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TALKING ABOUT TRUTH

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Malachi Haim Hacohen, *Karl Popper – The Formative Years, 1902–1945: Politics and Philosophy in Interwar Vienna* (Cambridge: Cambridge University Press, 2000)

John Kadvany, *Imre Lakatos and the Guises of Reason* (Durham, NC: Duke University Press, 2001)

Steve Fuller, *Thomas Kuhn: A Philosophical History for our Times* (Chicago: University of Chicago Press, 2000)

Science and science studies were on the front lines during the culture wars of the 1990s. On one side were those defending the purity of science, hostile to the contextualizations and deconstructions of scientific practice, strongly supporting the idea that science propels us forward. On the other were the skeptics who sometimes openly disparaged science and at other times more cautiously expressed reservations about the scientific enterprise. Through much of the decade, each side manned the trenches, fired salvos, and, like the Western front in 1917, looked for the decisive opening that didn't come.

Like so much else in the culture wars, the tendency to lock the discussion into two sides left little room for nuance. The subtlety that was genuinely there was largely ignored. Newspapers and news magazines didn't want to hear it. And why should they, when there were plenty of academics and scientists content to blast away at each other merrily? Wars—at least for the partisans—leave little room for complexity. The science wars also underscored something else—rumination on science takes place in the context of larger events. Nervousness about national mores in the long wake of the 1960s brought on the culture wars of the last decade, and battles about science were drawn into the roil. And it is not only the recent past that makes this clear. New work on the history of the philosophy of science underscores the same point.

In the past few years, three important books have appeared which try to assess three mid-century philosophers of science—Karl Popper, Imre Lakatos, and Thomas Kuhn. Each biography discusses scientific epistemology in relation to dramatic world events. Popper, Lakatos, and Kuhn each spun out a defense of science that was free of empiricist cant. Hovering behind their efforts were the great political dramas of the times—the Great Depression, the Nazi menace, Stalinism, and the Cold War.

Yet it is no surprise that these biographies are appearing now. They are a fitting punctuation to the culture wars. And they remind us of the range of ways in which the practice of science can be imagined. We should be grateful for these books. After the recent simplifications and polemics, it is reassuring to see historians trying to do justice to the subject's complexity.

Malachi Haim Hacohen's biography of Karl Popper is first-rate intellectual history. Popper, of course, is famous for his doctrine of falsifiability, the idea that science does not progress by positively proving its hypotheses but by allowing them to be rigorously tested and falsified. The ability to rigorously test theory, for Popper, marked the line between science and non-science. Since you can't really *disprove* that Jesus was the son of God, the idea isn't a scientific one. On the other hand, the Michelson–Morley experiment did undermine the idea that the universe is filled with ether. Consequently, the theory of ether, even though false, was still science. Popper's great originality was to suggest that empirical confirmation is not the core of the scientific enterprise.

Hacohen's book follows Popper from his birth in 1902 to the end of World War II. The thirty-page epilogue is an excellent place to get a bead on Popper's later career. Hacohen is careful with Popper's thought and adds hugely to our knowledge of the philosopher's personal life. The archival digging clears up much that was murky about the Austrian intellectual. There are shrewd observations about the Popper family fortunes which do a far better job of explaining Popper's outsider status in Viennese social circles than the breezier account recently published in David Edmonds and John Eidenow's *Wittgenstein's Poker*. Popper did come from a *haute bourgeoisie* family, but one that apparently suffered deep financial reversals during the inflation of the 1920s. By 1930, he had to scramble for a living. Hacohen also does an excellent job of parsing out Popper's politics. By the end of his life, Popper was often seen as a conservative. How far this extended back, how the Cold War mattered to Popper, and what exactly his relationship was with the Austrian libertarian Friedrich von Hayek—these were just some of the questions lacking serious research. Popper's 1974 autobiography was a start but, of course, such books need to be taken with a grain of salt. Popper, we now know thanks to Hacohen, briefly flirted with communism in 1918, but by the end of the 1920s he thought even the German Social Democratic Party incompetent. Social Democrats, he concluded by the middle of the 1930s, had done much that inadvertently helped bring Hitler to power. Yet despite such criticism, Popper remained committed to a soft, middle-class form of socialism. Into the 1940s, he continued to support liberal social reform, the basic point of view in *The Open Society and its Enemies*. The libertarian Hayek helped Popper find work and a publisher, and appreciated Popper's ideas about science, but the two men did not share the same politics.

