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Feyerabend and the Facts[1]

Paul Feyerabend likes to portray most of his predecessors as holding the view that the development of science has been essentially continuous. On this view, a branch of physical science may undergo a few "false starts", but once it has become really scientific (as astronomy did with, perhaps, Kepler and Galileo, or as optics did with, perhaps, Young and Fresnel) then any further changes in basic theory have been essentially cumulative. The new theory will standardly "penetrate to a deeper level" than the old but will leave the old "essentially" intact at its own level. Admittedly, at the common level the new and old theory's consequences may strictly speaking contradict one another but this is never more than a question of slight correction or "modification". This view is sometimes called the "layer cake" account of the development of science.

It is more difficult than Feyerabend seems to think to find philosophers who espouse this "layer cake" view in anything like the fully fledged form which he gives it.[2] But some philosophers have come close. This, for example, is how William Whewell described the development of astronomy:

> What Newton thus used and referred to as *facts*, were the *laws* which his predecessors had established. What Kepler ... had put forth as "theories" were now established truths, fit to be used in the construction of other theories. It is in this manner that one theory is built upon another; – that we rise from particulars to generals, and from one generalization to another; – that we have, in short, successive steps of induction. As Newton's laws assumed Kepler's, Kepler's laws assumed as facts the results of the planetary theory of Ptolemy; and thus the theories of each generation in the scientific world are (when thoroughly verified and established) the facts of the next generation. Newton's theory is the circle of generalization which includes all the others; – the

G. Munévar (ed.), *Beyond Reason*, 329–353.

highest point of the inductive ascent; – the catastrophe of the philosophic drama to which Plato has prologized; – the point to which men's minds had been journeying for two thousand years.[3]

Feyerabend opposes to this his own *discontinuity* view of the development of science. On Feyerabend's view, not only are successive theories in any field generally inconsistent with one another, some (perhaps most) shifts from one theory to the next have involved radical changes in basic metaphysical outlook. These changes cannot be dismissed, as some empiricists would dismiss them, as mere changes in window-dressing, since they have carried with them significant changes in the sorts of problems which scientists regard as important and in the sorts of explanatory solutions they tend to seek.

So far, of course, Feyerabend's discontinuity thesis is likely to win wide acceptance – not least from Popper, who has been arguing this thesis since the 1930s. Indeed – as the quote from Whewell suggests – the fully fledged continuity view could hardly survive the Einsteinian revolution. If scientists continue to talk of relativity (and quantum) theory as "extending" or "building on" their classical predecessors, or of Newton's theory as a "special case" of Einstein's, it is because they are thinking – usually implicitly – about continuity simply *at the empirical level.* Newton's theory postulates, amongst other things, an absolute and infinite space, an absolute time scale and velocity-independent inertial mass. Einstein's theory simply contradicts these claims. But, despite these radical discontinuities at the basic theoretical level, the newer theory of course explains the empirical success of its predecessor. It does so by entailing that its predecessor's lower level consequences, if not *strictly* accurate, are, in almost all practical experiments and observations, empirically indistinguishable from the truth. (In particular the new theory entails that this is true of those "old" experiments and observations previously taken to support the older theory). This, then, yields a weaker version of the continuity view that accepts that there have been radical discontinuities in science at the highly theoretical levels, but insists that these discontinuities do not extend to the "observational" or "instrumental" level. On this compromise view, there are no radical changes at the *lowest* observational levels in science (at the level of "crude facts" – see below). Moreover, while some switches of theory may have involved isolated losses of explanatory content (for example, Descartes' vortex theory seems to have explained why all the planets move around the sun *in the same direction* but Newton's theory does not explain this), successive theories have, at least in broad terms, explained more and more of the unchanging observational facts.[4]

Feyerabend has challenged even this weaker continuity view.[5] According to some of his claims, the discontinuities in science extend right down to the empirical level. Indeed there are no neutral observable facts; all "facts" are dependent on accepted theories and are subject to change as science develops. This, he implies, rules out the old idea that rival theories can be ranked in terms of how well they stand up to the neutral arbitration of the fixed, empirical facts, since there are no such facts. And the rejection of this idea takes Feyerabend a long way down the road towards old fashioned skeptical-relativism or "epistemological anarchism" (= skepticism plus a licence for intellectual tomfoolery).

Feyerabend (and others) have exposed some real difficulties of detail which beset straightforward accounts of the idea that scientific development has been continuous in even the weaker (empirical) sense. However, I have little doubt that this idea is fundamentally correct, and no doubt that Feyerabend has exaggerated the difficulties which it faces. The purpose of the present paper is to argue the latter claim. I shall argue that Feyerabend's shocking-sounding theses about the "theory-dependence" or "theory-impregnation" of facts turn out, on closer analysis, to consist of a "hard core" of good sense (good sense clearly articulated already by Poincaré and Duhem) surrounded by a "protective belt" of confusing rhetoric.

I shall first try to set out the "good sense" and then examine Feyerabend's two main arguments for the inevitable theory-dependence of facts to see whether they really take us beyond it.

I. 'SCIENTIFIC' AND 'CRUDE FACTS': POINCARÉ AND DUHEM

The term "fact" is a dangerously loose one, and has been responsible for a good deal of confusion. In discussing science, facts are generally contrasted with theories. The facts are the material against which the theories are tested. But, as is well known, in testing his latest theories, a scientist will generally take for granted all sorts of other theories, particularly when these have long been accepted. He will then standardly describe the "facts" against which he tests his latest theories in terms which presuppose these (usually "lower level") accepted theories. It is natural, for example, to talk of Newton's theory of gravity being tested in the eighteenth century against the "facts" about planetary positions. But the experimental astronomer might well regard herself as testing *theoretical* claims about planetary positions against the "facts" about when certain characteristic spots of light are sighted on the axes of suitably inclined telescopes. The "data" about planetary positions used in testing Newton's theory is certainly far from "crude"; it is arrived at *via* a calculation which transforms the "crude" data about the inclination of telescopes and the like into "data" about planetary positions. This calculation is informed by various *assumptions* – for example optical assumptions about the amount of atmospheric refraction; assumptions that are ultimately theories – well confirmed theories perhaps but theories nonetheless. Similarly we might talk of comparing theories about the constitution of matter with the "facts" about the behavior of certain particles in bubble chambers. But these "facts" are clearly highly interpreted ones - interpreted in the light of various theories (both about particles and about bubble chambers) in which we have come to have some confidence.

