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Riffing on Feyerabend: Direct Observation, Paraconsistentist Logic, and a Research Immanent Account of the Rationaliy of Science

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Abstract: Feyerabend's work, particularly his early papers contain important insights into the nature of science and scientific progress. I discuss his insights into the limits of empiricist foundationalism and positivism. I explain how the work of a number of philosophers has borne out Feyerabend's claims in startling and interesting ways. Nevertheless, I criticise Feyerabend's move from his attack on universal method to relativism. I point out that Feyerabend never confronted a well-developed research immanent view of the rationality of scientific change, which shows the limitations of the arguments in **Against Method**.

Keywords:
Feyerabend;
history and philosophy of science;
inconsistency;
paraconsistency;
philosophy of science;
rationality;
Shapere

Introduction

A foundation is a kind of origin. Feyerabend is an important critic of various kinds of foundationalism, theories which tell us the basis from which knowledge must be derived. He focussed on empiricist foundationalism, the theory that the basis for our knowledge and the meaning of key terms in our knowledge is derived from experience. After Feyerabend wrote many of his key works, his insights were deepened by other authors. I will here discuss some of his neglected papers, neglected aspects of **Against Method**, and the work of other authors who have deepened and strengthened the accounts Feyerabend presented. Many of these authors do not refer to Feyerabend's work and may well have been unaware of much of it. Nevertheless, Feyerabend had the important insights before them. I will also discuss an important challenge to Feyerabend's argument in **Against Method** that Feyerabend never discussed. I will not here discuss Feyerabend's relativism in any detail. I have elsewhere argued against Feyerabend's relativism. [1]

Feyerabend on Observation

What is directly observable and what do scientific theories describe? During the high tide of positivism, it was argued that what was directly observable were certain experiences. The semantics of a scientific theory were partly or wholly dependent on a direct tie to the content of experiences. The experiences are the foundation of our knowledge. Hanson, Popper and others argued for the claim that our observations are theory-laden. That is, that part of the content of experience is and must be dependent on background theories. This view was present already in Kant and in the influential nineteenth century philosopher/historian of science, William Whewell. It has become the accepted view. While Quine produced an influential critique of positivist dogmas of empiricism, he still declared that "[A]s an empiricist I continue to think of science as a tool, ultimately, for predicting future experience in the light of past experience". On the same page, he claimed that physical objects should be regarded as irreducible posits like the gods of Homer. Myths that are no doubt more useful than the gods of Homer, but still just useful myths. He further declared that posits at the atomic level or below should be treated in a similar manner. [2]

In some important, but now neglected early papers, Feyerabend went further than Quine, or those who argued for theory-ladenness. In "The Problem of Theoretical Entities", Feyerabend argued that various claims can be tested by using the

[1] See George Couvalis, **Feyerabend's Critique of Foundationalism**, Avebury, Aldershot 1989, pp. 136–143; George Couvalis, **The Philosophy of Science**, Sage, London 1997, pp. 111–139.

[2] See Willard Quine, **From a Logical Point of View**, Norton, New York 1961, p. 44.

same test, so that there is no conceptual connection between experience and what is being tested. For instance,

> "lifting a suitcase does not serve only to test the suitcase's weight. For example, after a long illness, we can lift a suitcase of a known weight as a test of our own strength and *not* as a test of the weight of the suitcase […] Or we lift the suitcase of a person who does not have a friendly disposition toward us, and we test his patience or our own nerve [...] the object being observed depends on the *problem* present, and that this object is not given by the simple act of observation […] Thus, we can conceive of lifting a suitcase as an observation of the intensity of the gravitational field at the location of this action […] (a more realistic example is the direct observation of a supernova by observing the sudden increase in brightness of a point of light in the sky)". [3]

An important point being made by this argument is that there is only a causal connection between a test and what is being tested, not a connection of meaning. What counts as a direct observation of something depends, as he says, on the problem we are dealing with — the theory we are testing. This means that experience cannot be a foundation of knowledge in the way in which positivist theory describes.

