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RISE, *GRUBENHUND*: ON PROVINCIALIZING KUHN*

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Bilingualism was Kuhn's solution to the problem of relativism, the problem raised by his own theory of incommensurability. In *The Structure of Scientific Revolutions*, he argued that scientific theories are separated by gulfs of mutual incomprehension. There is no neutral ground from which to judge one theory fitter than another. Each is formulated in its own language and cannot be translated into the idiom of another. Yet, like many Americans, Kuhn never had the experience of moving comfortably between languages. "I've never been any good really at foreign languages," he admitted in an interview soon before his death. "I can read French, I can read German, if I'm dropped into one of those countries I can stammer along for a while, but my command of foreign languages is not good, and never has been, which makes it somewhat ironic that much of my thought these days goes to language."[1] Kuhn may have been confessing to more than a personal weakness. His linguistic ineptitude seems to be a clue to his overweening emphasis on the difficulty of "transworld travel." Multilingualism remained for him an abstraction.[2] In this respect, I will argue, Kuhn engendered a peculiarly American turn in the history of science. Kuhn's argument for the dependence of science on the norms of particular communities has been central to the development of studies of science *in* and *as* culture since the 1980s. Recent work on the mutual construction of science and nationalism, for instance, is undeniably in Kuhn's debt.[3] Nonetheless, the Kuhnian revolution

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1 "A Discussion with Thomas S. Kuhn," in Thomas Kuhn, *The Road since Structure* (Chicago, 2000), 259.

2 Kuhn, *Road since Structure*, 249; see also 101, 175.

3 For a sampling, see Carol E. Harrison and Ann Johnson, eds., *National Identity: The Role of Science and Technology*, *Osiris* 24 (2009). Another fruitful avenue inspired by Kuhn's model of normal science lies in studies of the emergence of elite physical theories out of

cut off other avenues of research. In this essay, I draw on the counterexample of the physician–historian Ludwik Fleck, as well as on critiques by Steve Fuller and Ted Porter, to suggest one way to situate Kuhn within the broader history of the history of science.[4] To echo Kuhn's own visual metaphors, one of the profound effects of *The Structure of Scientific Revolutions* on the field of history of science was to render certain modes of knowledge production virtually invisible.

Although incommensurability struck many of Kuhn's initial readers as a problem desperately in need of solution, Kuhn himself suggested otherwise. In the evolutionary analogy that he pursued to the end his career, incommensurability was a necessary condition for the speciation of scientific paradigms. By promoting the proliferation of specialized subfields, incommensurability seemed to Kuhn to make modern science uniquely efficient. Prior to Kuhn's evolutionary metaphor, however, was the political one. In *Structure*, Kuhn likened scientific revolutions to a process of "Balkanization": "Scientific revolutions . . . need seem revolutionary only to those whose paradigms are affected by them. To outsiders they may, like the Balkan revolutions of the early twentieth century, seem normal parts of the developmental process."[5] Steve Fuller has glossed this as a claim that "a mature paradigm's inability to contain the spread of anomalies" is akin to the failure of the Austro-Hungarian and Ottoman Empires to "maintain order" among their national groups: "The resolution of a paradigmatic crisis by redistributing scientific labor to more specified domains of inquiry may be likened to the devolution of the old eastern empires into independent nation-states."[6] Fuller notes further that, for Kuhn, "Scientific knowledge, like language, is intrinsically the common property of a group or nothing else at all."[7] What could have seemed more self-evident to an American observer of world events in 1962? For a unique and fleeting moment in history, the nation state looked like an inevitability, a necessary step on the path to modernization. Through two world wars and their aftermath, by means of population exchanges and ethnic cleansing, Europe had been reconfigured into nearly homogeneous nation states. Decolonization was effecting a similar "rationalization" of the global map. What followed for the history of science was, in effect, its own Balkanization. As Avner Ben-Zaken has

highly disciplined cultures of puzzle solving: see David Kaiser and Andrew Warwick, eds., *Pedagogy and the Practice of Science: Historical and Contemporary Perspectives* (Cambridge, MA, 2005).

4 Steve Fuller, *Thomas Kuhn: A Philosophical History for Our Times* (Chicago, 2000); Theodore M. Porter, "How Science Became Technical," *Isis* 100 (2009), 292–309.

5 Thomas S. Kuhn, *The Structure of Scientific Revolutions*, 2nd edn (Chicago, 1970), 93.

6 Fuller, *Thomas Kuhn*, 168. See also Stefano Gattei, *Thomas Kuhn's "Linguistic Turn" and the Legacy of Logical Empiricism: Incommensurability, Rationality and the Search for Truth* (Aldershot, 2008).

7 Fuller, *Thomas Kuhn*, 71.

noted, one consequence of the thesis of incommensurability was the training of graduate students in "German science" or "French science," "Arabic science" or "Chinese science."[8] In Gestalt terms, students were trained to *see* science as located in nations, in some cases even before the existence of those nations.

