

In Conversation with Bruno Latour: Historiography of “Science in Action”

Bruno Latour’s *Science in Action*, published in 1987, is a guidebook for a scholar – historian, anthropologist, or sociologist – who wishes to “follow scientists and engineers through society.”¹ Latour leads the reader to several construction sites of “facts and machines,” urging the reader to open the blackbox of science and see the science in action.

How do we follow scientists and engineers? Latour suggests that we follow the “cycles of accumulation.” In his favorite example of cartography, the cycle starts with sending out an explorer, in his ship fully loaded with equipments, bearing a mission of drawing a complete map of the remote land. The explorer arrives in a remote land, meets with native people, draws a map on notebooks and sketchbooks, leaves the remote land, and finally returns to the metropolitan center with a map in his hand. The next explorer is sent out, this time not only with ships and equipments but also with maps drawn from the previous expedition. He comes back with another,

¹ Bruno Latour, *Science in Action: How to Follow Scientists and Engineers through Society* (Cambridge, MA: Harvard University Press, 1987).

arguably better, map. A new map is added to the existing piles of maps. Science, of which cartography is one branch, is none other than these repeated cycles of accumulation, Latour argues. It doesn't have to be people that are sent to draw maps or to "bring the lands back" to the center, and an expedition is not the only type of the cycles of accumulation. We can send instead probes to the inner surface of the earth (geology or seismology), build a huge particle accelerator (physics), set up an observatory to bring the atmosphere or the space back (meteorology or astronomy), attach an electrocardiograph to a patient's body (cardiology), or even send out inquiries with series of questions into the whole population (economics or sociology) (L: 225-7).²

Every part of this cycle matters, but Latour gives more emphasis on drawing a map than on other segments of the cycle. To Latour, a more general term for map-drawing is "inscription," and scientific instruments are equated with "inscription devices." He defines an instrument as follows: "any set-up, no matter what its size, nature and cost, that provides a visual display of any sort in a scientific text" (L: 68). Behind all scientific texts lie inscriptions, and all inscriptions are produced through instruments. The notion of scientific practice as inscription making constitutes a central element of Latour's trip to follow scientists and engineers. What is science in action? What do scientists actually do on an ordinary day in laboratory? They make inscriptions, Latour would answer. They make a cascade of inscriptions. An n th order inscription is abstracted into the next level, $(n+1)$ th order inscription. The inscription of the final order is what we see in scientific texts as diagrams, photographs, maps, equations, or tables.

² In this essay, the following abbreviations are used for the in-text notes. L: Latour, B: Burnett, G: Galison, and K: Kaiser. For example, (L: 225) refers to Latour, p. 225.

Latour's research program has influenced a generation of historians of science, who have produced many studies offering novel insights into the practice of science and engineering. In this essay, I examine how Latourian scheme – his notion of “cycles of accumulation” and his emphasis on inscriptions – have been adopted, exemplified, modified, or criticized in subsequent works in the history of science. The books selected here, though they differ in their particular topics, time periods, or main arguments, are in conversation with Latour in one way or another.

One point should be made clear before I begin my discussion on science as inscription or science as drawing. As I understand Latour, there is a distinction between *inscription* and *representation*, although one might say that to inscribe nature is to represent nature. There have been studies on scientific representation, so to speak. Many of them deal with the question of how nature, or a particular view of nature, is represented on prints, photographs, or films. They examine the representation of nature in scientific articles, science textbooks, scientific atlases (astronomical, geographical, medical), National Geographic documentaries, Hollywood films, or even commercial advertisements. Often lost in these studies, however, is the notion that all of these forms are the final products of many layers of inscriptions. While much attention is being paid to scientific representation at the final level, the intermediate process of scientific representation – the making of inscriptions through photographing, editing, tabulating, or simulating – is often overlooked. We should not merely ask what is represented or how it is represented at the last stage. Latour argues that we need to understand how a representation is *produced* or *manufactured* through many stages of inscription. The books I discuss here are concerned with just that: how scientific inscriptions are produced, transported, distributed, tested, and contested.

