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The working, prevalent philosophy of science has repeatedly shaped the way we have seen tools and innovation in science. This chapter is a reflection on the usual ways we have thought about innovation, especially how the blackboard and lab bench purportedly tie together – and what we might do to sketch a more reliable picture.

2.1 Positivism: observation before theory

Early in the twentieth century, a rigorous positivism held sway – a philosophy that emphasized observation above all else, held theory in relatively low regard, and utterly dismissed all that was not bolted securely to that which could be perceived directly. It was a view that dominated for decades among both philosophers and scientists – a perspective that shaped generations of dialogue about scientific propositions as those that were verified, confirmed, or falsifiable when confronted with empirical data. Those early-twentieth-century years were, in many respects, decisive in setting the popular conception of what made something scientific at all.

Ernst Mach, the Austrian polymath physicist-philosopher-physiologist-psychologist, was one of the most famous and productive leaders of the positivist movement in philosophy of science, but he was by no means alone. In his *Mechanics* (1883), he set out his credo about what truly grounded knowledge: “Nature consists of the elements given by the senses. Primitive man first takes out of them certain complexes of these elements that present themselves with a certain stability and are most important to him.” Even the objects around us (according to Mach) are not given immediately as objects – our grasp of them takes work. In the beginning only perception of a more primitive type exists. Mach again: “The thing is an abstraction, the name is a symbol for a complex of elements . . . That we denote the entire complex by one word, one symbol, is done because we want to awaken at once all impressions that belong together. [S]ensations are no ‘symbols of things.’ On the contrary the ‘thing’ is a mental symbol for a sensation-complex of relative stability . . . colours, sounds, pressures, times (. . . sensations) are the true elements of the world” [1, 2].

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Given that for Mach objects themselves were made up of more primitive, more basic sensations, it is to be expected that discoveries, concepts, and theories – indeed all forms of innovations – were (for Mach) composed of such sensations: “[I]t is by accidental circumstances, or by such as lie without our purpose, foresight, and power, that man is gradually led to the acquaintance of improved means of satisfying his wants. Let the reader picture to himself the genius of a man who could have foreseen without the help of accident that clay handled in the ordinary manner would produce a useful cooking utensil! The majority of the inventions made in the early stages of civilization, including language, writing, money, and the rest, could not have been the product of deliberate, methodical reflexion for the simple reason that no idea of their value and significance could have been had except from practical use” (see Ref. [2], p. 264).

As these and many related comments make clear, Mach held it to be obvious that our world, the only world worth speaking of, did not hold already-present things in it. Instead, objects of the world were the result of a laborious and often accidental assembly of that which was actually given to us: elementary sensations. Sensations slowly combined into complexes that made things (including our notion of “self”), and bit by bit the whole of our physical world could be put together. Innovation was crucial, but it did not come intentionally – that would be to put the cart before the horse, and, in Mach’s scheme, cart, horse, and even the notion of putting them together were all composed of sensations and their slow, exploratory, often randomly varied assemblies.

Over the early years of the twentieth century, Mach’s work formed the nucleus around which philosophy of science first condensed. Calling themselves the Ernst Mach Society (*Verein Ernst Mach*), a group of natural scientists, social scientists, and philosophers began meeting in Vienna to sort out a new, “unphilosophical” philosophy. For physicist-philosophers such as Rudolf Carnap and Moritz Schlick (along with many of their allies), the goal was to align the new formal logic of Gottlob Frege and Bertrand Russell with the sense-based positivism of Mach. These “logical positivists,” as they were known, understood science in all its variety to come down to observations logically combined. True, as they knew full well, there were theories, but these, they implied, came and went, as useful shorthand to codify observations.

As far as Carnap was concerned, science was predicated on the accumulation and manipulation of bits of sense-based observations, “protocol sentences” (as he called them) that were quite basic: “smell of ozone, here, 12 pm.” Manipulating, sorting, and composing these blocks into larger assemblies using logical relations (“if,” “and,” “or,” “then,” “there exists,” etc.) would lead to all the rest of real scientific utterances. According to the logical positivists, this rigorous construction was also a test: that which could *not* be constructed out of elementary sensations and logical combination was *not* worth building (not scientific). The unconstructable (or perhaps unreconstructable) residue was the metaphysical, and it should be consigned to the flames. As the troubled 1930s began to unfold, the logical positivists’ claim on the scientific touchstone had enormous appeal: the clear, universally accessible logical syntax of scientific statements could be used to counter the rising threat of propagandistic race talk by the Nazis, with their endless blather about national souls,

race, and mystical obscurantism. High-flying phrases such as “the spirit of the nation” were, for these philosophers, like fool’s gold, unable to stand up to the acid test of real science.