Historians of science since Kuhn have tended to assume that social organizations and social conflicts map neatly onto linguistic groupings. Instead of posing these lines of affiliation and division as historical questions, historians tend to take them as givens. The analysis of translation typically comes to a halt with the evaluation of the distance of the product from the original; the interpretation of the translation within its own context of production is largely neglected. In these ways, the incommensurability thesis has short-circuited much of the hard work of historical research.[9]

To see otherwise has meant turning incommensurability inside out: posing it as historical explanandum rather than explanans. This process began in the late 1980s. Peter Galison and Norton Wise began to consider cases in which material objects mediated communication between intellectual communities which did not otherwise share a language. Mario Biagioli showed how incommensurability, far from being a necessary outcome when groups espouse different theories, was a calculated strategy for maintaining socioprofessional autonomy. Bruno Latour and Michel Callon redirected attention away from isolated sites of science and towards the networks constructed as scientists engage the interests of other actors.[10] Yet these studies were not entirely free of the blind spot inherited from Kuhn. Still invested in issues of relativism and realism, they continued to seek broad functionalist explanations for achievements and failures of scientific communication. They stopped short of historicizing the phenomenon

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8 Avner Ben-Zaken, *Cross-cultural Scientific Exchanges in the Eastern Mediterranean, 1560–1660* (Baltimore, 2010), 3.

9 Roger Hart, "Translating the Untranslatable: From Copula to Incommensurable Worlds," in Lydia H. Liu, ed., *Tokens of Exchange: The Problem of Translation in Global Circulations* (Durham, 1999), 45–73. On scientific translation see Marwa Elshakry, "Knowledge in Motion: The Cultural Politics of Modern Science Translations in Arabic," *Isis* 99 (2008), 701–30; Scott L. Montgomery, *Science in Translation: Movements of Knowledge through Cultures and Time* (Chicago, 2000); Sander Gliboff, *H. G. Bronn, Ernst Haeckel, and the Origins of German Darwinism* (Cambridge, MA, 2008); Benjamin A. Elman, *On Their Own Terms: Science in China, 1550–1900* (Cambridge, MA, 2005).

10 Peter Galison, *Image and Logic: A Material Culture of Microphysics* (Chicago, 1997); M. Norton Wise, "Mediating Machines," *Science in Context* 2 (1988), 77–113; Mario Biagioli, "The Anthropology of Incommensurability," *Studies in History and Philosophy of Science* Part A, 2 (1990), 183–209; Michel Callon, "Some Elements of a Sociology of Translation: Domestication of the Scallops and the Fishermen of St. Brieuc Bay," in Mario Biagioli, ed., *The Science Studies Reader* (New York, 1999), 67–83; Bruno Latour, *Science in Action: How to Follow Scientists and Engineers through Society* (Cambridge, MA, 1988).

of incommensurability itself. How is it possible, in Kuhnian terms, for historians trained to perceive incommensurability as an epistemic principle to *learn to see* incommensurability instead as a contingent outcome of a specific historical situation? How is it possible for a discipline focused on situated knowledge to grasp knowledge in motion? How might historians awed by the robustness of scientific objects of exchange—"immutable mobiles"—begin to recognize the mutations that accrue to circulating facts?

This is not a question of a gestalt switch. A number of historians of science in recent years have trained themselves to see knowledge in motion by means of a creative adaptation of methods from world history, translation and postcolonial studies, and the history of material culture. Their insights have come from mapping networks of trade in specimens, books, and instruments; from delving into new sources, such as bilingual editions and multilingual marginalia; and from recovering forgotten actors, such as captive scholars, traders, and other cultural and linguistic "go-betweens."[11] Such studies set aside the Kuhnian assumption of "an essentially confrontational relation between disparate peoples or communities," in favor of a close examination of "global interconnections."[12] Rather than assuming any degree of commensurability, these scholars arrive at the common ground between cultures only via empirical analysis. For instance, Ben-Zaken has uncovered the "meta-language" of a myth of universal origins, which supported scientific exchange in the eastern Mediterranean *circa* 1600. Roger Hart has demonstrated the mutual intelligibility of concepts of existence between early modern Jesuits and Chinese scholars. And Pamela Smith has found evidence that similar alchemical practices and even cultural associations were associated with objects such as cinnabar, vermilion, quicksilver, and even lizards across premodern Europe, China, and the Arabic world.[13]

Nearly all this research on knowledge circulations has concerned the period before 1800.[14] In this sense, it does not necessarily stand outside the Kuhnian

[11] For example, Simon Schaffer, Lissa Roberts, Kapil Raj, and James Delbourgo, eds., *The Brokered World: Go-Betweens and Global Intelligence, 1770–1820* (Sagamore Beach, 2009); James Delbourgo and Nicholas Dew, eds., *Science and Empire in the Atlantic World* (New York, 2008); Ben-Zaken, *Scientific Exchanges*.

[12] "Introduction," in Schaffer *et al.*, *Brokered World*, xv.

[13] Ben-Zaken, *Scientific Exchanges*; Hart, "Translating the Untranslatable"; Pamela H. Smith, "Science in Motion in the Early Modern World," in Daniel Rogers, Bhavani Raman, and Helmut Reimitz, eds., *Cultures in Motion* (Princeton, forthcoming).

[14] Important exceptions are the work of Marwa Elshakry on late Ottoman Egypt, and of Jan Surman on late Habsburg central Europe. In both contexts, translators sought explicitly to create a scientific vernacular in the name of popular enlightenment. They pursued the epistemic–rhetorical ideals of "clarity and communicability" (Elshakry), though there was no consensus on how best to attain those ideals. Elshakry, "Knowledge in Motion," 721; Jan

