The Dilemma of Case Studies Resolved: On the Usefulness of Historical Case Studies in the Philosophy of Science[1]

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Abstract

Joseph Pitt argues in this volume that historical case studies are of limited value to philosophy of science. This essay replies to Pitt, showing that case studies, at their best, illustrate novel modes of evidential support and argumentation and present styles of scientific work calling for philosophical analysis over and above the standard analyses currently available. I adumbrate exemplary findings from case studies to illustrate modes of exploratory experimentation and to show how interdisciplinary co-operation within science can provide multiple independent means of access to theoretical entities. As the latter examples show, case studies illustrate some of the means used by scientists to support claims for the reality of theoretical entities in ways not standardly available from work performed within a single discipline. They also illustrate devices employed to correct systematic biases that stem from the commitments of each discipline taken separately. Such findings illustrate the power of case study methods.

Pitt's dilemma restated

Joseph Pitt argues for the following dilemma. If we generate general philosophical or methodological claims about science and then turn to case studies to test or support them, our sampling procedures and our interpretation of the cases are necessarily systematically biased. Our choice and interpretation of the case studies will be shaped by the methodological claim(s) to be tested. We will be prone to reinterpret the historical record anachronistically, in terms of the methodological thesis that is at stake, and to ignore those parts of science to which the thesis being considered is not germane. (This position is not unique to Professor Pitt; similar claims have been put forward in the writings of

Joe Agassi, Paul Feyerabend, Tom Kuhn, Imre Lakatos, Larry Laudan, and others.) Where cases are systematically chosen and manipulated to test methodological theses, the procedure is viciously circular and guilty of systematically cooking data to fit the investigation at hand.

Lakatos (1971, 1972) suggests a rather impractical way to escape this horn of Pitt's dilemma. Expose each philosophical methodology to all the cases considered by all competing philosophical methodologies. Exclude the trivially mistaken methodology that considers all science to be rational. Then, that methodology that maximises the rationality of science by evaluating the greatest number of the cases examined as exhibiting sound methodology is best, because it maximises the rationality science. Unfortunately, however, this test *assumes* that the methodology that maximises the *number* of cases considered to be rational has assigned credit and demerit correctly. No independent standpoint is available to certify the correctness of this assumption or to vouchsafe the particular assignments of rationality (or lack thereof) by this procedure. Others have put forward similar strategies, e.g., Larry Laudan (1977), who uses touchstone judgements by scientists rather than sheer numbers. They are all subject to similar objections.

On the other hand, if we embark on case studies without any philosophical issue in mind, on pain of hasty generalisation we cannot—and should not—draw any philosophical morals.[2] A series of case studies—even a few hundred case studies—does not provide a sufficient basis for generalising about science, which is as richly diverse as any human enterprise. To surmount this problem, the sample must be appropriate and not skewed by systematic bias—but to know that the sample is appropriate and unbiased we would already have to know how to determine what should count as an instance of good science and how typical it was. Conclusion: philosophers who start from case studies cannot prevent systematic bias or hasty generalisation. Methodological or philosophical morals drawn from case studies are untrustworthy without independent support. This problem is insurmountable.

This dilemma is often used (though not by Pitt) to support an anti-Kuhnian, top-down philosophy of science according to which philosophical considerations (usually epistemological, sometimes metaphysical) are needed to provide norms for science and standards of scientific knowledge. We need to work 'down' from philosophy to obtain an account of the nature aims, and methods appropriate to science and then select our case studies to test the conformity of workaday science to the resultant principles. Much work by Rudolf Carnap and Karl Popper fits this description. It is important to note that we find echoes of this stance in the work of such historically oriented philosophers of science as Lakatos and Laudan, both of them influenced by Popper. Among our contemporaries, I suggest that such Bayesian philosophers as Colin Howson and Urbach (1989) and Allan Franklin (1990) practice top-down philosophy. Top-down work is not now widely fashionable in science studies and is generally resisted in philosophy of science, but it is still enormously influential, not only among philosophers, but also among scientists and in the popular image of science. Top-down thinking infects anyone who holds that there is such a thing as *the* method of science or that science has an essence. Thus, for example, those who think that science, done properly, is necessarily self-correcting or is based simply on search for the truth or on describing phenomena economically are ascribing an essence to science and will approach cases in a top-down manner.[3]