History of science, by scientist-historians, largely followed suit. In 1939, the Unity of Science movement held a massive gathering at Harvard University, under the stewardship of chemist and university president James Conant. A world war later, after having run a good portion of the Americans’ scientific weapons projects, Conant urged the study of science through the positivist-inflected *Harvard Case Studies in Experimental Science* [3]. This was no accident. Having seen the enormous power of technical work during and just after World War II with the rise of the hydrogen bomb, Conant was terribly keen that all students, including – especially – the non-scientists, learn how the sciences worked. It simply would not be possible for citizens of the later twentieth century to carry on their civic lives without understanding what it was that had brought the modern sciences and all their applications into existence. Given that it seemed evident to him that students from outside the physical sciences could not learn the physics of microwave radar or nuclear fission in short order, the older, “simpler” experimental sciences would lead the way.

Boyle’s pneumatics, Dalton’s atoms, Pasteur’s fermentation – these and other examples from the history of science showed, Conant argued, “some of the ways in which the new ideas (concepts) have arisen from observation and experiment and the consequences they have entailed” (see Ref. [3], p. x). How these observations and experiments actually precipitated the theories (“great working hypotheses”) could “best be described by such words as ‘inspired guess,’ ‘intuitive hunch,’ or ‘brilliant flash of imagination.’” Only in the rarest of cases, Conant insisted, do such “great working hypotheses” emerge from “a logical analysis of various ways of formulating a new principle.”¹ Here was a history of science built explicitly and enthusiastically on the foundation of the logical positivists’ dream of a unity of science, a vision of theory cashed out in its entirety in its empirical correlates. Theory was important, true, but it was important not in leading the way in science, but as summarizing its achievements, which were, first and foremost, grounded in the laboratory in inspiration, hunch, and flash.

Discovery might have had its accidental origins (shades of Mach), but it was vouchsafed by the logical manipulation of observational material, organized and simplified by a commitment to mental economy. But, however accidentally it was composed, for Conant, and indeed for just about all the prewar and immediate postwar scientists, historians, and philosophers reflecting on innovation, one thing was clear: new science emerged from the unmoved prime mover – observation. It is of course a gross simplification, but, if one were to construct a cartoon picture of the positivist relation of theory and observation, it might resemble Fig. 2.1. Observations accumulate aggregatively and without break; theories do fall, one after the other, leaving the integrity and continuity of the sciences to the underlying stratum of sense-based observation.

For the logical positivists, Albert Einstein was both model and ally. When Einstein lambasted the notion of absolute space, absolute simultaneity, and absolute time (“there is

¹ See Ref. [3], p. xi.

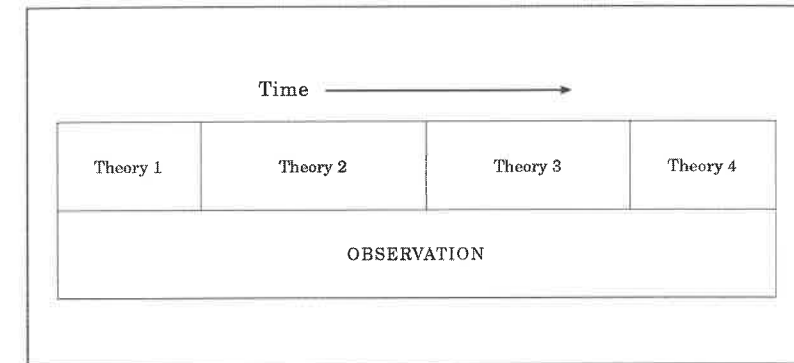


Fig. 2.1. Positivist periodization.

no cosmic tick tock”), his denunciation of the metaphysical fell on attentive ears. Indeed, positivists such as Schlick and his physicist-philosopher ally Philipp Frank both took Einstein himself to be a positivist. That Einstein refused the title might have given pause – but seemed to make little difference (on Einstein and positivism, see Ref. [4]).

On the positivist reading of history, the story of relativity was clear; deceptively so. A long train of observations had pushed the idea of a static ether to the limit of credibility. Experiments such as those concerning stellar aberration and the much more precise Michelson–Morley experiments set limits on our ability to detect motion through the ether. At first, the experiments showed that any detectable motion would not affect the rest of physics by more than terms smaller than v/c , the ratio of the motion through the ether divided by the speed of light. Then even better experiments by Michelson showed that the effects of motion would at most be a factor of $(v/c)^2$ smaller than the rest of the theory. In the case of the Earth orbiting around the Sun, that meant that the ether effects were *at the most* more than a billion times suppressed. Einstein (on this positivist reading) looked at this trend and eventually concluded that the trend was fatal for the ether – the right deduction was that in fact we would never detect motion through the ether. He then simply codified and secured this null result – drawing, as it were, the asymptote to the curve that had been bending for decades – and concluded that there is no ether. For the positivists, Einstein illustrated their motto perfectly. In the beginning, there was observation. Only in the end was there theory (according to the positivist picture): theory could summarize, conclude, even accelerate research, but theory was not the propelling central force of science.

2.2 Anti-positivism: theory before observation

During the 1960s, an inversion of that consensus occurred. Anti-positivism came to rule, and theoretical innovation increasingly came to be seen as the undisputed power of productive change in science. Where theory changed (so the mantra went), it pulled observation and experiment in its powerful wake.