Present day philosophers of science tend to portray their predecessors as naively assuming that there is an obvious and natural distinction between theoretical and (observable) factual claims. But Carnap is usually taken as the chief representative of the "older" approach and he quite explicitly stated (in his "Testability and Meaning") that "[t]here is no sharp line between observable and non-observable predicates". And, in his *Philosophical Foundations of Physics* (p. 226) he pointed out that scientists tend to stretch the notion of an "observable" a good deal further than do strict empiricist philosophers:

> Magnitudes that can be established by relatively simple procedures – length with a ruler, time with a clock, or frequency of light waves with a spectrometer – are called observables [by scientists]. The philosopher might object that the intensity of an electric current [say] is not really observed .... Only a pointer position was observed .... There is no question here of who is using the term "observable" in a right or proper way. There is a continuum which starts with direct sensory observations and proceeds to enormously complex, indirect methods of observation. Obviously no sharp line can be drawn across this continuum; it is a matter of degree ...

This theoretical/observational continuum had indeed been quite clearly mapped by the turn of the century – by the French conventionalists, notably Poincaré and Duhem. Poincaré, for example, introduced a distinction between "crude" and "scientific facts"[6]: a statement like "the current in this wire was 10 amps" is, if it is factual at all, a statement of "scientific fact" since it is informed by certain (no doubt well-accepted) scientific theories, while a statement like "the needle in this meter pointed to (or close to) the mark '10'" is a one of "crude fact". Similarly a statement about the wavelength of a certain kind of light expresses a scientific fact, while the corresponding crude fact would be about distances between the centers of two areas of maximum illumination in a certain pattern of light and dark stripes or the angle of deviation of a certain narrow beam of light. Duhem made a similar distinction between "practical" and "theoretical facts".[7]

It has long been recognized, then, that what scientists generally take as "facts" are dependent on theoretical assumptions. The sorts of "scientific" or "theoretical facts" that Poincaré and Duhem chiefly had in mind involve what are usually called "auxiliary" or "instrumental" theories: the theory of the galvanometer, or the astronomical and optical theories underpinning the use of the telescope to determine planetary positions. Still "more theoretical" theoretical facts are possible however. Indeed once a scientist becomes confident about his high level theories then he will confidently describe all particular situations in terms of those theories. In its widest sense (facts as opposed to fictions or perhaps to values) the term "fact" seems to cover anything that actually is the case - anything which is described by a true, synthetic, descriptive statement. This means that any synthetic, descriptive statement which we take to be true expresses what we take to be a fact; and at any stage in the development of science many quite high level theories will confidently be regarded as true. In this wide sense it presently seems to be a fact that space is curved in the presence of matter, that neutrinos exist, *etc, etc*. It would certainly not be stretching the notion of a "fact" too far to say, for example, that 19th Century scientists regarded it as a fact that the planet Mars moves in a way determined by the gravitational interaction between it and all the other bodies in the universe. Scientists even talk of *seeing* certain events occur – events that are then described in ways that are highly dependent on high level, though well-accepted, theories. A modern scientist might say, for example, "Here [pointing to some bubble chamber photograph] we see a high-energy pion enter bottom left and collide with a proton to produce a lambda hyperon and a kaon – of course the latter 'strange particles' being electrically neutral leave no tracks, but we can see that they have indeed been produced via their decay products further up the photograph". (I don't of course question the legitimacy of such talk. I do claim that it is *obviously* shorthand for a much more complicated statement involving crude facts (about lines on the photo) and a whole lot of theory – high level theories about the structure of matter and lower level instrumental theories about how bubble chambers work. These theories are not normally articulated but can be when necessary).

It is no news that scientific or theoretical facts in either this extended sense, or the more restricted sense implicit in Duhem's and Poincaré's treatments, are dependent on theory. And nor is it any news that such facts may be challenged and overthrown in the development of science. This simply means that the theories on which these facts relied were once accepted but were subsequently challenged and replaced. A well known example is the clash between Newton's theory of gravitation and some of Flamsteed's facts (clearly "theoretical facts") about planetary positions. The outcome was that Newton told Flamsteed how to "recalculate" these facts so that they were brought back into line with his theory. This simply means that Newton suggested a revision of one of the theoretical assumptions (specifically about the amount of atmospheric refraction) on which the original theoretical facts were based.

If Feyerabend is to teach us anything new here, he must surely do more than argue the truism that theoretical facts are dependent on theory. Unfortunately, as we shall see, his main arguments all depend on taking "facts" at a very high theoretical level.

But isn't it Feyerabend's point that all facts, no matter how "low level" or "crude", are really theoretical facts? Are we not, in making any assertion about the world, no matter how "empirical", really making certain assumptions, so that even the crudest of crude facts are assumption or theory-impregnated?

At any rate, if our factual statements remain objective - about the "external world" rather than our own present sensations – then the answer to these questions is obviously "yes": even in reporting that the end of a certain pointer coincided roughly with the mark "10" on some scale we are assuming that the pointer really exists, that we are not constantly hallucinating, that a malicious demon is not misleading us, and so on. But this admitted (though rather dull) fact should not blind us to a more important issue.

This is the question of whether in all cases of disagreement in science we can, by taking the empirical facts concerned at a *sufficiently crude level*, get all parties to agree on the facts and hence restrict their disagreements to acknowledged theories.

This will mean that the different theories can be compared by seeing how well they stand up to the *agreed* facts. *This does indeed always seem to be possible.*

Let's return, for example, to the Newton-Flamsteed case, and take the facts provided by Flamsteed at a "cruder" level: instead of taking Flamsteed as supplying (theoretical) factual statements like "planet *p* was at position (x, y, z) at time t", let's take him as supplying (crude) factual statements like "a certain characteristic spot of light" (specifiable in terms of its visual characteristics for a normal observer) was visible on the axis of a certain "telescope" when its angle of inclination was $\phi \pm \varepsilon$ and when a certain "clock" read $T \pm \delta'$.[8] There is, of course, no suggestion that Flamsteed and his assistants "misobserved" at this cruder level. Certainly Newton did not dispute *these* facts. The problem now is simply "the Duhem problem": Newton's theory (of gravity plus mechanics) has no logical consequences at this "crude" level. That theory, after all, is about the motion of massive bodies under the action of forces and *not* about spots of light viewed in telescopes. The smallest unit that has logical consequences at this "crude factual" level is not the theory itself but a theoretical *system* incorporating the theory but *also including the various astronomical, optical, and instrumental theories that underlie the original "scientific factual" statements about planetary positions*. This will mean, as Duhem forcefully pointed out, that any empirical refutation will have a large target – the refutation will be of a large theoretical system rather than of an "isolated" "individual" theory.

