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# THEORETICAL PLURALISM AND INCOMMENSURABILITY : IMPLICATIONS FOR SCIENCE AND EDUCATION*

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## *Introduction*

A central problem in the philosophy of science is : What are the methods by which we can reasonably expect to increase knowledge ? A fundamental part of the answer to this problem is : By means of criticism. Therefore a major task for philosophers of science is to develop a detailed theory of criticism. Much work has already been done — Popper has shown that the opportunities for criticism are increased when a theory has a high empirical content and argued that a theory can be more severely tested or criticized if it conflicts with our background expectations.

Lakatos has extended the theory of criticism by showing how a research programme can be criticized for being *ad hoc* or moving through"degenerating problem shifts". Agassi has introduced the notion of metaphysical criticism and Laudan has catalogued various conceptual problems which theories may encounter.

In the first half of this paper I wish to discuss an important additional component of the theory of criticism — what I shall call methodological pluralism. The advantages of working with a plurality of theories have been clearly stated in Feyerabend's writings but the importance of this contribution appears to have been neglected, partly perhaps because of the flurry of debate about the incommensurability thesis. The second part of my paper discusses methods for comparing radically different theories.

A complete critical methodology would also warn us about common strategies for avoiding criticism. Popper has discussed various ways in which theories may be protected from criticism. They may be couched in such imprecise language that it is impossible to tell what would count as evidence for or against them. Or they

may have a built-in *ad hominem* clause, such as "Your evidence against the theory doesn't count because you're defensive (psychology)/bourgeois (sociology)/a man (feminist politics)/not a teacher (education theory)."

As a corollary to my arguments for theoretical pluralism, I will illustrate two additional strategies for avoiding criticism. The first tends to be found in the area of natural science — and it is based on the assumption that the prevailing theory is fairly successful. Hence, the attempt to proliferate alternative theories is thought to be irrational or irresponsible. (I will call this Misguided Monolithicism.)

The second strategy for avoiding criticism appears to be more prevalent in the social sciences and related areas. Here one finds a diversity of theories which one might consider to be in competition, but they are carefully insulated from each other by claims that "these systems can't be compared because they use different concepts" or "they're really talking about different things (or at least different aspects of the same thing)" or "it all depends on your point of view". If proponents of both systems agree on such a division of labour, the possibilities for pluralistic criticism are reduced considerably. (I will call this Protective Partitioning.)

Thus I will exhort physical scientists to become more tolerant of diverse approaches, but advise social scientists to spend more time in trying to arrive at a more unified, coherent theoretical approach. Science curricula should reflect these methodological correctives.

### *1. The Trend Towards Unity in the Natural Sciences and How it Obscures Theoretical Pluralism.*

Among 'progressive' or 'enlightened' educators there is considerable agreement that the teaching of the liberal arts such as history, literary criticism, religion, political theory, etc., should be such that the student becomes aware of various ways of approaching these subjects and of the many positions and points of view which exist. For example, it is held that one should not only study democracy, but also fascism and communism. And to use one history book, regardless of its quality, would be considered by such people to be tantamount to indoctrination.

However, among science educators there seems to be no comparable tendency to present a plurality of views. One might present several sciences, e.g. biology and physics (although even these "separate" sciences are closely related), or use different pedagogical

methods to teach a particular science, e.g., 'discovery' methods and didactic lecturing, but there is only *one* periodic classification of the elements and only *one* set of Newton's laws. There seems to be no alternative chemical or mechanical viewpoints which *could* be presented and certainly no obvious reason for doing so. To be specific, the only reasons for using more than one physics text are relatively superficial ones, such as variations in the breadth or depth of coverage. The texts differ in *how much* content is presented and *how* it is presented. But different texts rarely make contradictory claims.

What theory about the nature and growth of science underlies these pedagogical practices ? It has traditionally been held that science evolves towards a single, coherent, comprehensive theory. By saying a theory is coherent we mean that it is *not* possible to axiomatize it in such a way that the axioms can be separated into two or more sets that are disjoint in the following sense :

(i) Not all the significant recognized theorems of the theory can be derived from one set alone (i.e., neither of the sets is a sufficient axiomatization of the theory);

(ii) No additional significant recognized theorem is derivable by conjoining the sets (i.e., the separated sets are sufficient to derive all the significant recognized theorems of the theory).

We can now clarify the claim that natural science tends to become more unified : During the historical development of science separate theories have generally eventually been replaced by a single theory which is coherent in the sense defined above.

There is little agreement amongst current philosophers of science about the exact relationship between successive theories in the history of science. (In stating the thesis above I deliberately used a vague term "replaced".) However, I think there is wide endorsement of the following claims : (1) Science does make epistemological progress (whether *progress* should be defined in terms of corroborated empirical content, problem solving capacity or heuristic power is quite another matter). (2) Progress in science is typically not cumulative, especially on the deepest theoretical level. (3) The relative coherence of the later theories in a field is typically achieved through earlier clashes between radically different research programs.

Thus our admiration for the power and scope of modern chemistry or optics or mechanics should not lead us to forget that these unified theories arose historically out of conflict between vialists and mechanists, wave theorists and corpuscularians, advocates

of energetics and atomists, etc. It should also be emphasized that the presence of radically different competing programs is a common feature of growing science, not just a temporary interim condition. Perhaps one could even re-write Kuhn, arguing that "normal science" is a rather abnormal phenomenon — one which only occurs in periods of what I will later call Misguided Monolithicism ! (Agassi makes a similar point in his *Science in Flux*.)