Thomas S. Kuhn was trained as a physicist (he did his thesis with Van Vleck at Harvard) and began his career thoroughly immersed in the world of positivism. His famous book *The Structure of Scientific Revolutions* (first published in 1962) first appeared in Volume Two of the University of Chicago Press series *Foundations of the Unity of Science: Toward an International Encyclopedia of Unified Science*, which was edited and organized by physicist-philosopher Rudolf Carnap and sociologist-philosopher Otto Neurath, along with their philosopher colleague, Charles Morris (among others). But Kuhn rebelled against the primacy of observation, celebrating in his tract a then-new anti-positivism as a thorough-going refutation of the idea that a universal protocol language of observation existed that would bind together any two theoretical structures. On the contrary, he insisted, the switch from Newtonian to Einsteinian physics was one that simply could not be mediated by observation statements alone. Far from it. For Kuhn, physicists speaking “Newtonian” and those who communicated in “Einsteinian” truly had only the most awkward and inadequate rules of translation.

Sometimes Kuhn and his allies deployed the visual metaphor of Gestalt changes – seeing the duck and seeing the rabbit were mutually exclusive. No neutral line element existed in the famous duck–rabbit drawing; every piece of the image played a role in one avenue of perception: a rabbit ear *or* a duck bill, but not both, and always one *or* the other. “Ships passing in the night,” “a conversion experience,” “talking past one another” – in various idioms and in a myriad of ways, the anti-positivists saw science as divided by unbridgeable fjords that separated different islands of theory.

What divided the blocks was clashes over the right framing theory. Instead of a picture of a continuous accumulation of observations, the anti-positivists saw blocks split by a discontinuity in theoretical conceptions. These warring paradigms differed over what the basic objects in the world were, what laws governed their interaction, and how we could come to know them. When theory broke, so too did the world of observations. Even the criteria that picked out certain observations as relevant would change: Newton does not explain why there are five planets (as Kepler wanted); he does not even change the number of planets and try to explain that. No, Newton simply threw out the question – the data were not otherwise explained, they were trashed. One philosopher wrote that “experimental evidence does not consist of facts pure and simple, but of facts analyzed, modeled, and manufactured according to some theory.” Or, to express this as an epigram, that same philosopher cited Goethe: “The most important thing to grasp is that everything factual is already theory” (see Paul Feyerabend [5], cited in Ref. [6], p. 791).

Historians of science joined the philosophers in a renunciation of the positivists’ “observation first.” Instead, they too launched studies to show how theory radically reconstructed instruments, experimental procedures, and even the possibility of discovery itself. One extended study aimed to show that tracks of positrons were there all along in cloud-chamber photographs, but only the Dirac theory of relativistic quantum mechanics made them perceptible. Hence, in very crude cartoon form, we have moved from Fig. 2.1 to Fig. 2.2.

Sure, in the case of relativity theory one could take the limit of $1/\sqrt{1 - v^2/c^2}$ as v^2/c^2 goes to zero and get unity. But, Kuhn insisted, this limit did not take one from Einstein’s

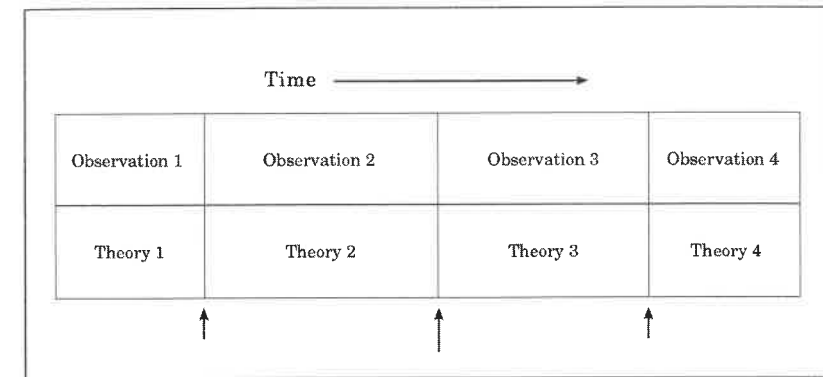


Fig. 2.2. Anti-positivist periodization.

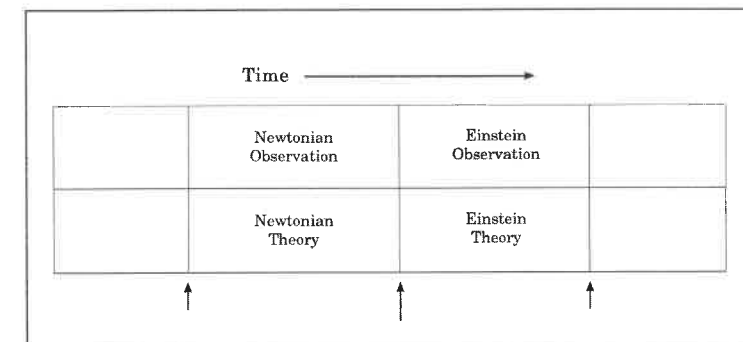


Fig. 2.3. Newtonian and Einsteinian blocks.

quasi-operationalist definition of space (x) and time (t) to the absolute meaning that Newton accorded them. Rulers and clocks, which were, for Newton, merely indicators of “relative” space and time, do not become “true,” “mathematical,” “absolute” space and time in the $v^2/c^2 \rightarrow 0$ limit. No, the anti-positivists repeated over and over again: science is discontinuous, broken into paradigmatic blocks by scientific revolutions. Between blocks, no traffic existed.