In another early paper, "On the interpretation of scientific theories", Feyerabend pointed out that the positivist account made statements describing causally independent situations semantically dependent. There is a conflict between doing properly scientific observations and positivist theory. Take one of his examples. Suppose we are doing celestial mechanics and trying to work out the mass of the sun from observations with a telescope and other data. We will have to allow for the refractive index of the earth's atmosphere, perceptual illusions caused by the functioning of our eyes, and many other things. The mass of the sun is causally independent of the refractive index of the earth's atmosphere, and of the workings of our eyes, and so on. Yet, according to positivism, the meaning of our statements about the mass of the sun is tied to the conditions under which we observe it. This is a clearly absurd result. Our statement is about the mass of the sun, not about these other things. We have to take account of these other things in doing our calculations because of interference effects caused by the light from the sun entering the earth's atmosphere, and because of the workings of our eyes, and so on. But

[3] See Paul K. Feyerabend, "The Problem of Theoretical Entities" (1960), trans. from the German by Daniel Sirtes and Eric Oberheim, in: Paul Feyerabend, **Philosophical Papers, Volume 3, Knowledge, Science and Relativism**, Cambridge — New York — Melbourne, Cambridge University Press 1999, pp. 19–20 [16–49] [emphasis in the original].

they have nothing to do with the mass of the sun. Feyerabend draws two conclusions from his argument. First, "*[T]he interpretation of a scientific theory contains metaphysical elements*", that is, elements that are non-empirical. Second, "*The interpretation of scientific theory depends upon nothing but the state of affairs it describes*". [4] Let us make clearer what Feyerabend is saying. Our senses are, for the purposes of science, only measuring instruments to be treated as like other measuring instruments. They are not at all the source of the meaning of scientific claims. Talking in positivist terms, our statements in a scientific theory are not only theory-laden, they are fully theoretical. No part of their meaning comes from experience.

Feyerabend was later to go further than claiming that scientific statements are fully theoretical. He argued that a science without experience is possible. In his 1969 paper, "Science Without Experience", Feyerabend argued that testing a scientific theory could be carried out by a computer which receives data from various devices and produces a yes-no answer to the experimenter. There is no need for the experimenter to use her sensations in testing a theory. However, he did not give convincing examples to bear out this claim. As we will see, others have done so.

Feyerabend presented a much more realistic picture of actual science than the positivists and their followers, who stuck with a supposedly "scientific theory" of meaning, which was based on nothing more than empiricist prejudice. Through much of the empiricist tradition there has been a confusion between empirical tests for hypotheses and empiricist theories about the origin of concepts. Empirical tests for theories, as Feyerabend pointed out, have to do with causal relations between a measuring device and a cause. They do not have to do with meaning relations between a hypothesis and a test.

In recent times, many philosophers have pointed out that what our measuring devices measure go way beyond anything we could experience. Take the case of temperature, we now have ways of measuring temperatures that are far too hot and far too cold for anyone to experience. Hasok Chang has done a detailed study of how these instruments developed. [5] If we were foundationalists, it would be

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[4] Paul Feyerabend, "On the Interpretation of Scientific Theories" (1960), in: Paul Feyerabend, **Philosophical Papers, Volume 1, Realism, Rationalism and Scientific Method**, Cambridge, Cambridge University Press 1981, p. 42 [37–43] [emphasis in the original].

mysterious how our measurement of temperature by using our bodies as very rough measuring instruments have been gradually and rationally replaced by much more accurate measuring instruments. After all, our bodies did not evolve to measure temperature as distinct from the heat conductivity of a medium, as we can see when we enter what we think is cold water from what is in fact colder air. How is it possible that we came to distinguish the conductivity of a medium from temperature when our sense of hot or cold has not evolved to distinguish them? How, indeed, did we learn to put aside the effect of our previous sensations on the detection of temperature; for, after all, if we have previously put our hands in "hot" water, water at a normal room temperature will feel cold. If we were positivists, surely the meaning of "hot" and "cold" must be closely tied to experience in a systematic way.

Jerry Fodor

Fodor started out as a critic of the sort of claim Feyerabend sometimes endorsed. In an influential paper, he argued that the processes producing experience are modular and insensitive to beliefs. For instance, while we can believe that the Müller-Lyer illusion is false, and believe that the lines in that illusion are the same length, this has no effect on our experience. The lines continue to look a different length. Thus, experience has a content independent of higher-level beliefs that can be used to test theories. [6] This undermined the claim that experience had no content independently of a high-level theory. However, Fodor soon changed his mind about the significance of his argument.