All the famous episodes from the history of science that are regarded as challenging "older" empiricist accounts can, I believe, be analyzed quite straightforwardly in this Duhemian way.[9] There is no need to talk of seriously corrigible observation statements, no need therefore to talk of, or even to concede the possibility of, "fact correction" and therefore of "overturning" a refutation; instead there is always an unchallenged refutation of a theory leading to the development of a new theory. It's just that the theories concerned are rather larger items than we are used to thinking of in this regard, being better characterized as theoretical systems consisting of a "central" theory (which may itself naturally decompose into "core" and "more specific" elements) together with a (finite!) set of auxiliary and instrumental theories.

So, returning to the example: the full theoretical system - including Newton's theory but also the various astronomical, optical and instrumental auxiliaries – clashed with Flamsteed's crude facts about telescopic inclinations. The latter were accepted on all sides. Hence the full theoretical system was unambiguously refuted and could certainly not "stage a comeback". The outcome of this clash, however, was the switch to a new theoretical system which differed from the old only in a "peripheral" assumption (about the refractive index of the atmosphere). Because the central theory in the old and new theoretical systems is the same, the temptation arises to speak of Newton's theory "surviving a refutation". But this temptation can (and should) be avoided. Duhem's main message is precisely that Newton's theory *on its own* was never refuted because it is not *refutable* at all by these crude facts. On the other hand the chunk of science that was empirically refuted by the crude facts – the larger theoretical system incorporating Newton's theory – neither did (nor could) "stage a comeback".

All cases of "fact correction" in the history of science can, I believe, be dealt with in this way. Indeed, if there weren't always some level at which the facts were agreed then the empirical character of science would surely be entirely lost.[10]

Of course scientists often disagree over theories and hence they may disagree over the "theoretical facts" - especially if we take these in the wider sense and allow them to be informed by quite high-level theories. For example, the Copernicans were confident that it is a fact that the earth is continually revolving both about its own axis and around the sun, while the Aristotelians were confident that it is a fact that the earth is stationary. We should not even *expect* agreement at this level.[11] But this disagreement does not preclude the two parties' finding more "practical" or "cruder facts" on which they would be happy to agree, and against which their different theories may be compared.

If it turns out that Feyerabend's arguments merely sustain the thesis that scientists standardly regard statements that are *clearly* informed by theoretical and corrigible assumptions as factual (because they take those theoretical assumptions for granted, at any rate *pro tem*), then we surely have every right to feel disappointed. What the Feyerabend propaganda machine seems to promise is some startling new thesis which goes beyond, or still better challenges, the old methodological lessons I have just outlined, all of which could certainly have been learned from Poincaré and Duhem. Let's turn then to Feyerabend's main arguments. We shall not, I fear, escape disappointment.

II. FEYERABEND'S 'OBSERVATIONAL IDEOLOGIES': GALILEO AND THE TOWER ARGUMENT

According to Feyerabend, the "evidence" or "facts" at any stage in the development of science will be 'constituted by older ideologies' (p. 55).[12] It would therefore,

> be extremely imprudent to let the evidence judge our theories directly and without further ado. A straightforward and unqualified judgement of theories by 'facts' is bound to eliminate ideas *simply because they do not fit into the framework of some older cosmology*. (p. 67)

We cannot assert any factual statement without presupposing the current "observational ideology". This means that the competition between theory and empirical facts is biased in favor of the *status quo* and so, in order to make progress, we must try to criticize this "ideology". Such criticism is extremely difficult to produce since the "ideology" operates as a presupposition; drastic measures must be taken:

> the first step in our criticism of customary concepts and customary reactions is to step outside the circle ... and either invent a new conceptual system ... that clashes with the most carefully established observational results and confounds the most plausible theoretical principles, or to import such a system from outside science, from religion, from mythology, or the ramblings of madmen (p. 68).

So far, Feyerabend's dramatic-sounding claims. His argument for them consists almost entirely of an analysis of a particular historical example: Galileo and the "tower argument".

Assume that, as the Copernican hypothesis asserts, the earth is really rotating towards the east. Many opponents of the heliocentric view argued that it follows from this assumption that if a stone is dropped from the top of a tall tower, the earth should have travelled some yards towards the east while the stone is falling and hence the stone should land some yards to the west of the base of the tower. Hence the well known result of this experiment – that the stone arrives (pretty well) at the base of the tower – refutes Copernicus' theory.

According to Feyerabend, Galileo could "defuse" this argument only by inventing a new "observational ideology" which radically altered the "facts" of the case so that they now *supported* rather than refuted Copernicus. This means that

> ... the experience on which Galileo wants to base the Copernican view is nothing but the result of his fertile imagination ...it has been *invented.* (p. 81)

What does this striking claim really amount to? The answer is: to no more than a confusing description of a standard piece of scientific practice that is much more straightforwardly and revealingly analyzed using Duhem's account of theory-testing and Poincaré's scientific/crude fact distinction.

If we take the facts in this episode at a sufficiently high theoretical level then nothing is easier than to present Galileo and his opponents as disagreeing over them. Aristotelians can be taken as stating that the result of the tower experiment is that the "real" motion of the stone is straight, while, in these terms, Galileo would assert that the "real" motion is curved. Since these "facts", when unpacked, seem to involve the idea of absolute space, it will come as no surprise that they are theory-dependent. But equally, nothing is easier than to produce statements of sufficiently "crude facts" on which both pro- and anti-Copernicans were agreed. Everyone agreed that the stone arrives (pretty well) at the base of the tower, i.e. everyone agreed that the statement "the stone landed some yards to the west of the base of the tower" is false. It is impossible to see how anyone's ideology could affect this, as, indeed, Feyerabend himself seems quietly to admit. ("The correctness of the observation is not in question" (p. 71)).

The anti-Copernicans just made a logical error, however, if they claimed that the above false factual statement follows directly from the theory of diurnal rotation. For, as Galileo is in effect pointing out, an auxiliary is needed for this inference: an auxiliary which implies that the stone, once released, "ceases to share" in the motion of the tower. Galileo simply pointed out in effect that experiments like this one refuted, *not* Copernicus' theory itself, but only a wider theoretical system; he proposed that the prime candidate for replacement is the above mentioned auxiliary rather than Copernicus' theory; and he went on to replace that auxiliary with a new one involving a principle of (circular) inertia. This principle, as Feyerabend puts it, denies that all "real" motion (that is, motion with respect to absolute space) is "operative" (that is, has effects that we can observe).[13] The replacement produces a new theoretical system which can be assessed in terms of how well it accords with the crude, unquestioned facts.