Softening the dilemma

Pitt's dilemma, I maintain, is ill-founded, but his argument helps clarify how best to make case studies philosophically useful. This position relies on the fact that scientific change is considerably more orderly than Pitt's Heracleiteanism suggests.[4] Yes, what count as *observations* change; yes, the *background assumptions affecting what is required of observation and theory acceptability* change; yes, the criteria for *acceptable evidence* change. Nonetheless, at any time, within particular domains (or problematics) many claims are better established than others and some techniques are more reliable and better rooted in observation than others. Furthermore, to overturn the relative epistemic position of such claims and techniques, highly specific burdens are placed on their critics. In effect, particular scientific communities exhibit enough epistemic consensus, at least within certain domains, that they have a rough and ready ordering of the relative vulnerability of certain key claims (see Burian, 1985, 2003). This is part of what gave Kuhn's muddy notion of a paradigm its initial plausibility. In favourable cases, the existence of this sort of local—or, perhaps, regional—consensus, enables those of us doing case studies to reach reasonable agreement about the boundaries of a case study, the relevance of some putative contextual factors, and so on. This provides a basis for greater optimism than Pitt manifests about what we can gain from well-executed case studies. Properly deployed, they can yield deeper understanding of science than alternative methods. Case studies ought to play a greater role in philosophy of science than the heuristic one to which he relegates them.

What are case studies?

Pitt's conception of case studies is narrower than mine, so it is worth clarifying our differences. Case studies are concerned with scientific work carried out during a limited time period and are usually restricted to a specified set of scientists, institutions, laboratories, disciplines, or traditions. But, case studies need not be focused primarily on individual scientists. They typically focus on work in what he calls a 'problematic' or a specified domain or topic—for example, on work on black body radiation, or locating black holes, or the material composition of genes, etc. In particular, there is no reason for them to be restricted to a particular science (e.g., physics) as conceived at a particular time. The issues examined may be quite heterogeneous. Thus, case studies can concern the research strategies of particular individuals or laboratories, differences between particular research programs, the impact of a new technology on a particular research problem, the impact of transforming an instrument for use in a new domain, or the impact of a particular patron on a domain of scientific work. The topics covered can be quite large. E.g., a number of people are engaged in interlocking case studies aimed at comparing treatments of heredity in different biological disciplines around the end of the 19th century. More often, case studies deal with somewhat narrower topics. Still, case studies that interest philosophers of science are typically organised around a focal issue of broader interest, for example the relationship between theoretical and technological innovation in certain lines of scientific work or the strengths and weaknesses of particular research methods or technologies in a certain domain.

Case studies that offer potential philosophical profit begin with some general knowledge of the focal issue toward which they are directed. They do *not*, however, proceed in hypothetico-deductive style to test (universal) philosophical theses about the place of values in science, or about scientific method, epistemology, or metaphysics.[5] There is a deep reason for this. Like Pitt and many others I do not believe that there is such a thing as *the* place of values in science, *the* scientific method, or *the* epistemology or metaphysics of science. Like him I consider close attention to context requisite for sound case studies. Unlike him (and this is a theme to which I will return), I believe that contextualisation does not undermine philosophical methodology (Burian 2003).

A response to the dilemma

Pitt's dilemma is persuasive only if one accepts flawed assumptions about science or misunderstands the proper application of case study methods. Science is not one thing. It cannot be properly characterised by abstract principles or by snapshots (i.e., by temporally isolated case studies). To understand what happens in a particular scientific episode, one must place it within a changing historical context, paying special attention to the special social contexts within which science is done and evaluated. Therefore, case studies should be grouped to take account of context and the relevant shifts in context in which the work was done. So far forth, Pitt and I agree, except perhaps for a trivial semantic difference, for he doesn't seem to think that what I call grouped case studies are case studies.

Given this, two ways of grouping case studies are quite powerful. The first is to construct longitudinal studies that follow the evolution of the problem and of scientists' ways of dealing with it.[6] The second is to conduct comparative studies of approaches to a problem taken by workers in different laboratories, disciplines, or locations, or by use of different tools and technologies. To be effective, such studies need to take account of the multiple settings within which scientific work takes place—theoretical, technical, instrumental, institutional, political, financial, national, ... Context here is like a set of matroshki, i.e., nested Russian dolls, except that the nesting can be continued indefinitely and in several directions. Nonetheless, for practical purposes (Pitt to the contrary notwithstanding) the contexts considered can usually be limited by requiring clear demonstration of their relevance to the execution of the scientific work or the historical resolution of the issues at stake. I maintain that the best use of case studies is 'bottom-up'. The point of case studies, after all, is to work with an appreciation of the scientific work in its context. To do their job case studies should yield improved understanding of how scientists and technologists solved (or failed to solve) problems, the methods they used or tried to use, how they made their tools interact, how they evaluated hypotheses and factual claims, and so on. If one counts work as genuinely scientific only if it meets a pre-set criterion or general aim (such as truth seeking), then one is not honestly working bottom up and risks misunderstanding the case.