In the wittily titled paper, "The Dogma that didn't Bark", Fodor argued against the Quinean Dogma that science is primarily about predicting experiences. As he pointed out, if that were the goal of science then the obvious strategy would be to have fewer experiences. So, "if all you want is to be able to predict your experiences, the rational strategy is clear. Don't revise your theories, just *arrange to have fewer experiences*; close your eyes, put your fingers in your ears, and don't

[5] See Hasok Chang, **Inventing Temperature: Measurement and Scientific Progress**, Oxford University Press, New York 2007.

[6] See Jerry Fodor, "Observation Reconsidered", *Philosophy of Science* 1984, Vol. 51, No. 1, pp. 23–43, https://www.jstor.org/stable/187729 [15.09.2023].

move. Now, why didn't Newton think of that?". [7] But scientists behave as if they want to find out further things about the world, not predict experiences. Fodor went on to describe experiments carried out in his laboratory.

The experiments were intended to test the hypothesis that understanding passive sentences takes a longer time than understanding active sentences in someone's native language, ceteris paribus. For instance, one experiment involved hearing active and passive sentences on earphones while attending to a display on a computer screen. The subject has to pronounce aloud any word she sees on the screen. The reaction time for a word paired with an active sentence is compared to the reaction time to the same word paired with a passive sentence. The elapsed time is measured and stored in a computer that carries out the experiment. The times are so short that only a computer could compare them. A material difference of 15 or twenty milliseconds is significant. The computer pools the data and only the differences in reaction times of 50 or 60 experimental subjects would have any importance. The data matrix is enormous. So, it is necessary for the computer to analyse the raw data and produce a result. The raw data would be beyond the capacities of human analysts to analyse. What is important is the statistical p value, which is not something that can be observed in any case. As Fodor points out, the data for a theory are just "*whatever confirms its predictions* and can thus be *practically anything at all* [...] So the data for big bang cosmology include "observations" of cosmic background radiation, the data for Mendelian genetics include the "observed" ratios of traits in the offspring of heterozygotes [...]". [8] Feyerabend could not have put the point better himself. While there is a difference between problems and theories, Feyerabend was really talking about testing theories when he used the Popperian jargon of "the *problem* present".

Fodor continues with some playful remarks on Quine's famous "Two Dogmas of Empiricism" by stating that "[T]he observability of data is thus the third dogma of Empiricism". [9] He then imagines a future science in which you plug an experimenter's cortex into a computer which feeds her the data so that there's no sen-

[7] Jerry Fodor, "The Dogma that Didn't Bark", *Mind* 1991, Vol. 100, No. 2, p. 202 [201–220], https://www.jstor.org/stable/2254867 [15.09.2023][emphasis in the original].

[8] Fodor, "The Dogma…", p. 208 [emphasis in the original].

[9] Fodor, "The Dogma…", p. 208.

sory input at all. This goes even further than Feyerabend in undermining the baleful influence of positivism.

Shapere on Direct Observation

Shapere's first Feyerabendian insight was to go further than Feyerabend by arguing that whether something is directly observed or not is dependent on the source of observation and how the information is received. Further, not only could there could be a science without experience, such a science already exists. He argued that physicists were right to talk of directly observing the centre of the sun in an experiment that started in 1967, which seems to have been unknown to Feyerabend when he wrote "Science without experience".

The experiment involves capturing a neutrino in a drum of cleaning fluid 5000 feet beneath the earth. The neutrino will react with an isotope of Chlorine in the cleaning fluid to produce argon; the argon in turn be removed from the tank by bubbling Helium through it, and then the argon is separated from the Helium by a charcoal trap which registers on a proportional counter, so that the number of neutrino captures are counted. A computer keeps track of the counts. The whole procedure is carried out to capture neutrinos from the centre of the sun in order to directly observe nuclear reactions at the centre of the sun. Neutrinos interact chemically very weakly, and they are not interfered with in travelling from the centre of the sun. The siting of the tank screens out other irrelevant particles.