Hacohen also has very competent renderings of Popper's major books and essays. Most important, though, Hacohen has the best account to date of Popper's relations with the Vienna Circle of logical positivists. Like Popper, early twentieth-century Viennese philosophers like Otto Neurath and Moritz Schlick devoted themselves to developing a logic of science. Unlike earlier positivists, these central Europeans placed just as much emphasis on the power of formal logic and linguistic analysis as they did on the gathering of empirical evidence. The relations between Popper and the group have never been clear, and some scholarship at least implicitly puts Popper uneasily close to them.[1] Hacohen shows that while key logical positivists—notably Rudolph Carnap and Moritz Schlick—supported Popper's work, and saw it as highly original, Popper was, intellectually and personally, firmly outside the circle. Personally, Popper was too much of a pain in the neck to belong, yet too important to ignore. Popper participated in some key Vienna Circle events of the mid-1930s, which gave the impression that he was closer to the group than he actually was. Hacohen argues forcefully that Popper was intellectually distant from the logical positivists. Popper's philosophy of science, with its famous defense of falsifiability as the cornerstone of science, was meant to reform metaphysics, not overthrow it as the Vienna positivists hoped. Moreover, the Viennese positivists understood logical analysis and empirical experience to be the bases of knowledge. Popper shared their passion for logical analysis but rejected the idea that empirical testing could firmly establish "truth." Popper saw himself "as a heterodox Kantian, and the positivists as precritical philosophers"(p. 209).

John Kadvany has done his own biographical digging, putting together an absolutely riveting story of Imre Lakatos's life and work. As for his life, Lakatos was long known to have been a Hungarian communist who lost the faith in the mid-1950s, made his way to England, studied with Popper, and took up a post at the London School of Economics. Lakatos was a strong anti-communist and a vocal opponent of the student rebellions of the 1960s. As for his work, Lakatos is conventionally understood as having expanded on Popper's notions of scientific research. Lakatos came to believe that Popper's ideas about falsification were not so much wrong as simplistic. Contrary to what Popper had claimed, one refutation, according to Lakatos, did not undermine a whole scientific theory. It took multiple failures for a theory to be jettisoned. A scientific research program, according to Lakatos, had an outer and an inner shell—the former being the

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1 See, for example, Victor Kraft, "Popper and the Vienna Circle," in *The Philosophy of Karl Popper*, ed. Paul Schilpp (Chicago: Open Court, 1974), 185–204; Friedrich Stadler, *Studien zum Wiener Kreis* (Frankfurt: Suhrkamp, 1997).

protective realm that could be criticized and tested for falsifiability, the latter the core beliefs that could not be touched without the whole enterprise collapsing. The outer shell, among other things, protected the central core from attack. Only when the outer shell (the "protective belt" was Lakatos's term) stopped doing its job did the inner essence become vulnerable ("degenerate" was Lakatos's term). A research program could be judged progressive if it kept answering new questions, kept generating new facts.

Kadvany reexamines Lakatos's life in two crucial ways. He provocatively argues that Lakatos never gave up his Hegelian roots, that his key insights into science reflect his continued faith in the philosophy of history he learned in a Budapest intellectual climate where George Lukács's Hegelianized Marxism marked advanced thinking. Lakatos's *Proofs and Refutations: The Logic of Mathematical Discovery*, his 1961 dissertation and his major contribution to the philosophical history of science, was certainly not Marxist, but it was covertly historicist. So, too, were his key essays on the philosophy of science—contributions like "Falsification and the Methodology of Scientific Research Programmes" or "History of Science and its Rational Reconstruction." The Marxist political economy so important to Lukács was gone, but in these essays Lakatos continued to emphasize the omnipresence of error and how science advanced by incorporating error into new theory—Hegel's *Aufhebung*, in other words.