Hence, what we are left with is something like Fig. 2.3, a specification of Fig. 2.2 for the instance of special relativity. The old scheme has been turned on its head: in the positivists’ picture of science, observation came first and stood as the ground on which all else was built, whereas for the anti-positivists, theory is at the root, and divisions of theory cause resultant cracks to propagate all the way up into the observations. As one philosopher-historian put it back in 1976, “Operations and manipulations [Kuhn] feels, are determined by the paradigm and nothing could be practically done in a laboratory without one. A pure observation language as the basis of science exactly inverts the order of things” (see Donna Haraway [7], cited in Ref. [6], p. 791).

2.3 Intercalation²

Inversion is a transformation that leaves much unchanged. Indeed, Kuhn's theory-first treatise, *The Structure of Scientific Revolutions*, famously first appeared in Volume Two of Foundations of the Unity of Science, which, as noted above, had been co-edited by the two best known logical positivists of all – Carnap and Neurath. In effect, both positivist and anti-positivist schemes aimed to find a single engine of change in science, the one in observation, the other in theory. Both, I would argue, prepare us poorly to understand important aspects of tools, inventions, and innovations in science in the past, much less those under way now.

As I see the difficulty, two principal and interrelated difficulties face the positivist/anti-positivist alternatives: the problem of language and the problem of instruments.

2.3.1 Language

For the positivists, the hunt for a universal language was a matter of the highest importance, both politically and scientifically. Their view was that natural languages often did nothing but divide peoples, served to encourage civilization-threatening nationalism, and had to be countered (certainly this was Carnap's view) with a concerted effort to foster such universal languages as the simplified jargon Basic English. Philosophically, Carnap and Neurath also both believed that language had unnecessarily divided philosophical approaches. In one of his early books (*The Logical Syntax of Language*, 1934), Carnap argued that one could effectively translate between different modes of speech and that the formal mechanism of correspondence between these modes would resolve many "pseudo-problems" that had long set philosophers at loggerheads. Scientifically, the key for both Carnap and Neurath was the development of a "protocol language" that would cut across all philosophical theories and provide the observational base against which all might be compared.

Kuhn explicitly and emphatically rejected the positivists' picture of language. Instead of a universal *passe-partout* protocol language (in fact many such languages), he insisted that *no* complete and effective translation between theories existed. The language of a chemistry that included oxygen was simply and irreducibly untranslatable into the language of phlogiston. True, one could awkwardly and piecewise shift one into the other, but such efforts were fundamentally limited: only in a very limited and awkward sense was phlogiston translatable into "absence of oxygen."

It seems to me that what we faced in the transition from positivism to anti-positivism was an overreaching reaction: *yes*, the idea of a protocol language utterly devoid of theoretical presuppositions strains credulity, but *no*, the absence of a language valid for all theories and all times does not imply that every scientific theory is utterly untranslatable into another. Or, put more formally, accept for the moment that no single observation language exists that translates between any two scientific theories; this does not rule out scientists finding

² For more on inter-languages in science, see Ref. [6], especially Chapters 1 and 9.

experimental means of comparing two specific theories. An observation language does not exist for two arbitrary theories x and y , but for any two proximate theories x and y there may well exist a set of experiments with which to evaluate them. (Logically: "there exists for any" is not the same as "for any there exists.")

Let's come back to language. Kuhn, in his *Structure*, compares speaking Newtonian and speaking Einsteinian with the translation between distinct natural languages such as French and English. But this is problematic for at least two reasons. First, no Newtonian committed to absolute space and time couched in theological terms was trying to speak to Einstein and his followers. Those Newtonians had been dead for a good 200 years. What *was* on offer against Einstein's special theory of relativity was a host of "new mechanics" by Lorentz, Poincaré, Bucherer, Abraham, and many others. These various theories made predictions for the path of an electron through crossed electric and magnetic fields – and all of them, Einstein included, expected that the experiments would eventually have something to say about which theory best fit the data. Einstein, it should be said, had suggestions for improving the experiments – at a certain point he doubted that one of the experiments was using quite so effective a vacuum as advertised, but he never once said that such experiments were in principle incompetent to litigate between predictions. Does that mean that statements about effective transverse mass in these electron-scattering experiments were "theory-free" in the sense of protocol statements? Of course not.