The experiment was very important, for it showed that the number of neutrinos was considerably less than had been anticipated. This led to further work which resulted in a Nobel prize. [10] Shapere's paper was written long before the Nobel prize was awarded, but he was clearly aware of the importance of the experiment.

Shapere stated the sense in which neutrinos are direct observations of the centre of the sun. He said physicists use "directly observable" to mean the following:

"*x is directly observed (observable) if:*

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[10] See https://www.nobelprize.org/prizes/physics/2002/summary/ [15.09.2023].

(1) information is received (can be received) by an appropriate receptor; and

(2) that information is (can be) transmitted directly, i.e., without interference, to the receptor from the entity x (which is the source of the information)". He continues "specification of what counts as directly observed (observable), and therefore of what counts as an observation, is a function of the current state of physical knowledge, and can change with changes in that knowledge".[11]

Whereas in his early work Feyerabend had been inclined to dismiss everyday talk as trivial and irrelevant, Shapere argued that there is an important continuity between the use of "directly observed" in everyday language and in the language of Physicists. Everyday talk of observation has two aspects, perceptual and epistemic. In everyday contexts, the two coincide. We gain evidence by perceiving with our sensations. If we perceive something under optimal conditions (for instance, we are not blinded by a bright light), we can speak of directly observing something. However, the Physicist's usage is a rational extension of everyday epistemic usage. Science has shown us, for instance, that we are only sensitive to a small part of the electromagnetic spectrum, let alone our sensitivity to many other ways in which we can acquire evidence about the world. So, we build instruments to capture information about the world. These instruments can detect things we cannot detect with our ordinary sensations. The more we learn, the more we rationally modify our notion of what is directly observable.

An important point in Shapere's argument is that what we know from science indicates that photons interact with all sorts of things before they get to our eyes. This means that detecting photons, in the way in which we do with our eyes, is an unreliable method for directly observing the centre of the sun. By contrast, what we know from science indicates that neutrinos do not interact during their transit to the drums of detergent. This why physicists call getting information from neutrinos a "direct observation" of the centre of the sun.

Shapere goes on to criticise the philosophical tradition of taking generalised doubt to be a serious matter. (Consider, for instance, Descartes' taking seriously the claim that a malignant demon might be causing all of his perceptions). By contrast, he argues that such generalised doubts are not taken seriously in science, for good reasons. Science is highly predictively successful and has transformed

[11] Dudley Shapere, "The Concept of Observation in Science and Philosophy", *Philosophy of Science* 1982, Vol. 49, No. 4, p. 492 [485–525], https://www.jstor.org/stable/187163 [15.09.2023] [emphasis in the original].

our lives. It has done this by only taking seriously specific doubts that are grounded in evidence (such as evidence about the working of particular instruments).

Shapere also points out that, contrary to positivistic lore, scientists do not start from minimum observation statements of experience, such as describing what is seen on a photographic plate as a "speck" rather than an image of a star. He objects that even describing something on a photographic plate as a speck requires prior knowledge. In any case, reference to sense-data is too impoverished to function by itself as a basis for knowledge. To be of any use in knowledge gathering, the so-called speck must be thought to be something on the basis of a rich background knowledge based on previous scientifically reliable information. In science, we describe what we see according to the strongest vocabulary justified by previous work in the area and by background knowledge. This is a practice in turn justified through the success of science. [12]

Shapere's discussion of direct observation in a complex scientific experiment which relies on a great deal of background knowledge of causal interactions furthers Feyerabend's account of scientific observation and testing of theories. It also hints at something I will discuss in detail later, which is that knowledge gathering is a research immanent practice in which previous success fundamentally alters the knowledge acquiring enterprise itself.

Feyerabend on Logic

In two early papers, "An Attempt at a Realistic Interpretation of Experience" and "The Problem of Theoretical Entities", Feyerabend pointed out that there are situations of which the phenomenologically adequate description is inconsistent, even though some philosophers have claimed that this is impossible. Feyerabend relied principally on an important paper by Tranekjaer-Rasmussen. [13] As Feyerabend put it,

[12] See Dudley Shapere, "The Concept of Observation in Science and Philosophy (summary version)", in: Dudley Shapere (ed.), **Reason and the Search for Knowledge**, Dordrecht, Reidel 1984, pp. 349–350 [342–351].

