that are then tested; 3) reality is integrated, and scienti—

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7 Auguste Comte (1798–1857) authored positivism in the 1830s, a philosophical and political movement that enjoyed widespread influence in the latter half of the nineteenth century (Bourdeau 2021). As much a philosophy of social development as philosophy of science, he maintained “that each of our leading conceptions—each branch of our knowledge—passes successively through three different theoretical conditions: the Theological, or fictitious; the Metaphysical, or abstract; and the Scientific, or positive” (Comte 1853). According to Horkheimer and Adorno, Comte’s formal positivist philosophy followed the Enlightenment trajectory: “To the Enlightenment, that which does not reduce to numbers... modern positivism writes it off as literature” (Horkheimer and Adorno 1993, 7).
Scientific methods can be applied to study all phenomena—physical, organic, psychological, and social by the same objective means; 4) progress characterizes scientific pursuits, and faith in that progression promises evermore comprehensive laws of nature. Accordingly, from facts, determined by objective methods, scientists derive hypotheses that are closely examined by experimentation that then are placed in some ordered construct. Such models are then formalized in predictive theories more successful than previous ones. At least, so it was thought.

Several assumptions in this sequential development deserve attention. The first is that the inductive scheme by which individual empirical observations are generalized “presupposes metaphysics,” namely “an antecedent rationalism,” the first principle that dwells in the deep reaches of science’s philosophical structure (Whitehead 1925, 62). Accordingly, 1) the world is material and ordered; 2) this order may be discerned by detached empirical observation, neutral rational description, and objective analysis; and 3) laws will emerge from this inquiry that will remain inviolable. Why nature corresponds to human mathematical and objective descriptions remains enigmatic, but the empirical product of that method has been highly successful and thus approximates a depiction of the real as truth, and so on. The method “works,” but as David Hume noted with suitable skepticism, why it works is not logically self-apparent.

A second profound metaphysical assumption builds on the lingering Aristotelian notion of natural kinds, where the “thing-hood” of nature’s objects science examines seem self-evident. These entities are assumed to exist as contained within a simple location of placement that in turn depends on a certain understanding of the space-time continuum (Whitehead 1925, 69–70). However, quantum mechanics radically upturned a universe of discrete objects existing in fixed coordinates of space and time. This is important for our discussion, because with a simpler philosophy of physics, the real is effectively localized and captured as objective entities. Such “things,” waiting in nature for human discovery, rested upon what Whitehead called the “Fallacy of Misplaced Concreteness” (ibid., 52). In other words, the abstract descriptions of nature arising from modern science have paradoxically been conceived as concrete realities, when in fact they are constructions of human invention. They are real, but their reality depends on how they have been partitioned from the array in which they exist. Humans apply the partitioning borders of “things” through measurement or definition, which in turn are constructed with human

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8 See chapter 6, footnote 13
tools (machines) and, ultimately, human cognition (ibid., 72). Simply, as Kant had noted earlier, human “understanding” is the lawgiver to Nature, as dictated (and limited) by human “reason.”

Whitehead’s observations draw from a long line of argument about “natural kinds.” Are things that we dissect from the panoply of experience to become individual elements that may be counted, or are they differentiated and acknowledged as items by intrinsic, natural characteristics? Much of contemporary post-positivist comment builds on the understanding that natural categories are imposed, and while used because of their functional utility, their authenticity is always in question. As already noted in the preceding chapter, James argued that the furniture of the world is delineated by human interest and need, in other words, “things” do not arise naturally, but are accounted by making and categorizing choices:

We carve out groups of stars in the heavens, and call them constellations, and the stars patiently suffer us to do so—though if they knew what we were doing, some of them might feel much surprised at the partners we had given them.... What shall we call a thing anyhow? It seems quite arbitrary, for we carve out everything, just as we carve out constellations, to suit our human purposes.... The permanently real things for you are your individual persons. To an anatomist, again, those persons are but organisms, and the real things are the organs. Not the organs, so much as their constituent cells, say the histologists; not the cells, but their molecules, say in turn the chemists. We break the flux of sensible reality into things, then, at our will. We create the subjects of our true as well as of our false propositions. We create the predicates also. Many of the predicates of things express only the relations of the things to us and to our feelings. Such predicates of course are human additions. (James 1987b, 597–98)

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9 “In the seventeenth and eighteenth centuries, the principle of causation had been put into ever more successful use by practicing scientists, at the same time doubt had been cast upon it by philosophers.... Kant, however, argues that a genuine necessary connection between events is required for their objective succession in time, and that the concept of causality in which this connection is expressed is imposed on experience by our own thought as an indispensable condition of its possibility. The human understanding, therefore, is the true lawgiver of nature, and the successes of modern science are due to its conduct of its inquiries in accordance with a plan whose ground lies a priori in the structure of human thought (B xii-xviii)” (Guyer and Wood 1998, 21).

10 Originally posited by Plato in Phaedrus 265d–266a. For contemporary accounts see Campbell, O’Rourke, and Slater, 2011.
Thus things become facts when suitably identified (in the positivist opinion, independent of human value). This positivist view leads to the third critical assumption, one that I would describe as the positivists’ ethos: facts are conceived as independent of value and thereby protected from subjectivity. With this approach, positivists invoked critical distinctions between scientific facts and contaminating non-epistemic values. In short, facts and values resided in split domains. However, this fact/value division proved to be muddled.

The effort to regard facts independent of values did not include the value of objectivity that became the positivists’ cardinal precept. During the nineteenth century, objectivity shifted in meaning as standards were revised to accommodate new methodologies. Data from machines radically replaced the personal report with one written in a neutral voice and a universal perspective. In other words, a report might have been written by anyone given the setting and circumstances of the investigation. And because true knowledge possessed no individualized perspective, a community of observers would warrant the discovery. Agreement on the significance of a finding testified to the veracity of the facts under discussion and then the significance and meaning of the facts might be discussed. In the end, a hypothesis, or even a theory would emerge. Universal accessibility independent of personal bias became the key criteria (Tauber 2009a, 52–54). The singular subjective observation was thus co-witnessed and translated into a shared public finding through the data derived from a machine.