Sending Out People and Probes

Since cartography, or Portuguese expedition for map-making, was Latour's favorite among many "cycles of accumulation," it seems appropriate to start with D. Graham Burnett's *Masters of All They Surveyed* (2000).³ Burnett sounds very much like Latour when he writes he is "particularly interested in how certain explorers – lightly equipped, underfinanced, more or less solitary – struck out into difficult environments, spent months and even years wandering in the interiors of continents far from home, and came back with maps, maps that ended up on negotiating tables in London, Paris, and Berlin" (B: 3). Burnett notes clearly that "the obligation of a geographical explorer was to return from a cartographic blank with a map" (B: 86). When the historian follows an explorer's traveling cycle, the science (of geography in this case) ceases to seem to be diffused "from metropolis to periphery" and, instead, "ideas, instruments, whole disciplines appear to have been carried *back* to Europe" (B: 6, my italic). Then Burnett's central question becomes this: how maps were drawn in the periphery and brought back to the center.

This seems to be a meaningful suggestion in the historiography of maps. There had once been histories of maps as straightforward descriptions of geographical nature, in which pioneering explorers were often characterized in a hagiographic way. On the other hand, more recently a new way of looking at maps was suggested: maps as texts that can be read and deconstructed. In these revisionist histories, the heroic explorers are not given a prominent place they had before. Burnett's suggestion is to put the explorers back in the field and constantly

³ D. Graham Burnett, *Masters of All They Surveyed: Exploration, Geography, and A British El Dorado* (Chicago: University of Chicago Press, 2000).

connect their traces on the land back to the well-polished maps on the table at the center. Yes, maps are texts and symbols, but they are not disembodied signs floating randomly. They are painstakingly crafted through “the explorer’s active engagement with the land” (B: 6-13). How did the explorer, then, manage to make reliable inscriptions as he proceeded through the land?

Instruments: Uses and Meanings

As Burnett shows throughout his book, the actual making of maps is a task much more complicated than that in Latour’s schematized cycle. Whereas Latour’s explorer Laperouse only stayed on shore and drew a map there, Burnett’s protagonist Robert Schomburgk had to step *into* the lands of British Guyana, performing a traverse survey. Crucial to understanding this next segment of the cycle is the instrument. Schomburgk’s stepping into Guyana is analogous to a scientific instrument’s probing into nature in both literal and figurative senses. Literally, Schomburgk carried many kinds of instruments that were simply indispensable in finding his location and route.⁴ Good instruments and accurate measurements were a significant part of the Humboldtian ideal of science that many explorers were eager to follow. Burnett writes that instruments “became the symbol of the scientific traveler himself and inseparable from his scientific identity” (B: 93).

Figuratively, the geographical explorer who is equipped with measuring instruments and stepping into unknown lands is found in any scientific discipline. Virtually no scientists face

⁴ These included chronometer, reflecting telescope, sextant, theodolite, azimuth compass, artificial horizon, pendulum, thermometer, barometer, electrometer, and hygrometer (B: 94).

nature with their bare hands and eyes, whether in the laboratory or in the field. When they do, they simply can't function as scientists. If stripped of instruments, they are no different from non-scientists who have read some science textbooks. Latour puts it this way (though in a different context): "You can very well claim that Ohm's law is universally applicable in principle; try in practice to demonstrate it without a voltmeter, a wattmeter and an ammeter" (L: 250). Whenever scientists investigate into unknown aspects of nature, they need to be equipped with proper instruments. In this respect, the distinction between instruments for traverse survey and other laboratory sciences becomes less significant than their similarity as mediating apparatus. The instruments are the interface between the nature and the scientist, where inscriptions are produced. This sounds like a common sense, but the instruments as the meeting point of the physical world and the paper world have not commanded historians' attention as much as they deserve. If you want to study a scientist, find out first what kind of instrument he or she is using, and then observe what the scientist does *to* the instrument and *with* the instrument. This is Latour's message.