All of this exemplifies exactly Duhem's analysis of theory-testing. And this way of presenting Galileo's intellectual strategy shows clearly that it constitutes no sort of argument for the view that "anything goes", for the view that, even in episodes which the rationalist applauds, facts and logic have frequently been overturned. No one did (nor, so far as I can see, *could* sensibly) challenge the crude facts here; and it is proper deductive logic that governs the whole analysis – and in particular points to the deductive gap between the basic Copernican claim and the (crude) result of the tower experiment. To talk of Galileo "inventing experience" is absurdly obscurantist: aside from occasionally enjoyable though consistently confusing rhetoric, Feyerabend on this point seems to add nothing to Duhem.

III. FEYERABEND AND EMPIRICAL CONTENT: BROWNIAN MOTION AND THE SECOND LAW OF THERMODYNAMICS

Feyerabend has, several times, argued a further claim about "theory impregnation" of facts:

> Not only is the description of every single fact dependent on some theory ... but there also exist facts that cannot be unearthed except with the help of alternatives to the theory to be tested and that become unavailable as soon as such alternatives are excluded.[14]

It follows from this that a theory's empirical content will depend on what rival theories exist – the invention of a new rival will generally increase, while the suppression of a rival will generally decrease, the empirical content of a given theory.

Again this *sounds* like a radical challenge to orthodox methodological views. And again Feyerabend's main argument consists of the consideration of a particular historical example (though also, in this case, of a "generalization" of the example). The example concerns Brownian motion and its relevance to the second law of thermodynamics. Feyerabend states:

> It is now known that the Brownian particle is a perpetual motion machine of the second kind and that its existence refutes the phenomenological second law. (p. 39)

He then claims that had it not been for the development (particularly by Einstein) of the new statistical-kinetic theory neither the *relevance* of the Brownian motion to the second law, nor the "fact" that the motion *refutes* that law, would ever have been spotted.

On the relevance point Feyerabend is factually wrong. He claims that without Einstein's development of the kinetic theory, it is at least highly likely "that the Brownian particle would have been regarded as an oddity" unconnected with the phenomenological theory (ibid.). In fact, a whole string of physicists (including Exner, Gouy and Poincaré) acknowledged, before Einstein's work, that the Brownian motion constituted an important difficulty for the phenomenological theory. But is Feyerabend right on the methodological point about refutation? Is he right that the "fact" of Brownian motion was capable of refuting the phenomenological theory *only once* the statistical-kinetic theory had been developed?

Feyerabend is, I assume, not merely making the (surely uncontroversial) heuristic claim that a new theory may suggest new tests of an old theory (that is, highlight hitherto unconsidered empirical consequences of the old theory as ones that it would be especially interesting to test) and that these may turn out to refute the theory. This, of course, has happened quite often and has been well-documented. Nor is he, I presume, making the still less controversial claim that historically the phenomenological theory of heat came to be *rejected* only once there was something better (viz. the statistical-kinetic theory) to put in its place. Feyerabend is, I take it, making the seemingly much stronger *logical* claim: that the actual empirical content of a theory depends on what rival theories exist – that the articulation of some new rival extends the class of empirically decidable statements which, if true, would refute the theory. On closer analysis, however, this challenging-sounding thesis again fizzles out disappointingly. The thesis can be defended, but only by interpreting it in such a way that it becomes mundane.

Feyerabend argues (p. 40) that "a direct 'refutation' of the second law which would consider only the phenomenological theory and the 'facts' of the Brownian motion is impossible". Such a demonstration of inconsistency would have required a demonstration that the Brownian particle is a perpetual motion machine of the second kind, and this, in turn,

> would have required: (a) measurement of the exact *motion* of the particle in order to ascertain the change in its kinetic energy plus the energy spent on overcoming the resistance of the fluid; and (b) it would have required precise measurements of temperature and heat transfer in the surrounding medium ... Such measurements are beyond experimental possibilities. (p. 40)

Feyerabend's requirement (a) seems to be concerned with testing for a perpetual motion machine of the *first*, not the second kind. But the full details of the case are not important. The main point is Feyerabend's explicit admission that the "measurements" involved here not only were, but *are*, "beyond experimental possibilities". That is, Feyerabend admits that the "fact" about the Brownian particle which refutes the second law – the "fact" that it is a perpetual motion machine of the second kind – could not and cannot be established experimentally either before or after the development of the kinetic theory. In what sense, then, can he claim that the development of the kinetic theory increased the empirical content of phenomenological thermodynamics?

The kinetic theory does entail that a small particle on the surface of a fluid constituting a closed system in thermodynamic equilibrium is constantly being bombarded by the molecules of the fluid. The particle may move under this bombardment and thus work may be done (against the resistance of the fluid) without a further source of heat at a different temperature being involved. Subject to various auxiliary conditions being met, this is what the kinetic theory says is "in fact" going on in various experimental situations and such a description is inconsistent with the phenomenological second law. But only in a stretched, indeed overstretched, sense can this be said to be an *experimental* refutation. And only in this same overstretched sense can the new kinetic theory be said to have extended the *empirical* content of the phenomenological theory.

The "fact" concerned – that the Brownian particle is a perpetual motion machine of the second kind – is clearly a very "scientific" one. If we allow any "fact" no matter how "high level", no matter how "theory impregnated", to count as part of a given theory's *empirical* content then, it is of course trivially true that empirical content will depend on what rivals exist. The photon theory of light entails that whenever I see a beam of light my eyes are "in fact" being bombarded by a stream of photons. This "fact" contradicts the classical wave theory but was "unavailable" until the new theory was articulated. Hence the "empirical content" of the old wave theory was extended by the introduction of the photon theory! Similarly, since the general theory of relativity implies that when an astronomer follows the path of a planet he is "in fact" seeing a massive body move along a geodesic in space-time and since this contradicts the Newtonian account, it could presumably in this sense be said that the articulation of the general theory of relativity increased the empirical content (and led to a new experimental refutation) of Newton's theory!

Once it is accepted that new deep level theories standardly contradict their predecessors, it is not at all surprising that a new theory's fully interpreted account of some *particular event* should contradict the corresponding account of that same event given by the old theory. Of course these accounts are empirical in some sense – they are open to revision in the light of evidence, but they are certainly not anything like *directly* checkable on the basis of experience. It does seem at any rate most of the time that Feyerabend is talking of the "empirical content" of a theory in something like Popper's sense, and whatever the specific unclarities of his account, Popper clearly took a theory's "empirical content" to consist of "basic statements" which are undoubtedly spatio-temporally singular and which – at any rate originally – were meant to involve no *high level* theory. I need hardly say that the singularity requirement alone excludes statements about *perpetual* motion machines from any theory's empirical content thus construed.