Standardized equipment and techniques universalized scientific practice so that the first-person report could be replaced by the abstract “scientist,” an authority who would leave the human only as a machine among machines. This positivist ideal carried profound implications. Constructed in opposition to the romantic view of the world that privileged the individual’s perspective and subjective experience, positivism denied any cognitive value to personal judgments. Individual experience, positivists maintained, cannot be extrapolated into a scientific description. “Noble,” “good,” “evil” or “beautiful” are human projected qualities of men or events, and while such adjectives may be applied to nature, in doing so a human sentiment is assigned to the phenomenon. In reaction against the Romantics, positivists sought instead to radically objectify nature, banishing all human prejudice from scientific judgment. The total separation of observer from the object of observation—an epistemological ideal—reinforced the positivist disavowal of value as part of the process of observation. One might interpret, but such evaluative judgments had no scientific or objective standing.
Accordingly, these precepts portray the scientist as vanishing, absorbed by her machines. But if one steps back from the persona of the scientist as a social entity and attempt to portray her subsumed beneath the epistemological demands of what Thomas Nagel (1986) calls the “view from nowhere,” a “paradox of scientific subjectivity” emerges (Fox Keller 1994). This posture refers to the ostensible goal of a completely detached observer, one independent of subjective foibles and prejudices, whose conclusions come from “somewhere else.” But pray tell, how is the scientist removed from the interpretation she offers, a process that synthesizes and judges the facts to construct a model or theory? For positivist science, this element was largely ignored. Seemingly, the facts fell into place logically and, putatively, independent of human interpretation. I entered the field after this austere view had been effectively challenged. However, unrepentent Standard View stalwarts defended the positivist standing of facts and the claims of Objectivity and Truth. To this third characterization of science, I now turn.

Catching the Tramp Steamer

While there were stirrings that the gap between science and the humanities was smaller than I thought, I knew science as only a technical exercise based upon a methodology devoid of the subjective. Both Kuhn’s first edition of *The Structure of Scientific Revolutions* (1962) and Polanyi’s *Personal Knowledge* (1958) had been published before I attended college. Although influential and now canonical, they escaped my serious attention, but not my memory of their first trivial encounter in 1965. I distinctly recall a seemingly casual glance at these books belonging to a college classmate, Rick Adler. He had enrolled in a history of science course that included the seminal texts—Kuhn’s *Structure* (1962) and Charles Gillispie’s *The Edge of Objectivity* (1960). I remember picking them off his desk and casually glancing at several pages. We had a short exchange about the course, but Rick’s hesitant interest quelled my own.

We were, unbeknownst to us, amid a revolution that would reject the dominant positivist philosophy of science. Kuhn, Feyerabend (1975), Polanyi, Toulmin (1953) and others promoted non-formalistic accounts of how science was

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11 The canonical text was issued in 1970 and two later editions (1996 and 2012) included general commentaries.

12 Note the date, 1953, of Toulmin’s text that closely follows Quine’s seminal papers that were instrumental in dislodging key tenets of the logical positivist school (Issacson 2004; Tauber 2009a, 92-100). Toulmin’s introductory text emphasized how the contextualization of scien-
performed. The fundamental shift follows two key tenets: first, science does not develop according to some overriding logic or method. Instead, scientific knowledge is a pragmatic product that incorporates creative and interpretive functions. Second, once that constructive door opens, the knowing agent introduces an interplay of epistemic and non-epistemic values that influence knowledge acquisition and its application. These positions built on what Arthur Eddington called, “selective subjectivism,” in which to account for the ways in which the scientist selects the object of study within chosen epistemological constraints (Eddington 1933). But the next generation coupled specific subjective factors (e.g., aesthetic, psychological, political, gender influences) to pragmatic opportunism to erect their sociologically-sensitive constructivism (Zammito 2004). By the time I graduated, these mid-century critics had set the groundwork for a revolution in the ways the scientific enterprise was understood by overturning the descriptions of science based on the ordered, restrictive methods of rational advancement built on positivist precepts. That revision allowed me to again approach my collegiate integrative project, but now on terms that allowed for the bridge-building I originally sought. I find the timing ironic. I didn’t even know Kuhn and company were having a party. I hadn’t been invited. I awoke two decades later to empty bottles and scattered debris. The celebrants were just saying adieu by the time I again engaged my abandoned collegiate interests.

One of the main modernist-postmodernist battles had been fought over the standing of science, when the major conceptual breakthroughs had been made in philosophical (Quine), cognitive (Polanyi), methodological (Feyerabend), and historical (Kuhn) sectors. It seemed as if I had been sitting on the dock mending nets while the boat had taken off for rich fishing grounds. I was left to catch the tramp steamer and proceeded with my own historical/philosophical research with a wary eye and an ear turned to the cacophony.

The seismic reevaluation of science as an objective and neutral pursuit was launched by Kuhn’s *The Structure of Scientific Revolutions*, a work that ignited controversy about the legitimacy of positivist precepts. Prior to 1960, what passed for philosophy and history of science is now called the Standard View. In large measure a hagiography of “scientific method,” “scientific rationality,” “scientific objectivity,” and “scientific progress,” the Standard View portrayed scientific problems largely informed the “answers” provided and thus offered an early declaration of the non-epistemic variables of scientific construction.

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13 Eddington’s argument was extended in his *The Philosophy of Physical Science* (2012, 16–21).
science as logical in its ordered definition of the real. Despite the positivists’ best efforts, when each of these categories was placed under a critical lens, they failed their own cognitive standards. The “idolatry” of science fell to a more circumspect respect. Kuhn led that reappraisal. He argued that the view of the autonomous, rational growth of scientific thinking, that is, of science as logically progressing and possessing universal and unwavering objective criteria to describe nature conflates science’s declared ideal aspirations with the heterogeneous nature of its enterprises. In refutation, Kuhn maintained that scientific change occurred non-incrementally in sudden leaps, or what *Structure* described as “paradigm” shifts. Kuhn’s original notion of paradigm has suffered misapplications, overuse, and conflicting interpretations (Masterman 1970). Yet, he introduced an abiding revisionist view of scientific advance that challenged logical progression and championed a pragmatically derived picture of the world (Hoyningen-Huene 1993).