Hence, if neither the universal – even transcendental – concept of observation statements *nor* the "island empire" view of scientific languages as utterly disjunct holds good, then how should we understand the local, particular role of the language of experimentation vis-à-vis theory? We need to look at what often *actually* happens at the boundary between natural languages on the ground. Where seafaring fishing cultures encounter fixed village people who cultivate crops, they develop coordinative jargons to effect their trades. Where French met the Native American language, Miskito, and the West African languages (Akan, Igbo, and Twi), a hybrid language, Belizian Kriol, evolved. In general, anthropological linguists have studied, classified, and characterized a whole series of languages that range from very particular purpose-specific jargons through more elaborated pidgins to full-fledged creoles that are sufficiently articulated and structured to allow one to grow up in them.

It is something like this that we need in science when we want to analyze what is happening, for example, when chemistry and biology began their fateful encounter. First, a few terms and techniques were coordinated, and then a more elaborate set of procedures, experiments, and joint work were developed. In the long run we have biochemistry – drawing on both "parent" scientific languages, but rich enough to support the full gamut of intellectual and institutional structures from graduate programs, journals, and conferences to the professional identity "biochemist." I think of the formation of these hybrid fields of work as "trading zones," real and virtual spaces where different scientific cultures make highly productive inter-languages and combine laboratory techniques.

Broadening our picture of scientific language to allow for these intermediate and joining languages would be very useful in understanding innovation and invention in science – it

would give us the beginning of a vocabulary sufficient to go beyond the simplistic labels we often use: “collaboration,” “symbiosis,” “interdisciplinarity.” It is not that these terms are wrong, but rather that they just relabel what it is we want to understand, rather like Molière’s dormative power. Instead, we could begin to ask much more interesting questions: Where do the calculational techniques come from? For example, are they borrowed from biology or chemistry? From where does the scientific vocabulary emerge? What kind of equipment is used, and is it principally taken from previous biology or earlier chemistry? Contrary to the positivists’ view, innovation is not just a recombination of experience. Contrary to the anti-positivists’ view, innovation is not simply driven by revolutions in theory. The fabric of scientific languages is woven in complex ways, often from very different scientific fields with their various languages. But even language, too narrowly conceived, is not enough.

2.3.2 Instruments

Flipped images though they are, positivist and anti-positivist descriptions of scientific invention and innovation are similar in another way. Both are at pains to speak of theory versus observation. Observation, however, is only a very particular rendition of the enormously heterogeneous category of experimentation. After all, experimentation in physics could mean tinkering with a new device, running a billion-dollar detector at the Large Hadron Collider at CERN with 2,500 collaborators, or it might refer to a painstaking nanotube manipulation in a clean room. Worse, the label “observation” obscures the often distinct role of instrument designers and makers.

In physics, at least in many of its branches, it is often quite useful to distinguish three subcultures of the field: theorists, experimentalists, and instrument makers. Of course, there are overlaps, and in some subfields of physics the same person may be both theorizing and experimenting. Still, some journals specialize in instruments, others focus on theory, and still others take experimentation as their *raison d’être*. By subculture I mean to say that all three arenas recognize themselves as participating in a broader culture of physics – but nonetheless maintain a degree of quasi-independence. When physicist Carlo Rubbia and engineer Simon Van der Meer shared the 1984 Nobel Prize in physics for the work leading up to the discovery of the weak bosons, the W and the Z, the Prize Committee stated that they wanted to recognize not only the planning and direction of the experiment, but also the immense operation that this kind of physics had become.

More broadly, it is possible to parse the history of physics and its innovations in terms of these subcultures, as well as their interaction. Take particle physics. We could follow the development of theories from Maxwellian electrodynamics through Einsteinian relativity, quantum mechanics, up through quantum electrodynamics, and out through the vast broadening of quantum field theories. Such an account would follow the grand periodization of theory, taking as its break points such moments as 1905 (special relativity), 1915 (general relativity), 1926 (quantum mechanics), and so on. Or, one could follow experiments and

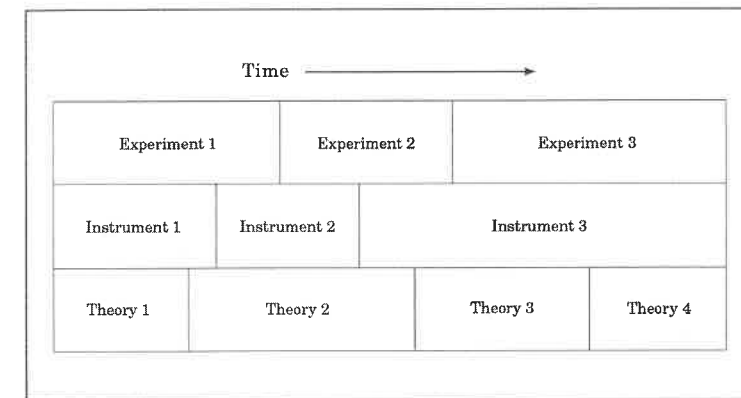


Fig. 2.4. Intercalated periodization.

the discovery of particular objects and interactions: the electron, alpha particles, nucleons, fission, quarks, gluons, and weak neutral currents.