Ideally, reflexive self-consistency, would require me to argue against Pitt's dilemma by working bottom up, from case studies. However, space limitations prevent this approach; here I limit myself to adumbrating two exemplary cases from my work. Interested readers can examine my other case studies, with both longitudinal and comparative components (e.g., Burian 1993a, 1993b, 1994, 1996, 1997, 2000; Burian & Gayon 1999; Burian *et al*. 1991, 1996; Thieffry & Burian 1996; Zallen & Burian 1992).

In the present context, the philosophical issues to be addressed are these:

(1) How do biologists deal with complex situations for which they do not have adequate theories? More specifically, what can we learn from case studies about ways in which biologists have employed powerful techniques where almost no high theory is relevant?

(2) What light can case study work shed on epistemological aspects of the interactions among scientists trained in different disciplines?

The two examples show that methodologically and philosophically useful case studies need not proceed to grand conclusions by induction from absurdly small samples. And by producing findings that cannot be gotten from more abstract 'armchair' philosophical work they will show the way out of Pitt's dilemma.

Dealing with complex questions in the absence of high theory

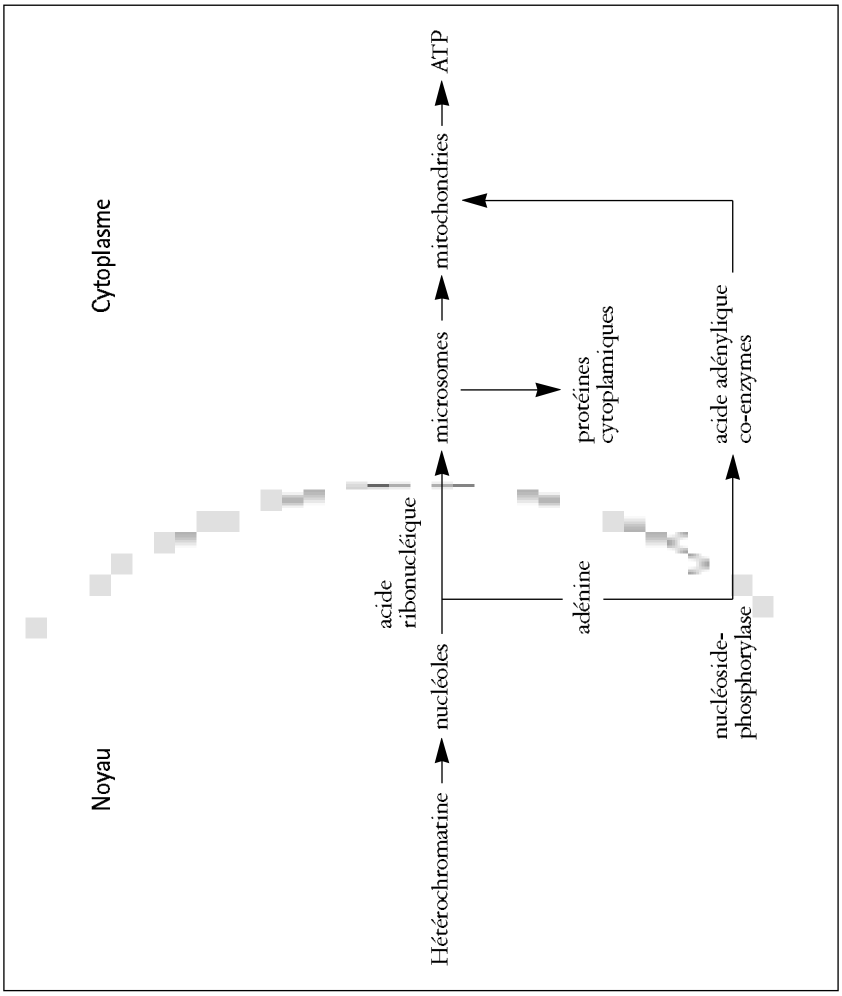
I start with a series of studies that began with virtually no guidance from theory and near total ignorance of the answers to the questions at hand. They illustrate the reliance on powerful exploratory technologies and techniques and illustrate the early impact of the ever more powerful battery of tools available in the twentieth century for use in exploratory experimentation in the absence of high theory. The cases studied concern the localisation and distribution of nucleic acids started by Jean Brachet (1909–1988) in 1927 (see Burian 1997 and Fantini 2000). Brachet, was very much a hands-on experimental scientist, published on this topic more or less continuously from 1929 well into the 1970s, continually devising and exploiting new techniques. His 1957 book, *Biochemical Cytology*, was the bible of methods in that field for a number of years.