**Quine**

Despite the undoubted influence of its argument, the ultimate influence of *Structure* rests on its promotion of deeper philosophical arguments and other shifting elements in the study of science. By 1960, the positivist program was already unwinding from within, and Kuhn drew upon a profound philosophical reassessment led by Willard Quine (Friedman 1999; Zammito 2004; Hyl10207). How Quine’s insights have been extended to the foreground of science studies cannot be overemphasized, for he, more than any other figure, unhinged descriptions of scientific practice and its pragmatic logic from formal accounts. Quine’s critique must be understood within the context of the logical positivist movement that arose, in large measure, as a revolt against the idealism that dominated philosophy at the turn of the century (Hylton 1990; Giere and Richardson, 1996; Tait 1997). For these positivists, language became the forum in which to examine science’s philosophy. They maintained that scientific method is the only source of knowledge, and that a statement is meaningful only if it is “scientific,” in other words, empirically verifiable (thus their alternate label, “logical empiricists”).14 Statements alluding to some transcendental reality were regarded as meaningless, since they could not be verified. With such criteria,  

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14 The Vienna Circle were composed of an inner cadre led by Moritz Schlick and then a larger, more diffuse group of philosophers whose respective positions took form in response. For collected papers see Sarkar 1996; for critical comments, see Ayer 1952; 1959; Friedman 1999; Richardson and Uebel 2007; for popular overviews, see Sigmund 2017 and Edmonds 2020.
metaphysics could be ignored. Moreover, the knowledge criteria of science defined *knowledge*, more generally, and thus discourses that failed the standards of empirical investigations were dismissed from analytics altogether.

The Vienna Circle analysis of language pursued both “negative” and “positive” plans. The first sought to dispense with “non-science” (a major focus of concern) by establishing a linguistic conception of analytic truth that would provide an account of the non-empirical character of logico-mathematical knowledge. Without appeal to metaphysical principles or abstract entities (like concepts or ideas), these positivists attempted to establish the *a priori* status of logic and mathematics compatible with radical empiricism by showing the truth of such propositions through logical analysis. Having putatively secured logic and mathematics and having pushed metaphysics aside, they were then freed to pursue the second aspect of their agenda, namely, assess epistemology in the same linguistic manner. Their philosophy thus became the analysis and clarification of meaning with the use of logic and scientific method. Accordingly, language was viewed as a system for solving problems; from another vantage, philosophical problems were characterized as confusions bestowed by language itself, or as Wittgenstein famously noted, “philosophical problems arise when language goes on holiday” (Wittgenstein 1968, 19e). Accordingly, the aim of linguistic analysis was to solve philosophical problems, namely, “to shew the fly the way out of the fly-bottle” (ibid., 103e). These efforts, however, failed. (Note, Wittgenstein would not formally associate with the group given his general suspicions of these efforts.) With this new opening, a spectrum of options ranging over varieties of naturalism, pragmatism, constructivism, and relativism have made their respective claims.

Logical positivism’s failure had many sources, but for our present purposes the issue may be reduced to a single fault: For the Vienna Circle, the key to cognitive significance rested on mutually exclusive criteria, that is, based on logic or on fact. Thus, meaningful statements either were analytic-independent of empirical considerations and reliant on language alone (as Quine wrote, “grounded in meanings independently of matters of fact,” [1980a, 20]). The alternate, synthetic statements, were assertions which were verified or falsified by empirical procedures, in other words, “grounded in fact” (ibid.). Mathematical and logical statements were regarded as analytical (tautologies) and true-by-definition. Such propositions are helpful in organizing cognitively meaningful statements but are not verifiable by examining the world. In contrast, synthetic truths are empirical. Indeed, the demarcations – theory/observation, discovery/verification, fact/value – rested on this more fundamental division between synthetic and analytic statements.
The analytic/synthetic division so understood originates with Kant, who argued in the *Critique of Pure Reason* that sensory experience requires mental (cognitive) synthesis, while analytic statements are tautological and rest within their own internal logic and definition. For instance, the truth of the statement, “All unwedded men are bachelors,” depends solely on the definition of “bachelor” and thus is an analytic statement. “I dropped the ball” is a synthetic statement. Its truth content is assessed by determining whether I in fact dropped a physical sphere that bounces, and if not, whether my statement refers to having failed an assignment or responsibility or some other referent. In short, synthetic judgments require some interpretative, empirical operation and thus are distinguished from analytical statements. Or at least so it seemed.

The so-called analytic-synthetic distinction collapsed under Quine’s critique. He showed that synthetic statements could not be completely separated from analytic elements that supported them. To say that “Caesar crossed the Rubicon” cannot suffice as a synthetic statement because the very meaning and significance of that sentence requires a vast network of supporting facts, definitions, and interpretations, which, in turn, create a *web of beliefs*. Quine argued that theories in their entirety hold empirical significance. On this view, the line demarcating the synthetic from the analytic vanishes. If the distinction itself has no firm footing, then the so-called “two dogmas of empiricism” fail and the putative logical basis of scientific theory constructed by the Vienna Circle comes crashing down. Quine’s critique had broad ramifications, for, if correct, then belief systems (and science is the most easily conceived as such a system) cohere through various kinds of epistemological and linguistic linkages that extend throughout the network of ideas. In other words, scientific theory is a grid with interlocking synthetic and analytic components. Change one, and the entire structure must accommodate as a fully integrated whole.