*2. The Methodological Advantages of Theoretical Pluralism*

There are many historical precedents for the introduction of alternative theories before the prevailing theory is discarded but are there stronger reasons for recommending this methodology other than "successful scientists do it" ? Why is external criticism, i.e., criticism based on an external system, a valuable addition to internal criticism ?

In "Explanation, Reduction, and Empiricism", Feyerabend briefly mentions several advantages of constructing and entertaining alternative theories instead of only criticising the prevailing theory from within. I will now elaborate on his suggestions, making free use of philosophical ideas which Feyerabend does not endorse, and illustrate them with a few examples.

*a) Scientific advantages : The use of alternative theories tends to increase the severity of testing and may lead more quickly to new discoveries.*

The proponents of a theory (T) may have a fairly clear idea of which 'parts' of the theory have been severely tested, in which domain careful experiments have been carried out, etc., but the theory itself cannot tell them which prediction of the theory to test next. There are always an infinite number of tests which could be carried out. However, the invention of an alternative theory (T'), i.e., one which *contradicts* the theory in hand, will insure an exact specification of which experiments are important, namely those which would constitute a 'crucial experiment' between the two alternatives.

Subjective Bayesians would quickly add that the mere existence of an alternative theory is not enough — after all ~T is always available. What really is required for severe testing is a somewhat plausible alternative.

Consider two examples of severe tests of Newtonian theory — that provided by Galileo's law stating the constant acceleration of falling bodies and that arising from Einstein's prediction of the gravitational bending of light. It happened Newton's theory passed one test and failed the other, but both tests were provided by attractive alternative theories. By contrast, an experiment to find out whether radioactive billiard balls will rise in a vacuum has, to my knowledge, never been carried out — the reason being that there is no plausible alternative physics which has that as a consequence. Hence, verifying that particular prediction of our theory would be of minimal significance.

There is a second way in which working with a family of alternative theories may lead to an increase in knowledge. Consider a theory T which makes claims about a domain $D$ but not about domain $d$; it may be that according to T, $D$ and $d$ are held not to be related, or perhaps the phenomena in $d$ have never been observed. Suppose that according to a strong alternative theory T′, phenomena in $D$ and $d$ may be given a unified explanation. This will either refute the claim of T that they are distinct *or* leave T open to the claim of being incomplete if it cannot be extended so as to cover domain $d$.

An historical example : The early phlogiston theory explained a certain series of chemical reactions in terms of the transfer of phlogiston. It was a tenet of this research programme that weight was a physical property of materials and that the qualities chemists talked about were independent of physical properties. To Lavoisier, who first formulated the law of conservation of matter, weight changes were an important method of investigating a chemical reaction. His theory of combustion *simultaneously* explained both the increase in weight of a metal heated in air and 'chemical' changes (such as the inability of the combustion product to react with nitre). Faced with this competition, the phlogistonists first postulated that phlogiston had negative weight, then said that phlogiston had no weight but affected the density of materials in some peculiar way, and finally were reduced to claiming that although phlogiston was indeed lost in combustion, oxygen was also gained. At this point phlogiston was a mere metaphysical vestige which was scientifically redundant; it was therefore dropped.

The alternative theory's attempt to invade the new domain $d$ and to unify $D$ and $d$ may not succeed. However, its ambition may stimulate an extension of the other theory. For example, in this century, a chemist, reasoning by analogy to the chemical theory of

stable electron shells, pointed out that there was a rough periodicity when the relative stability of nuclei was plotted against the number of nucleons. This effort to extend the shell theory of chemistry to cover properties of the nucleus prompted the physicists to come with much more sophsiticated and successful theories of the nucleus. (Who would have thought that a mere chemist could be useful to a theoretical physicist !)

A new alternative theory which is based on different metaphysical principles and relies on different heuristic models may lead to discoveries that would have been very difficult to make, given only the old theory. Thus the wave theory of light led to Young and Fresnel's interference experiment. Feyerabend points out that the discovery of Brownian motion would have been practically impossible within a classical thermodynamical framework.

There are also pragmatic advantages to a pluralist methodology. If a family of theories is available, there is a good possibility that at least one of them will accomodate each of the data items which may eventually be considered relevant. *There is less motivation for ad hoc hypotheses and less likelihood that data will be left unsystematized and thereby neglected or forgotten.*

Grimaldi's discovery of diffraction was easily explained and thus preserved by the wave theory of light; the corpuscular theory could handle it only with difficulty by means of *ad hoc* modifications. The phlogiston theory attempted, albeit with limited success, to explain the colour changes accompanying the calcination of metals; the oxygen theory made no attempt to explain colour changes and even today this data lies scattered throughout handbooks since little theoretical work is being done on the colour of solids.

The advantages of working with a plurality of theories discussed above seem sufficient to explain the prevalence of this practice throughout the history of science. Feyerabend also suggests some psychological and social consequences of theoretical pluralism which also help explain the desirability of this methodology. Let us now consider them.

*b) Psychological advantages : According to Feyerabend, working with a plurality of theories leads to more conceptual flexibility and less dogmatism.*

Much research would need to be done to ascertain exactly what

sort of experiences lead to these psychological traits (which I assume to be desirable ones) — we all know that travel *can* lead to chauvinism as well as to an appreciation of other cultures. What I find exciting is the suggestion that a proper understanding of the nature of science should not lead to an authoritarian, monolithic view of science nor to a 'scientific' personality (in the pejorative sense).