By the time he emerged in Anglo-American philosophical circles in the early 1960s, Lakatos was writing in an Anglo-American analytic style that masked his covert central European historicism. He sounded like Popper but thought—secretly—like Lukács. This historicism, according to Kadvany, reached right to the heart of Lakatos's criticism of Popper. Popper had argued that once it was shown that a theory contradicted the facts, the theory would be jettisoned. This ignored, in Lakatos's estimation, the fact that contradiction was a part of *all* scientific theory. Some debunking experiment was not, in itself, grounds to get rid of anything. Falsifiability was far more complex than Popper imagined. It took a much more complicated rotting of a research program for a theory to fail. Only in retrospect do we think that some key experiment destroyed a weak scientific theory. The owl of Minerva appears at dusk.

Kadvany's second point is biographical—and stunning. Delving into Lakatos's Budapest background, Kadvany found that Lakatos was a far more committed Stalinist in the 1940s than is usually thought.[2] In the most shocking revelation of

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2 Contrast this paragraph, for example, with the biography included in Imre Lakatos and Paul Feyerabend, *For and Against Method: Including Lakatos's Lectures on Scientific Method and the Lakatos–Feyerabend Correspondence*, ed. Matteo Motterlini (Chicago: University of Chicago Press, 1999), 401–5.

the book, it appears that during World War II Lakatos, a resistance cell leader, engineered the suicide of a woman who potentially compromised the group's secrecy. He convinced her to kill herself, in other words, for the good of the cause. People who knew Lakatos at the time described him as "diabolically clever" and "a fanatical Communist who believed the end justified the means" (p. 288). As a student and working in the Ministry of Education after the war, Lakatos helped undermine the independent university system and pull it into the communist orbit. In 1947, the Party sent him to work on Budapest's Eötvös College. He was an aggressive, confrontational activist, "disrupting college activities" (p. 289), helping organize communist students, attacking the record and probity of non-communist college administrators, and ultimately, in 1950, helping force those administrators to resign. He was, Kadvany argues, ruthless and committed to creating a fully fledged Stalinist state. Despite this, he was arrested in the early 1950s as ideologically unreliable, spent a couple of years in a prison camp, and then made his way to England in 1956.

Kadvany does not integrate this biographical material into the flow of the book, unfortunately. The author says that he wants to use these events to show one more way that truth unfolds through history, but readers get no discussion of how Lakatos behaved in England, his values, his treatment of colleagues, his politics. The personal biography of Lakatos, as opposed to the history of ideas, simply stops in 1956. It is well known that Lakatos vehemently opposed the student disruption of university activities in the late 1960s, a reversal of his own role in 1947–8.[3] But it would have helped this truly original and fascinating book to include some discussion of Lakatos's later life.

Steve Fuller, in his *Thomas Kuhn: A Philosophical History for our Times*, has his own take on the relationship between his protagonist's life and work. And none of it is good. For Fuller, Kuhn's famous 1962 book should be understood as an expression of Cold War ideology. *The Structure of Scientific Revolutions* reeks of the questions it does not ask—questions of power, domination, and the role of science in the national security state. Kuhn's take on revolutions is similarly bad for Fuller. There is, Fuller posits, an analogy between scientific and political revolutions. Kuhn, according to Fuller, leaned far too heavily on normal science and far too little on revolutionary science, an emphasis that supposedly reflects his political conservatism. Kuhn's emphasis on normal science and the laboratory also pushed later science studies scholars like Bruno Latour to do phenomenologies of lab practice, leaving unexplored the more political,

3 See "A Letter to the Director of the London School of Economics" (March 1968), in Imre Lakatos, *Mathematics, Science, and Epistemology: Philosophical Papers, vol. 2* (Cambridge: Cambridge University Press, 1978), 247–53.

external social backdrop to science. Kuhn's understanding of scientific practice as autonomous, disconnected from social forces, was disastrous, according to Fuller.