With the analytical logic of the positivists dismantled, Quine effectively argued that theories are tested as ensembles, not singly, because 1) any scientific statement can be held true if adequate revisions are made elsewhere in the system; and, conversely, 2) no statement is immune to change, since truth claims are made within the context of the whole, and not even analytical statements are free of such adjustment. As Quine wrote, “our statements about the external world face the tribunal of sense experience not individually but only as a corporate body” (Quine [1951] 1980a, 41). Two points deserve emphasis. First, language, and by extension, belief systems including scientific theory, achieve stability by balancing all respective elements within a holistic construction. (This basic holistic notion set the stage for Kuhn’s idea of the para-
The web of beliefs acts as a kind of buffering system for accommodating new elements and bestowing meaning on them by their coordination within the entire system (Quine and Ullian, 1978).

The second element describes the relativity of the process because each system has its own coordinates, its own inner logic, its own weight relative to other systems. This simply means that language fits loosely to the world, and the way words or statements link to the world is arbitrary (invented) and thus indeterminate (open to interpretation or re-definition), except as integrated within some “coordinate system” (Quine 1969a). When this view is applied to science, theory becomes a ‘language’ in which facts are coordinated within an interlocking network of other facts that must accommodate new findings (data) as meaningful within the system-as-a-whole. Quine’s “under-determination thesis” maintains that for any set of facts, linkage lines will be adjusted until the most congruent, predictive, and pragmatically supportive structure is found. In other words, the other facts of that system will adopt the new fact as consistent with the already established ‘meaning’ of the system.

Quine’s deconstruction laid the groundwork for positivism’s dismemberment as a governing theory of scientific practice and development. Following his insight, 1) a logical scheme for scientific evolution could not be formulated; 2) an analytic decipherment of scientific theory could not account for its coherence or utility; and 3) as Kuhn amplified in his own presentation of the “paradigm,” only a seismic adjustment will alter the system’s basic character (further detailed in Zammito 2004; Tauber 2009a, 92-99). Quine placed science under a scrutiny that inaugurated a revolution in characterizing its truth claims and objective methods. Instead of some idealized notion of truth or the singular truth quotient of any singular fact, all the elements of knowledge—facts, hypotheses, theories, the diverse values supporting each, the linguistic structures and metaphors, the larger social and cultural determinants, and so forth—contributed to what he called “a web of beliefs.” Like a web, any alteration of one part signified an adjustment that would either accommodate or reject that component. Once incorporated, all the other supporting elements must adjust to the integration of the new part (Quine [1951] 1980a, 42-3).

In repudiating the “imagined boundary between the analytic and the synthetic,” Quine espoused a “more thorough pragmatism” (Quine [1951] 1980a, 46). The pragmatic, local descriptive alternative he offered maintained that the reality sought by scientists was a metaphysical aspiration, discerned by substituting their linguistic analysis for a traditional metaphysics (Quine 1969b). He argued that we must be satisfied with the picture offered by our investigations
but claim no more (1969a). Truth can only be defined within a particular framework. Neither language nor scientific conceptual schemes mirror nature, and thus assessing the success of any scheme is based on pragmatic criteria. These are adequate for the task at hand, albeit “truth” assumes a modest stance. The process is piecemeal, yet progressive (Quine [1951] 1980b, 78-9; Tauber 2009a, 96-98, 201, n. 10).

Accordingly, the belief system is not dependent on what is really there, but rather on the success with which it works. And it “works” through observation and the hypothetical-deductive method, which then offers a “conceptual scheme” of the real. And conceptual schemes, like frames of reference in relativity theory, serve to provide a perspective. Reality is then only our best theory. So, for Quine, truth can be no more than a product of this pragmatic approach, an approach whose limits we better understand but whose success is beyond any final logical analysis. Yet pragmatic results do arrive and are adjudicated by public identification and abstraction. We are not, as he said, in “cosmic exile,” but approach the real with confidence, albeit conforming to a good measure of skepticism (Quine 1960, 275).

Quine is the key transitional figure between the positivists and the post-positivists who followed them, and in that role, we see an unresolved tension: On the one hand, Quine’s commitment to natural epistemology places him firmly within the realist camp. On the other hand, his epistemology leads to a radical reassessment of science’s putative ability to capture reality in some final fashion:

As an empiricist I continue to think of the conceptual scheme of science as a tool, ultimately, for predicting future experience in the light of past experience. Physical objects are conceptually imported into the situation as convenient intermediaries – not by definition in terms of experience, but simply as irreducible posits comparable, epistemologically, to the gods of Homer. Let me interject that for my part I do, qua lay physicist, believe in

15 In his landmark essay, “Epistemology Naturalized.” Quine makes epistemology a branch of psychology in this famous passage: “Philosophers have rightly despaired of translating everything into observational and logico-mathematical terms. They have despaired of this even when they have not recognized, as the reason for this irreducibility, that the statements largely do not have their private bundles of empirical consequences. And some philosophers have seen in this irreducibility the bankruptcy of epistemology...But I think that at this point it may be more useful to say rather that epistemology still goes on, though in a new setting and a clarified status. Epistemology, or something like it, simply falls into place as a chapter of psychology and hence of natural science” (Quine 1969a, 82).
physical objects and not in Homer’s gods; and I consider it a scientific error to believe otherwise. But in point of epistemological footing the physical objects and the gods differ only in degree and not in kind. Both sorts of entities enter our conception only as cultural posits. The myth of physical objects is epistemologically superior to most in that it has proved more efficacious than other myths as a device for working a manageable structure into the flux of experience. (Quine [1951] 1980a, 44)

These conclusions, in toto, were devastating to any normative account of science. If foundations were disassembled, what remained other than convention, consistency, and consensus?

Quine’s ontological relativism, affording only pragmatic criteria for knowing the world, provided later social constructivists a route to pursue a more radical endpoint. Quine distanced himself from them, much as Kuhn professed hostility for the Kuhnians, who carried his work to an extreme relativism that he similarly had not envisioned. Forgetting that Quine embraced scientific reality as defined by a naturalized epistemology, more specifically, physical science, the irony is self-apparent. After all, his own naturalizing epistemology is strongly supportive of science writ-large, as his later work clearly states (Quine 1995). In many respects, he held a traditional view of the objective as the best approximation of approaching the real. I note this attitude because Quine wears two personae. The first is conferred by the power of his critique of positivism. That endeavor drew from the same tradition of logical analysis that spawned the Vienna Circle, and Quine, trained as a logician, was very much a member of that tradition. Second, he firmly committed to the authority of scientific knowledge, hence his naturalism. The latter position proved to have little impact on later philosophical developments as compared to his dismissal of the logical positivists’ program. That re-appraisal set the course for post-positivist philosophies of science for the next half-century.Ironically, like Kuhn, he could not forecast how his critique would be used by later commentators, who appropriated his ideas in a radical re-formulation.