Brachet began work on nucleic acids long before they were suspected to be genetic material. His initial focus was on biochemical embryology; many of his early investigations were directed to finding out the distributions and roles of nucleic acids in the development of a great variety of embryos. From roughly 1940 to 1952 his findings led him into the problem of understanding protein synthesis. Crystallised RNAse, which degrades RNA but not DNA, became available in 1938. By 1940 Brachet was able to stain RNA and DNA differentially in successive microtome slices, allowing him to make rough quantitative estimates of the distribution of DNA and RNA within embryos and cells. Many organisms had about as much RNA in the cytoplasm of their eggs as they had DNA in the nuclei of their cells by the time they formed a gastrula, i.e., when they had hundreds or thousands of cells. During this time, the amount of RNA decreased in proportion to the increase of DNA. Given this and other evidence, Brachet hypothesised that RNA is a precursor required for synthesis of DNA.

From 1940 to 1952 he gradually refined his techniques and hypotheses. By 1952, before most geneticists were convinced that DNA is the genetic material, he had developed dramatic support for a series of strikingly new claims. Here are a few, reflected in the sole diagram of a little book he published in 1952 (Figure 1; Brachet 1952: 115).[7] DNA occurs exclusively in the nucleus of cells; some RNA (probably formed under the influence of DNA) is found in the nucleus; RNA is exported from the nucleus; most RNA is found in the cytoplasm. A major fraction of RNA is in ribosomes (themselves discovered during this period); ribosomes also contain protein and, when proteins are formed, they seem to grow on, or conjoined to, ribosomes. Finally, to be able to produce significant amounts of protein, cells must make or contain plentiful RNA.

The ramifications and wider connections of this work require detailed attention, and belong at the heart of a full-fledged case study, but a simple point suffices for the present. No theoretical principle, guideline, or expectation about the distribution of the nucleic acids shaped Brachet's findings. Indeed, when he began his work, the orthodox view was that RNA was plant nucleic acid and DNA animal nucleic acid—a view thoroughly undermined by his earliest work. His work cannot be properly understood as based mainly on hypothesis testing or as shaped by clear-cut theoretical assumptions until quite late in the game. It was *exploratory experimentation*, based on an expanding toolkit of methods for tracing the spatio-temporal distribution of DNA, RNA, proteins, and other materials in cells and tissues during the development of a wide variety of organisms. It built on the opportunistic adaptation of new techniques, such as the use of radioactive tracers, to 'triangulate'[8] on the locations of the substances he was investigating. Brachet's exploratory experiments produced findings of crucial importance for work on protein synthesis and for understanding *which* proteins are made when—issues into which Brachet was drawn on the basis of these exploratory experiments, not on the basis of prior theoretical commitments.[9]

Figure 1. The scheme of protein synthesis redrawn from Brachet (1952: 115)



This embryological diagram IS not a declaration of the 'central dogma' that information is transferred only from nucleic acid to protein (Crick 1970). Nonetheless, note the approximation to subsequently accepted details of the roles of the nucleic acids in the mechanisms of protein synthesis and that the feedback loops convey no information.

Interim conclusion

An interim moral follows easily. First, (theory centred!) top-down philosophy of science is systematically biased. Exploratory experimentation is widespread and comes in many modes. It usual distinguishes three major employments of experiments used in connection with theory: hypothetico-deductive, inductive, and abductive inference. Experiments can test hypotheses, they can generate and/or support hypotheses by induction and they can determine which of the available explanations best fits the experimental results. Thus, the category of exploratory experimentation plays no significant role in standard philosophy of science. Nonetheless exploratory experiments are easy to find and are common in most sciences. The case study literature is needed to correct the narrow theory-centrism ad myopic vision of philosophers of science.