paradigm. It was an essential point for Kuhn that science only became science once it acquired the capacity for incommensurability with respect to the "laity." In "The Route to Normal Science," he distinguished between the "pre-history" and the "history proper" of a science, the latter denoting its professional incarnation. He argued that the emergence of a paradigm had the power to "transform a group previously interested merely in the study of nature into a profession or, at least, a discipline." Kuhn was well aware of the stigma against professionalization and narrow specialization, inherited from a Victorian culture of gentleman naturalists. And he was highly self-conscious about his own efforts to straddle the divide between science and humanism. Nonetheless, he implied here that the bias against specialization was anachronistic: "Although it has become customary, and is surely proper, to deplore the widening gulf that separates the professional scientist from his colleagues in other fields, too litttle attention is paid to the essential relationshp between that gulf and the mechanisms intrinsic to scientific advance." Scientists owed their efficiency as problem solvers to the "unparalleled insulation of mature scientific communities from the demands of the laity and of everyday life . . . The most esoteric of poets or the most abstract of theologians is far more concerned than the scientist with lay approbation of his creative work, though he may be even less concerned with approbation in general." Thanks to this isolation, scientists were able to take "a single set of standards for granted." Normal science, for Kuhn, was an activity of "technical" puzzle solving, with crises arising exclusively from "technical" problems. From this point on, Kuhn argued, the worlds of science and everyday life became incommensurable. Communication between scientists and laypeople became a matter of more or less successful "translation." For instance, "Electrical research began to require *translation* for the layman before the end of the eighteenth century, and most other fields of physical science ceased to be generally accessible in the nineteenth."[15]

With this claim, Kuhn was transforming nothing less than the meaning of "science." Science in the eighteenth century had been equated with the interests of humanity; in the nineteenth century it had been praised for its "humanistic value" and defined (by Thomas Huxley) as "trained and organized common sense." Now it was to be something entirely removed from human interests. Kuhn was redefining modern science as *technical* knowledge. As Ted Porter notes, the technical denotes "not just what is difficult, but what is inaccessible and, by general consent, dispensable for those with no practical need of it."[16] Porter

Surman, "Figurationen der Akademia. Galizische Universitäten zwischen Imperialismus und multiplem Nationalismus," in Doktoratskolleg "Galizien," ed., *Galizien – Fragmente eines diskursiven Raums* (Innsbruck, 2010).

15 Kuhn, *Structure*, 20, emphasis added.

16 Porter, "How Science Became Technical," 298.

argues that technicality is a cloak behind which experts hide in order to avoid the scrutiny of a democratic society.

This principle of the "incommensurability" of science and everyday life is one of the few elements of Kuhn's magnum opus that has gone largely unanalyzed. With few exceptions, even scholars thinking critically about the "public communication of science" take this model for granted. Alan Irwin, the coiner of the increasingly trendy term "citizen science," writes repeatedly of the "incommensurability" between the perspectives of scientists and citizens.[17] It is impossible to know to what extent Kuhn was responsible for this assumption. Certainly, others in the US were making similar claims at the time. As Porter notes, even those in the Cold War United States who spoke of science as a public endeavor and a pillar of democratic society—men like Merton, Conant, and Oppenheimer—thought of science as a highly technical and exclusive pursuit.[18] Yet 1962 was also the year of publication of two seminal manifestos in support of public reason—Rachel Carson's *Silent Spring* and Jürgen Habermas's *The Transformation of the Public Sphere*. It was also the era when an iconic "technical" scientist, Werner Heisenberg, raised the call to expand public debate on science policy.[19] Consider, too, the logical positivist tradition to which *Structure* was originally meant to contribute (as a volume in Neurath's *International Encyclopedia of Unified Science*). In a suggestive footnote, Fuller has observed that the Kuhnian model of scientific–lay incommensurability clashed with the assumptions of his central European forebears, many of them influenced by socialism. Fuller asks us to

> consider the endearingly naive beliefs ... that one could contribute to [science] by speaking in terms of universally available observations and logically transparent claims ... Popperians preferred 'plain speaking' and ad hoc critiques of jargon and scientistic obfuscation ... However one wishes to judge their specific efforts, these exiles from Vienna clearly believed that science was within the reach of more than just the people who happen

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17 Alan Irwin, *Citizen Science: A Study of People, Expertise and Sustainable Development* (New York, 1995), 122, 124, 127. Irwin's recent work takes a more critical view of presumed barriers to expert–lay communication and and the consequences for "public" assemblies staged under the banner of citizen science. Alan Irwin, "The Politics of Talk: Coming to Terms with the 'New' Scientific Governance," *Social Studies of Science*, 36 (2006), 299–320. Anthropologists have fruitfully interrogated the divide between expert and lay knowledge; see especially Jean Lave, *Cognition in Practice: Mind, Mathematics and Culture in Everyday Life* (Cambridge, 1988).

18 Porter, "How Science Became Technical," 306. Porter dates the association of science with the technical to the early twentieth century.

19 Cathryn Carson, *Heisenberg in the Atomic Age: Science and the Public Sphere* (Cambridge, 2010).

to get advanced degrees in scientific subjects and regularly spend time in research sites. In our Kuhnified world, this is no longer the case.[20]

In this light, Kuhn's claim for the fundamentally technical character of science was arguably revolutionary.

To make that interpretation plausible, I propose to reconsider a text that is often read alongside *Structure* by incoming graduate students in the history of science. Although little noticed upon its publication in 1935, Ludwik Fleck's *Genesis and Development of a Scientific Fact* was, according to Kuhn, the source of the sociological dimension of his own argument.[21] Ever since Kuhn made this claim in the preface to *Structure*, it has been difficult to read Fleck's work as anything but a first approximation to Kuhn's incommensurability thesis. Read in his own context, however, Fleck seems to express something quite different.