Another important psychological consequence of methodological pluralism cited by Feyerabend is the following : "Intuitive appeal will lose its paralyzing effect." How different this is in spirit from nearly all science education today, which strives so hard to make abstract concepts intuitively acceptable to students. All discovery methods of teaching assume that modern scientific theories, the refined result of hundreds or thousands of years of exploration, can be easily 'discovered' through a purified intuition in proper contact with nature. What we need to encourage is the ability to think in 'counter-intuitive' ways, or, if you like, to develop a plurality of intuitions !

*c) Social-political advantages* : In order that science continue to grow through the competition of theoretical alternatives, it is necessary that institutionalized science not become identified with any particular theory, regardless of its apparent superiority at the moment. It is also necessary that institutions be set up to encourage both critical debate and the creation of radical alternatives.

Although more research on the sociology of science needs to be done, it appears that contemporary institutions increasingly inhibit theoretical pluralism. As science changes from being the preoccupation of financially independent, freely communicating gentlemen joined only by what Goodman calls "an international anarchist confederation"[1] into the occupation of tightly organized teams, the nature of whose work is largely controlled by the availability of funds and whose findings are often considered to be 'classified information', we may well expect its radical, creative aspect to diminish, although a considerable amount of articulation of the prevailing paradigms can easily continue.

### *3. Misguided Monolithicism*

At certain points in history it has been thought that the received theory was of such merit that it would be irresponsible to

'waste' time in trying to devise alternatives to it. I will call this practice 'Misguided Monolithicism' — I conjecture that historical studies would show that science always stagnates when such a view prevails[2].

There are many reasons for Misguided Monolithicism. It may be due to sheer prejudice or power-seeking (e.g., agricultural science in the Lysenko period); the success of the prevailing theory may make any alternatives appear unthinkable (e.g., mechanics in 18th century England), there may be arguments that certain concepts within the preferred theory are *necessary* components of any future science (see Feyerabend's discussion of quantum mechanics and ordinary-language theories of mind); sometimes there are arguments that only a limited range of techniques can be used (c.f. Frye's remarks on literary criticism). These arguments ruling out the *possibility* of introducing radical alternatives can be answered individually. However, like arguments about the possibility of particular cases of reduction, they often seem rather pointless. It is *always* true that the attempt to generate alternative theories (like the attempt to reduce one theory to another) is good methodological practice. And one cannot rule out success by *a priori* arguments — one can only indicate the difficulties involved.

Note that *internal* criticism can continue under Misguided Monolithicism — not all scientific activity ceases. However, really radical questioning through the consideration of alternative systems is not permitted. Popper once proposed a demarcation criterion which separated science from systems which did not permit of empirical falsification. It is just as important to distinguish between theories which encourage radical criticism and those which give *a priori* arguments which severely limit the kinds of explanatory alternative which can be considered[3]. Some fundamentalist Christian sects invite criticism based on a linguistic analysis of the Bible — it is only the authority of the Book which is not open to debate.

*4. A Diversity of Theories is Not Enough*

While browsing through the library I came across a book entitled *Twenty-seven Theories of Personality*. Like any admirer of physical science, even without reading it I cannot suppress a shudder. But is this not a paradigm case of the methodological pluralism which is being recommended ?

A mere proliferation of theories is not enough. None of the advantages of theoretical pluralism cited above accrue unless the various theories are used for mutual criticism. They must be deliberately juxtaposed and compared.

Let us briefly review the possible results of such a comparison and the heuristic implications of each.

*a) We may find that the theories give inconsistent predictions.* In this case the theories are alternatives and at least one is false. However, it may happen that each of the theories works fairly well within a limited domain (e.g., the pre-Boltzmann equations for black body radiation). Such a situation points to the existence of a more general theory of which the present theories are special cases[4]. Or it may happen that one theory presents factors which must be controlled in order for the other one to hold — the existence of the field of social psychology is due to a situation of this kind.

*b) The theories are empirically identical but expressed with different formalisms.* (This is a rare occurence in the history of science and the equivalence of the Schroedinger and Heisenberg representations of quantum mechanics is still being debated.) Should such a case arise, however, the uninvestigated models or positive heuristics associated with the two theories should still be compared — we may find that the two formalisms have different natural extensions.

*c) The theories are independent.* One must search for a unifying theory; if it should turn out that there really are twenty-seven independent theories of personality, we would find the situation very unsatisfying. Science, as we understand it, does not consist of an unconnected set of empirically tested statements. Empirical content is not enough — the positing of a large number of relations between phenomena is also essential. I do not believe that the full import of this intuitive requirement has yet been fully explored. Mach tried to explain it by saying that the aim of science is economy of thought, Popper by claiming that the aim is explanation. Whewell thought that unifying theories (i.e., theories which were arrived at by a "consilience of inductions") were important because they have a special claim to truth; I have claimed that such theories always survive in a corrected, modified form in future theories. It is just because natural science tends to become *ontologically* unitarian that

philosophers and scientists have been so slow to recognize the existence of and the advantages of *methodological* pluralism.

