EXPLAINING CHANGE IN SCIENCE

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Philosophical theories of scientific change abound and, for the most part, they have one thing in common: they are theories of rational justification for changing scientific theories. That is, they are not about science *per se*, where science is construed as a social process whose main activity is the generation and testing of ideas about the composition and structure of the material universe. The kinds of theories of scientific change I have in mind are exemplified by the work of Popper (1959), Lakatos (1970), Kuhn (1962), and Laudan (1977). These are philosophical theories whose focus is a theory of scientific rationality and which attempt to provide a justification for abandoning one theory in favor of another. They often proceed by examining the logic of the language of support for scientific theories. Science ought to change, on these views, when current theories are shown to be defective because of failed predictions, or inadequate evidence, or decreasing problem solving ability. Built into these accounts is the assumption that rational scientists accept theories which meet these conditions. In short, these are all theories in the positivist tradition of the philosophy of science, where the center of attention is the logic of philosophical concepts about science. And, to no one’s surprise, these treatments of the topic of change in science are sterile and unconvincing.

There are also non-philosophical accounts of scientific change—one hesitates to call them theories—that do pay attention to the social processes of science. Good history of science, both internalist and externalist, social and institutional, provides us with much valuable insight into the workings of the sciences. Then there are the sociological treatments of scientific activity. These, in general, are not so helpful, for they ignore the subject matter of scientific theories and the role it plays in the activity of scientists, concentrating only on the scientists, imposing on them a variety of unsubstantiated psychological motivations for their actions.

In this paper, I am not going to worry about history and sociology, although what I am arguing in favor of has need of both done well. Here I am concerned to develop a *philosophical* account of change in and of scientific theories which really is about science. The heart of this project is to see mature science as an historically contextualized social process embedded in a technological infrastructure. A technological infrastructure is a complex set of mutually supporting individuals, artifacts, networks, and structures, physical and social, which enable human activity and which foster inquiry and action. Thus, for any particular technological infrastructure of science, the science is but one component of the technological infrastructure. The other components are, strictly speaking, not science, i.e., do not directly deal with the investigation and understanding of nature. However, without them that particular scientific activity would not be possible at that time and in that place. This implies that the activity we call science needs a social environment, which it does, and that science does not proceed in a vacuum by itself, which it does not, and that the engine of science is technological, not logical or psychological. Scientific change results in a change in the scientific explanation of the structure and functioning of nature. It, in turn, is the result of changes in the technological infrastructure within which the explanations are generated. For example, the launch of the Hubble space based telescope is providing the impetus for the development of new cosmological theories. Likewise, the creation of the technology of gene-splicing paved the way for new theories of genetic development. And, I will argue later, the more sophisticated and mature the science, the more embedded and indebted to its technological infrastructure it will be.

Now, to speak of the obvious, the account given above is loaded with contentious notions. To accomplish the goal of a philosophically sophisticated and historically accurate account of scientific change, I am proposing some new vocabulary and some different ways of conceptualizing familiar issues. Therefore, to begin with I will spend some time unpacking some of the more superficially obnoxious claims. After doing a little philosophical work here with some examples, I will explore some of the unsettling consequences of this explanation of scientific change.

Let me begin by providing some rationale for introducing new terminology and for offering new definitions of familiar notions. Elsewhere I have argued about the evils of reifying technology, science, government, etc. (Pitt, 1999, in press) Reification, making an abstract or general noun into a thing in the world, is responsible for a category mistake with real world consequences. It allows for the misapplication of normative assessments, resulting in claims like "technology is threatening our way of life." Nothing could be more preposterous. Technology is doing no such thing. It is the application by people of specific technologies in certain ways that sometimes creates problems. In short there is some truth to the bumper sticker that reads "guns don't kill, people do." I have simply translated that insight into a general reluctance to talk about "technology" *simpliciter*. I have also gone further than merely displaying a reluctance, I have offered and defended a definition of technology, angering some, which redirects our attention to people, and reduces the emphasis on artifacts; thus, technology is humanity at work.