Fleck (1896–1961) grew up in a middle-class Jewish family in Lwów (German: Lemberg; today Lviv, Ukraine), in the eastern part of the Habsburg crownland of Galicia. Galician Jews attended Polish-language schools and tended to identify strongly with Polish culture; Polish nationalists in turn looked to Jews for political support. At the same time, Galician Jews were strongly loyal to the tolerant Habsburg dynasty and its supranational state. Like most educated Galician Jews, Fleck was fluent in German and Polish and could likely understand Yiddish and read Hebrew as well. As an adolescent, during and after the First World War, he would have encountered a growing number of Zionists with a wide variety of religious and political orientations. As interwar censuses indicate, the relationship between language and national identity was not entirely predictable for Galician Jews: Yiddish-speakers might identify nationally as Poles, for instance. In 1918, when Fleck was twenty-two, Lwów was caught in the civil war between Polish and Ukrainian nationalists; then, until the Second World War, the city was part of the new independent Poland. By the 1930s, anti-Semitism was unofficial state policy. A Jew in this multinational city could easily become suscipicious of any form of unitary identity.[22]

Scholarly life in Lwów was closely tied to that of Vienna, but the city also nurtured its own intellectual ideals.[23] Fleck trained as a physician and bacteriologist in Lwów and Vienna at a time when the Polish medical community was particularly concerned with developing Polish as a scientific language.

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[20] Fuller, *Thomas Kuhn*, 314.

[21] Kuhn, *Structure*, vii.

[22] Ezra Mendelsohn, *The Jews of East Central Europe between the World Wars* (Bloomington, 1983), esp. 42–3. On the fraught relationship of Jews to central European nationalist movements see Yuri Slezkine, *The Jewish Century* (Princeton, 2004).

[23] See the essays by Giedymin, Wolniewick, and Markiewicz in Robert S. Cohen and Thomas Schnelle, eds., *Cognition and Fact: Materials on Ludwik Fleck* (Dordrecht, 1986).

The first major Polish-language medical journal, *Medical Critique* (1897–1907), devoted a section of each issue to questions of language.[24] Fleck himself continued to publish papers in Polish throughout his career. This interest in scientific "vernacularization" was common throughout eastern central Europe at the time. Scientists pursued a two-pronged strategy, simultaneously claiming priority by publishing in a *Weltsprache* and cultivating a local scientific culture by publishing in the languages of the "small" nations.[25] These central Europeans promoted science in part as an element of national culture. Consequently, they set high standards for publications for a popular audience, insisting that these represent original work and not "plagiarize" German sources.[26] Against this background, the gap between Fleck's book and Kuhn's begins to crystallize.

Despite Kuhn's own sense of indebtedness, his sociology was a radical departure from Fleck's. Fleck's notion of a "thought collective" stands opposed to Kuhn's "scientific community" as a functionalist concept to a realist one. "The concept of the thought collective," Fleck wrote, "is not to be understood as a fixed group or social class. It is functional, as it were, rather than substantial, and may be compared to the concept of field of force in physics. A thought collective exists whenever two or more persons are actually exchanging thoughts." Fleck's analogy to a force field diverged from that common in Gestalt psychology, where a field was taken to be independent of and prior to its components. For Fleck, the collective was merely a "functional" concept, and its boundaries were transient and fluid. It was explicitly not a "community," for its membership and leadership "do not coincide with the official hierarchy and organization."[27] Fleck's choice of the word *Kollektiv* constituted what one translator has called "a highly contentious terminological issue."[28] It was a deliberate rejection of *Gemeinschaft*, with its increasingly exclusionary and antimodern connotations. Indeed, a German reviewer attacked Fleck in 1936 for shunning *Gemeinschaft*

[24] Ilana Löwy, *The Polish School of Philosophy of Medicine* (Dordrecht, 1990), 132.

[25] Michael Gordin, "The Unpleasant Instance of the Periodic Table: Translating into a Priority Dispute," and Jan Surman, "Communication of Representation? Linguistic Policies in Polish, Czech and Ukrainian Science in the Late 19th Century," both presented at the American Historical Association meeting, Boston, Jan. 2011.

[26] Tomáš Hermann, "Originalita Vědy a Problém Plagiátu. Tři výstupy Emanuela Rádla k jazykové otázce ve vědě z let 1902–1911," in Harald Binder, Barbora Křivohlavá and Luboš Velek, eds., *Místo národnich jazyku ve výuce, vědě a vzdělání v Habsburské monarchii 1867–1918/Position of National Languages in Education, Educational System and Science of the Habsburg Monarchy 1867–1918* (Prague, 2003); cf. Surman, "Figurationen."

[27] Ludwik Fleck, *Genesis and Development of a Scientific Fact*, trans. Frederick Bradley and Thaddeus J. Trenn (Chicago, 1979), 102, 103.

[28] "Preface," in Fleck, *Genesis and Development*, xvi.

in favor of *Kollektiv*, "currently the keyword of the Russian conception of social life."[29] *Kollektiv*, more precisely, was a term Fleck borrowed from statistics.

> If workers in highly exact sciences such as physics do not shrink from making use of statistical data, such as average values or probability values, which correspond not to any "actual" appearance but to a hypostatized fiction, and indeed consider an "actual" appearance much less "genuine" than this fiction, we shall probably have no reason to fear any damage caused by the introduction of the thought collective.[30]

German social scientists had long derided statistical models of this sort, with their implication that a society was no more than an aggregate of autonomous individuals. Wilhelmine critics argued that statistical descriptions could not account for the emergence of a higher social unity. Norton Wise has captured their attitude in the principle that "German individuals do not sum."[31] By contrast, a Fleckian thought collective was precisely an aggregate. This became clear when Fleck went on to distinguish this basic concept from certain "stable" thought collectives or "thought communities"—and here he conspicuously introduced the term *Gemeinschaft*. Where Kuhn would stress the efficiency of exclusionary scientific communites, Fleck pointed to their "intolerance":