**The Turn of the Screw**

Kuhn’s *Structure* drew from two powerful pragmatist arguments that developed non-formal notions of science’s logic: 1) Quine maintained that revision of a web of beliefs may be made in any number of ways and that the criteria that guide the choice between different logically and epistemologically possi-
ble revisions are *pragmatic* (Quine [1953] 1980a; 1980b), and 2) Nelson Goodman (1953) argued that justificatory practices have no foundational basis and are, instead, governed by evaluative values that are variously interpreted within the context of successful practice. In other words, the scientific values that guide research (e.g., accuracy, simplicity, coherence, etc.) are themselves derived from fruitful investigative performance and unifying interpretation. Thus, *usefulness* determines their justification where practices and the rules governing them are pragmatically formulated in tandem and their dialectical harmonization serve to justify each.

That Quine, Goodman, and Kuhn were working together at Harvard in the early 1950s is a fascinating confluence of influences, whose impact on later American philosophy can hardly be over-emphasized (Misak 2013; Mladenovic 2017, 155ff.). Yet, I must note, that of the many contributors to the pragmatic inflection of American philosophy, Kuhn stands out as the pivotal figure, for his defrocking the positivist program reached well beyond philosophers’ deliberations with immediate and direct impact in sociology, economics, political science and the humanities.¹⁶ Beyond the notoriety (and more importantly, confusions) of ill-defined concepts of “paradigm” and “incommensurability,” Kuhn undermined the status of what constitutes certain knowledge. Instead of formalistic accounts of logical progression, he emphasized the fluidity of discovery and interpretation. And that orientation impacted all of academia by setting the stage for re-appraisals of the status of *facts* that later critics argued were not so much discovered as constructed. Furthermore, facts do not just coalesce into models and theories but fall into place given a larger scaffolding of what Ludwig Fleck had described as the “thought collective” (Fleck 1979). Fleck’s seminal work inspired Kuhn and an entire generation of social constructivists, by first showing how facts are the product of complex social negotiations of evidentiary findings, where the fact emerges within a collective of contributors who negotiate whether 1) data constitutes a *fact*, and then 2) how the fact is placed within a model or theory. Interpretation thus provides both the datum’s status and its meaning. Data assembly does not occur in a vacuum. Analysis is not controlled by neutral logic. Human interest is always at play.

¹⁶ On the 50th anniversary of *Structure of Scientific Revolutions*’s publication, *The Guardian* noted the pervasive presence of “paradigm shift” in contemporary discussions of organizational change and intellectual progress. “A Google search for it returns more than 10 million hits... and it currently turns up inside no fewer than 18,300 of the books marketed by Amazon. It is also one of the most cited academic books of all time. So, if ever a big idea went viral, this is it” (Naughton 2012).
In the contested arena of modeling evidence, so-called “non-epistemic” factors play a crucial role. These are derived from sources outside the laboratory (social context, historical forces, economic impact, etc.) that impinge on the business of what is researched and how those results are applied. So, the scientist, while consciously aware of fulfilling epistemic standards, does not easily escape less obvious influences originating beyond the laboratory.17 This process, immensely complicated and still not clearly understood, denies the positivist hopes for a “view from nowhere,” where some universal, essentially uncontested neutral research simply pictures the world as it is.

If the rationality underpinning scientific discovery and theory fails any prescribed method, the revisionists argued that scientific investigation was a pragmatic process drawing from various human cognitive and social resources to construct knowledge. When the study of nature and the study of society were perceived as inexorably linked—not only interwoven in a trivial social sense but locked together at their deepest roots—a novel picture of science emerged, one that was hotly contested. As might be expected, when Truth and Reality are at stake, many took note, because this post-positivist orientation radically alters our very confidence in the objective distillation of reality. Instead of capturing the real, the constructivist argues that what depends on how “the what” is seen and why it is being viewed. Her wedge drives at human fallibilities: the irrationality of much of how we think, the hidden prejudice of emotion, the bias conferred by experience, and the recognition of ignorance where we assume knowledge. In this reappraisal, objectivity assumes new contours. Indeed, without a logic of rational development based on objective appraisal, final truth forever retreats, leaving only facts that cohere in the theoretical, technological, and methodological arrangements of the time.

When radical “Kuhnians” extended their constructivist positions to the point of relativizing scientific findings, polite disagreement grew into wide-spread polemics. In a rare display of arcane philosophy spilling into the public sphere, debate over the truth claims of science ignited a far-flung battle over the extent

17 While Newtonian physics has been interpreted as emerging from the distinctive political climate of seventeenth-century British political culture (Jacob 1976; Toulmin 1990), examples drawn from the life sciences are more convincing. For example, studies of the social behavior of apes reveal differences of interpretations that follow gender divisions. Donna Haraway observed that male primatologists generally see aggressive dominance patterns as prominent, while women scientists observe cooperative relationships as framing social ape behavior (Haraway 1989b). Teasing apart the cultural threads pertaining to how racial science supported Nazi ideology is a particularly notorious example of objectivity compromised.
to which scientific facts are constructed, as opposed to discovered. The discussion on both sides was dominated by rhetorical hyperbole, indeed, the vitriol reached hysterical proportions. For example: “There is no goddess, Truth, of whom academics and researchers can regard themselves as priests or devotees” (Heal 1987–1988, 108). The defense responded with impassioned and sometimes strident rebuttal, for instance, “It is downright indecent for one who denigrates the importance or denies the possibility of honest inquiry to make his living as an academic” (Haack 1996, 60). Accordingly, one would justifiably banish such “cultural garbage” propounded by academic “slobs” and their collective “gangs” (Bunge 1996, 110; 96; 97). Taking the sole proprietorship on honesty could not foster discussion, much less a resolution (Ross 1996). I watched in amazement and then drew my own conclusions (Tauber 2009, 133ff.).