[13] See Edgar Tranekjaer–Rasmussen, "On Perspectoid Distances", *Acta Psychologica* 1955, Vol. 11, pp. 297–302.

> "there are statements that are subjectively completely certain in a particular observational situation [...] and which contain a contradiction [...] Subjects were asked to compare the lengths of three lines: a, b, and c. The result of direct observation (whose absurdity, for the most part, only subsequently appears to the subjects, who are occupied with the correct description of what is observed) is that a=b; b=c; but a>c". [14]

Feyerabend discussed an objection by Ayer, in which Ayer argued that it only seems that a=b etc., which is not inconsistent. Feyerabend pointed out that:

> "[T]his solution does not work. What I observe is not that a seems equal to b. The impression is not indefinite and uncertain. I observe that a=b. The element »seems« does not appear *in* the perception, but only serves to hint that the following report concerns a perception and not a physical object. Thus, the situation can be grasped with a single glance, so »seems« belongs to the beginning of the description and is equivalent to »I perceive that«, and that is what we have claimed — the existence of a direct description of a perception which contains a contradiction". [15]

Ayer's resistance, and the resistance of many others, to describing phenomenology as inconsistent partly results from an attachment to the view that the contents of our experience are a foundation for our other knowledge. However, in this context, a more important source of resistance is an attachment to standard modern logic, which is based on the formal systems developed by Frege and Russell. A central assumption of various positivist thinkers is that there is one true logic and that that logic is Frege/Russell logic or one its variants. Built in to those logical systems is ex contradictione quodlibet, the principle that a contradiction implies every other proposition. Accepting this principle means that individual contradictions have no structure of their own — they are not different from one another because they are logically equivalent. Thus, their individual features cannot be described adequately. Yet, as is obvious from Feyerabend's discussion, particular contradictions have highly specific features. The fact that they have highly specific features has led many logicians, particularly in Australia, to abandon ex contradictione quodlibet. Abandoning this principle leads to the development of paraconsistent logics, which are logics that allow for the possibility of true contradictions. [16]

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[14] Feyerabend, "The Problem...", p. 33.

[15] Feyerabend, "The Problem...", p. 34 [emphasis in the original].

[16] For a detailed and interesting discussion of the advantages of paraconsistent logical systems which abandon ex contradictione quodlibet, see Richard Routley, Robert Meyer, Valerie Plumwood, and Ross Brady, **Relevant Logics and Their Rivals 1**, Ridgeview Publishing, Atrascadero 1982. For

Chris Mortensen on Inconsistent Geometry

Chris Mortensen has devoted himself over some years to systematically describing the details of the inconsistent geometry of inconsistent pictures. He has developed a paraconsistent logic and topology, a variant of group theory, matrices, and other mathematical tools to describe the inconsistent geometry involved in various inconsistent pictures. He has classified at least four kinds of inconsistent geometry. Unfortunately, the details of his analysis are too intricate and formal for a paper of this kind. So, I can only note here that Feyerabend's initial brief and suggestive remarks on an inconsistent geometry were prescient, and that recent work by Mortensen has described in detail various kinds of inconsistent figures in a rich and complex formal theory that does not reduce all the various kinds of inconsistencies to equivalents to one another, as one would expect if Frege/Russell logic and its variants were the one true logic. Inconsistent figures Mortensen analyses in detail include the Schuster fork, Escher's inconsistent Necker Cube, and the Penrose triangle. [17] By carrying out this project, Mortensen has significantly extended an insight found in Feyerabend. He has shown through the analysis of a range of concrete examples that only a paraconsistent logic and mathematics will allow us to describe accurately the phenomenology of inconsistent pictures. In this way, he has shown in detail that the standard logic students are taught, as if it is the one true logic, is not an adequate foundation for the study of phenomenology.

Escher's inconsistent pictures are well known. Mortensen has produced a significant work on Escher's predecessor who studied and produced a range of impossible pictures, Oscar Reutersvärd, and discussed his relationship with Escher and others. A range of pictures await detailed formal analysis. [18]

I note here, however, that Mortensen confines himself to phenomenology. He distinguishes between weak paraconsistency, which holds that inconsistent figures can be coherently described but do not exist in the external world, but only

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a more technical account, see Alan Anderson, Nuel Belnap, and Michael Dunn (eds.), **Entailment 1**, Princeton University Press, Princeton 1976.