> The organic exclusiveness of every thought commune goes hand in hand with a stylized limitation upon the problems admitted. It is always necessary to ignore or reject many problems as trifling or meaningless. Modern science also distinguishes "real problems" from useless "bogus problems." This creates specialized valuation and characteristic intolerance, which are features shared by all exclusive communities.[32]

Unlike Kuhn, Fleck suggested that the exclusiveness of the thought collectives of modern science was not inevitable, nor was it necessarily desirable. Only the tendency to read these two texts side by side has obscured this fundamental difference. Fleck's translators, for instance, cite the Polish term *zespół myślowy* as an equivalent for the German *Denkgemeinschaft* (as distinguished from *kolektyw myślowy*/*Denkkollektiv*). However, *zespół* (band, team, crew, set, cooperative, collective, complex, etc.), from the root *społ* (together) is closer in meaning to the German *Kollektiv* than to *Gemeinschaft*.[33]

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29 Hans Petersen, "Ludwig Flecks Lehre vom Denkstil und dem Denkkolektiv," *Klinische Wochenschrift* 15 (1936), 239–242, 240.

30 Fleck, *Genesis and Development*, 181.

31 M. Norton Wise, "How Do Sums Count? On the Cultural Origins of Statistical Causality," in Lorenz Krüger, Gerd Gigerenzer, and Mary S. Morgan, eds., *The Probabilistic Revolution*, vol. 1 (Cambridge, MA: 1987), 395–426.

32 Fleck, *Genesis and Development*, 104.

33 Thanks to Edyta Bojanowska for this definition.

Kuhn's sense of the discomfort of "transworld travel" was also a radical departure from Fleck. In pre-1939 Lwów, one might say, such passages were a fixture of everyday life. As Fleck put it in a 1929 essay,

> Every thinking individual, as a member of some society, thus has his own reality, in which and according to which he lives. Indeed everyone has many, partially contradictory realities: the reality of everyday life, a professional, a political, and a little scientific reality. And secretly he has a superstitiously fatalistic reality that makes of himself an exception. For every perception, for every knowledge system, for every entry into social relations, there corresponds a distinct reality.[34]

Where Kuhn stressed the mental contortions necessary to move between paradigms, Fleck made it seem natural for an individual to live in constant transition among contradictory "realities." This contrast becomes more vivid if one compares Fleck's account to a more famous passage of central European literature—which appeared, coincidentally, just a year after Fleck's essay on multiple realities. In *The Man without Qualities*, Robert Musil described an indeterminacy of identity that bears similar marks of central European experiences during and after the Great War:

> For the inhabitant of a country has at least nine characters: a professional, a national, a civic, a class, a geographic, a sexual, a conscious, an unconscious, and possibly even a private character to boot. He unites them in himself, but they dissolve him, so that he is really nothing more than a small basin hollowed out by these many streamlets that trickle into it and drain out of it again, to join other such rills in filling some other basin. Which is why every inhabitant of the earth also has a tenth character that is nothing else than the passive fantasy of spaces yet unfilled.[35]

If Fleck's intercollective mediator resembles Musil's citizen with "at least nine characters," the similarity is not fortuitous. It was a tenet of Austro-Marxism that the state must stand above the nations and accommodate multiple allegiances; as Karl Renner put it, "We must put a double network on the map, an economic and an ethnic one."[36] A quintessentially central European sense of the fluidity of identity seems to have guided Fleck's attention to the role of the individual as a mediator among thought collectives—"as a vehicle for the intercollective communication of thought."[37] Fleck opened up the question of intercollective

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[34] Fleck, "On the Crisis of 'Reality,'" in *Cognition and Fact*, 47–58, 49; translation slightly modified. Thomas Schnelle notes in his essay for *Cognition and Fact* that Leon Chwistek was another philosopher of "multiple realities" active in Lwów at the time.

[35] Robert Musil, *The man Without Qualities*, vol. 1, trans. Sophie Wilkins (New York, 1996), 30.

[36] Rudolf Springer (pseud. Karl Renner), *Grundlagen und Entwicklungsziele der österreichisch-ungarischen Monarchie* (Vienna, 1906), 208.

[37] Fleck, *Genesis and Development*, 110.

communication just where Kuhn would later close it off, for Kuhn had no more to say about the experience of "transworld travel" or multilingualism than that he imagined it to be difficult.

A crucial feature of Fleck's image of overlapping realities was the interpenetration of the spheres of science and everyday life. "Everyday reality" was, for Fleck, the common soil nourishing all specialized spheres of thought. "So-called commonsense, as the personification of the thought collective of everyday life, has become in this same way a universal benefactor for many specific thought collectives." Fleck in fact traced the origins of modern science to the everyday sphere in which a variety of ordinary human activities converged:

> Surely there had always existed thinking typical of the natural sciences. It was to be found among the artisans, the seamen, the barber-surgeons, the leather workers and saddlers, the gardeners and probably also among children playing. Wherever serious or playful work was done by many, where common or opposite interests met repeatedly, this uniquely *democratic* way of thinking was indispensable.