Among the more outrageous claims, a significant core validity remains (Ross 1996; Labinger and Collins 2001). For me, the Science Wars catalyzed a reassessment of the legitimate claims of different ways of knowing, now with a more circumspect view of objectivity and the subjectivity that played its own role in the creation of knowledge. How science draws on domains of imagination beyond logic and radical objectivity to achieve its insights draws from a hermeneutical perspective and with that hybrid, I saw a bridge between what I had previously thought were two diametrically opposed ways of thinking. Instead of dichotomy, with a deepened understanding of the larger historical and philosophical context in which knowledge evolves, division turned into a continuum, where the objective and subjective elements of creativity find synthesis. A resolution to my youthful quandary began to coalesce around this realization and the scholarship that followed my early immunology writings excavated that idea.

**Science and the Quest for Reality**

I consolidated my thinking about these matters in an unexpected format. In the mid-1990s, shortly after publishing *The Immune Self*, I was asked to edit an anthology describing science as an intellectual and social enterprise. The opportunity to compile *Science and the Quest for Reality* came as a surprise (Tauber 2009, 133ff.).

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18 The debate reached a climax with Alan Sokal’s hoax, in which he argued that gravity was a fiction, one that society had agreed upon but only because of the ideological blinders supporting this concept. His paper was published in a reputable journal (*Social Text*) and later revealed in *Lingua Franca* as a parody (Editors 2000; Sokal 2008; Sokol and Bricmont, 1998).
I did not solicit the project but was invited by the editors of the series, whom I did not know. I suppose they thought me suitable because I led the Center of Philosophy and History of Science, however, they probably did not realize that I did so as an interim appointment. I was no expert, and, in fact, I was no more than an enthusiastic novice in the field. The Center had an excellent library and I spent much of a year reading there. In that sanctuary next to my office, I educated myself in the multiple disciplines comprising science studies, and as I sampled that literature, I discovered a rich articulation of my own ill-formed views. Indeed, the project concretized my ideas about science and forced me, in making editorial decisions, to define my own opinions.

*Quest for Reality* presented a wide swath of topics: the status of scientific realism (constructed), the nature of scientific change (heavily indebted to Kuhn), the boundaries of science (the laboratory walls are porous), and, closest to my over-riding interests, “science and values.” I undertook this venture at the height of the Science Wars, and as mentioned, I found myself in “the center of the road.” There, I firmly held to the standards of objectivity that had ruled my own laboratory while acknowledging (through my historical studies of immunology) the constructivist elements (i.e., the self metaphor) and deeper philosophical commitments (reductionist versus holistic thinking). The book became a declaration of sorts about my own views of science and the reality it presents. My summary (presented in a long Introduction) described general themes about the debunked “Standard View” of science and an Epilogue enunciated lingering romantic themes I would soon develop in my later scholarship.

*Quest for Reality* effectively captured my own sympathies for the post-Kuhnian historians and the post-positivist philosophers of science and thereby became a key reference for much of my later work. Indeed, in many respects, that collection of papers legitimated a major reappraisal of my own laboratory research and the scholarship about immunity that followed. However, the anthology could not cover my full interests and one of these concerned how to interpret science in its historical evolution as a system of ideas.

The birth of modern science and the developments encapsulated in the “scientific revolution” have had seemingly endless commentary, and while interpretations differ, my guiding questions centered on the place of *understanding*

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19 The book joined a series entitled “Main Trends in the Modern World” that included texts devoted to propaganda, the metropolis, the middle class, social movements among other topics.
in the enterprise. For me, understanding bridges the immediate scientific concern, the need for explanation, and the humanistic, referring to the larger sense of meaning and contribution to a coherent view of the world and oneself within it. Beyond the guiding principles of prediction, control, and manipulation of nature, with an eye directed towards the pragmatic, understanding spans the cognitive criteria of an objective-based appraisal of science’s product with a subjective appreciation of scientific findings that fall into a larger personal orbit. This humanistic aspect seemingly had lost its place in the history of science, one that was on prominent display in the sixteenth century and again during the Romantic period (Richards 2001). That issue would become an important focus of my evolving scholarship.

The humanist orientation represented one of the two massive intellectual tributaries that had joined to create modern science. Francis Bacon emphasized the material benefits of empirical investigation. In contrast, the humanists like Erasmus and Montaigne, regarded the scientific refinement of “pure” ideas as a means of attaining intellectual coherence that afforded a better understanding of God’s material creation and His divine laws. For them,

to increase comfort, or to reduce pain, was secondary to the central spiritual goal of Science. Rejecting both in method and spirit Bacon’s vision of humanly fruitful science, Descartes and Newton set out to build mathematical structures, and looked to Science for theological, not technological, dividends. (Toulmin 1990, 105)

The repercussions of this attitude reflected radically differing attitudes about the philosophical import of scientific knowledge. The humanists accepted the latitudes of objectivity. Dispensing with “intellectual exactitude, with its idolization of geometrical proof and certainty” for a “practical modesty, let them live free of anxiety, despite uncertainty, ambiguity, and pluralism” (Toulmin 1990, 105). In other words, focused on the primacy of humane concerns, the practical challenges and concrete problems of everyday life, they rejected “the rationalist move of decontextualizing the problems of science” (ibid., 80).

In contrast, Descartes and his followers developed a novel strategy by pursuing a philosophy based upon universal abstractions (and the geometric method) to establish irrefutable knowledge that adhered to standards of logical

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20 For difference between prediction and understanding as the primary aim of science, see Toulmin 1960.
proof. This is the origins of the “scientific method” in which twentieth-century positivists followed the same decontextualized objects of study, which were subject to examination by rationally validated methods that were in turn underwritten by demonstrable, logical arguments. With this observation, Toulmin hit on the focal point of my own philosophical concerns. In contrasting these two points of view, he highlighted the dual origins of modernity: the humanist sixteenth century, grounded in classical literature, the prosaic world of law, politics, religion, and medicine coupled to a scientific universalism rooted in seventeenth century natural philosophy.