We have reviewed the useful information that can be gained by comparing theories which appear to be alternatives. Yet in many areas (this is especially prevalent in the social sciences), it is thought that there is no need to make such comparisons. I will call this practice "Protective Partitioning". As with Misguided Monolithicism, there are a variety of reasons for Protective Partitioning. There can be personal or political motives for Balkanizing certain disciplines; many of our contemporary institutions encourage specialization and low-risk research. However, there are also philosophical arguments given for why certain pairs of theories cannot be compared. If these arguments were persuasive, they would show methodological pluralism to be impossible; all so-called alternative theories would really be independent ways of looking at the world — different, incomparable "forms of life". Let us briefly examine some of these arguments.

### *5. Arguments that Alternative Theories Cannot Be Compared*

It is ironic that most of the very arguments which are now used *against* the possibility of comparing radically different theories were given by Feyerabend to argue against the position which I have called Misguided Monolithicism !

This has led to such confusion that it is perhaps worthwhile to attempt a 'rational reconstruction' of Feyerabend's major points; (even though such a simplification will not do justice to the subtlety of Feyerabend's thought !) :

(i) What is most important is that science develops through criticism.

(ii) Putting restrictions on the concepts which may be used hinders the growth of science by weakening criticism.

(iii) To convince natural scientists, who are very susceptible to Misguided Monolithicism, [due to the convergent way in which natural science has grown] of this, it is necessary to stress the incommensurability of certain theories in the history of science.

(iv) The problem of specifying the linguistic relationship between such theories in detail is not an interesting or important problem (at least not for the philosophy of science)[5]. I might add that contrary to Kuhn's claim, there is generally no important *communication* problem between natural scientists who hold

competing theories — there is simply disagreement[6].

However, in the social sciences, there already exist a plurality of theories; comparing them is a very hard and discouraging job because the theories tend to be of limited validity and if taken as making more general claims, they all seem to be instantly refuted. How convenient it would be if it turned out to be impossible to compare them ! Perhaps, instead of searching for universal theories of high content, one could add to the understanding of Nature by showing the richness of possible points of view ! To borrow Feyerabend's motto, "Let a thousand flowers bloom !" And let it be forgotten that the aid of our investigations was to *improve* our theories through criticism.

If such a justification of Protective Partitioning were to become widely accepted, I predict it would lead to the stagnation of science even more quickly than would Misguided Monolithicism. It is for this reason that we must seriously examine these arguments.

*a) Arguments based on a difference in instruments and rules of evidence :* If T and T′ contain claims about what counts as evidence and if these do not agree, then the theories are not talking about the same data and cannot be viewed as competitors.

Thus, according to this position, Galileo could not use the data from his telescopic investigations to argue against Aristotle's theory, because Aristotelians did not accept as evidence any data obtained by 'distorting' or tempering with the senses. Or, it is held, there is no 'real' conflict between para-psychology and traditional science, because the orthodox scientists do not accept the statistical techniques by which Pratt and Rhine obtain the very data which their theory is designed to explain ! Or, if a certain theory requires tht all psychological testing be through forced-choice questionnaries, then, it is held, that theory can never compete with a theory which allows Rohrschach-type testing.

In brief, if the data fields are not the same, there can be no logical conflict between the two theories.

*b) Argument based on a difference in low-level descriptive terms :* In this case, the holders of T and T′ both use at least some of the same instruments and observations techniques, but they describe their results in different ways. Hence, the explananda statements of the two theories are different and the theories can no be logically inconsistent.

For example, if T tries to explain the Hungarian *revolution* of 1956 and T′ tries to explain the *counter-revolution* which took place in Hungary in 1956, according to this position it is impossible to compare the theories in any logical way, because their explananda sentences are different. Likewise, for theories about dephlogisticated air and oxygen; mental intentions and behavioural propensities; devil-harboring and schizophrenia; the period of revolution of the earth around the earth (Ptolemy) and the period of revolution of the earth around the sun (Copernicus).

In brief, if the data are not described with the same concepts, there can be no logical conflict between the two theories.

*c) Argument based on a difference in explanatory concepts :* According to this position, which is an extension of the one above, two theories whose explanatory laws differ, *must* also disagree on their description of the data; i.e., the meaning of any term is dependent on the entire system. Hence, any two different theories can *never* be logically comparable.

For example, even if Galileo and the Aristotelians both called Jupiter a "planet", what they intended by this was quite different — the Aristotelians were speaking of an immutable heavenly object not subject to the terrestrial laws of motions; Galileo was referring to a physical object which could undergo change and which could be described in terms of a single, universal physics. If a new object were discovered in the heavens and the Aristotelians said it was a planet and Galileo said it wasn't, we could *not* say that their claims were contradictory because they were endowing the word with different senses.

*d) Argument that theories are discussing different aspects of the same 'thing' :* Although two theories may appear to refer to the same object or event, they are really talking about different aspects of the object or event — they provide *different* perspectives, but are not *competing* theories.

Thus we may speak of the political, economic, or ideological causes of a war, but these are just different approaches. They start from different premises and argue in different ways, using somewhat different data. Or one could give a physicist's explanation of an explosion or a chemist's; a sociological treatment of suicide or a psychological one; one can describe a painting in physical, materialistic terms or in aesthetic, experiential ones.

Such systems may be in some sense complementary; they can not be in logical conflict.