By "democratic," Fleck meant a form of knowledge that both resulted from and served a free and open confrontation among different points of view. Like conventionalists, Fleck noted that the result was "independent of the individual," precisely because it was "socially conditioned," i.e. because it was constructed through the "collaboration and communication of many individuals, as many as possible." Again, Fleck's vision of the scientific process was essentially statistical: the stability of knowledge was the result of the law of large numbers. Thus, for Fleck, the true robustness of scientific knowledge derived from its being "democratically constructed" by a mass collective (*die Masse*); not, as for Kuhn, from its isolation within a strictly bounded community.[38]

In comparing science to artisanal work, Fleck partly anticipated the celebrated thesis of Edgar Zilsel on the "sociological roots of modern science." Zilsel began to lecture on this theme in Vienna in 1930 but did not publish until 1942. In Zilsel's account, science did not become possible until scholars gave up their disdain for manual labor. Historically, the scientific method originated with craftsmen: "Having outgrown the constraints of guild tradition and being stimulated to inventions by economic competition, they were, no doubt, the real pioneers of empirical observation, experimentation, and causal research." The full import of Zilsel's thesis has only recently been appreciated, thanks to Pamela Smith's

[38] Fleck added that "every democracy has its little untruths," its displays of power; so too the sciences—practical and democratic though they are—must have "their own natural philosophy and their own *Weltanschauung*." This cynical note must be read in the context of interwar Poland, where parliamentary democracy was increasingly a front for authoritarianism. Fleck, *Genesis and Development*, 109, added emphasis; *idem*, "On the Crisis of 'Reality,'" 50, 55, 57.

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investigation of the implicit epistemologies of Renaissance craft traditions.[39] For my purposes, the key point is how far Fleck and Zilsel stood from Kuhn in their understanding of "science": one a "democratic" form of knowledge, rooted in the craftsman's engagement with nature, and shading off into the activities of everyday life; the other exclusive, mathematical—in short, "technical."

"Natural science," Fleck concluded, "is the art of shaping a democratic reality and being guided by it—thus being reshaped by it." Fundamental to Fleck's concept of science was a reflexivity or capacity for self-correction. He likened the work of science to that of "a river cutting its own bed."[40] True, the energy of a flowing river changes the water's own course. But energy is also lost through friction with the riverbed. To complete the metaphor, Fleck must have had a notion of the source of this resistance to the course of science. What was the terrain into which the river had to carve itself?

It was, I would suggest, the common ground of "everyday reality." And the force of its resistance was primarily linguistic. Far from imagining scientific communication as an exchange of "immutable mobiles," Fleck argued that "even the simple communication of an item of knowledge can by no means be compared with the translocation of a rigid body in Euclidean space." Fleck, the Polish-German-Jewish philosopher–physician, was of necessity attuned to the ways in which meanings were transformed by communication across collectives. "Communication never occurs without a transformation," he argued. Like Biagioli some fifty years later, Fleck was suggesting that the process of "translation" was prior to the emergence of incommensurability. Moreover, Fleck explicitly identified this process as creative, an observation that resonates with recent work in translation studies. In his case study of syphilis, he determined that communication between thought collectives "offers new possibilities for discovery and creates new facts. This is the most important epistemological significance of the intercollective communication of thoughts."[41] When Fleck wrote of comparing "the meaning of the words 'force,' 'energy,' or 'experiment' for a physicist, a philologist, or a sportsman; the word 'explain' for a philosopher and a chemist, 'ray' for an artist and a physicist, or 'law' for a jurist and a scientist," he was *not* arguing for incommensurability in Kuhn's sense. He was just as interested in continuities of meaning as in differences. And he found continuities in particular between science and "popular" knowledge. Unlike the hermetic boundaries that Kuhn imagined between professional science and the public, Fleck envisioned a gradation from "esoteric" (expert) to "exoteric"

39 Pamela H. Smith, *The Body of the Artisan: Art and Experience in the Scientific Revolution* (Chicago, 2004).

40 Fleck, "On the Crisis of 'Reality,'" 54.

41 Fleck, *Genesis and Development*, 110.

(amateur) circles and beyond to the general public. Communication kept this a porous boundary. Thus one could always locate "items of popular knowledge from other fields within the depths of these sciences."[42] Indeed, the process of communication with the public reacted back on the most esoteric cores of specialized fields of knowledge. Expert knowledge tended to be "exhaustive," encumbered by footnotes, lacking "clarity" and "unsuitable in any practical case." In this form it was impossible to see "more general and recurrent elements," and one always remained at a distance from "fundamental concepts." It was only through the communication of knowledge to the public that such jargon took on the qualities of basic, systematic knowledge. "Certainty, simplicity, vividness originate in popular knowledge. That is where the expert obtains his faith in this triad as the ideal of knowledge. Therein lies the general epistemological significance of popular science."[43] In this sense, the public was the vital source of friction in the flow of science.

By contrast, language confined within a thought collective became, in Fleck's judgment, "lifeless" (*lebensfremde*). What had once been malleable became "reified and objectified" and the products were but "technical terms." For Fleck, the language barrier between experts and laypeople was an *outcome* and not, as for Kuhn, a *cause* of the professionalization of science. "A contrast between *expert* and *popular* knowledge is hence the first effect of the general structure of the thought collective in science." Language functioned quite differently in Fleck's argument than in Kuhn's. Fleck, for example, did *not* liken mutual incomprehension between successive thought collectives to a language barrier; instead, his historical account of changes in thought-style instead emphasized *style*, both artistic and rhetorical.[44]

Fleck's description of how communication with the public reacts back on science itself is one of the most profound and least appreciated of his insights. The logical positivists could agree with Fleck on this point as on little else: scientific language needed to be held to a standard of universal intelligibility. As Fuller notes, Ernst Mach had made this point explicitly, arguing that "the tractability of science to common modes of experience should constrain the development of science nearly as much as science should revise and discipline common modes of experience." Erwin Schrödinger expressed a similar conviction in his 1932 essay "Is Science a Fashion of the Times?" (cited by Fleck in 1947). Schrödinger considered what happens when a specialist must explain to a layman why he studies what he does: "I mean that you will try to defend the reason *why* you

[42] Fleck offered the example of the popular origins of the "etiological idea of disease entity" (*Genesis and Development*, 121).