Peter Galison's *Image and Logic* (1997) does this with detectors in microphysics.⁵ *Image and Logic* is often cited for its concluding chapter on the now famous "trading zone," a space where different subcultures in sciences interact at the local level despite their potential disagreement over more fundamental issues. But *Image and Logic* is, more than anything else, a history of detectors in physics and of physicists' relation with detectors. By definition, detectors – cloud chamber, bubble chamber, etc. – detect things and phenomena that would not be detectable by bare human senses. They therefore "mediate between the microworld and the

⁵ Peter Galison, *Image and Logic: A Material Culture of Microphysics* (Chicago: University of Chicago Press, 1997).

world of knowledge” (G: xviii). One might even say that nature shows itself to us only through various detectors, producing pictures or numbers (although Latour wouldn’t like this model of nature dictating to humans). By attending to detectors, Galison is directly addressing the question raised by Latour: how to follow scientists in action with their peculiar instruments. Galison asks, “Why were these instruments created, reproduced, and used in particular ways in particular places?” (G: 5)

However, detectors do more than that. For they are located “at the intersection of epistemology, physics, technology, and the social structure of physics.” By their unique place, “they fix what it is like to *be* an experimenter” (G: xviii, original italic). Galison writes that experimental physicists’ changing relationship with their instruments reflects, and at the same time constitutes, the change over time in their professional identity. There can be more than one way to characterize this relationship between the instruments and their users. When Burnett examined Schomburgk’s employment of instruments, the keyword was *discipline*. What was needed for a successful exploration was “more than a good instrument.” “A good man” was also essential, since “good observations were contingent on good discipline” (B: 99, 103). Burnett rightly emphasizes the discipline Schomburgk maintained (or claimed to maintain) to himself and his crew. Part of the discipline was drawn from the naval tradition of navigation, which ensured safe, disciplined passage through land and sea. No less important was the strong, even “monastic,” self-discipline in attending the instruments, which let the explorer “serve as the amanuensis for nature, ... a transparent agent for the transcription and transmission of positions” (B: 105). In short, the discipline you establish in yourself and your workers in relation with the instruments will determine how accurate and reliable your exploration/inscription can be.

For Galison, the keyword to examine the relation between experimental physicists and detectors is *control*. At stake here is the control over the particular “machines of physics” and over the experiments in general. Thus Galison summarizes the history of twentieth century physics in terms of control:

It has been a long, irregular, and often broken road between a time when it was unthinkable that a physicist be anything *but* someone who built equipment, designed procedures, manipulated experiments, wrote up results, and analyzed them theoretically to a time when it would be a matter of near-universal consent that someone could count himself (or, more rarely, herself) as an experimenter while remaining in front of a computer screen a thousand miles from the instrument itself (G: 5, original italic).

New instruments and techniques such as nuclear emulsion brought into physics new actors who in turn began to weaken physicists’ control over experimentation. These included chemists, “unskilled workers” such as women scanners, and also industrial companies. They began to produce materials and devices for experiments or even to make crucial observations for physicists. The work of these new actors in physics could no longer be entirely controlled by physicists. As physics grew bigger, it moved beyond individual physicists’ supervision. “With the loss of control came anxiety,” writes Galison (G: 233). “The anxiety of the experimenter,” according to Galison, has been a very significant characteristic of experimental physics throughout the twentieth century. It first began as a question regarding new machines and new people. However, as the simulation technique entered the main arena of physics toward the end of the century, their anxiety about the meaning of experiment came to border upon “the *alienation* from what they considered the classical activity of experimentation” (G: 45, my italic).

One may ask this question: what would Latour think of Galison’s seriousness with the “anxiety” or even “alienation” of the experimenter? The answer can only be speculative here, but it might help understand some differences between the perspectives of Latour and Galison. On

the successive developments of new detectors, Latour would comment simply that scientists become more and more powerful by forging associations, or networks, with better and better instruments. Constructing bigger, better machines concentrates resources onto scientists' laboratory, endowing them with more strength to mobilize more humans and non-humans alike. More sensitive probes and more delicate machines bring stronger allies, empowering those at the center. Therefore, no worries at all.