But theories and experiments would not exhaust the physics; one could, and in my view should, understand the history of experimental means (instruments) as having its own, partially autonomous history [6]. We could distinguish an *image tradition*, ranging from the cloud chamber and nuclear emulsion to the vast industry of bubble-chamber physics in the 1950s, 1960s, and 1970s. We would see as well a *logic tradition*, a form of experimental work based more on electronic counting and sorting that would begin with linked Geiger–Müller counters back in the early 1930s and progress through the spark chambers and into the ever-more-elaborate wire chambers. Each had its own advocates, its own techniques, its own lore – and eventually, starting in the mid 1970s, the two traditions began to merge, forming electronic images. It may well turn out that this story – the story of the creation of the manipulable digital image – is the major scientific event of the late twentieth century. Medicine was transformed by nuclear magnetic resonance imaging (MRI), computerized axial tomography (CAT/CT) scans, and functional MRIs. Astrophysics is hardly imaginable without digital optical, infrared, and other imaging techniques joining the traditional purview of observational astronomy to the techniques of radioastronomy. Geology also formed its own trading zone, binding traditional morphological techniques to seismic studies in their own form of seismic earth tomography. If one were to construct a new cartoon picture by analogy to the earlier ones, it might resemble the intercalated periodization shown in Fig. 2.4.

Hybrid instruments drawn from different traditions were certainly not linguistic in the narrow sense of the term. However, I think it is worth expanding our notion of trading zone to include not only terms, but also material objects. In this way we could think of science as more capacious: physics, on this reading, would be seen as a constantly evolving set of trading zones among and within the different subcultures of instrument making, experimentation, and theorizing.

It seems only fair to return one last time to relativity theory, to see how it might look different from both the positivist's and the anti-positivist's accounts. Take the case of Poincaré's trajectory toward his understanding of time and simultaneity – complex enough to illustrate the point, not so involved as that of Einstein [8].

Contrary to his image as a head-in-the-clouds mathematician, Poincaré had superb engineering training at the Ecole Polytechnique in Paris, where he both studied and later taught. Among his many technological engagements, Poincaré was, during the late 1890s, a member of the Paris Bureau des Longitudes. One of the major tasks of this organization – perhaps its single greatest challenge – was the determination of longitude at faraway points so that the world's great landmasses could be mapped. Now finding latitude is a relatively easy affair: in the Northern Hemisphere, exaggerating somewhat, it amounts to finding the elevation of the North Star. But, given that the world turns on its axis, finding longitude is a complicated business, involving the comparison of the stars overhead *at the same time* as the overhead stars are located at some distant point. Finding ways of determining distant simultaneity was a principal concern of the Bureau.

At first, longitude finders would carry clocks with them from their starting point. If local time was six hours different from their portable Paris time, then the explorer would be one-quarter of the way around the world. But clocks hate being moved; they despise the vibrations, temperature changes, and alteration in humidity. When telegraphy became possible, and dramatically after the first submarine cables were installed across the Atlantic after the American Civil War, it became possible to shoot time with nearly the speed of light to distant observers. Even then a small correction would be needed – a correction corresponding to the fact that, however small a time delay, light took some time to get from, say, Paris to New York City or Washington DC. Thus, it became routine to take this time of transfer (using round-trip measurements) into account when finding longitude.

Poincaré knew all this – he had to. In 1899 he was elected to direct the Paris Bureau of Longitude. Hence, when he penned a famous *philosophy* article in January 1898 on the nature of simultaneity, defining it as the coordination of clocks by the exchange of an electromagnetic signal taking into account the time of transfer, this was not merely a thought experiment. It was, in effect, a very specific trading zone in which signal-exchange clock coordination was *both* a piece of very practical cartographic work *and* a philosophical argument for the this-worldliness of the nature of time and simultaneity. But, in 1898, Poincaré's argument had nothing at all to do with the electrodynamics of moving bodies. This is quite clear: in 1899, he showed (in a Sorbonne physics lecture) that Lorentz's new electrodynamics with its (purely mathematical) notion of local time ($t - vx/c^2$) does *not* lead to a significant change from "classical" electrodynamics – so Poincaré stated that he would ignore the correction. The two streams (philosophy/technology and the electrodynamics of moving bodies) had not yet crossed, as shown in Fig. 2.5.