*6. Methods for Comparing 'Incommensurable' Theories*

Before discussing the above arguments in detail, it should be noted that there are two different conclusions which have been drawn from them :

(i) The conclusion that a rational comparison between alternative theories *can* and *should* be made, but that it is impossible to give a *general* characterization of the process. This position would be somewhat analogous to a claim that translation between Chinese and English is possible by a bilingual person, and one can debate the accuracy of particular translations; however, neither adequate dictionaries nor a mechanical translation procedure can be devised. This appears to be Feyerabend's general position.

(ii) The conclusion that for a human being to truly understand two radically different theories, he must be in some sense schizophrenic, i.e., there is no possibility of real communication between the two language games or two forms of life. This is similar to Kuhn's position.

I will wish to argue against both of these positions, but it should be noted that the first position, which holds that human translation *is* possible, does allow criticism to take place. For example, instead of demanding that his opponent drop his terminology, the critic can take on that framework and may then criticize it internally, *perhaps by using the discoveries made within his own preferred framework !* Thus although the Eddington expedition would very probably never have occured without the invention of Einstein's theory, the results of that expedition could be put in terms that were of great interest to Newtonians; and the same procedure can be used in the social sciences.

To consider another case, may we not agree that the Hungarian Revolution *was* a counter-revolution as far as Marxist theory is concerned even though we may wish to criticize the Marxist theory of the causes of the counter-revolution, etc. ? We can accept the Marxist definitions for purposes of argument, but then go on to criticize the *theory* of revolutions *Marxist* and counter-revolutions *Marxist* by showing that there are counter-examples to some of the generalizations set up between the terms as they have defined them. Perhaps one could argue that this particular counter-revolution

*Marxist* was not preceeded by foreign ideological intervention *Marxist* if their theory claims that this is always the case.

If the data for the counter-example is well-confirmed, such an attack may be devastating to the theory. At the very least, it forces proponents of the theory to modify or limit their claims. Admittedly, there are often practical difficulties in obtaining such data. And we must insure that the theory is not so vague that it is difficult to know what would count as a counter-example.

This would seem to be an obvious way to proceed, but the erroneous belief that no constructive debate is possible without agreement on the highest level theoretical terms (and their underlying value systems) seems to persist ! Note that this is never the case in the natural sciences. For example, no two successive chemical theories have ever had the same concept of 'element'. [ There seem to be remnants of the old essentialist quest here. Historically, the failure to find the 'essences' or 'true natures' of things has often led to relativism. But the fact that words gain their meaning through convention, and that different conventions coexist, does not indicate that truth is relative !].

Let us consider a typical example of the kind of confusion which often arises :

> Certain regimes or movements, in imitation of Mussolini's Italy where the term first became popular, have called themselves or have been called by others "fascist." But this description can be used in many ways : as a term of mere abuse, or as an inaccuracy based on inadequate knowledge of the facts of the history of the country concerned, or as a political device used for propaganda purposes. Therefore if you se up a conference of this type [to discuss the nature of facism] the first essential would seem to be to specify the particular regimes and movements to which alone the term "fascist" is, by definition, to be applied. The alternative (the one apparently adopted at Reading) was to treat "fascism" as some kind of a vague abstraction to which everyone could give what meaning he chose, just occasionally glancing at Germany, or Italy, or Argentina, or Roumania : if the facts did not appear to fit the model, then other countries — Japan, Spain — could be brought in to rescue it. As Dr. Woolf, the promoter of the Reading exercise, points out in his introduction, the "qualification of fascism expanded or contracted according to the conceptual

> approach adopted." (Naturally, what did he expect ?) The conference discussions showed that if rapid economic and social transformation is the real touchstone of fascism, then the countries of Central and Eastern Europe have to be excluded; analysis of "fascist economic systems" lets in Japan, but cuts out Argentina; but viewed as a "potentially revolutionary movement," "fascism" applies to Roumania, but not to Japan. And so forth. The result in this volume is a series of vague generalizations by the sociologists – with the historians occasionally putting them right on the facts.
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> The argument in discussion tended to be circular. For example, one criterion offered in order to discover "fascism" was to see who benefited from it. But what do you do about Soviet Russia, says a critic, where the labor movement has been destroyed and both workers and peasants have been drained of their savings (as much as under "fascism") to build up development ? Oh, but "fascism" does not always destroy the labor movement, says another participant : Look at Argentina, where it in fact created such a movement. And so it goes on. Are we any wiser at the end ? Schapiro (1970).

I would agree with the reviewer that sloppy definitions which protect circular arguments must be weeded out. However, we should not conclude from this that the first item on the agenda of the conference should have been to agree on a definition of *fascism*. As the reviewer points out we could investigate fascism in the following way : We begin by collecting together what Bacon would call "striking instances" of what are ordinarily considered to be fascist regimes. (In other words we *begin* with a limited *extensive* definition of *fascism*.) However, the reviewer does not describe the next step : According to our problem or proposed line of enquiry, we may classify these examples in different ways by proposing varying intensive definitions of *fascism*. One definition might stress the actual political workings of the regime, the behavior of the police, etc. Another definition might focus on the legal code, constitution or other official stated policy of the country. Each of these alternative definitions might rule out a few of the examples we started with. However, this is no cause for complaint if (a) the new class of fascist regimes is clearly specified; (b) an interesting theory accopanies the definition.