[43] Fleck, *Genesis and Development*, 115.

[44] Ibid., 133–45.

are interested . . . And you will become aware of the fact that only now, in your discussion with your colleague, have you reached those aspects of the subject that are, so to speak, nearest your heart."[45] By the same token, Zilsel's thesis specified that science originated when scholars began to *talk* with craftsmen. In all these cases, communication with the public was assumed to be therapeutic to science. It was the form of resistance necessary to keep the scientific river on course.

This function of public communication was not, however, an insight original to any of these philosophers. As Fleck himself would likely have argued, it stemmed from the "common sense" of his own everyday reality. In contrast to Britain and Germany, where a distinct class of scientific "popularizers" arose in the late nineteenth century, recent research suggests that scientists in imperial Austria took upon themselves the responsibility of communicating with the educated public. Rather than seeing this as a mark of delayed modernization, we might view it as a conscious attempt to bridge scientific and vernacular discourses.[46] Equally significant was the culture of the Viennese press. Intellectual historians are likely familiar with that culture through the figure of Karl Kraus, the "anti-journalist." Kraus was a writer whose purpose was to expose the ways in which modernity disfigured language itself.[47] He attacked the sensationalistic, self-obsessed journalism of his day as a reflection of the hypocrisy, arrogance, and complacency of bourgeois culture. He is less well known as a perceptive critic of science. In Vienna in 1899, at the age of twenty-five, Kraus founded his mouthpiece *Die Fackel*. In its wide-ranging early attacks on bourgeois sexual mores, psychoanalysis, and nationalism, a consistent theme was the hubris of modern science and technology.

As I explore elsewhere, some of Kraus's most acid critiques of *fin de siècle* journalism before 1914 were those dealing with the reporting of natural disasters. These events furnished him with the tell-tale images of his increasingly "apocalyptic" vision. A better-known example of Kraus as scientific watchdog is his 1909 essay on the "discovery—or, as it has also been called, the conquest" of the North Pole. Kraus identified in the reporting of that feat a fatal combination of political chauvinism and technological arrogance. The dispute over the priority for the discovery was, to Kraus, a typical case of the machinations of science and the press. Each prolonged the dispute in order to bolster its own authority; each, while claiming to speak for "truth," ran roughshod over it. The essay's

45 Erwin Schrödinger, "Is Science a Fashion of the Times?" in *idem*, *Science, Theory, and Man*, trans. James Murphy (New York, 1957), 5.

46 Mitchell Ash, "Wissenschaftspopularisierung und Bürgerliche Kultur im 19. Jahrhundert. Essay-Rezension," *Geschichte und Gesellschaft* 28 (2002), 322–34.

47 Paul Reitter, *The Anti-journalist: Karl Kraus and Jewish Self-Fashioning in Fin-de-Siècle Europe* (Chicago, 2008).

question "What is truth?", twice raised, receives no semblance of an answer. In the conclusion, Kraus drew together the North Pole expeditions with the recent exploits of aviation to illustrate the recklessness of the modern race to conquer nature. "Progress, with its head down and its legs up, kicks away in the atmosphere and assures all crawling spirits that it dominates nature." Kraus reduced the much-vaunted heroism of modern science to images of effete self-delusion.[48]

Kraus attributed the power of modern science to the performative quality of its jargon, its rhetorical voice or "tone" (*Tonfall*): "With the right tone one can conquer the world as a whole," he remarked in 1910. "Scream murder and a murder is committed; whisper abracadabra and it is religion; write dynamo exhaust pipes and it is science."[49] The occasion for this reflection was a minor earthquake in Vienna and the prank it inspired. Over lunch the day after the tremor, a group of engineers fell into conversation about "the unprecedented idiocy [*Schmockerei*] of the newspaper earthquake reports in general and those of the *Neue Freie Presse* in particular." Suddenly, seized by "a wild desire," an engineer by the name of Alfred Schütz left the table; when he returned he read his friends the earthquake report of one "Herr Dr. Ing. Erich R. v. Winkler, Assistant at the Central Laboratory of the Ostrau-Karwin coal mines." It consisted of Winkler's observations of the earthquake's effects on a train's compressor—down to the last technicality, and in the name of the "ceaseless efforts of our mining authorities for the protection of the lives of the miners." What would live in infamy, however, was the following sentence: "A wholly inexplicable occurrence is, however, that already half an hour before the start of the quake, my mine-dog [*Grubenhund*], asleep in my laboratory, gave conspicuous signs of the greatest disquiet." It helps to know that, to a mining engineer, a "Grubenhund" is a cart for carrying coal. The letter reduced Schütz's friends to hysterics, but they refused to believe that a newspaper would print it. Schütz bet that the content of the report was irrelevant; only the tone mattered. Indeed, the letter appeared the following morning in the *Neue Freie Presse*. Kraus was naturally the prime suspect.

The incident gave birth to the concept of the *Grubenhund* (see Fig. 1). Schütz defined the term narrowly as a false report that sneaks by editors but can be clearly recognized by readers. Thanks to his many imitators, however, the term came to be used more broadly to identify a tactic of resistance against the obfuscating jargon of technical experts. Schütz himself later cast his prank as an apolitical act of enlightenment: "The *Grubenhund* is the symbol of the spoofing of pretended universal knowledge, the protest against the assumed authority of the printer's

[48] Karl Kraus, "The Discovery of the North Pole," in Harry Zohn, ed. and trans., *In These Great Times* (Manchester, 1976), 48–57, 55.