Working across disciplines: Jacob and Monod

In a recent memoir, Jacob (1998: 47) writes: 'In the early days of molecular biology, at mid-century, most research was the product of teams of two, of duos, of pairs'. He mentions Beadle and Tatum, Luria and Delbrück, Perutz and Kendrew, Watson and Crick, Jacob and Monod, Meselson and Stahl. Many more such pairs could be added to this list, each coming from different disciplinary backgrounds, but each, in their collaboration, worked to understand the same entity or process. This phenomenon demonstrates that the relationships between exploratory experimentation and high theory are many and varied. Brachet was always suspicious of over-dependence on theoretical assumptions, but François Jacob and Jacques Monod, like most of the pairs listed, mixed exploratory experimentation with testing of predictions from high theory.

At one key phase of their collaboration, Jacob and Monod simply followed the time course of events when one strain of bacteria donated well-defined genetic material into bacteria of a different, but also well-defined genetic constitution. This case is particularly interesting, because this experiment was part of a series that relied heavily on hypothesis testing. Still, some key experiments of the series did not test hypotheses and yielded results that neither confirmed nor infirmed the high theory involved, but opened up new doors for studying what we now call regulation of gene expression. Here I can only provide a few hints of what a full case study would show about the intricate interrelation between exploratory experimentation and theory involved in their work. Nonetheless, it will become clear that the relationship between exploratory experiments and high theory depends on the domain under investigation, the available knowledge of that domain, and the power of the available research tools. Consequently, the relationship of exploratory experimentation to high theory cannot be analysed from first principles or from philosophical accounts of the use of experiments to test scientific theories and explanations in theory-driven situations.

Monod and Jacob worked together for five years on control of the synthesis of ß-galactosidase (the key enzyme required to digest lactose) in *E. coli.* Their collaboration eventuated in the discovery of messenger RNA and in the proposal and analysis of the operon model of the regulation of gene expression.[10] Because of space limitations, I focus on one central point. Jacob contributed one key set of experimental techniques and biological insights, acquired in collaborative work with Elie Wollman on bacterial and bacteriophage genetics. These techniques included manipulation of bacterial genomes to contain specific variants of specific genes and experimental protocols to test the effect of foreign DNA on genetically distinct recipient bacteria. They yielded maps of bacterial genes, records of the time course of gene expression, and studies of the effects of newly synthesised gene products on bacteria and their properties.

Monod contributed another set of experimental tools and skills, based on meticulous biochemical analysis of the metabolism of ß-galactosidase production and of lactose digestion in bacteria. He possessed an enormous stock of genetically distinct bacterial strains that produced variant enzymes related to the synthesis of ß-galactosidase. Although most of Monod's bacteria contained the gene required for synthesis of ß-galactosidase, they did not normally make the enzyme except in extremely well defined conditions—when the medium contained no glucose, but was plentifully supplied with lactose. Monod realised that if he could discover the mechanism controlling the production of ß-galactosidase, he might gain general understanding of control of protein synthesis. He developed a rich armamentarium of inducers, inhibitors, and modifiers of ß-galactosidase synthesis so he could test which steps were blocked and ensure that specific genes were expressed (or not expressed) in cells of particular genetic constitutions under well-defined conditions.

Because Monod did not have tools for manipulating DNA, for mapping genes, and for controlling crosses between bacteria and Jacob did not have a system of enzymes subject to the fine-grained controls that Monod had developed, neither could have done alone what they did together.

Jacob and Monod showed that ß-galactosidase production and that of related proteins began at full speed within three seconds of the entry of DNA containing a functional ß-galactosidase gene into a foreign cytoplasm—provided that cytoplasm did not have an inhibitor to block expression of the ß-galactosidase gene. But exploratory experiments extending the key experiments showed that in about an hour, something new—an inhibitor in the cytoplasm!—blocked further expression of those genes (Pardee *et al.* 1959).

Both Monod's and Jacob's methods were needed to design the decisive experiments. But the techniques alone were not sufficient. They were needed to resolve a sharp, partly discipline-based, disagreement between Monod and Jacob that came to a head in 1957. Monod believed that nothing could act directly on genes to alter their effects.[11] In contrast, Jacob, experienced with activation of bacteriophage genes, believed that biological proteins could act on genes in living organisms. By combining their fine-grained technologies, they designed experiments to resolve their differences. This dispute and the experiments it engendered helped them develop and test the operon model. Without the battle to achieve experimental resolution of the discrepancies between their beliefs the technologies alone would not have led them to the operon (Jacob 1973, 1998).