Galison seems skeptical about the win-win association between human scientists and non-human instruments. Where Latour sees alliance, Galison recognizes tension. If all sciences are basically about making inscriptions, Galison might ask, what happens to a scientist when the inscription process becomes increasingly (and someday, fully) automated but at the same time nearly incomprehensible for an individual? Can the scientist really become more powerful when s/he has little to do for the experiment? Can the scientist really make associations with non-humans when s/he has little control over them? Galison also disagrees with Latour's characterization of scientists as gender- and class- neutral. Latour doesn't pay attention to the relation between, for instance, physicists (most of whom are male supervisors) and scanners (most of whom are female unskilled workers). Women scanners may be seen as just another group of allies in Latourian scheme, but to Galison they raise more important questions that can connect physics laboratories with outside cultures. Seen this way, machines in a laboratory are not just mechanical devices with specific uses. They are "dense with *meaning*, not only laden with their direct functions, but also embodying strategies of demonstration, work relationships in the laboratory, and material and symbolic connections to the outside cultures" (G: 2, my italic). It is the historian's job, then, to make sense of the instruments' uses and meanings.

Inscribing the Nature?

From instruments full of uses and meanings, let us now move onto the inscribing act, the production of the *n*th order abstractions. The question here is a semiotic one. Science, as much as map-making, is a sort of semiotic practice, which lets you deal with signs and symbols instead of touching directly the messy physical nature. You cannot just go out and deal with the “real” nature every time you want to make a statement about nature. The nature has to be *scaled down* and *inscribed* in order for you to examine it. Then the questions are: how do you establish a non-arbitrary, meaningful connection between the physical world out there and the inscribed world on papers? Or, is there such a connection at all?

Returning to Burnett’s story, how do you tell whether the map on gentlemen’s table is a faithful, reliable inscription abstracted from *terra incognita*? In his favorite cartographic example, Latour says that this is assured “by coding every sighting of any land in longitude and latitude (two figures).” Once all sets of paired codes are sent back, “the shape of the sighted lands may be redrawn by those who have not sighted them” (L: 224). Burnett’s argument, however, is that finding in the jungle a corresponding point of each (longitude and latitude) coordinate on the blank map was never sufficient for those gentlemen. As Burnett aptly notes, “it did not take an explorer to tell you that the point 2° 55′ N by 58° 48′ W *existed*.” “The question was, What stood at that point?” (B: 15, original italic) What was needed in addition to all the paired coordinates was “a fixed point” or “a landmark,” visible and tangible. The explorer’s task, according to Burnett, was not so much being a docile “amanuensis” of nature as capturing and fixing a solid landmark amid the transient landscape. It was not the coordinates of the land that he had to bring

back to the center. There had to be spectacular landmarks that could please and assure the center.

In Burnett's account, inscription was not merely writing down what the land dictates. It often required "active engagement with the land." How could one best relate a point on the paper to a point on the ground? One had to rigorously identify a landmark standing there and then actually had to draw it. Only then, "could the map be reconciled with the ground." Burnett also says, "Only then could the map become a testable proposition" (B: 111). In other words, only by active engagement of an explorer – a probe himself – does the map become a scientific inscription.

Extending this argument into the concluding remarks, Burnett contrasts those who made bodily engagement with the land against those gentlemen in the imperial metropolis who attempted to resolve territorial disputes based only on well-polished maps and documents. Without doubt, Burnett sympathizes with the former and asks, "what it means to 'possess' land where one does not 'dwell.'" Being questioned here is not only the imperial premise. The meaning of scientific inscription (in this case, cartography) is also reconsidered. For when one makes territorial claims with a map while sitting thousands of miles away from the land, one is also making a strong semiotic connection between the physical world and the paper world. The gentlemen in imperial metropolises believed that they could "'divide' and 'hold' land by means of *representations* of the place rather than by *participation* or *engagement* with it, by reliance on *mimetic* artifacts rather than local and dynamic *methexis*" (B: 264, original italic). This is exactly what Latour meant when he explained how a random place, through repeated cycles of accumulation, grew to be the center by "acting at a distance" (L: 222). And this action at a distance is both corporeal and semiotic.

A similar question arose in microphysics, too. How do you maintain connection between the physical nature and its inscriptions? Even before that, what kind of method should you pursue in order to inscribe nature? The image tradition and the logic tradition were two competitors in this endeavor. They were two different types of inscriptions: “golden event versus statistical demonstration, the objectivity of passive registration versus the persuasiveness of experimental control, vision versus numbers, and photography versus electronics” (G: 25). Common to both traditions, however, was the concern about the connection with nature.