I was to treat these two perspectives, seemingly in perpetual conflict, ten years later in *Science and the Quest for Meaning* (2009), in terms of the romantic reaction against the positivism of its own period (summarized below in chapter 12). But whether examined as did Toulmin in the early modern period or in later times, a constant theme is reiterated, namely the status of certainty and how uncertainty serves to organize the philosophical infrastructure of science.

In early modernity, the humanists were skeptical of religious dogma and the certainty in which it was carried. They argued for temperance and toleration, they held practical doubt about the value of “theory” for human experience, and most saliently for our discussion, the limitations of reaching unquestioned Truth or unqualified certainty in theology, natural philosophy, metaphysics, medicine, or ethics (Toulmin 1990, 24–28). This skepticism was challenged by the scientific revolution, and more particularly by the philosophy elaborated by Descartes: opinion/argument yielded to proof and thus formal logic trumped rhetoric, general principles were sought to offer some uniformity to the messiness of the particular, so abstraction displaced the case-by-case adjudication of concrete diversity, and, correspondingly, timelessness, in the form of the permanent displaced the transitory as the object of study. In short, the ambiguities of uncertainty were philosophically countered by a philosophy designed to attain certainty or unambiguously knowledge.22

As discussed in later chapters, this search for certainty was effectively thwarted not only because of the constitutive complex character of nature but

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21 Objective stringency becomes a corollary requirement for such a system (Daston and Galison 2011; Daston and Lunbeck 2011).

22 “No formalism can interpret itself; No system can validate itself; No theory can exemplify itself; No formal language can predetermine its own meanings; No science can forecast just what technology will prove of human value” (Toulmin 1990, 31–35). Toulmin interprets the Quest for Certainty as a reaction to the social and political uncertainties generated by the catastrophe of the Thirty Years War, 1618–1638.
for good philosophical reasons as well (Toulmin 1990, 105). I will not further delve into this nest of issues here and this cursory review must serve as a preamble for describing how Descartes’s efforts directed at rebutting skepticism evolved into the positivism of the nineteenth and twentieth centuries that further developed his quest for certainty. For now, suffice to note that the fate of this pursuit ties together not only science’s own pragmatic aims, but has also effectively parsed the differing ways of knowing that so befuddled me. Sorting out that dilemma eventually opened the door to contemporary science studies whose refractions of scientific method and theory construction profoundly impacted my understanding of the scientific endeavor. Science and the Quest for Reality became a waystation on that road.

I continued to examine the central theme of post-Kuhnian science studies which asserts that despite the appeals of neutrality and objectivity of the individual knower, subtle, subjective elements remain at play in analysis. Simply, interpretation is integral to the scientific enterprise and thus personal factors cannot be purged. And here, at the juncture of objectivity and the panoply of subjective factors influencing interpretation I found the synthesis that I sought since I first appreciated the tension in science pitted against hermeneutics. Accepting this general standpoint, I examined philosophical issues pertinent to the knowing agent that arose during the Romantic period. It was then that the oppositions evident at the objective-subjective interface were most prominently debated. In the romantics’ response, I discovered resonances in contemporary science studies that offered an enriched understanding of positivism’s replacement with constructivist models.

A resolution beckoned. The alternatives that had remained in waiting eventually declared their rightful position in the hierarchy of my thought. My “re-education” led to far-ranging repercussions. A revised view of the subjective-objective division emerged once I achieved success as a biomedical researcher and reconsidered my original imbroglio from a far different vantage point. I found that the rigid contrasts bandied about were less dichotomous than originally presented, for the oppositions that had guided my professional life were exaggerated and untenable. With these new understandings, I built the bridges that would connect the oppositions that had so belabored my conflicted identifications. Before turning to that matter, I offer a brief outline of my philosophical reconsiderations about science, a commentary drawn from both personal experience and critical re-evaluations, whose pragmatic orientation provides a hard-won equipoise.
REFLECTIONS

With my writings on immunology, a new self-consciousness slowly developed. Doing “normal science” (Kuhn’s term for the work of ordinary investigations), I had no reason to question the standards organizing the methods employed and the strategies adopted. No meta-views of the performance or theory were required. Sounding the deeper intellectual currents was utterly irrelevant. After all, the myopic view of the bench scientist requires no such perspective. And with good reason. Scientists work within a research tradition that has been proven highly successful in yielding degrees of certainty unobtainable with other ways of thinking. Why fix what isn’t broken? Indeed, to become “theoretical” would interfere with the daily business of laboratory experimentation. Science as practiced is an epistemic affair, and I, like all the successful investigators I knew, became absorbed by the doing.

This commonplace attitude, unadorned by theory and pragmatic in practice, suffices for research scientists as a legitimate description of how they work. The ordinary attitude is that empirical data adjudicates hypotheses and while interpretation may be disputed, the investigative process depends on the gathering of factual material. I first learned this basic tenet as a child from Sergeant Joe Friday of the 1950s television show, Dragnet. He would interrupt a rambling witness and direct him, “Just the facts sir, just the facts.” He then would draw the proper conclusions and apprehend the criminal (Dragnet 2022). As discussed, such a simplistic schema misrepresents the complexity of how facts are constituted and the latitude of interpretation that may arise. Interpretation may be prejudiced by “extra-curricular” influences (e.g., Eddington’s promotion of Einstein’s relativity theory; tobacco-sponsored research disputing toxic effects of smoking; Gregory Mendel’s manipulation of genetic data; Pasteur’s tribunal that rejected spontaneous generation) and the rhetorical battles of disputed conclusions (Strevens 2020, 41 ff.).

However, these circumspect considerations are not the mettle of the apprenticeship in which I enrolled, nor, for that matter, is post-Kuhnian philosophy of science integral to the education of students in the natural sciences. Being critical and objective are, of course, cardinal precepts, but the discrepancy between the investigative ideal and the realities of scientific discourse and competition belie the positivist Standard View.