[17] See Chris Mortensen, **Inconsistent Geometry**, *Studies in Logic*, Volume 27, College Publications, London 2010.

[18] See Chris Mortensen, **The Impossible Arises, Oscar Reutesvärd and his Contemporaries**, Indiana University Press, Bloomington 2022.

in phenomenology, and strong paraconsistency, which holds that contradictions exist in the external world.

Feyerabend briefly claimed that logical laws might have to be abandoned in the light of research in his later work, and he showed a clear awareness of alternative logical systems. However, he did not explicitly discuss paraconsistency or paraconsistentist logical systems. [19]

Feyerabend on Method

In early papers such as "Explanation, Reduction, and Empiricism", and in the various editions of **Against Method**, Feyerabend argued against prevailing views that were not only prominent in positivist thinking but even in various radical critics of positivism such as Popper and Lakatos. A key part of his line of argument was the methods for acquiring significant knowledge changed over time. Another part of his line of argument was to criticise the cumulativist assumptions of many historians and philosophers of science. He argued that science has not accumulated knowledge over time, because later theories are sometimes radically conceptually different from their predecessors. This was turned into a defence of relativism in the later part of **Against Method**. There have been many criticisms of many of the central claims of **Against Method**. For instance, Feyerabend's claims about Galileo, his central case study, are largely false or misleading, and he did not correct problematic claims in later editions. [20] Nevertheless, there was an important point on which Feyerabend was correct. There is no universal scientific method in the form of precise formal rules that has been used by successful scientists or the scientific community.

An important weakness in Feyerabend's critique of a universal method is that he never gave a detailed argument that that critique implied relativism. Instead, he assumed that the lack of a universal method in some way implied relativism, and proceeded to develop a relativist account. In this way, Feyerabend ac-

[19] See Paul Feyerabend, **Against Method,** Third Edition, Verso Books, London 1993, pp. 195–197.

[20] See, for instance, Alan Chalmers, **Science and its Fabrication**, Open University Press, Milton Keynes 1990. I have summarised some of the major criticisms in George Couvalis, "Feyerabend, Critique of Rationality in Science", in: Byron Kaldis (ed.), **Encyclopedia of Philosophy and the Social Sciences**, Volume 1, Sage Publications, London 2013, pp. 356–359.

cepted a crucial assumption of the foundationalism that he had done so much to criticise, namely that if there is not a universal method that is given before we even begin research, we are condemned to relativism.

An Important Challenge: Shapere on Method

Dudley Shapere agreed with criticisms of universal method and cumulativism, and expanded on them. He also agreed with the view that the conceptual scheme of science has changed radically over time. However, he did not draw the relativist conclusions that Feyerabend drew from these facts in **Against Method**. Instead, he argued that although science had changed, it had changed in rational steps without benefit of a universal method. Unfortunately, Feyerabend never confronted Shapere's line of argument in any detail despite his preference for a pluralist epistemology. Instead, his criticisms focussed on Popper's and Lakatos' attempts to come up with a universal method.

In an important paper, Shapere summed up the foundationalist view of the origin of our knowledge stating it as the view that "*there is something which is presupposed by the knowledge-acquiring enterprise, but which is itself immune from revision or rejection in the light of any new knowledge or beliefs acquired*".[21] He distinguished four variations on this theme. First, that there are ontological claims which must be accepted before inquiry is possible. Second, that there is a universal method not subject to alteration. Third, that there are rules of reasoning which can never be changed. Fourth, that there are concepts employed in or talking about science which cannot be altered in the light of new knowledge. Feyerabend had argued in detail against these claims. For instance, **Against Method** was a critique of the second claim; some of his early papers were critiques of the fourth claim.

Shapere mentioned the line of argument in favour of the foundationalist view that there cannot be good reason for change unless there are universal standards. The proponents of foundationalism argue that there are only two alternatives, relativism or accepting the timeless universal standards. Note that, as we have seen,

[21] Dudley Shapere, "The Character of Scientific Change" (1983), in: Dudley Shapere, **Reason and the Search for Knowledge**, Reidel, Dordrecht 1984, pp. 205–260 [emphasis in the original].