C.T.R. Wilson, an important figure in the image tradition exemplifies this concern very well. His ingenious development of the cloud chamber, which was originally designed to reproduce and record the real atmospheric phenomena at a small scale, came from the tradition of “morphological sciences,” of which Humboldt, who appears in Burnett’s story as well, was the ideal model. Victorian scientists who worried about the detachment of scientific theories from the real nature saw the possibility of reconnecting to nature in their studies of geology, meteorology, hydrology, etc. Galison says, when Wilson went to Cambridge, a world center of analytical experimentation, “the morphological combined with the laboratory to yield the mimetic; the mimetic-photographic joined with the analytic to yield the image tradition” (G: 137). As the cloud chamber was assimilated into the analytical tradition at Cambridge and used in researches with little bearings on “the authentic conditions of nature,” however, Wilson began to worry. Is science to reproduce and imitate nature or to dissect and analyze it? What is it that the cloud chamber produces (or reproduces) in the laboratory? A small-scale nature? Or just artificial phenomena? What is a scientist’s job?

These questions intensified as the computer came in and played an important role in

experimental physics. Starting as a fast electromechanical calculator, the computer has gone through many redefinitions: “a piece of the instrument, an instrument in its own right, and finally (through simulations) a stand-in for nature itself” (G: 44). The transition “from computer-as-tool to computer-as-nature” brought about serious consequences and concerns (G: 692). Computers were no longer confined in the duty of calculating experimental data. Experiments were now performed *inside* a computer. But is it really an experiment? Can an experimenter only watch the computer screen and still call himself or herself an experimenter? Where was the nature, or “reality,” that the experimenter had long been supposed to work on? The clear distinction that was once made between the physical world “out there” and the inscriptions it made “on here” was no longer pertinent. And in this perplexing topology of nature, instrument, simulation, and reality, the experimenter seemed to wonder where to stand. The questions of what is inscribed in science and who is inscribing it came to the fore with computer simulations.

But, again, Latour takes a very simple position on simulations: it’s only a matter of degrees of abstraction. If the rehearsal flight in the flight simulator is the n th order abstraction, the actual flight is only the $(n+1)$ th. The n th order abstraction of the flight simulator draws together all the possible information about future flight and calculates them into a virtual reality. In this sense, what simulation does is a *temporal* version of the cycles of accumulation, which usually refer to *spatial* movement in Latour’s other examples. Instead of bringing the land back to the center, the simulation brings the future back to the central computer and lets the future created in the computer. It is another action at a distance. By this action at a distance, when it comes to the actual flight, the space pilot is “not really going into the unknown, as Magellan did crossing the strait that bears his name.” “He had been there already hundreds of times, and

monkeys before him hundreds of times.” For Latour, the (arguably) complete simulation of space flight is only “admirable” (L: 247-8). The computer simulation attracts not merely future information but also money, institutions, monkeys, and people, and in doing so, it becomes the “center of calculation” which produces high order abstractions and creates extended networks of technoscience. The higher degree of abstractions you produce, the closer to the center you approach. Unlike Galison’s experimenter, Latour’s actors never get lost in confusion about who they are and what they do. They simply get more powerful.

Through the concept of calculation, we may now enter the discussion of theoretical physics and of the last book selected for this essay, David Kaiser’s *Drawing Theories Apart* (2005).⁶ At first glance, theoretical physics seems the least appropriate for a study of scientific practice and material culture. To those who think so, however, Latour had this to say: “Doing a history of scientific ‘theories’ would be as meaningless as doing a history of hammers without considering the nails, the planks, the houses, the carpenter and the people who are housed, or a history of cheques without the back system.” Then how to write the history of scientific theories? Latour’s point is that working on theories is none other than making calculations. It is another act of making abstractions, from the n th order to the $(n+1)$ th. There is no such a thing as an “*abstract* theory”; there are only such *abstractions* as made from the act of calculation (“we will abstain from ever using the words ‘abstraction’ and ‘theory’ in adjectival or adverbial forms”) (L: 242-3). As we follow abstractions and calculations, we will be led to the “center of calculation.” Latour gives a clear instruction on how to get there and beyond:

⁶ David Kaiser, *Drawing Theories Apart: The Dispersion of Feynman Diagrams in Postwar Physics* (Chicago: University of Chicago Press, 2005).