An example from natural science of how this works : Glass is not a member of the class of liquids according to the ordinary usage of that word. However, scientists have re-defined *liquid* (in terms of molecular disorder) in such a way that it is true to say that glass is a liquid. This permits them to make useful and interesting generalizations such a "Heat is liberated when a liquid turns into a solid" and to explain why the process of 'melting' glass does not require the normal heat of fusion.

However, a marble-maker would probably want to define *liquid* in the ordinary way because he wishes to use generalizations such as this : "Marbles cannot be made out of a material which is liquid at room temperature."

Several things should be noted :

(a) It makes no sense to ask which is the "true", of "real", or "essential" definition of *liquid.*

(b) There is a large, but not complete overlap between the extension of the terms $liquid_{Marble\text{-}maker}$ and $liquid_{Scientist}$

(c) The scientist who understands the marbleman's use of the term can criticize the marbleman's assertions. For example, he can point out that the statement "Dried-out molasses is a $liquid_{Marblemen}$" is false although the statement "Dried-out molasses is a $liquid_{Scientist}$" is true. Likewise, for the scientifically-trained marbleman.

Thus we see that there may be 'human bridges' between theories which allow criticism to take place. However, since it is sometimes denied that this is possible, it is also desirable to present formal 'bridges' which permit theories to be compared.

First, the obvious point will be made that the comparison of two theories T and T′ can only be made within a meta-theory $T_C$. *Within* Euclidean geometry one can not even state that its five axioms are independent. And the claim that Euclidean geometry and Riemannian geometry are inconsistent can only be made in a meta-theory. Therefore, I agree with statements that if one were to compare cultures, one would have to go 'outside' of a least one of them and speak from the perspective of another system. However, I disagree that this is any argument against the possibility of comparing them. The discovery of meta-theories was an important 20th century contribution to philosophy and one which cannot be gratuituously rejected.

Secondly, it should be noted that no two theories can be asserted to be inconsistent or compatible or independent of each

other without also asserting something about the interpretation of the two systems. If *Pa* is a theorem in T and ~*Pa* is a theorem in T′, the theories are inconsistent only if the two instances of *P* and *a* are given the same interpretation. In the case of natural languages, the claim that they *are* given the same interpretation can be denied (e.g., using the written languages, $\text{Pain}_{French}$ v.s. $\text{Pain}_{English}$) or affirmed (e.g., $\text{Compliment}_{French}$ v.s. $\text{Compliment}_{English}$). So the claim that T and T′ are inconsistent can only be made in a meta-system that includes a claim, which in the case of natural languages is an arguable *empirical* claim, about the interpretations of the terms of T and T′.

There has been much discussion in the literature about how scientific terms are to be interpreted. Since for criticism, it is most important to discover the inconsistency of two theories, let us first focus on that relationship. It seems that there are several notions which are valuable.

(a) First one can use the interpretive methods suggested by an extensional approach to meaning. One checks for 'stimulous synonymy' in paradigm cases, shows holders of the two theories pictures and asks, "In this case, would you say ... ?", etc.

Of course, it is a practical impossibility to exhaust the extension of most terms so any conjecture about extensional identity will be a fallible empirical one[7].

Occasionally one finds extensional equivalence when the intensions are dramatically different.

Thus one might find that

$$\text{"pig}_{radical}\text{" and "PIG}_{conservative}\text{"}$$

(i.e., Pride, Integrity, and Guts)[8] have the same extension (i.e., porkers and policemen) although there may be important differences in the intensions of these words. (Perhaps the radical dictionary defines *pig* as any flithy, despicable animal, while the conservative may define *pig* as a praiseworthy creature doing a job in filthy circumstances !)

Two theories may also be extensionally inconsistent even if there is only a partial overlap in the extension of their terms. Suppose T contains the predicate *P* which denotes the direction determined by a line which is perpendicular to the average horizontal in all countries bordering the Mediterranean and which is directed into the ground (the early Greek's notion of *down* ) while T′

cotains the predicate $Q$ which denotes the vector perpendicular to the horizontal anywhere on the earth's surface. If T predicts that Grecian urns in temples do not fall down and T′ predicts that they do, the theories are inconsistent even though their denotation sets of *down* (and *fall* and *urn* perhaps) only partially overlap.

It is also interesting to note that if T says that bodies in Australia fall "down" and T′ also says that bodies in Australia fall "down", nevertheless the theories are inconsistent because the states of affairs which are predicted are mutually exclusive, given the assumption that a body cannot fall in two directions at once.

(b) In cases of purported incommensurability it will generally be impossible to establish shared intensions. However, we often rely on an intermediate notion, which is an extension of what Feyerabend calls "local grammar" — I will call it "limited systematic significance".

Let us consider an actual historical case of such a suggested identity and analyze the kinds of evidence that could be brought forward to support it. How could one argue for the suggestion that the phrase "loses phlogiston" in Stahl's chemistry and the phrase "combines with oxygen" in Lavoisier's theory have the same "limited systematic significance" or "local grammar" ? One could note that all of the experimental occasions on which these phrases were used were the same. One could also note that many of the explanations and theoretical assertions made using these expressions were isomorphous. For instance, within Stahl's theory one could reason as follows (this is based on one of Stahl's discussions and I will try to indicate both the form of explanation and some of the independent evidence for the explanans) :

> "Why is tallow a reducing agent, i.e., a material which can be used to prepare metals ? (Metal workers had long known that a metal calx could be converted into a metal through the addition of tallow.) One can explain this with the phlogiston theory. When a metal is calcined it loses its phlogiston — to change the calx back into metal, phlogiston must be restored to it. (Thus metallic zinc can restore iron calx; metallic iron can restore copper calx; charcoal, which is eminently combustible and hence contains phlogiston, can also serve as a phlogiston donor.) Tallow, as evidenced by its combustibility, contains phlogiston. (There were other tests for containing phlogiston which Stahl could also have relied on — including

> reactivity with nitre and the ability to change acid of sulfur back into sulfur, Stahl's famous 'sulfur synthesis'.) Therefore, tallow is a reducing agent."