For me, the gulf between the laboratory and post-positivist commentary on science was highlighted in an encounter during the early 1990s, shortly after I had joined the Department of Philosophy. In conversation with Steven Wein-
berg, the Nobel laureate (physics 1979), he asked (in a somewhat inebriated state), “What the hell has philosophy ever done for science?” I responded, “Not much. But it’s not about science, it’s about philosophy.” I probably was a bit intoxicated myself, so the discussion didn’t assume any intellectual traction, but basically my off-hand response summarized my view at the time, one developed before I wrote my critical studies of immunology. That judgment was based on what I would now call “textbook philosophy of science,” which deals with verification theories; falsification; nature of laws and theories; types of scientific reasoning (hypotheses, deduction, induction, probabilistic); search for truth (indirect tests, auxiliary hypotheses, coping with empirical findings); realism/antirealism; constructivism and the various discussions spawned by Kuhn’s revolt against the Standard View. No doubt, philosophy of science so regarded had become a central focus of contemporary epistemology, and to the extent that it served as the context of my own work, highly pertinent. However, I could see little impact of that body of work on the practice of science.23

I would answer Weinberg differently now. Although authors are not necessarily the best judge of their work, I regard my writings on immunology as contributing to both philosophy and science. Regarding the science of immunology, I have offered a revision of research goals and a reformulation of immunology’s basic theory. This seems to me to be doing science with a very practical impact. If my reoriented perspective takes hold, the science will change dramatically. I take no personal credit for this shift, if it occurs, because I am joining a cadre of scientists who hold similar views and are doing the experimentation that is pushing this program forward. Whether it succeeds is another matter, altogether. In the meantime, I have satisfaction in still participating in the scientific discourse.

And regarding philosophy, I have identified basic issues that deserve further attention and opened the science to philosophical scrutiny. Descriptions of extensive metaphorical thinking are relevant to philosophy of science more generally, and my ideas about cognition, information, and definitions of individuals are pertinent to other areas of biology and psychology. Philosophy of ecology, a fledgling discipline would profit as well. But I recognize that this immunology scholarship is in its infancy and has had limited appeal for the larger philosophy community. Even philosophers of biology have remained

23 A recent study has attempted to quantify the impact of philosophy of science on the natural and social sciences. The method of using a citation model is approximate, at best, but nevertheless, evidence is presented that philosophers are not ignored, albeit their impact seems minor (Khelfaoui et al, 2021).
outside my small interactive circle. From this perspective, the impact of my writings probably has a more immediate influence on immunologists. The reason is simple: few philosophers have expertise in immunology and thus find my writings impenetrable. The scientists at least know what I am arguing about.

Academic silos stand tall and well-defended. For instance, philosophers of physics assume sophisticated knowledge of relativity theory or quantum mechanics, and when they engage their uninformed colleagues, they could be speaking Chinese and the difference in comprehension for the non-specialist would be negligible. Such segregation was reinforced as I acquainted myself with the philosophy of biology literature. The practitioners of this subdivision were predominantly concerned with evolutionary biology and closely related areas. Of those philosophers working in this specialty, the best ones were intimately knowledgeable of the science, had spent time in laboratories devoted to the subject, and wrote papers that were explicitly concerned with examining data, the models in which they were placed, and the scientific conclusions drawn. The topics were technical and firmly set in the practical problems of group selection, species, taxonomy, evolutionary mechanisms, adaptation, altruism, and to a limited extent the nature of genes, definition of function, and criteria of individuals as pertinent to evolutionary dynamics. Oftentimes, I thought their papers, published in philosophy of science journals, could have easily appeared in scientific publications instead.\footnote{My impressions have been confirmed: other areas of biology (development, ecology, microbiology, cognitive science, genetics, neuroscience, sociobiology, etc.) comprise less than half of the subject matter of published work in philosophy of biology, and much of that commentary was considered in light of issues in evolutionary biology. This distribution had little correspondence to the actual research conducted in biology (Pradeu 2017).}

Scholars addressing general questions such as the nature of biological laws, causation, complexity, reductionism, and biological models reflected different sets of interests.

In short, philosophy of science is a highly specialized field, so given the character of the discipline, I accepted working within a small circle of scholars.\footnote{Segregation is not isolation and I have had the opportunity to work with several research immunologists, who were interested in immune theory, most notably, Frank Austen, Irun Cohen, Melvin Cohn, Antonio Coutinho, Robert Schwartz, and Nelson Vaz; and; theorists: John Stewart, Francesco Varela, Zvi Grossman; historians: Andrea Grignolio, Peter Keating, Ilana Löwy, Ann-Marie Moulin; Arthur Silverstein, and Thomas Söderqvist; philosophers: Alex Rosenberg, Thomas Pradeu, Henri Atlan, and Kenneth Schaffner; and direct collaborations with Leon Chernyak, Eileen Crist, Scott Gilbert, Scott Podolsky, Sahotra Sarkar, and Bartlomiej Swiatczak.} That of course had consequences. For example, at a faculty meeting in which we discussed the prospect of adding a philosopher of biology to the staff,
a senior colleague, Jaakko Hintikka, stated we lacked such expertise because I did not qualify. His view of the field centered on what he saw as the province of evolutionary biology, and having no expertise himself, his impressions were his sole basis for judgment. I stormed out of the room in frustration and disgust.

On reflection, Hintikka’s opinion cannot be easily dismissed. He sensed that my concerns were not entirely oriented to elucidating the infrastructure of immune theory; he probably also intuited that my philosophical interests were broader than might be contained in philosophy of science. Indeed, he was correct. I had a second demanding agenda, one centered on identity, albeit disguised—at least during the early stages of my philosophical excursions. In later chapters I will explain how the agency of knowing and identity, more generally, posed in different formats shifted my science studies from their original focus and with that inflection, my philosophical enterprise took on new complexity. As described below, that redirection had several sources, but the tributary originating in the depiction of reality offered by contemporary science seems to me of special significance.