Feyerabend gradually fell into relativism after radically criticising the foundationalist view. Contrary to Feyerabend, Shapere argued that both that the dichotomy is a false one, and that the four variations of the traditional view are all incorrect. Even though the given criteria at any stage for an explanation do mark out a range of possible explanations, the knowledge attained can lead to a change in the criteria — a "rational feedback" mechanism is involved because what is discovered by science changes the criteria themselves, as indeed it changes much else.

Shapere's view might be described as a research immanent rationalist view of the development of science rather than a research transcendent rationalist view. On his account, radical differences between the beliefs, methods, and concepts used by researchers at two different epochs do not lead to relativism or irrationalism if we can show that at every point, a change is rational on criteria rationally accepted at the time. Shapere's arguments for his view have never been adequately addressed by Feyerabendians. I will only briefly discuss some of his line of argument to give an idea of the detail and subtlety of Shapere's approach.

We have already seen how Shapere argued that what is directly observable changed rationally in the course of what was discovered in science. Already, as I have pointed out, measurements of temperature transformed science by allowing various important distinctions to be made. To the ancient Greeks, like Aristotle, heat was a qualitative property. The very idea that various aspects of it could be measured, and measured precisely, does not seem to have occurred to Aristotle or his followers. This changed with Galileo, who constructed an early thermometer and showed that amounts of heat were measurable in many situations, at least in a rough and ready manner. Using widely available common-sense criteria of the time, this changed the view of temperature radically.

Feyerabend often talked as if various aspects of the Aristotelian view made it a kind of closed system which could not be criticised from the outside. However, the Aristotelian view was enmeshed in much of the common-sense of the time, which contained much else apart from that view. Various moves were available to undermine the plausibility of the Aristotelian view, as we can see in the case of temperature. Alan Chalmers and Stillman Drake have shown in some detail how common-sense arguments were used to undermine criticisms of the use of the telescope.[22] For the Aristotelians to try to make Aristotelianism a closed system was merely a manoeuvre to save them from embarrassment. Feyerabend exaggerated for rhetorical purposes and created a kind of world view that was closed off from outside influences — something which did not exist at the time, or, indeed, at any time. In the real world, cosmological hypotheses are enmeshed in a variety of real-world practices that can be used to undermine them. Closed relativist world-views are a construction of the anthropologically minded rather than a reflection of actual societies. Actual societies are much more complex and engage in real world practices which can be used to contradict hypotheses. In Italy in Galileo's time there were many useful practices involving careful measurement and the use and design of instruments.

Various surprises occurred as a result of research on temperature towards the end of the eighteenth century. As a result of the surprising data provided by the use of early thermometers, it became clear that there is not one thing "heat". It was realised that putting in the same amount of a hot substance would raise the temperature of others substances to quite different levels. It was also realised that putting in heat into ice would not raise its temperature for a long time until it relatively suddenly turned into water. As a result of attempts to make sense of various measurements of temperature, Joseph Black and others distinguished specific heat, and various kinds of latent heat from temperature.[23] Heat conductivity was also later distinguished from temperature and measured. The ontology of heat was radically changed over time. This was all partly a consequence of the widespread design and use of ever better steam engines. It is not an accident that one of the principal theorists of the new science of heat was Joseph Black, who was assisted by the now well-known James Watt. However, the view that there is a fluid heat substance, caloric was prevalent for some time, for good reasons. Nowadays, it is recognised that the ontology of temperature is quite different from the onto-

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[22] See CHALMERS, **The Scientist's Atom and the Philosopher's Stone...** An amusing example of Galileo's use of common-sense criteria is that Galileo responded to the claim that the moons of Jupiter might be artifacts with the ironic remark that he would "pay 10, 000 scudi to anyone who made a telescope that would create satellites around one planet and not around others", Stillman DRAKE, **Galileo at Work**, University of Chicago Press, Chicago 1978, p. 166. In a society with an important and influential craft and instrument making tradition like the Northern Italy of Galileo's time, remarks like this had a significant impact.