[49] Karl Kraus, "Nach dem Erdbeben," *Die Fackel* 13/338 (1911), 18–24, 22; my translation.

Fig. 1. Grubenhund caricature (1911), in Arthur Schütz, *Der Grubenhund: Experimente mit der Wahrheit*, ed. Walter Hömberg (Munich: Fischer, 1996), 106.

ink in everything, but especially in technical matters."[50] Kraus credited the *Grubenhund* with having "unmasked the scientific voice." It had exposed the complicity of science with the press: "For science is by nature so constructed that surprises are not excluded, and its credit rests on novelty. In sending up journalism, it proved their identity and bedded down with it."[51]

Given Vienna's public culture of scientific communication and critique, it may not be a coincidence that two of the most astringent early critics of Kuhn's incommensurability thesis had Viennese roots. Karl Popper (1902–94) was born into a family of progressive Jewish intellectuals in late imperial Vienna; in turn, he and other Viennese scientists and philosophers of his generation decisively influenced Paul Feyerabend (1924–94), who earned his doctorate in Vienna in 1951.[52] Both Popper and Feyerabend recognized the normative dimension of Kuhn's notion of "normal" science, its implication that dissent *should* be suppressed. As Stefan Gattei explains, Popper believed that

> Critical discussion ... is always possible, and the contrary thesis (i.e. the incommensurability thesis, the idea that different frameworks are like mutually untranslatable languages) is a dangerous *dogma*—"the central bulwark of irrationalism." The "myth of the framework" exaggerates a difficulty into an impossibility: however difficult, there is nothing more fruitful than the clash between different cultures of ideas. Denying this possibility is a mistake, since authentic progress springs from it.

[50] Arthur Schütz, *Der Grubenhund: Experimente mit der Wahrheit*, ed. Walter Hömberg (Munich: Fischer, 1996), 38, my translation.

[51] Kraus, "Nach dem Erdbeben," 21.

[52] Malachi Haim Hacohen, *Karl Popper, The Formative Years, 1902–1945: Politics and Philosophy in Interwar Vienna* (Cambridge, 2000); Paul Feyerabend, *Killing Time* (Chicago, 1995).

Incommensurability, in other words, however often taken for granted as a problem, reveals itself rather as a *solution*, an all too easy way out of problems: instead of confronting them, we deem them insurmountable, label them incommensurable and set them aside.[53]

The coincidence that Feyerabend's "Explanation, Reduction, and Empiricism" appeared in the same year as *Structure* has overshadowed the vast differences between the two men. Like Popper, Feyerabend objected to the quietism of Kuhn's thesis: incommensurability "emphasizes difficulties, dwells on them, makes theories about them instead of trying to get out of them."[54] Drawing on the anticlerical rhetoric of Austria's liberal political tradition, Feyerabend argued in 1962 that "whereas unanimity of opinion may be fitting for a church, or for the willing followers of a tyrant, or some other kind of 'great man,' variety of opinion is a methodological necessity for the sciences and, a fortiori, for philosophy." Tellingly, Feyerabend also insisted on the richness of everyday languages, which "contain a well-developed and sometimes very abstract ontology." [55] These Viennese critics were rightly wary of Kuhn's equation of science with the technical.

In short, the long shadow cast by *Structure* illuminated certain dimensions of science while obscuring others. It highlighted the elements of knowledge production that are local and technical at the expense of those that are distributed and vernacular. Gradually, this balance is being righted by studies that track imperial networks, knowledge brokers, distributed cognition, collective observation, and practices of translation. Still, the Kuhnian framework sometimes intrudes where you would least expect it. Deborah Harkness, for instance, in her path-breaking study of vernacular knowledge practices in Elizabethan England, claims that she is providing a method for studying Kuhnian "normal science."[56] As we have seen, however, normal science in Kuhn's sense is, by definition, conducted in a technical language that is incommensurable with the vernacular. In addition, it is often unclear how these methods might be applied to more recent science. As long as historians do not question Kuhn's definition of modern science as technical knowledge, they are likely to remain deaf to the vernacular critiques and "common sense" that may continue to inform science. The question is how to bring technical and popular science into the same frame of analysis. Fleck suggested an answer—namely that historians consider how the process of "popularization" reacts back on the language and values of science

53 Cited in Gattei, *Thomas Kuhn's "Linguistic Turn"*, 53, original emphasis.

54 Cited in ibid., 137.

55 Paul Feyerabend, "Explanation, Reduction, and Empiricism," in *idem*, *Realism, Rationalism, and Scientific Method: Philosophical Papers*, vol. 1 (Cambridge, 1981), 44–96, 76, 78.

56 Deborah Harkness, *The Jewel House: Elizabethan London and the Scientific Revolution* (New Haven, 2007), 257.

at its technical core. Standing in the way of such an analysis is the Kuhnian metaphor of the "translation" of science between experts and popular audiences. Likening expert–lay communication to translation hides its dialogic aspects. It may even make historians complicit in the political stalemate of the present environmental crisis. As the sociologist Brian Wynne has recently argued, the translation model of scientific communication invites citizens to "sit back, and wait to be told what they must do, rather than go out and learn as well as take their share of responsibility for what could have been presented as a more complex, multidimensional and inherently indeterminate set of human problems, which citizens and their representatives can and should help define."[57]

[57] Brian Wynne, "Strange Weather, Again: Climate Science as Political Art," *Theory, Culture, and Society* 27 (2010), 289–305, 300.

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