In some major ways, Fuller is certainly correct. Kuhn's uncritical assumption in *The Structure of Scientific Revolutions* that autonomous, self-regulating professionalism is the way science works becomes even more striking when contrasted with the more radical work of J. D. Bernal, the radical physicist and sociologist of science from the 1930s and 1940s. For Kuhn, truth was what the scientific community said it was. His epistemology was a version of philosophical conventionalism. Fuller is sympathetic to Bernal, who brought to center stage the question of what science actually does or does not do to benefit citizens at large, a question completely absent from Kuhn's work. Nor is Fuller wrong to compare Kuhn to Chance, the strange, disconnected observer-protagonist of Jerzy Kosinski's novel *Being There.* (I myself have long compared Kuhn to Andy Warhol, another blank observer whose breakthrough year was 1962.) There was what seems to be a determined effort by Kuhn—throughout his career—to avoid thinking about the critical consequences of his insights, to be calmly looking on from the sidelines without there being anything at stake in the outcome. And Kuhn's equation of autonomous professional communities with the nature of the scientific enterprise becomes more pronounced in his later work.[4] Even as resolute a defender of the social order as Imre Lakatos would deride Kuhn as an "elitist."[5]

Yet despite these insights, this is a frustrating book. There is Fuller's tone. He is a sort of know-it-all, always looking for the least generous interpretation. Kuhn is never just wrong or lacking insight: he is always the "Cold Warrior," a phrase hauled out time and time again by Fuller without a really clear sense of what it means, save that it's clearly bad. This book paints a one-dimensional Kuhn, whose work on the history of science is on a par with some of the worst excesses of the Cold War. Sometimes Fuller engages in gratuitous political slandering, claiming, for example, that for Kuhn "unrestricted" criticism "verges on treason" (p. 177) but presenting no evidence to support this extravagant claim, no evidence that Kuhn thought his statements on science reflected beliefs about the political world, and no discussion of what that key word "unrestricted" actually means in this passage. It does hint, though, at some sort of relationship between Kuhn and

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4 See Thomas Kuhn, *The Road since Structure: Philosophical Essays, 1970–1993*, ed. James Conant and John Haugeland (Chicago: University of Chicago Press, 2000).

5 Imre Lakatos, "The Problem of Appraising Scientific Theories," in *Mathematics, Science, and Epistemology*, 114–16.

McCarthyism.[6] The book, moreover, is sloppy. Fuller sees himself as a polymath, talking about a huge range of intellectual and political trends of the twentieth century. Yet he does not handle all of them with the same level of reliability (the contrast with the scrupulous scholarship of Hacohen and Kadvany is striking). Fuller is utterly romantic about the New Deal. I can't see how he can have actually read Daniel Bell's *The Coming of Post-Industrial Society* and still describe Bell as calling for "a harmonious social order supported by a benevolent technocracy" (p. 240). Fuller argues that Kuhn borrowed from the political science realism of the 1950s but only provides very weak analogical evidence. He often relies on leaps to make a point. He darkly introduces the "Pareto Circle" at Harvard, a set of moderate and conservative social scientists developing alternatives to Marxist social theory in the 1930s and 1940s, the most famous of whom was Talcott Parsons. Fuller wants to connect Kuhn to them, but he has too many sentences like "To be sure, Kuhn never cited Pareto"(p. 168). He tries to make up for this by citing the "intellectual environment" at Harvard. Given the prominence of Pareto, "it is only natural to conclude" that Kuhn's concept of revolution "owed more to Pareto than, say, Marx." Put like this, it's a no-brainer. Who has ever claimed that Kuhn owed anything to Marx? But it also shows Fuller's sleight-of-hand. Why does it *have* to be posed as Pareto versus Marx?

So consumed is Fuller with placing Kuhn in his proper Cold War context that he pushes some of his interpretations into boxes they don't easily fit in. This is a shame, because, as I noted above, his initial impulse to place Kuhn in his own time was a wise one and Fuller makes some useful points. It is too bad that this isn't a more careful book.

All three philosophers painted alternatives to twentieth-century versions of positivism. Popper's neo-Kantianism, Lakatos's neo-Hegelianism, and Kuhn's neo-conventionalism all rejected empirical philosophies of science. At the same time, however, all remained very supportive of scientific endeavor. All three liked science and wanted to put it on a solid footing.