[23] See Duane ROLLER, **Case 3: The Early Development of Temperature and Heat**, Harvard University Press, Cambridge 1950.

logy of specific heat — specific heat is a quantum phenomenon, whereas temperature is caused by the vibration of molecules. Still, at each point, changes in the ontology of heat were arrived at rationally, as careful studies of the work of Joseph Black, Lavoisier, Perrin, and others show. [24]

A similar result has occurred recently with research on pain. The idea that there is one simple thing, "pain" has been overthrown on the basis of research on reports from patients, and on the basis of anatomical research carried out according to current standards. Chronic pain is distinct from sharp pain, and the affective aspect of pain has been distinguished from the somatosensory features of pain. It is even possible for subjects to experience pain and describe it clearly but report that it does not bother them. The simple everyday concept of pain has been replaced in medical research by more sophisticated categories as a result of careful research. [25] It has been pointed out that Plato already had a rudimentary account of what has been discovered in modern research, which he arrived at through his reflections on cognitive aspects of pain. [26]

The above two case studies are useful illustrations of Shapere's point that ontology can change radically in a rational manner. There is no need to invoke relativism in explaining any change in science.

Let me now turn to logic. Shapere rightly points out, as one of his examples from the history of science, that the early versions of the calculus were inconsistent. The inconsistent version of calculus was at the very heart of early modern science because it was being used systematically in calculations. Nevertheless, scientists worked out how to use them to make precise predictions that were confirmed while avoiding the problems raised by inconsistency. So, this shows that science is not bound by consistency when abandoning consistency is fruitful. Scientists did not follow Berkeley in rejecting the use of the calculus because it was inconsistent. Of course, for mathematicians the inconsistency needed to be re-

[24] See Robert Fox, **The Caloric Theory of Gases**, Clarendon Press, Oxford 1971; Alan Chalmers, **The Scientist's Atom and the Philosopher's Stone: How Science Succeeded and Philosophy Failed to Gain Knowledge of Atoms**, *Boston Studies in the Philosophy of Science*, Vol. 279, Springer, Dordrecht 2009.

[25] See Valerie Gray Hardcastle, **The Myth of Pain**, MIT Press, Cambridge 2001; Nicola Grahek, **Feeling Pain and Being in Pain**, MIT Press, Cambridge 2011.

[26] See George Couvalis and Mathew Usher, "Plato on False Pains and Modern Cognitive Science", *Philosophical Inquiry* 2003, Vol. 25, No. 3, pp. 99–115.

solved if it could be, as indeed it was in the nineteenth and twentieth centuries. But as Shapere points out, consistency is not a fundamental requirement of the scientific enterprise. [27]

Brown and Priest have spelled out the strategy used by physicists in the seventeenth and eighteenth centuries to deal with the inconsistency of the calculus, and suggested other ways in which the strategy can be used fruitfully when confronted with an inconsistency. [28] We have already seen in a discussion of Mortensen's work that to adequately describe the phenomenology of inconsistent images or perceptions, we need to use a paraconsistentist logic and mathematics. Unfortunately, Feyerabend does not seem to have followed up his early insights into violations of the laws of Frege/Russell logic in his later work in any detail. He seems to have been unaware of significant work by paraconsistentist logicians such as Routley.

Much else can be said about Shapere's arguments. As I have emphasised, Feyerabend never engaged in a debate with those arguments. They pose a fundamental challenge to Feyerabend's move from a rejection of a universal method to relativism.

Epilogue

We have seen that Feyerabend's work, particularly his early work, contains important insights into the nature of science and logic that were developed in detail by later researchers. However, he failed to discuss a serious challenge to his move to relativism developed by Dudley Shapere. How far can we go with a research immanent account of rationality without falling into relativism? I do not know. Shapere's neglected work constitutes a well-worked out alternative to relativism. It is time Feyerabendians took it seriously.

George Couvalis

[27] See Shapere, "The Character …", p. 235ff.

[28] See Bryson Brown and Graham Priest, "Chunk and Permeate, a Paraconsistentist Inference Strategy, Part 1: The Infinitesimal Calculus", *Journal of Philosophical Logic* 2004, Vol. 33, No. 4, pp. 379–388, https://doi.org/10.1023/B:LOGI.0000036831.48866.12.

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