[F]irst look at how the observers move in space and time, how the mobility, stability and combinability of inscriptions are enhanced, how the networks are extended, how all the informations are tied together in a cascade of re-representation, and if, by some extraordinary chance, there is something still unaccounted for, then, and only then, look for special cognitive abilities (L: 246-7).

Kaiser seems to accept Latour's method of study (although we will see his disagreement with Latour soon). Kaiser focuses on material and practical basis of theoretical physics: calculations on paper. It is an action, and since it is an action, its style, format, or function is transmitted through face-to-face contact and instruction, i.e. pedagogy. However abstract the theories may get, they ultimately have to be *calculated* and the equations have to be *written down* and all of them have to be taught on blackboards and papers. If one accepts these premises, even theoretical physics can be observed in the way the sciences of maps and detectors are observed. This is the strength of Latourian scheme. It enables one to put different sciences on the same table. The order of appearances of the three books (Burnett, Galison, and Kaiser) in this essay only reflects the different orders of abstraction at which these disciplines (cartography, experimental physics, and theoretical physics) usually operate. Theoretical physicists do not send out explorers or employ detectors, but they nevertheless produce a cascade of inscriptions on paper forms, from scratch papers to published articles, increasing the order of abstraction. Kaiser follows the production, concentration, and distribution of these abstractions and their makers. Theoretical physics now seems more "studiable" than it did before (L: 241).

Immutable Mobiles, or the Fixity of Inscriptions

While Kaiser echoes Latour in following theoretical physicists and observing what they

do with inscriptions, he also modifies one of Latour's key concepts regarding the cycles of accumulation. "Immutable mobiles" (or "immutable and combinable mobiles" as Latour also called them) are those objects produced by inscription and transported back to the center, and then combined with other such objects. A map is a perfect example. It is mobile while the actual land is not. It is immutable while a native man's drawing on the sand is not. So by drawing a map on paper, you bring the remote land back to the center while you are not really taking the actual land with you. This notion of "immutable mobiles" has been widely applied to the study of pictures, photographs, and diagrams in science. Examining the use and circulation of Feynman diagrams in theoretical physics, however, Kaiser raises a question about the "immutability" of scientific diagrams. In fact, "little about the diagrams remained 'immutable,'" writes Kaiser. Physicists have adopted and then adapted the diagram for their own research purposes sometimes much different from Feynman's. Kaiser even argues that it was the diagram's "plasticity," not "immutability," that "furthered their staying power" in physicists' daily routine (K: 281-2).

The "plasticity" or mutability of diagrams Kaiser suggests also enables one to reconsider the meaning of "mobility" as well. If the form and content of the diagrams is not fixed, how can they be transported or circulated securely? For Latour who was concerned only about the immutable ones, the transportation of inscriptions was usually a mechanical issue. It was guaranteed by making sure that the ship does not sink in the ocean or that the electric circuits do not break down. This was not the case for Feynman diagrams, however. The Feynman diagrams' movement was highly contingent on physicists' actual travel from institution to institution. In emphasizing the role of human carriers of diagrams, Kaiser cites Robert Oppenheimer: "The best way to send information is to wrap it up in a person" (K: 60). In fact, it was the "postdoc

cascade” from the Institute of Advanced Study (of which Oppenheimer was the director) that made possible the wide “dispersion” of Feynman diagrams. Where those postdocs moved and taught, there emerged new diagram users. Where they didn’t, the chance was much lower for dispersion. Textbooks for Feynman diagrams, which were also important media for dispersion, came much later than the period of personal contacts. Kaiser’s argument is a major revision for Latour’s notion of diagrams as immutable mobiles. Whereas Latour attributed the power of diagrams to their capacity to travel *anywhere*, Kaiser highlights the instances in which the diagrams could not be dispersed to particular places precisely because the diagram users of the U.S. could *not* travel to those places (e.g. the Soviet Union in the cold war era).