The operon provided a beautiful vision of the regulation of gene expression. Although it was fiercely debated at first, it quickly gained wide acceptance. The operon is a highly integrated system of control circuitry governing the behaviour of several genes. At least one of the genes in the operon produces a soluble product dispersed through the cytoplasm that, perhaps in conjunction with other molecules, interacts with the DNA. In so doing, it creates a feedback loop that regulates expression of all of the genes controlled by the operon, itself included. In effect it throws a switch that blocks the transcription of mRNA for the genes controlled by the operon. In the Jacob-Monod system, such a protein, called a repressor, prevents the production of ß-galactosidase except in the absence of glucose and presence of ß-galactosidase.

Conclusions

Like Jacob and Monod, the members of each of the pairs on Jacob's list drew on different disciplinary backgrounds or employed distinctive tools or technologies for gaining access to a common subject matter. Typically, they also encountered divergent assumptions and resorted to exploratory experiments to resolve which (if any) of their disparate intuitions was correct. The central moral here is obvious: discrepancies in theoretical assumptions can be resolved when there is adequate experimental access to the entities of concern and sufficiently powerful tools to resolve significant ambiguities in the interpretation of the experiments. In general, collaborators reached agreement about the issues at stake when they could satisfy themselves that they had gained access to the same underlying processes and entities and resolved their disagreements about the modes of action involved.

It is worth stressing that the issues concerned highly theoretical entities. The operon and the repressor—a molecule that was utterly uncharacterised biochemically—are typical theoretical entities! The collaborations on Jacob's list succeeded because the available tools enabled scientists to triangulate on theoretical objects (e.g., bacteriophage genes) and behaviours (semi-conservative (?) replication of DNA) even when the collaborators differed sharply about what those entities were or how they behaved. Epistemologically speaking, scientists with sharply different fundamental assumptions were able to isolate some properties of specific theoretical entities or processes with definite locations in space and time.

By thus localising their experimental objects, the collaborators pinned down facets of the behaviour of the objects under investigation, which they tested decisively against a background of shared results. Such investigations are not guaranteed success, but the general pathway by means of which to resolve such questions is clear.

Highly particular knowledge is needed to pin things down well enough to ensure contact with the same processes, entities, and properties. This makes it difficult to provide a general characterisation of what is required to 'triangulate' on theoretical entities or processes. Initial disagreements may turn on differences in what is being talked about, on false assumptions about the behaviour of specific entities, on experimental artefacts, on defective reagents, or …—the list of possible sources of error continues indefinitely. To resolve such disagreements, one needs specific local knowledge about the interaction of specific techniques with the materials or behaviours in question. Without that knowledge, the disagreements may prove intractable; one cannot determine whether the parties have actually fixed on the same entity, behaviour, or cause of the phenomena in question. In forefront work, such as the exploratory work discussed here, it is necessary to forge routinised means of access to the theoretical entities, processes, or behaviours under investigation in order to secure the desired triangulation. It will not be possible to develop a satisfactory general account of what it takes to resolve such issues.

This point about local knowledge illustrates a grain of truth in Joe Pitt's claim that science is subject to Heracleitean flux. Typically (although not in the instance of the *E. coli* operon) the highly specific knowledge of particular systems is relatively evanescent, often for highly contingent reasons. Two examples: we cannot review the data from the Mercury series of rockets because there are no longer any computers available whose operating system can translate the electronically stored data into a form legible to humans. Second, because of scientific fashions, no one now knows the embryology of many of the marine organisms that were well studied in the late 19th century, though, typically, the data for this work are more readily available than the Mercury data.

We now have the basis for a strong reply to Pitt's dilemma. On the view that underlies this paper, science has no essence. Its standards of argument, of adequate evidence, and of the adequacy of theories change—and should change—with time, subject matter, and setting. At the same time, as my case studies show, the changes involved are not Heracleitean, but orderly and very strongly based on evidence. Thus, the lack of an essence and continual flux do not imply radical relativism or loss of contact between science and an external world.