If the empirical content of a theory is restricted to statements at a lower or "cruder" factual level, then nothing Feyerabend says suggests that a new theory can extend a given rival's empirical content. Indeed he more or less explicitly admits, as we saw, that when "empirical content" is understood in this more restricted way, his claim no longer applies. On the other hand, if *any* consequence of a scientific theory is counted as empirical (even any singular consequence) then Feyerabend's dramatic *sounding* thesis becomes true but trivial.

Is there a way of understanding Feyerabend's claim that makes it both true and interesting? Ronald Laymon has argued that there is.[15] In order to prepare the ground for Laymon's analysis, we should first see that the whole historical episode concerning Brownian motion (just like its Galilean counterpart) can be told quite simply and revealingly in Duhemian terms – without any bewildering talk about inventing experience, consulting the ramblings of madmen, new rivals extending the empirical contents of given theories and the rest.

Suppose we insist that the "facts" of the case are to be (relatively) crude ones about whether or not certain small particles do or do not move noticeably in an irregular fashion, or about whether or not a set of small particles suspended in a certain medium adopts a given spatial distribution in certain specifiable circumstances. It is a logical fact that the phenomenological theory on its own has no consequences which are testable at this level. That theory on its own does imply, however, that a particle on the surface of a fluid will not undergo accelerated motion, *provided* it is not subject to an external disturbance (accepted scientific theories supply various disturbances that *might* be operative) and *provided* the liquid on whose surface the particle rests is in thermal equilibrium. In order to obtain a theoretical system which *is* testable at this relatively crude factual level, then, some further assumptions must be added to the basic phenomenological theory *T*. These extra assumptions ought to be well-supported but they will nonetheless clearly be conjectural and corrigible.

These extra assumptions sanction the inference from the fact that some particular physical system is known to satisfy certain (crudely) observable conditions to the (conjectural) conclusion that the system also satisfies the (not directly ascertainable) conditions of being closed against external disturbance and in thermal equilibrium. Different auxiliary assumptions *A* of this kind, specifying different observable circumstances under which a system could be taken to be closed and in thermal equilibrium, were produced over time. The original auxiliaries were such that the whole system *T & A* was inconsistent with "crude facts" about Brownian motion – i.e. the system predicted no noticeable movement of the particles.

The natural first move, however, was to try to mend this inconsistency by modifying *A* rather than *T* i.e., by conjecturing that hitherto unsuspected perturbations or sources of disequilibrium were present in what were originally thought to be closed systems in thermal equilibrium. A particularly popular modifying assumption was that the light needed to view the phenomenon was causing local movements away from thermal equilibrium, which in turn explained the motion of the Brownian particle consistently with the phenomenological theory. Any such move leads to a different theoretical system *T & A'* which may make certain new "crude" predictions. This particular modification predicts, for example, that if, in the same experimental set-ups as before, the light used to view the motion is greatly reduced then so is the extent of the motion. This factual prediction again turned out to be false, however.

In Lakatosian terms, the phenomenological program, at least in this regard, made no progress. Eventually an entirely new central theory *T'* (the statistical-kinetic theory) was proposed as a replacement for the phenomenological theory. *T'* together with some extra auxiliaries *and the old auxiliaries* about which systems were (at least approximately) closed and in thermal equilibrium made startling new predictions. In particular, Einstein showed that a set of particles suspended in a certain medium would have a certain precise spatial distribution. This "crude" prediction could be checked and was found to hold.

In other words, the most important methodological role played by (relatively crude) empirical results was not to refute any particular system but to *verify* a prediction of the new theoretical system built round the new "central" theory. This brings the episode precisely into line with the general methodological views of Duhem (and of Lakatos).

Laymon suggests, in effect, that the above Duhemian analysis points to a reasonable way in which we might speak of the development of the kinetic theory having produced an extra empirical refutation of the phenomenological theory – one which, just as Feyerabend claims, would not have been "available", or would have been "hidden", without that new theory.

In the attempt to produce a theoretical system based on the phenomenological theory that was consistent with the observations of Brownian motion, various replacements had been suggested for the "old" auxiliary, A, characterizing those systems that are (to all practical intents and purposes) closed and in thermal equilibrium. These suggested replacements had failed (that is, the systems amended so as to include them had either not been independently testable or had failed independent tests). Nonetheless, the original auxiliary, A, could not be regarded as firmly accepted. The new theoretical system based on the statistical-kinetic theory but also incorporating the "old" auxiliary A had, however, then scored impressive predictive success. This confirmed that new system and in particular, says Laymon, confirmed A. This in turn 'refutes' (or at any rate "firms up" the refutation of) the phenomenological theory T. The conjunction T & A had all along been refuted by O (the observations of the Brownian particle); the predictive success (in Perrin's experiment) of the conjunction T' & A gave us extra reason to think A true and therefore extra reason to think that it was T rather than A that was refuted by O.

This *may* have been what Feyerabend had in mind, if only obscurely. But it certainly does not account for all of Feyerabend's explicit claims. For example, far from the facts about Brownian motion being "irrelevant" to the phenomenological theory ahead of the development of the statistical-kinetic theory (as Feyerabend claims), the analysis makes it patent what relevance Exner, Gouy and others saw here: after all there was *some* reason to think A might be true in advance of the development of T' (proposed alternatives to A having failed when conjoined with T) even if there was still more reason to think A true after the development of T'. Indeed, this is, on the present analysis, hardly a case of a "*hidden* refuting fact", but rather a fact that was well-known to *threaten* the phenomenological theory, though it was also known that the threat might be averted if some successful replacement could be found for the auxiliary A.

But laying aside the question of how accurate or generous Laymon's analysis is as a gloss on Feyerabend, the main point is that it fails to improve on the Duhemian analysis previously given – indeed it seems to me to constitute a (small but) definite step backwards compared to that analysis. Even if Laymon is right that this is Feyerabend's message, that message is *at best* a rather confusing reformulation of the old message that should already have been received from Duhem.