Now these considerations clearly have ramifications for my main notion regarding scientific change, which is a technological infrastructure. On this account, a technological infrastructure is that assembly of different forms of work relations among people which makes the doing of science possible. To put it in this way automatically includes the people, artifacts, institutions and networks which constitute the environment within which work occurs. Described in this fashion, it entails that appeals to any specific development in a science must be historically contextualized, because science must involve the working relationships which make that particular form of social activity what it is at that time. Thus, there can be no general rule or universal explanation for changes in a science, beyond the recognition that what happened was a function of a multiplicity of factors working at that time. In short it depends on the institutions within which scientific activity occurred and the sources of support for that activity—for example, today it might be the National Science Foundation; in 17th-century Florence it was either the university or the court of the Medici—the people, the politics, social influences and fads, etc. The institutions themselves are the contingent product of a variety of historical and social forces.

At this point I need to interject a caveat to forestall shouts of glee by postmodernists. By recognizing the historical contingency of science, it does not follow that science is, therefore, only one activity among others, none of which can claim some sort of epistemic virtue which allows it to be identified as the premier knowledge producing activity. The evidence, which is all around us, is that, in fact, and let me stress the fact of the matter, in fact, scientific activity, of all our activities, is the best at producing the knowledge which allows us to understand and manipulate the natural world. The historical contingency of any particular scientific success or failure does not undermine the fact that nothing has provided us with the scope and depth of knowledge science has. And by science, I mean the set of activities associated with the totality of specific investigations into the structure and make-up of the universe. Now to return to the topic at hand.

If we are going to talk about scientific change, we need to talk about specific scientific changes and the contexts in which they occurred. But, it might be asked, how do we identify the context? The answer is that if it *is* a specific context, then it will be an historical item, locatable in space and time. The technological infrastructure will then be that set of working relationships without which that specific scientific development could not have happened. (Identifying the context is one thing, understanding it is another. This is where the history and the sociology come in.)

At this point two objections come up: (1) to assume that one can identify factors contributing to certain scientific developments, in the counterfactual context that were one of these factors not present, the developments in question would not have happened, suggests a commitment to a dubious sense of social causation; (2) to claim that if a technological infrastructure is that without which the scientific development could not have happened, then is not the door opened to including everything? Let us consider these in order.

First, I am not proposing an account of social causation. Rather, I am offering a justification for selecting the relevant factors for producing an accurate description of a technological infrastructure. Thus, in the historical context under discussion, given the kinds of mechanisms, tools, tool-makers, groups, patronage systems, etc., that actually existed, is it possible to give an adequate explanation of how what happened happened without including factor x or y? In so arguing, it may be the case that several different causal factors are appealed to, but no one single account of causation is being assumed. Thus, the grant from NSF which funded the laboratory in which the crucial experiment took place is causal, but not in the same way that flipping the switch on the microscope is.

The second objection asks whether we are not opening the door to including everything, since it seems that with a little ingenuity, anything can be shown to be relevant to something. To take a trivial example, if we want to explain the change from a geocentric theory of the structure of the universe to a heliocentric theory, then surely this will require that we not only detail the standard and familiar events (Copernicus and the calendar), players (Kepler and Galileo), institutions (the Medici court and the Catholic church) , but that we also consider such factors as the educational and familial backgrounds of those who supported the change and those who did not, and the political and economic factors that infused their thinking, the geography of the lands they own, the number of servants they maintain, *ad infinitum*. Where do we stop? The garden of Eden?

Obviously this is not a desirable result. Further, since what actually happened in the past occurred in the seamless flow of time, fixing a context will always be arbitrary to some extent. However, the solution to the problem is one which appears naturally when we are setting it up this way. The point to stress is that the relevant factors to be included as constituting any specific technological infrastructure of science are the ones which make a difference as to whether or not the event in question would have happened. When we are speaking of science, two related criteria for selecting relevant factors come to mind: (1) making a difference means making a difference in the epistemic content of the change in question, and (2) explanatory coherence. Let us now look at each of these in turn.

Making an epistemic difference. Remember we are talking about a theory of change in the process of science. So, if scientist X is led by reason of personal ambition to establish his own laboratory rather than continue to work in Renowned Scientist G's laboratory, and X fails to get funding, and no publishable findings are produced, then it is unlikely that this is a factor to be included in the relevant factors explaining the success of Renowned Scientist G's laboratory in discovering a new mechanism. Someone might try to argue that had disgruntled scientist X continued in G's laboratory, given his disruptive personality, the eventual success of the lab would never have occurred. Now that*is* a counterproductive counterfactual, and does not contribute to our understanding of why G's lab produced the results it did. Hypothesizing as to what might have happened does not affect what did happen.