In August 1900, Poincaré gave a major philosophical talk at a philosophy convention re-emphasizing his philosophical argument (still no physics). Only in December 1900, after Poincaré had gone back to a thorough review of Lorentz's work, and after *personally* writing back and forth to the British about the Paris–Greenwich time-coordination effort

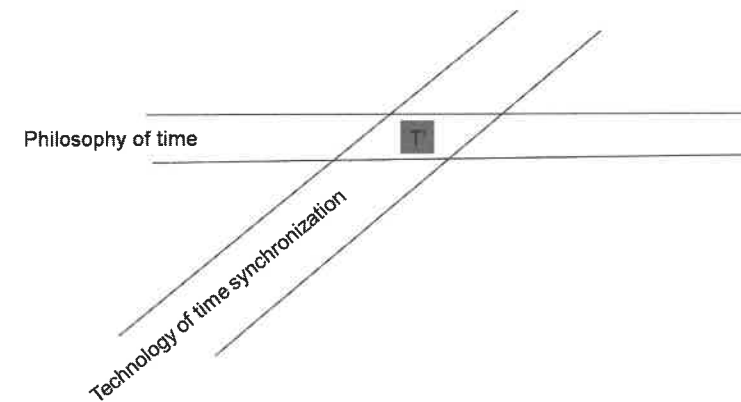


Fig. 2.5. Crossing of simultaneity – philosophy and technology (Poincaré, January 1898).

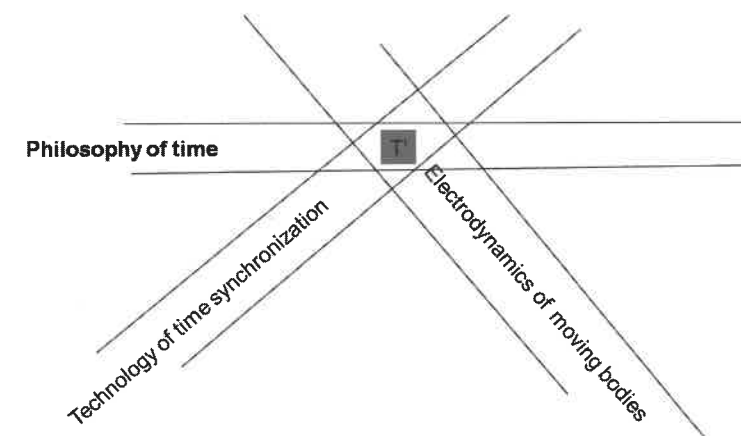


Fig. 2.6. Crossing of simultaneity – philosophy, technology, and physics (Poincaré, December 1900).

(already in his prestigious position as director of the Bureau des Longitudes), did Poincaré reverse course by re-interpreting Lorentz's local time. Given that in that December 10, 1900 talk the philosophical–longitude account of the necessity of signal-coordinated clocks is extended to the case in which there is motion and signaling through the ether (which is how he re-interpreted Lorentz's local time), we can see that the original philosophical and technological insight of January 1898 was part of the intensification of interest in coordinated clocks as a defining feature of Poincaré's emerging understanding of simultaneity talk. Viewed this way, the French physicist's homage to Lorentz of December 10, 1900 was a confluence of these three streams: philosophy, technology, and physics, as shown in Fig. 2.6.

Hence, should Poincaré really be seen as deriving his abstractions from observations? Or should his observations be seen to be fixed by his theoretical presuppositions? Neither

all-too-simple account helps much. Poincaré stood at this triple intersection, moving in philosophical, technological, and physics circles. It is this combination, this joining of concerns in a trading zone, that made possible his innovation in the nature of time. Does this mean that technology, physics, and philosophy lost their separate identities? Of course not. It means that in this restricted region – this arena of time coordination – his thinking and innovation drew from all three streams.

2.4 Conclusion: trading-zone innovation

Before ending, I would like to come to another intersection, one very much on our minds here – that of Charles Townes with tools and innovation. In one interview, Townes, reflecting on the work he had done during and just after World War II, said this: “Now Columbia University . . . had worked on magnetrons during the war, due to Rabi . . . who had been up at the MIT Radiation Laboratory. They had a lot of microwave equipment in the K band region and the X band region . . . they had a lot of equipment. They were eager to see that it was used well, and . . . this was why they wanted me to come. And I was glad to come because they had all of this equipment so that I could get started very fast . . . Both [Bell Labs and Columbia] were well equipped with surplus war materials” (see Ref. [9], p. 59).

Tools mattered. They did not depend on a particular theory – neither Columbia nor Townes himself was out to test one particular problematic area of theoretical physics. Nor did these microwave tools emerge from a raft of theory-independent observations in need of a summarizing theory. Instead, and this is, I believe, very often the case, a horizon seemed to be opening through a new constellation of instruments. That being said, work with instrumentation was not blind to theory. According to Townes, those who thought about stimulated emission and amplification tended to be physicists, while those who thought about the wave aspect of microwaves were primarily engineers. Great physicist though he was, John von Neumann (according to Townes) thought about photon avalanches, but not about feedback sharpening up the line or allowing a continuous generation on a discrete frequency. Townes, among very few others, stood at that particular intersection where the physicists’ quantum effects and the engineers’ wave phenomena crossed. It was the *combination* of stimulated emission *and* feedback with coherence that was important in the maser. Stimulated emission was being discussed widely among several key physicist colleagues, but, as Townes put it, “the physicists had been so . . . thoroughly taught to treat photons as photons and discrete particles that they simply weren’t looking at it very much as a continuous wave, which an engineer had been taught to do.”³