In Lavoisier's theory one argued as follows :

> "Why is tallow a reducing agent which can be used to prepare metals ? To change a metal calx into a metal, we must remove the oxygen. (This can be done by chemicals which combine readily with oxygen, such as another metal or charcoal. That they combine with oxygen is indicated by their reaction with air, nitre, etc.) Tallow reacts readily with oxygen, as evidenced by its combustibility. Therefore, tallow is a reducing agent."

As chemists and historians of chemistry from Lavoisier onward have pointed out, there is a complete parallelism in such accounts which can be seen if we correlate certain phrases such as "loses phlogiston" and "combines with oxygen". Therefore, if one theory should predict that any material which loses phlogiston will decrease in weight[9] and the other theory predicts that any material which combines with oxygen will increase in weight, we can declare the theories inconsistent.

In summary, any two theories expressed in natural languages may only be related with the help of one of these three kinds of hypotheses about the interpretation of the terms used. With the help of such a hypothesis (which may itself be challenged), the theories can be compared and some of their theorems (or predictions) may be shown to be inconsistent. In such cases a crucial experiment is possible.

Sometimes it is only possible to compare theories with the help of a hypothesis which is definitely *not* about linguistic considerations. Suppose T asserts that the craters on the moon result from volcanos and T′ asserts that the craters result from meteors. Let us assume that these words are all being used as in ordinary English. These theories only become inconsistent if we add the hypothesis that it is not the case that meteors always strike and only strike the central core of just-erupted volcanos !

And to return to an earlier example — Newton's laws of motion and Kepler's laws of planetary motion give (approximately) the same predictions only if we add the empirical claim that our solar system is such that the planets are separated from the sun and from

each other by large distances and their mass is small compared to that of the sun. Not even an approximation of Kepler's laws can be derived from Newton's laws without the addition of this boundary condition. However, once the boundary condition is accepted, we can compare the two theories.

Logically, debate about the appropriate $T_c$ should precede the actual comparison of T and T′. However, it should be remembered that scientists usually move directly to the stage of conducting crucial experiments, especially if they are easy to carry out. (Feyerabend has pointed out the sterility of arguments about whether two terms 'really mean the same'.) The only interesting debates about $T_c$ occur when there are disagreements about non-linguistic matters — such as rules of evidence. These may well have to be settled before the original theories can be compared. But debates over experimental techniques contribute directly to the progress of the science in hand. It was quite proper to severely question the relevance of telescopic data — this was a debate about optics, not mechanics, but valuable nevertheless.

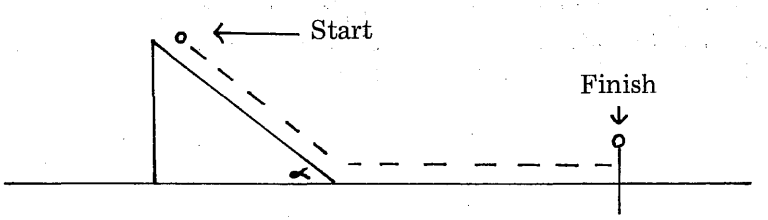
Most of the above discussion has dealt with examples in which the competing theories are eventually shown to be inconsistent. But are there not many cases where the so-called 'alternative' theories are really as unrelated as ravons and writing-desks ? For example, if one asks, "Why did Mr. W die ?", one can give a medical answer (e.g., stroke), a psychological answer (overly achievement-oriented), a sociological answer (member of the junior executive class), an economic answer (capitalist-competitive system), an astrological answer (the unlucky conjunction of two planets), a theological answer (original sin), or a physiological answer (oxygen deficiency in the brain).

In support of these answers, the various theorists would bring forth different sorts of evidence, such as X-rays, graphs, statistics, star-charts, Bibles, what have you. All of these accounts refer to the same event, Mr. W's death, but do so from entirely different points of view. In addition, these accounts are not simply related in any sort of sequential or nesting fasion. One might claim that death was caused by oxygen-lack, which was caused by the stroke, which was in turn conditioned by X's being overly achievement-oriented. But this latter condition is certainly not caused by X's working in a certain sort of job in a certain sort of economic system. There may be various complex interconnections between some of the accounts, but there are a diversity of systems among which there may be

various more or less close relationships. But these are not contradictory explanations; perhaps they are complementary, in some loose sense. Moreover, the number of possible explanations appears to be very large and proponents of the 'different aspects' argument would claim it is best to view these as autonomous explanatory modes.

I am not in major disagreement with much of the above analysis, but I would like to suggest a different methodological conclusion. By *trying* to combine various smaller theories one may make new discoveries. One might discover that one theory was reducible to another, or that they were contradictory, or that both were oversimplified but could be corrected by a more detailed, unifying system.