Case studies cannot and should not be expected to yield universal methodologies or epistemologies. Rather, they yield local or, better, *regional* standards, and fallible ones at that. As my discussion of the Jacob-Monod collaboration shows, to combat divergent assumptions it is crucial that the tools employed pin down the same entities or behaviours. One does not get much general guidance about how to do this from case studies. Unless one is dealing with bacteria, DNA, RNA, and, perhaps, a limited class of proteins, the example of Jacob and Monod will not be much help. But the heuristic principle that the case study yields is, I believe, generally useful: to resolve the choice between conflicting theoretical assumptions, try forging and using tools that are experimentally adequate to the task of identifying the entities and behaviours at stake with great specificity. With this we see that case studies like those explored today can make useful points in the methodology and philosophy of science.

These considerations allow us to escape the second horn of Pitt's dilemma. Given that science has no essence, we cannot expect to find universal philosophical principles or methodological rules that provide more than heuristic guidance. We have no option but to work in, and study, particular contexts and do our best to find valid, but limited generalisations. Such work, inevitably, risks hasty generalisation and sample bias. We must do our best to combat these risks, and we have tools that enable us to detect some of our failures, but they are risks that no one can escape. The dilemma that Pitt originally posed should be taken seriously only by those who believe in a universal or wholly objective scientific methodology or in the adequacy of fundamental philosophical guidance for those who would seek truth in empirical matters. Case study work helps to reveal how fragile and poorly supported those beliefs are.

Notes

1 This paper, presented at the IFF/IFZ in Graz, has three previous instantiations. I am grateful to Joe Pitt for the stimulus to take up these issues. The paper has been improved by comments from colleagues and audiences in meetings at Como (at a meeting on 'Science and Culture'), the Center for History of Recent Science at George Washington University, the Department of Philosophy at Virginia Tech, and the IFF/IFZ in Graz.

2 A useful analogy: 'About thirty years ago there was much talk that geologists ought only to observe and not theorise; and I well remember some one saying that at this rate a man might as well go into a gravel pit and count the pebbles and describe the colours. How odd it is that anyone should not see that all observation must be for or against some view if it is to be of any service!'—C. Darwin to Henry Fawcett, September 1861, from Francis Darwin, *More Letters of Charles Darwin* (London: Murray), pp. 194–195, as reprinted in David Hull (Ed.), *Darwin and his Critics* (Harvard University Press, 1973), p. 277.

3 Ironically, Pitt himself does not accept these flawed assumptions about science, which help to make the arguments against the philosophical value of case studies plausible. Indeed, as we will see, he supports many of the uses of case studies that I advocate in this paper.

4 The term 'Heracleitean' comes from an earlier version of Pitt's paper for this volume. It refers to his claim that 'all of the concepts we use to discuss science [not just *observation*] are in constant flux' as are the core doctrines of the scientific theories put forward in the leading theories of different eras.

5 For an attempt along these lines, see Donovan *et al*. (1988).

6 Although he does not consider large-scale longitudinal studies to count as case studies, in his contribution to this volume Pitt explicitly recognises their value and importance.

7 Brachet's work, described here, was completed before publication of the Hershey and Chase (1952) experiment, which showed that when bacteriophage infect bacteria, their protein remains attached to the outside of the bacteria and their DNA—and apparently only their DNA—is injected into the bacteria, where phage multiply. And Brachet's results were published the year before Watson and Crick announced their famous findings of the structure of the DNA molecule.

8 This term has seen increasing usage for the employment of distinct and independent techniques to localise or examine properties of theoretical entities. Ian Hacking, Susan Leigh Star, William Wimsatt, myself, and, I believe, quite a few others have used it for this purpose

9 There is much more to say about the ways by which the entities at stake here—e.g., ribosomes and, for that matter, DNA and RNA—are constituted. They resemble, in certain respects at least, what Hans-Jörg Rheinberger calls 'epistemic objects', which are formed and reformed as the objects under investigation as characterised by the theoretical-plus-practical means of identifying them. For ribosomes see Rheinberger (1995, 1997a) and for a more general treatment of 'epistemic things' see Rheinberger (1997b).

10 This work is described *in extenso* by Judson (1996, esp. chap. 7). Shorter recent treatments can be found in Morange (1998, chaps. 12 and 14), and Burian and Gayon (1999).

11 The title of chapter 7 of Judson (1996) is a quotation from Monod expressing his attitude at the time: 'The gene was something in the minds of people as inaccessible as the material of the galaxies'.

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