³ On von Neumann and coherence, see Ref. [9], p. 90. Townes: “I went over the problem that one needed to produce very small structures, in order to get resonators . . . At that point, I realized, well, there were natural resonators, and I’d already been thinking about spin resonators as a possible resonance circuit, the natural resonators really were molecules. Yet molecules, I had told myself before, could never generate anything above black-body radiation. And suddenly, I realized, that really isn’t true. After all, they don’t have to be in thermodynamic equilibrium . . . populations could be inverted, and they can amplify, and – well, but then how do you interact with them? I thought of a cavity . . . in sending a molecule through the cavity . . . separating states and . . . and you had an ideal system, and it didn’t take very long to calculate . . . [I]t would just about work” (see Ref. [9], pp. 79–80).

Again and again, physicists had worked with photon avalanches, population inversions, stimulated emission – but not in combination with ideas of coherence and feedback. “I think,” Townes contended “that [coherence and feedback] came out of electrical engineering, my own contacts with circuitry, electrical engineer oscillators . . .” (see Ref. [9], p. 91). Innovation simply isn’t reducible to models of pure inference or pure deduction; there is a combinatorial aspect to making something new that involves a putting together of domains, interests, constraints, and resources that, so often, were held apart.

In understanding Townes’s work on coherence and feedback, it simply isn’t helpful to make the physics or the engineering approach primordial – it only obscures the history to gloss Townes’s position *either* in terms of an overarching theory that determined everything *or* in terms of a series of “observations” that led, inexorably or even accidentally, to the theory and realization of the emission of coherent radiation. The whole point – the essence of this kind of innovation – is that Townes was, at that moment, with that equipment and that set of concerns in Columbia’s physics department, straddling the two cultures: a physicist interested in the quantum states of atoms and an engineering-inflected scientist emerging from years of thought about miniaturizing and controlling the resonant cavities of magnetrons and their associated microwaves.

Looking around us, this kind of trading-zone innovation may well be characteristic of our era. Think of the nanosciences, where surface chemistry, electrical engineering, and atomic physics have combined in an ever-shifting configuration. At stake are new objects, nano-scale devices, that sit at the boundary of science and engineering. At the opposite end of the abstract–concrete spectrum lies string theory – there, too, there is an ever-widening trading zone in which algebraic geometry and quantum field theories have found common cause, borrowing bits of both and not only producing novel theoretical-physics objects (rolled-up dimensions, branes), but also bending what a previous generation might have understood as utterly incompatible physical or mathematical intuitions or the seemingly unbridgeable gap between mathematical proof and theoretical demonstration.

If we are going to understand the remarkable play of disciplines, techniques, and tools that marks the contemporary scientific scene, if we are going to grasp innovation, we are going to need a picture freed from the reductive schema that divide science into theory and observation. We’ll need to be able to handle not only the complex skein of constantly recombining technical traditions, but also the even more complicated ways in which science sometimes crosses into realms of thought far from the laboratory bench.

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3

The future of science¹

FREEMAN J. DYSON

3.1 Optical SETI

We are here to wish Charlie Townes a happy birthday and to thank him for his many brilliant contributions to science and to education. I am particularly grateful to him for one of his less famous contributions, the invention of optical SETI. Optical SETI means searching for extraterrestrial intelligence by looking for optical flashes in the sky. The SETI enterprise was started by Philip Morrison and Giuseppe Cocconi, who suggested in 1959 that we should try to detect alien friends and colleagues in the sky by listening to their radio signals. Frank Drake and Otto Struve lost no time in starting a radio SETI program at the Green Bank Observatory in West Virginia, listening for signals that might have been transmitted by aliens in orbit around a few nearby stars. The radio SETI enterprise has continued to flourish from that time until today, with searches of ever-increasing scope and sensitivity as our data-processing technology improves. A big new radio SETI observatory is under construction at Hat Creek a couple of hundred miles to the north of Berkeley. But already in 1961, only two years after Cocconi and Morrison and only one year after he had invented the laser, Charlie Townes suggested that searching for laser radiation would be another good way to find aliens (Schwartz and Townes, 1961). He observed that laser beams would be about as efficient as radio transmitters for communication over interstellar distances. There was no strong reason for the aliens to prefer radio to laser communication. A well-balanced SETI program should cover both possibilities.

It took a long time before Charlie's suggestion led to any action. The technology of lasers took a long time to catch up with the technology of radio. A few pioneers began sporadic optical SETI programs in the USSR and America. Finally, Paul Horowitz at Harvard and David Wilkinson at Princeton organized an optical SETI collaboration, with a telescope at Harvard and a telescope at Princeton working together, carrying out a systematic search that is still continuing today. Sad to say, David Wilkinson died soon after launching the

¹ An excerpt from this chapter also appeared as an article entitled "Make Me a Hipporoo" in *New Scientist*, February 11, 2006, pp. 36–39 (www.newscientist.com) by mutual agreement of the publishers.