From the perspective of the philosophy of science and its history, these three books can be seen as revealing counter-traditions to mainline scientism. Figures like Ernst Mach and the Vienna Circle in central Europe, or A. J. Ayer in Britain, formed the positivist core about which alternatives were spun. And certainly, the backdrop to each of the three philosophers is their desire for an alternative to positivism of any sort. Yet the collective stories of Popper, Lakatos, and Kuhn can also be seen as challenging the idea that some core positivism defined

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6 Here Fuller's innuendo replaces scholarship. There are other archival papers available that will possibly shed light on these matters, but Fuller, given his penchant for "philosophical" history, did not sully himself with the dust of the archives.

twentieth-century philosophy of science. There was too much outside of that box. Popper, Kuhn, and Lakatos were just part of it. Henri Poincaré, Pierre Duhem, George Canguilhem, and Michael Polanyi were conventionalists of one sort or another, that is, believing that what counted as "truth" depended on what the community of scientists accepted at a given time. None had much faith in the idea that truth was defined by the correlation of propositional statements to some external reality or facts. Thomas Kuhn might be seen, not as articulating something so shockingly new, nor, as Fuller wants, as fundamentally a Cold War apologist, but as a thinker working in a deeper twentieth-century tradition.[7] Recent work on the early years of the Vienna Circle has suggested that neo-Kantianism was far more important than earlier thought, that Poincaré was a source as much as Ernst Mach.[8] Hacohen observes, correctly I believe, that Popper saw himself as the valiant neo-Kantian fighting off the pre-critical philosophers of the Vienna Circle, but Popper might have been wrong. Popper might be seen as arguing for a second neo-Kantian front rather than a sharp pre-critical alternative to positivism. In all sorts of ways, any straightforward history of twentieth-century philosophy of science seems up for grabs. One message of these three biographies is that far more needs to be done to sort out the intellectual history of reflection on science in the twentieth century.

Still, it does remain striking that all three searched out non-positivist understandings of science. However complicated the paths taken by twentieth-century philosophy of science might have been, each of these three philosophers suggested midway points between positivism in all its varieties and recent postmodernism. All three, in other words, filled some space between Ernst Mach and François Lyotard. Popper filled it with the doctrine of falsifiability, arguing that science gets better not by confirming truths but by debunking unsupportable claims. Lakatos extended that but, if Kadvany is to be believed, the Hungarian also snuck in the historicist faith-in-the-long-run. Truth emerges as history unfolds. Kuhn voiced a version of conventionalism, suggesting that science cannot support itself as the empiricist bromides claimed, but that scientists do their jobs pretty well nonetheless.

Moreover, as these books make abundantly clear, the musings of Popper, Lakatos, and Kuhn all developed in the midst of huge, cataclysmic political

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7 For two essays which place Kuhn in broader traditions of twentieth-century philosophers of science, see Gary Gutting, "Thomas Kuhn and French Philosophy of Science," and John Worrall, "Normal Science and Dogmatism, Paradigms and Progress: Kuhn 'versus' Popper and Lakatos," both in Thomas Nickles, ed., *Thomas Kuhn* (Cambridge: Cambridge University Press, 2003), 45–100.

8 See, for example, Michael Friedman, "Kuhn and Logical Empiricism," in Nickles, ed., *Thomas Kuhn*, 19–44.

events—the failure of liberal politics in central Europe and the expansion of communist and American global power after World War II. For Popper, the breakdown of liberal Vienna (his father worked in the administration of the last liberal mayor of Vienna) hovered in the background. Popper made his dramatic epistemological breakthroughs as the Great Depression settled in and Hitler came to power in Germany. Lakatos struggled with issues of intellectual responsibility and science in the face of Nazism, Stalinism, the Cold War, and, finally, the student protests in the West of the 1960s. Fuller grounds his whole understanding of Kuhn in the politics of the Cold War.

Just as I would see the long residues of the 1960s as the appropriate backdrop to fights over science in the 1990s, so too does it seem just common sense to place Popper, Lakatos, and Kuhn in relation to the dramatic events that shaped, not the particulars of their thought, but the issues that pressed in on them. Science may or may not be a product of its times—but rumination on science certainly is. As the history of ideas about truth gets written, it cannot, at least in the mid- and late twentieth century, be severed from the politics that mattered so much. Peter Galison, in *Einstein's Clocks, Poincaré's Maps: Empires of Time*, connects Poincaré's musings about science to new attitudes to time emerging at the turn of the twentieth century.[9] While Galison does not ignore politics, it is industrialization, time management, and the more general excitement and nervousness about the "modern age" that is the heart of his contextualization. Can it be that the most important contexts for discussions of science shift—culture at one point, politics at another, new technology or science at still a third? The recent literature certainly suggests that questions like this deserve more attention from students of science.