This leads to the criticism of Latour’s another concept, “centers of calculation.” Kaiser argues that the Institute of Advanced Study cannot be considered as an example of “centers of calculation,” to which people and things are supposed to return with inscriptions, thus empowering the center. According to Kaiser, the Institute could establish its prominent status “precisely because its graduates did *not* continue returning to the Institute.” Instead of coming back, they stayed where they went and made the local place a small niche from which the diagrams would be communicated and dispersed anew. In suggesting a new model of diagram circulation, Kaiser is criticizing the imperialist model of scientific practice by Latour, in which every stage is about gaining or losing power and thus about becoming a center or a periphery. Kaiser therefore writes, “Pace Latour, educational missionary work and imperialist exploitation need not be the same thing, past abuses notwithstanding” (K: 111). The apt title of his book *Drawing Theories Apart* also reveals his uneasiness with Latour’s suggestion that science is

mainly about “Drawing Things Together” (K: 7).⁷ Although he may have taken significant insights from Latour, Kaiser ends up refuting many key aspects of Latourian scheme.

“Pace Latour”

The imperialist implication of Latour’s model, briefly noted above by Kaiser, poses an interesting historiographical question to Burnett and others. As I mentioned above, Burnett’s history of map-making in British Guyana fits very well with Latourian model, even without a reminder that cartography was Latour’s favorite example. Latourian scheme has been very effective in explaining the remarkable expansion (or success, some might want to say) of empires. Its explanatory power is not limited to science and technology but applied to every aspect of the empire business. In mentioning the recurring attention to the role of cartography in the making of empires, therefore, Burnett refers to Latour: “by making places portable they [maps] conformed to (and even exemplified) the Latourian notion of the immutable mobiles” (B: 6). To roughly summarize, Burnett’s story is basically about how Latourian cycles of accumulation were completed and repeated in British Guyana. And he is very successful in showing that. At the same time, however, Burnett is critical of the imperial projects that attempted to “possess land” by making “immutable mobiles” and transporting them to the imperial center. As an intellectual, he wants to help undermine the colonial legacy left on the land, though only in an epilogue-style conclusion. Burnett thus suggests, “At the very least we might make an effort to remind

⁷ Bruno Latour, “Drawing Things Together,” in Michael Lynch and Steve Woolgar (eds.), *Representation in Scientific Practice* (Cambridge, MA: MIT Press, 1990), pp. 19-68.

ourselves of *the ephemeral nature of the lines* we draw on the ground” (B: 261, my italic).

Sympathetic with the people of previous colonies who are currently struggling with the imperial legacies, Burnett seems to burden himself with the task of showing the “fragility” of the imperial boundaries. Here, his moral stance, which is the insistence that the imperial lines be erased or at least be redrawn by locals, is somewhat conflicting with his useful historiographical framework that is predicated on the very fixity and immutability of those lines. Is some reconciliation needed?

More questions arise. Would Latourian scheme continue to be inspiring for historians of science, despite its imperialist implication? Would the recent critiques of colonial technoscience undermine the efficacy of Latourian model of technoscience? The answers are to be found in subsequent studies that may accept, modify, criticize, or abandon Latour’s research guide. And I am sure that there will be many of them.

Epilogue: History as Cycles of Accumulation?

Could history as scholarly practice be explained by Latourian scheme? If one dares to try, a historian may be said to send out a probe into the ocean of the archives. Or rather, the historian is the probe itself. Since the historian cannot bring the whole archive back to his office, he prepares inscriptions, taking notes and making photocopies, the historian’s “immutable mobiles.” They are the n th order abstractions from the raw materials of the past to which the historian has no direct access. The historian transports, compares, and combines the inscriptions he made from the archive. The historian then produces the $(n+1)$ th order abstractions when he writes an article

or a book. Articles and monographs are sometimes abstracted into history textbooks, which can be regarded as the $(n+2)$ th order abstractions. Being higher order abstractions, textbooks may not give us detailed, in-depth knowledge, but they have stronger influence on people and society, which is why we debate on them so fiercely. What would be, then, the “center of calculation” of history? If one must specify a center, it would be the historian’s office, to which the inscriptions from archives are brought back. At this point, however, the Latourian explanation of history proves inadequate. Whatever amount of materials is brought back to the center, it is not likely to empower the center very much. The historian always remains weak. What is worse, who would bother to follow the historians in action? [The End]