Here is a simple example from science education. Suppose one presents an inclined plane apparatus (see diagram below) to two students who have been doing experiments with it, and then proceeds to roll a ball down it, mark where it stops, and ask, "Why did the object go to point *d* ?" One student might answer, "because the plane was elevated at an angle $\alpha$." (Perhaps he has been investigating how d is a function of $\alpha$ for the case of rolling balls.) The other might answer, "because the object was a relatively friction-free ball." (She has been determining how *d is* a function of the friction between object and plane for the case of fixed $\alpha$.)




What are we to say of the relationship of these explanations ? First, note that they are certainly not independent. The first system holds the value of the coefficient of friction constant and at a low value and discusses the effect of angle. The second holds the angle constant and discusses the effect of friction *(f)*. To improve the accuracy and to predict what would happen with a wooden block at a different angle, we would need to use both theories together.

Secondly, when one tries to combine the theories to make predictions or explanations, we discover something very interesting : One cannot simply "sum up" the two theories because it turns out

that the coefficient of friction depends on the angle ! So one is led to a new discovery — namely the interaction of the two variables influencing $d$. The result is a unified system which relates $d$, $\alpha$, and $f$.

Of course, one *may* find that the two (or more) separate theories cannot be combined into a single coherent theory, but as was pointed out above, it is still good methodological practice to attempt the unification. (I, personnally, am always more in sympathy with grand theoretical enterprises which may fail than with defensive attempts to argue that something is impossible or too difficult.)

*Conclusion*

The discovery by historians and philosophers of science of the positive role of radically different frameworks within the development of science reminds us to not only tolerate but actively encourage diverse points of view, especially in the more staid, established disciplines.

However, neither criticism nor the growth of knowledge will ensue unless these diverse frameworks are made to clash. Sometimes considerable effort is required to compare radically different systems. But as Kuhn emphasized at a meeting in Philadelphia twenty years after *The Structure of Scientific Revolutions*, just because the diagonal and sides of a unit square are incommensurable doesn't mean that you can't compare their lengths.

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NOTES

*An early version of this essay was discussed in the Department of History and Philosophy of Education, Ontario institute for Studies in Ediucation, Toronto, Spring, 1970. Bernard Davis made many valuable criticisms and suggestions.

[1]This was discussed in Paul Goodman's seminar on "Deschooling the Society", given at CIDOC, Cuernavaca, Mexico, March, 1970.

[2]One classic example is provided by the neglect of the continental drift hypothesis.

"When German meteorologist Alfred Wegener came to document the first full-blown theory of continental drift ... he cited Anarctic data nineteen times. But challengers of his thesis insisted that his case would not be valid until he could show that Antarctica had been occupied by the same species of animals that occupied the other continents of the Southern Hemisphere. Wegener was unable to offer any such demonstration. Although he did show parallel relationships in many species of plants and primitive organisms, he died with his theory unaccepted by all but a handful of scientists.

Wegener's proposition was not only unpopular. It faced rigid taboos throughout most of the Northern Hemisphere during the first half of the century. To side with him was to risk professional ostracism from the geology departments of leading universities. If Southern Hemisphere scientists had not of necessity lived in the midst of evidence that Suess and Wegener were right, the notion of drifting continents might have been lost for a long time." (Lear, 1970).

[3]But are there not always some aspects of a theory which must be insulated from criticism ? For a reply to this point, see W.W. Bartley, *The Retreat to Commitment*.

[4]An elucidation of this point is to be found in Post (1971).

[5]Feyerabend (1962) writes : "...as soon as this demonstration has been carried out, in the very same moment, the idiom of T′ must be given up and must be replaced by the idiom of T. Of course, one need not go through the laborious and very uninteresting task of analyzing the context of which T′ is part. All that is needed is the adoption of the terminology and the 'grammar' of the most detailed and most successful theory throughout the domain of its application.

"One hears frequently that a *complete* replacement of the grammar and the terminology of the 'old language' is impossible because this old language will be needed for introducing the new language and will, therefore, infect at least part of the new language. This is curious reasoning indeed if we consider that children learn languages without the help of a previously known idiom. Is it really asserted that what is possible for a small child will be impossible for a philosopher, a linguistic philosopher at that ?"

"This automatically takes care of whatever incommensurabilities may arise, and it does so without any linguistic detective work (which therefore turns out to be entirely unnecessary for the progress of knowledge)."

[6]For example, a popular textbook in both Britain and France during the phlogiston-oxygen debate was entitled : Leçons élémentaires d'histoire naturelle et de chimie; Dans leqeuelles on s'est proposé, 1) de donner un ensemble méthodique des connaissances chimiques acquises jusqu'à ce jour; 2) d'offrir un tableau comparé de la doctrine de Stahl & de celle de quelques Modernes : pour servir de résumé à un cours complet sur ces deux sciences. Paris : 1782.

[7]See Quine (1970).

[8]This definition comes from a letter to the editors of *Time Magazine*, March 9, 1970.

> Piggy Pride
> Sir : You aren't going to like this, but your favorite whipping boys, the Chicago police, now refer to each other as PIG. The term is not used,however, in a derogatory sense since the three letters point up the motto awarded them by a grateful public : Pride, Integrity and Guts.
> Chicago JOHN H. SHINSKE

[9]It was only a late (and minor) variant of the original phlogiston theory which actually asserted this.

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