If these books upset the picture of positivism as the core philosophy of science in the twentieth century, they do confirm another basic idea of twentieth-century intellectual history: the 1960s and 1970s were a great divide. For while the story of positivism triumphant needs to be rethought, critical sentiment did take a significant turn a few decades back. Even this needs to be stated carefully. We should not confuse this shift with less science being done. We should not forget that there were always intellectuals nervous about science—romantic, religious and traditionalist objections to science are easy enough to come by in the past three centuries. Nor is it difficult to find past scientists complaining that the larger cultural commitment to their work was faltering. None of this is new.

In the 1970s, though, the critique happened in new places and seemed to be more threatening. And this has been sustained. Here's what I think was new:

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9 Peter Galison, *Einstein's Clocks, Poincaré's Maps: Empires of Time* (New York: W. W. Norton & Co., 2003).

people who professionally thought about science—science reporters, sociologists of science, and philosophers of science—now joined the skeptics. This new turn reflected, my current guess is, not an outright hostility in the culture at large but certainly a more general ambivalence about science, technology, and medicine.[10] By the 1970s, it was no longer the hoary humanist complaining about soulless scientism; it was now Paul Feyerabend, the extravagantly anarchistic philosopher of science. It was not some Protestant minister fighting the truth claims of Charles Darwin, it was Bruno Latour questioning the truth claims of Louis Pasteur. It was not Lionel Trilling pining for literary values in the *Partisan Review*, it was the exposé of Jean Heller of the Associated Press denouncing the now infamous Tuskegee syphilis studies.[11] Once people who wrote professionally about science added their voices to the critique, it made the criticism sustained rather than ephemeral and episodic. It gave the critique institutional homes, instead of being encased in a fleeting book of the season. These new homes for criticism certainly helped to propel, for better or worse, the last generation's complaints about science, medicine, and technology. It is this writing that led to the flare-up of the "science wars" of the 1990s. It is not surprising, then, that two of the books under review also stake out positions on current notions of science (Kadvany does not say much about the present). Hacohen and Fuller, despite their very sharp differences, share some broad similarities. Each suggests a variant of one of the most common contemporary way stations between positivism and postmodernism—the call to deliberation and debate.

For Hacohen, Popper is still the answer. Hacohen is quite upfront about Popper's problems as a human being. Popper was rude, reclusive, high-handed, domineering, unable to bear criticism, and more and more hidebound in his views as he got older. But Hacohen makes all this so apparent in order to reclaim Popper's thought. Yes, Popper was no great shakes as a human being, Hacohen seems to be saying, but that does not undermine his philosophy. In the closing pages of his book, as Hacohen discusses the present, he highlights Popper's defense of critical discussion. The commitment to rational debate is still the best alternative out there, Hacohen reasons, and we would do well to pursue it. It is through giving reasons—and testing their validity—that we get closer to the truth. Hacohen defends liberal universalism against the charge of racism and sexism. "Only in a deliberative democracy (or the Open Society)," he writes, "are

10 In 1970, for example, the *New York Times* reported that the sharp increase in interest in the history of science was "closely linked to a growing tension, even revulsion, among students and public over the consequences of science and technology . . . ." See "Rise of History of Science is a Reply to Technology," *New York Times* (Feb 18, 1970), 49.

11 James Jones, *Bad Blood: The Tuskegee Syphilis Experiment*, rev. edn (New York: Free Press, 1993), 203–19.

intersubjective criticism and politics proximate" (p. 545). As Hacohen presents his defense of deliberative science and politics, his Popper starts to sound suspiciously like Jürgen Habermas. To be sure, despite their differences, there are elements in both that do sound close. Popper's 1965 essay "The Myth of the Framework" is a spirited and impressive defense of discussion.[12] And as early as *The Open Society and its Enemies* in 1945, Popper was claiming that "argument, which includes criticism, and the art of listening to criticism, is the basis of reasonableness."[13] Some of the seminal moments that created the image of Popper as a conservative and positivist were his highly publicized debates with Habermas and Adorno in the 1960s. Hacohen, in contrast, now emphasizes Popper's liberalism, his distance from positivism, and his closeness to Habermasian discourse ethics. There really was not as much difference between them as was thought in the heat of the 1960s.[14]