The clearest way to express Duhem's point about theory-testing is that *only theoretical systems*, and not "single" scientific theories, are empirically refutable – because only such systems have directly checkable empirical consequences. Why then strive so hard to talk of particular components of such theoretical systems themselves being "refuted"? The important role played by observation in this case (as, I believe, in general) was the *verification* of an independent prediction of the new theoretical system. The conjunction of the auxiliary A and the phenomenological theory was *all along* refuted;[16] either conjunct separately was *all along* irrefutable. The question all along (the "Duhem question") was which of the two conjuncts to *replace* in the light of the observations. (It might of course have been both). Several attempts to replace the auxiliary while retaining the phenomenological theory had failed to gain any independent support (excess confirmation); replacing the phenomenological theory and retaining the auxiliary *did* lead to a new prediction which was experimentally confirmed. This gave good (and new) grounds for the *replacement (not* refutation) of the phenomenological theory. Of course it is hardly surprising that the question of whether or not a given theory gets *replaced* by another depends on what rival theories exist!

Alongside his analysis of the Brownian motion example, Feyerabend produces a "generalization" of the example. Does it reveal any further way in which his thesis might be interpreted so as to make it both true and interesting?

Feyerabend writes:

We may generalize this example as follows: assume that a theory T has a consequence C and that the actual state of affairs in the world is correctly described by C', where C

and C' are experimentally indistinguishable. Assume furthermore that C', but not C, triggers, or causes a macroscopic process M that can be observed very easily .... In this case there exist observations, viz. the observations of M, which are sufficient for refuting T, although there is no possibility whatever to find this out on the basis of observations alone. What is needed in order to discover the limitations of T implied by the existence of M is another theory T', which implies C', which connects C' with M, can be independently confirmed, and promises to be a satisfactory substitute for T where this theory can still be said to be correct. Such a theory will have to be inconsistent with T, and it will have to be introduced not because T has been in need of revision, but in order to discover whether T is in need of revision.[17]

There are certain rather obvious difficulties with this – in particular various confusions of states of affairs with descriptions of them. Let me begin then by trying to elucidate Feyerabend's message as best I can.[18]

We are presented with a theory T which has the statement C as a logical consequence. Although C is not directly empirical testable, we are eventually going to discover that it is false: the "actual state of affairs" being described by C', inconsistent with C. There is, as yet, no question of an experimental refutation of T, however, since C and C' are "experimentally indistinguishable". Feyerabend clearly intends that this be altered by the invention of the new theory T'. The articulation of T' is, somehow or other, to lead to a "new" experimental refutation of the old theory T. The new T', in Feyerabend's story, certainly entails (the true) C' and also "connects" C' with some "macroscopic process M that can be observed very easily". Since we are aiming at a refutation of T we are presumably going to need a sentence describing the process M – let P be such a sentence.

Now, according to Feyerabend, C' "triggers" M. I suppose this means that the state of affairs described by C' causes (*via* the operation of natural laws) the process M. According to Feyerabend the new theory T' "connects" C' with M. I interpret *this* as meaning that T' entails the conditional sentence $C' \rightarrow P$. (P, remember, is a description of M). It follows, since T' entails C', that T' entails P. And this in turn means that T' receives (via the empirically true P) an empirical confirmation not shared by the older theory T.

We have then an "excess verification" of T', but where is Feyerabend's promised new refutation of the old theory T? So far none has cropped up, though Feyerabend clearly intends that one should have. I presume then that when he says that C (which is, remember a consequence of T) does not "trigger" M, he intends us to assume that C does "trigger" (or rather that it would, if it really held, "trigger") some different "easily observable macroscopic process", N say (where N might be simply the non-occurrence of M). If we let Q be a description of N, then, since M rather than N is the case, Q is empirically false (and, of course, logically inconsistent with the empirically true P).

If C "triggers" N, then, on the same interpretation as before, this is described by the conditional sentence $C \rightarrow Q$. Now while it may be true that C "triggers" N, our access to such information is, of course, exclusively through theories. Some (presumably accepted) theory, some putative law of nature, must imply the conditional $C \rightarrow Q$.

Which precise theory might have $C \rightarrow Q$ as a consequence? Only two theories appear (at any rate explicitly) in Feyerabend's story. The old theory T was not refutable by M before T' appeared on the scene and so T cannot entail Q. (Q remember states that N is the case and hence is a false description of the real observable state of affairs M). But this means that it cannot be T which entails $C \rightarrow Q$. (This is because T definitely entails C and so, if it entailed $C \rightarrow Q$, it would also entail Q, and hence be refuted by M – all without any assistance from T').

The only other theory explicitly mentioned by Feyerabend is the new theory T'. Is it T' that entails $C \rightarrow Q$? This might at first glance seem to capture Feyerabend's intentions. If T' does indeed entail $C \rightarrow Q$, then, given that T entails C, we have that T' entails $T \rightarrow$ Q. Since *Q is empirically false it seems that the introduction of* T' has indeed allowed us to derive a new testable (and actually false) consequence from T. This can quickly be seen to be mistaken, however. In Feyerabend's story T and T' are inconsistent (they entail the mutually inconsistent C and C' respectively). This means that T' entails the conditional $T \rightarrow$ R, for *any* statement R. So if this were what Feyerabend had in mind, he would be committed to the absurd position that the articulation of *any* theory inconsistent with a given one immediately increases the given theory's empirical content to encompass *every* expressible empirical statement.

If we take Feyerabend's "generalization" of his Brownian motion example at anything like face value, then it threatens to melt away entirely. But perhaps he meant something rather different from what he actually says. Again Laymon's conjecture seems plausible. Feyerabend is implicitly assuming that the theory which reveals that C "triggers" N is some *auxiliary* assumption A, quite separate from either T or T'. This would mean that, since T entails C and A entails $C \rightarrow$ Q, then the conjunction T & A would, *unlike* T alone, have the extra (and empirically false) consequence Q. A may initially be somewhat uncertain, and so if the conjunction of that very same A and a new central theory T' makes some startling new and correct prediction, then we may come to have extra faith in A. It seems fair then to say that, in these circumstances, this reason for extra faith in A is at the same time an (admittedly *indirect)* reason for extra faith in the falsity of T.

I have already indicated that this seems to me an exceptionally generous gloss on Feyerabend's words. For example he does say in describing his "generalization" of the Brownian motion example that, in the type of case he is delineating, the new theory is needed in order to "discover the limitations of [the old theory] implied by" the old observations. This seems clearly to imply that, in advance of the articulation of the new theory T', there was no reason at all for adherents of T to feel threatened by the observations. But Laymon's gloss certainly does not capture this implication: the process may have lent *extra* support to A but A will certainly not have been plucked out of the blue; it will have already seemed at any rate a very plausible assumption with some empirical support. Hence T will already have seemed threatened by the "old" observations.