That was a negative example of sorts. Let us look at a positive example. In a complete explanation of the impact of the Hubble space based telescope on cosmology, it will be important to include an account of the resources available to the U.S. shuttle program which made it possible for the needed adjustments to be made to the telescope after it was launched and it was discovered that the main mirror was defective. That is, an adequate account of the new changes that are taking places in cosmology due to the observations of the Hubble would not have taken place were it not possible to fix the mirror. And yes, it is important to relate the fact that the Hubble as launched was defective; otherwise, we relapse into the let-us-only-tell-about-successes mode of history of science, which results in an inadequate explanation of why cosmological theories changed. It is inadequate because it ignores factors relevant to having those changes take place. In particular, it explains the acceptance by astronomers of the findings of Hubble observations and their willingness to allow those findings to force changes in their theories. For if the mirror had not been repaired, then the value of the resulting observations would be diminished. That it was repaired, using already agreed upon techniques, is very important. It made it possible for the Hubble telescope to be calibrated. And as Alan Franklin (*Perspectives on Science*, 1997, 5:1:31) argues,

> Calibration, the use of a surrogate signal to standardize an instrument, is an important strategy for the establishment of the validity of experimental results. If an apparatus reproduces known phenomena, then we legitimately strengthen our belief that the apparatus is working properly and that the experimental results produced with that apparatus are reliable.

If the Hubble could not be calibrated, then no scientific results would be forthcoming. Important for our purposes is recognizing that the calibration of instruments is crucial to using the instrument to generate new information, but it is not itself doing science. The science can only take place after the instrument is calibrated. But clearly calibration of instruments constitutes just what we have been talking about as part of a technological infrastructure, just as the instruments are part of it.

Now what we want from a philosophical theory of scientific change is an account which explains why this happened rather than something else. Consider the following: for many years I was puzzled by the fact that while everyone acknowledges Galileo's contribution to the Scientific Revolution and the importance of his last book, *Discourses on Two New Sciences*, nevertheless, Galileo's own form of scientific methodology seemed to have died with him. There is no Galilean school of physics; there are no clear Galileans as there are Newtonians. Why is this so? It took me twenty years, but I think I now have the answer (Pitt, 1992). As it turns out, Galileo's use of geometry is the key to understanding his science. To this end, it is also important to realize that his commitment to geometry was so strong that he urged others not to take up the study of alegra, the new mathematics being introduced. The reason there are no Galileans is that Galileo, for all his greatness, picked the wrong form of mathematics with which to work. The cumbersome proofs of geometry were quickly being replaced by faster and easier-to-use algebraic methods. Galilean science died because geometry was replaced by algebra and then by the calculus. (It is a bit more complicated, but that is the heart of it.)

But why did Galileo stick to geometry? That requires explanation. An easy and ready account is that he was getting old, and he was virtually blind when he finished the *Discorsi,* which he had been working on virtually all his adult life. It would have been rather difficult to change mathematical methods at this late stage. This would seem to be reasonable. But there is one more thing, something that really makes a difference—for many centuries the Latin translation of Euclid's geometry in use had a flawed version of Book 5. In 1544 a new translation of Archimedes appeared which included the correct version of Euclid's Book 5, in which a clean account of Definition 4 is given. It is a definition which had been badly garbled both Boethius and by the Arabic translators. Its correct form reads: "Magnitudes are said to have a ratio of one to another which are capable, when multiplied, of exceeding one another." Galileo took his own definition of ratio from this relatively new translation of the definition and made it the basis for the derivation of most of his theorems. Because Galileo insisted on not compounding magnitudes of different types and because of his demand for complete rigor and proof (following Archimedes), Galileo thought hehad the basis for a new mathematical method. Why did he not adopt algebra? Because he thought he had a new method of his own.