Fuller offers a different defense of debate. Contemporary discussion of science, he argues, is trapped in two different ruts—the phenomenological description of practice (Kuhn's legacy to science studies) and the layperson's assumption "that airplanes are kept up in the air by 'the laws of physics'"(p. 315). Fuller wants science questioned, current practice always put up for debate. Although Fuller is committed to debate and reason-giving, his notion of argument is considerably different from Popper's. Fuller draws largely from the philosopher Stephen Toulmin. All discussion is rhetorical, according to Toulmin, even discussion of science. This does not mean that reason is irrelevant, just that the relevant reason is based on practical life instead of formal logic, mathematics, or any sort of formal method. Toulmin, unlike Popper, believed that questions of fact and emotion overlap in argument. Nor is there any sharp line between science and society. The rhetoric Fuller privileges is civic, providing "training in public speaking for democratic citizens of all walks of life" (p. 314).

Fuller's reliance upon Toulmin is intriguing. Toulmin's first important book, *The Uses of Argument*, was published in 1958, four years before Kuhn's more famous work. In other words, Kuhn and Toulmin were contemporaries. Their interests overlapped. Both wanted an alternative to logical positivism. Both were indebted to Wittgenstein. Lakatos lumped them together as philosophical

12 Karl Popper, "The Myth of the Framework," in Popper, *The Myth of the Framework: In Defence of Science and Rationality* (London: Routledge, 1994), 33–64.

13 Karl Popper, *The Open Society and its Enemies, vol. 2: The High Tide of Prophecy: Hegel, Marx, and the Aftermath* (London: Routledge, 1945), 214.

14 Popper wrote later in life that it was Habermas who first labeled him a "positivist." See Popper, "Reason or Revolution?", in *Myth of the Framework*, 66–7. For the exchanges of the 1960s, see Theodor Adorno *et al.*, *The Positivist Dispute in German Sociology* (New York: Harper & Row, 1976).

conventionalists and therefore "elitists." Yet Fuller sees it differently. Fuller, via Toulmin, wants rhetoric to reaffirm "its democratic roots and public service orientation" (p. 314). He wants a "citizen science" instead of a "professional science" (pp. 418–19).

Fuller's deliberation is civic and populist; Hacohen's is—at least as presented in the closing pages of his book—civic but more restrained. Fuller's debate seems to take place in the public assembly amid the swirl of crowds, Hacohen's in scientific journals, Op-Ed pages, congressional hearing rooms, or university seminars. Fuller's accent is on "rhetoric", Hacohen's on "rationality."

One of Fuller's antagonists is Bruno Latour, whom Fuller associates with the unfortunate turn of science studies in a phenomenological direction. It is ironic, then, that Latour, in *We Have Never Been Modern*, comes as close to Fuller's model of science and society as anyone writing, arguing for a republican science where scientists, policy makers, politicians, and citizens all get a say in the process.[15] It is a sign of the power of discursive interchange as a contemporary intellectual model that it will appear in various guises: rhetoric, discourse ethics, deliberative democracy, republicanism, participatory democracy. Discursive ethics of various sorts have emerged as one of the most potent alternatives to recent postmodernism. Both Hacohen and Fuller polemicize against postmodernism as they defend debate. Fuller is far more radical than Hacohen. Despite the differences, however, both represent the current fascination with deliberation as a key intellectual resource.[16] Moreover, this faith in rational deliberation has become, in our time, a contemporary way of maneuvering between positivism and irrationalism. In other words, it represents the most recent variation of the same basic concern that gnawed at Popper, Lakatos, and Kuhn in the middle decades of the twentieth century.

15 Bruno Latour, *We Have Never Been Modern*, trans. Catherine Porter (Cambridge, MA: Harvard University Press, 1993), 142–5.

16 For a brilliant intellectual history that suggests alternative ways of thinking about communication, see John Durham Peters, *Speaking into the Air: A History of the Idea of Communication* (Chicago: University of Chicago Press, 1999).