The main point, however, as before, is that, even if this is what Feyerabend intended, it represents no more than a rather confusing redescription of Duhemian point. Duhem pointed out what we normally take to be a single scientific theory T will indeed have no empirical consequences unless conjoined with auxiliaries. But conjoining T with an auxiliary does not increase the empirical content of T (increase it from the empty set if Duhem is right): rather, there are here two *separate* theories T and T & A both of which have fixed and unchanging empirical contents, the stronger second generally having higher empirical content than the weaker first. If *only* T & A and not T alone can be refuted, why talk at all of T being refuted? Why talk of the refutation of T being "firmed up" by the success of T'? This is certainly unnecessary and almost certainly confusing: the episode can be described perfectly well and the reason for the replacement of T by T' explained without talking about any refutations of T, and *a fortiori* without talking about any *extra* refutations of T.

In sum, then, it does seem to me that Feyerabend's claims about "theory-impregnation" amount *at best* merely to confusing ways of putting old methodological points. If we take Feyerabend at his word and thus as claiming that the empirical content of a *given, fixed* theory is dependent on what rivals to it have been articulated, then his thesis is either trivial (if we take an extended notion of what counts as "empirical") or false (if we take empirical consequences at the "crude" level). If we try to save Feyerabend from this dichotomy by interpreting him Laymon-fashion, then, although the interpretation constitutes an important methodological point, it is one that was better developed already by Duhem and which it is confusing to express in terms of a given theory's sprouting extra empirical consequences.[19]

IV. FEYERABEND AND 'KUHN LOSS'

I believe that Feyerabend makes many of his most startling-sounding methodological points in *Against Method* (and not just those explicitly discussed above) by operating at the level of *highly* theoretical "facts".

For instance, the famous claim that content is always lost during scientific revolutions (an important premise in his argument for "incommensurability") is sustained by examples like the following. Content about the specific gravity of phlogiston was lost during the Chemical Revolution; content about the characteristics of witches was lost during the switch to more modern theories of mental illness; content about the ether was lost during the relativity revolution.[20]

But *of course* there are losses of content at these highly theoretical levels. *Of course* the "facts as currently accepted theories allege them to be" change as currently accepted theories change; *of course* the "theoretical facts" change as the auxiliary and observational theories which underpin them change. The crucial fact for the rationality of science, however, is that we arrive at unchallenged and unchanging facts if we go "low" enough. The experimental results can – it seems – be described in agreed terms neutral between high level rival theories, between, for example, the phlogiston and oxygen theories in a way that then permits rational adjudication between the two high-level theories in terms of how well they stand up to the neutral facts. No one ever thought that statements about the specific gravity of phlogiston were likely to play this neutral role. But statements about the combined weights of the products of certain reactions (reactions described differently by the two theories of course) might play this role. What experimental results *of this kind* could be accounted for by Priestley's phlogiston theory but not by Lavoisier's oxygen theory? Feyerabend never says. Indeed, although his radical "epistemological anarchist" claims certainly require a denial of the thesis that if we go low enough we arrive at facts which are, if not theory free, at any rate neutral between the rival theories at issue, none of his arguments and *none of his examples* supplies a shred of evidence for that denial.

I have no doubt that there are genuine cases of "Kuhn" or "Feyerabend loss" even when we restrict "lost" content to the crude factual level. But such losses as do occur are altogether more minor and transient and altogether less threatening than Feyerabend in particular would like them to appear. The scientists involved regard the "loss" as a real problem to be solved as quickly as possible; the "explanation" that is lost in jettisoning the old theory is seldom truly satisfactory; and – of special importance – in all the cases I am aware of, *failing* to make the switch to the new theory would have led to "losses" of codified content several orders of magnitude higher than the loss actually sustained in making the switch. Given these circumstances, even if we insist that empirical content be *low level* before it possibly counts as "lost", the phenomenon hardly constitutes a major problem for the idea that the switch from the old theory to the new is a switch from a good to a still *better* theory. And certainly the sort of "lost content" cited by Feyerabend is just no problem at all for this idea: having repudiated the whole idea of phlogiston or witches or the ether, the proponents of the new theories in Feyerabend's examples would not even seek to restore *this* lost content. It *obviously* cannot be a problem to "lose" content that is premised on theoretical assumptions that the later theory implies are unambiguously false. Feyerabend's examples do not support "incommensurability" in any interesting sense.[21]

Feyerabend seems in a variety of ways to have bamboozled philosophers into quite erroneously believing that he is making startling new challenges to more orthodox wisdom.

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NOTES

[1] This paper was written in 1979 and published in German translation in 1981. In view of the very tight deadlines for the present publication I have been unable either to rethink the arguments or to take the subsequent literature into account. Instead I

have done no more than attempt to sharpen the presentation of the arguments as they stood in 1979.

I thank Peter Urbach and John Watkins for some incisive critical comments on a draft of the original paper. A series of extremely useful discussions with Elie Zahar helped to clarify my mind about some of the issues raised.

[2] Indeed some of Feyerabend's own formulations – especially when he is discussing the "consistency requirement" – rule out the possibility of the new theory's even modifying the old. Surely philosophers have always been aware that new theories standardly at least modify their predecessors: though admittedly some of them may have "idealized away" these modifications in the attempt to develop a precise logic of science.

[3] William Whewell, *History of the Inductive Sciences*, Part II, pp. 138–139. Notice even here, however, an equivocation: Whewell talks of Kepler's laws assuming 'the results' of Ptolemy's theory and not of course the theory itself (which clearly contradicts Kepler's basic Copernican heliostaticism); is Whewell then claiming anything more than continuity at the level of accepted empirical and observational results?

[4] This compromise view is stated very clearly, for example, by John Watkins: 'Typically [the new theory] S_2 will more or less flatly repudiate the ontology of [the old] S_1 ...[while at] the empirical level there will be near continuity between the predictive implications of the old theory S_1 and the new S_2.' (p. 396 of his "*The Unity of Popper's Thought*: in P.A. Schlipp (ed): *The Philosophy of Karl Popper*, Vol. I).

Peter Urbach reminded me of the Descartes-Newton example of "Kuhn loss".

[5] Or, at least, he has often seemed to. See below, pp. 337–39.

[6] See his *The Value of Science*, Dover PB, especially Part III, Chapter X.

[7] See Duhem's *The Aim and Structure of Physical Theory*, Atheneum, especially Part II, Chapter IV.

[8] The terms in scare quotes are to be understood purely nominalistically. As Duhem again pointed out, even to describe some construction of metal and wires as a 'galvanometer' or some metal and glass arrangement as a 'telescope' is normally taken to involve certain theories. But by understanding 'telescope' as simply picking out 'that bit of machinery over there' or 'clock' as 'that thing over there with a dial and "hands"', the factual statement can be stripped of even these low-level instrumental theories (though these are so firmly entrenched that one would scarcely ever bother).