This example is instructive for several reasons. First, it helps me make the point that geometry is used by Galileo in the same way that a hammer is used by a carpenter. In short, it is very much a technology. It is a tool which enhances human capacity for changing the world. Second, not every change in science is fruitful. In epistemology it is important also to explain how we make mistakes. No adequate epistemology can neglect to do that. In the history and philosophy of science it is equally important to explain failures and dead ends. It is not enough to merely account for the successes. And it is not sufficient to say that X failed where Y succeeded because X was irrational. (I find it somewhat rewarding to note that it takes work in the philosophy of technology to accomplish what philosophers of science have been unable to.) Third, it is worthwhile noting that despite the fact that Galileo's methodology failed to attract adherents, geometry was not discarded as false or useless. It remains a viable tool.

Finally, this example puts us in a position to turn to the second criterion for selecting factors to define a context and subsequently a technological infrastructure. The determination of whether various factors should be included in the determination of an historical context must meet the criterion of explanatory coherence. If the things to be included do not contribute to the coherence of the explanation being offered, they should be eliminated. I think the role of the new translation of Euclid's Definition 4 helps to explain why Galileo selected the method he did for his proofs and why there were no Galileans to take up his research program. The fact that he did not marry his long time mistress does not. Nor is it relevant that at this time Cardinal Richelieu held power in France.

Let us now turn to the question of how an historical context contributes to our understanding of a technological infrastructure for science. It becomes one when the factors selected can be shown to make an epistemological difference with respect to specific scientific developments, thereby explaining what happened in a manner which brings the relevant factors into a coherent story. That it is a *technological infrastructure* is a function of the fact that it identifies the players—human, artifactual, epistemological, institutional—and their interrelations in which the events in question took place.

A mature science is a complicated thing. It is not merely a theory. By concentrating on the logical structure of theories, philosophers of science have done some good things, but they have not made it possible to do the important philosophical job, which is, as Wilfrid Sellars put it, "To see how things, in the broadest possible sense, hang together, in the broadest possible sense" (Sellars, 1963, p. 3).

Concentrating on the logic of theories does not tell us how science gets done. Before there are data to be used as evidence, there are laboratories and the places where the laboratories are located. And where they are located makes a difference. For example, different kinds of pressures apply in commercial labs as opposed to laboratories in universities. There are different objectives to be met. In some commercial labs, the emphasis is on commercially viable results. In some academic labs the emphasis is on securing grants to ensure the continuation of the research program (and the generation of overhead for the university administration to play with). In addition to the kinds of issues just noted, science includes laboratory assistants, experimental apparatus, the interactions among the members of the community (no, I am not talking about the social construction of scientific results) which fuel ideas and techniques. In short, if we play out the list of things we need to consider, we will find ourselves looking at the full scope of the working relations among those people involved in the investigation of nature. And if technology is humanity at work, then those relations and players constitute a technological infrastructure.

In closing, it seems appropriate to consider the down side of the view I am proposing. Cosmology is the science concerned with explaining the universe as a whole. It uses data gathered from a variety of instruments, telescopes of various kinds in varying locations. These instruments themselves embody numerous theoretical assumptions, from optics to electronics to the manufacture of ball bearings. The increased use of computers to manipulate data incorporates yet another wide ranging set of assumptions, some of them having to do with computer languages, others with the reliability of hardware. The kind of explanations cosmologists generate do not, therefore, merely rely on the evidence pure and simple. The question, to my mind, is, how much of the theory is a function of the technology? In mature sciences, it appears that the more embedded the science is in its technological infrastructure, the more the infrastructure drives the science. Thus when we attempt to ascertain the cause of a change in theory, we will find it increasingly difficult to point to specific causal factors. I suppose we could simply say that it is the Hubble telescope that is forcing us to revise our cosmological theories. But that would simply be false. How that instrument is used, the kinds of support systems it requires, and how they influence the generation of images, cannot be ignored. If what I have been suggesting is correct, then we need to know a great deal more about the supporting systems and the environment in order to understand just what it is the science is telling us. And when the science is thus embedded in its technological infrastructure, changing scientific theories can only be accomplished by rejecting the technological infrastucture or by finding another theory which uses the same infrastucture, at which point the science is still captive to the technology. Thus, explaining scientific change will require a full account of the technological infrastructure of that science if we are to understand what kind of a change we are witnessing.

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