[9] In fact, Duhem himself was not as clear as he might have been here: creating the impression that there are two independent reasons why it is impossible to test a scientific theory "in isolation" – (a) the need for auxiliaries and (b) the theory-impregnation of the 'facts'. (Indeed some of Duhem's less well considered remarks seem to endorse the sort of 'holism' that many philosophers take from Quine, although his arguments clearly establish only a much more modest view – 'largism' as I have called it elsewhere. Not the view that 'the whole of our knowledge is involved in any test' – whatever that might mean – but the claim that the smallest unit which has genuinely (i.e. crude) empirical consequences is a good deal larger than one might initially think and certainly larger than the 'single' 'isolated' theory).

[10] This position is closely analogous to the one developed by Popper, notably in his *The Logic of Scientific Discovery*. Popper's position is, however, rather obscured by a

reluctance to admit that the facts become intuitively more secure, 'less corrigible' as we go 'lower down'. This reluctance is part and parcel of Popper's "anti-foundationalist" insistence that we can always further test any basic statement. But I have never been able to see exactly how this could be done (assuming of course, that the 'basic statement' has already been intersubjectively checked and accepted). How, for example, could the statement 'The needle seemed to me to point to around "10" on the scale' possibly be 'further tested'? In those cases (of statements of theoretical fact) where it does seem superficially to make sense to talk of 'further testing' a basic statement, this is always better described as testing some hitherto accepted auxiliary theory. In our example although one might talk loosely of Flamsteed's (planetary position) data being 'further tested', the situation is much more clearly described, as above, as one where an underlying theoretical assumption, in terms of which the (theoretical) data had hitherto been expressed, came to be questioned.

[11] This is why some of Hanson's arguments in his *Patterns of Discovery* (particularly about Kepler's and Tycho's disagreements over the 'facts' of a sunrise) seem so trivial.

[12] This, and all other page references without further details, are to Feyerabend's book *Against Method*, New Left Books, 1975.

[13] Feyerabend claims that Galileo here denied an idea that was almost universally held before him. I could detect no trace of a serious argument for Feyerabend's claim, however, which seems to face a range of well-known counterexamples.

Nor could I see any reason for Feyerabend's dismissal as a 'propaganda exercise' of Galileo's surely perfectly reasonable (and hardly novel) point that a principle of relativity, or non-observability of shared motions, had long been accepted with respect to some experimental situations – e.g. aboard smoothly sailing ships. Of course Galileo's circular inertia principle – although it solves this particular experimental problem – proved unacceptable: that is, the Copernican system incorporating it met further experimental refutations and the circular inertia principle was eventually replaced. But the fact that Galileo's solution of this problem was unsuccessful (or only partially successful) again hardly constitutes grounds for claiming that he broke the rationalist's rules of science. Both the reason for Galileo's manoeuvre and for its limited success can be straightforwardly explained on a Duhemian analysis.

[14] See p. 175 of his paper 'Problems of Empiricism' in R. Colodny (ed): *Beyond the Edge of Certainty*, University of Pittsburgh Press, 1965. And *Against Method*, p. 39.

[15] Laymon, "Feyerabend, Brownian Motion and the Hiddenness of Refuting Facts', *Philosophy of Science*, 44 (1977), 225–247.

[16] I am deliberately simplifying here of course: no doubt a full analysis of the deductive structure of this test would reveal further components of the theoretical system beyond simply T and A.

[17] 'Problems of empiricism', p. 197. As Colin Howson pointed out to me, Feyerabend's story is superficially incoherent: how can 'there exist observations sufficient for refuting T', but at the same time, 'no possibility whatever to find this out on the basis of T and of the observations alone'? If observation of M is sufficient to refute T, then surely T must entail some statement falsified by M. I endeavour to resolve this mystery below.

[18] In my attempted clarification of Feyerabend's claims, in order to avoid an excess of quotation marks, I have not gone to the lengths of pedantically differentiating between

uses and mentions of sentences. I trust however that this will cause no confusion, whereas I think that some of Feyerabend's formulations are confusing.

[19] The above constitutes an elaboration of an argument which I sketched in a review of *Against Method* (the review appeared in *Erkenntnis*, 13 (1978), 279–295). Paul Feyerabend's reply ('Life at the LSE?) appeared as Chapter 5 of Part III of his *Science in a Free Society*, NLB, 1978. As far as I can tell, he there partly gives in and partly accuses me of reverting to positivism. No doubt my use of the scientific/crude fact distinction will also be branded positivist – but this hardly constitutes an argument against it. (The title of the section of Feyerabend's book in which his reply appears is 'Conversations with Illiterates': no doubt then it was deliberate policy, sparked by Paul's celebrated sense of humor, consistently to misspell my name, and to brand at least two of my claims as 'disingenious').

[20] The phenomenon (or alleged phenomenon) of "losing" codified empirical content by rejecting one theory in favor of a "revolutionary" new one is often called, of course, 'Kuhn loss'. Kuhn's own cases (in for example his *The Essential Tension*, Chicago, 1979) are rather better than Feyerabend's – though, in all honesty, not much. One example that Kuhn cites more than once is the following: the phlogiston theory could, but Lavoisier's oxygen theory could not, explain why metals are "more similar" to one another than are metallic calxes. This seems to me an extremely strange "empirical fact" – indeed an extremely strange fact. The notion of 'similarity' is of course multiply ambiguous: one wonders in what precise sense the metals are supposed to be "more similar" to one another than the calxes. (I need hardly say that this 'fact', if it be one at all, does not count as a crude fact in my or Poincaré's sense). The alleged explanation by the phlogiston theory of this strange fact seems, if anything, stranger still. The "explanation" according to Kuhn is that all the metals contain phlogiston. If this is an explanation, then the claim that coal and petrol and the Koh-i-Nor diamond all share the common ingredient of carbon should predict that they are all "similar" to one another. Well, they certainly are similar to one another in that they all contain carbon!

[21] Feyerabend's apparent admission (*Against Method*, p. 279) that theories which are incommensurable in his sense (whatever that sense may be) may nonetheless be commensurable 'instrumentally' seems to condemn him once again of having confused everyone into believing that he is arguing a really challenging unorthodox view when he is actually arguing a thesis that could hardly be denied. (Of course insisting on the importance of the 'instrumental' (i.e. low level empirical) comparability of theories does not commit one to an overall instrumentalist view of theories).