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How to be a good empiricist: a plea for tolerance in matters epistemological

> 'Facts?' he repeated. 'Take a drop more grog, Mr Franklin, and you'll get over the weakness of believing in facts! Foul play, Sir!'
>
> Wilkie Collins *The Moonstone*

I CONTEMPORARY EMPIRICISM LIABLE TO LEAD TO ESTABLISHMENT OF A DOGMATIC METAPHYSICS

Today empiricism is the professed philosophy of a good many intellectual enterprises. It is the core of the sciences, or so at least we are taught, for it is responsible both for the existence and for the growth of scientific knowledge. It has been adopted by influential schools in aesthetics, ethics and theology. And within philosophy proper the empirical point of view has been elaborated in great detail and with even greater precision. This predilection for empiricism is due to the assumption that only a thoroughly observational procedure can exclude fanciful speculation and empty metaphysics as well as to the hope that an empiristic attitude is most liable to prevent stagnation and to further the progress of knowledge. It is the purpose of the present paper to show that empiricism in the form in which it is practised today cannot fulfil this hope.

Putting it very briefly, it seems to me that the contemporary doctrine of empiricism has encountered difficulties, and has created contradictions which are very similar to the difficulties and contradictions inherent in some versions of the doctrine of democracy. The latter are a well-known phenomenon. That is, it is well known that essentially totalitarian measures are often advertised as being a necessary consequence of democratic principles. Even worse – it not so rarely happens that the totalitarian character of the defended measures is not explicitly stated but covered up by calling them 'democratic', the word 'democratic' now being used in a new, and somewhat misleading, manner. This method of (conscious or unconscious) verbal camouflage works so well that it has deceived some of the staunchest supporters of true democracy. What is not so well known is that modern empiricism is in precisely the same predicament. That is, some of the methods of modern empiricism which are introduced in the spirit of anti-dogmatism and progress are bound to lead to the establishment of a dogmatic metaphysics and to the construction of defence mechanisms which make this metaphysics safe from

refutation by experimental inquiry. It is true that in the process of establishing such a metaphysics the words 'empirical' or 'experience' will frequently occur; but their sense will be as distorted as was the sense of 'democratic' when used by some concealed defenders of a new tyranny.[1] This, then, is my charge: far from eliminating dogma and metaphysics and thereby encouraging progress, modern empiricism has found a new way of making dogma and metaphysics respectable, viz., the way of calling them 'well-confirmed theories', and of developing a method of confirmation in which experimental inquiry plays a large though well controlled role. In this respect, modern empiricism is very different indeed from the empiricism of Galileo, Faraday and Einstein, though it will of course try to represent these scientists as following its own paradigm of research, thereby further confusing the issue.[2]

From what has been said above it follows that the fight for tolerance in scientific matters and the fight for scientific progress must still be carried on. What has changed is the denomination of the enemies. They were priests, or 'school-philosophers', a few decades ago. Today they call themselves 'philosophers of science', or 'logical empiricists'.[3] There are also a good many scientists who work in the same direction. I maintain that all these groups work against scientific progress. But whereas the former did so openly and could be easily discerned, the latter proceed under the flag of progressivism and empiricism and thereby deceive a good many of their followers. Hence, although their presence is noticeable enough they may almost be compared to a fifth column, the aim of which must be exposed in order that its detrimental effect be fully appreciated. It is the purpose of this paper to contribute to such an exposure.

[1] K. R. Popper, *The Open Society and its Enemies* (London: Routledge and Kegan Paul, 1945, reprinted, New Jersey: Princeton University Press, 1953).

[2] It is very interesting to see how many so-called empiricists, when turning to the past, completely fail to pay attention to some very obvious facts which are incompatible with their empiristic epistemology. Thus Galileo has been represented as a thinker who turned away from the empty speculations of the Aristotelians and who based his own laws upon facts which he had carefully collected beforehand. Nothing could be further from the truth. *The Aristotelians could quote numerous observational results in their favour.* The Copernican idea of the motion of the earth, on the other hand, did not possess independent observational support, at least not in the first 150 years of its existence. Moreover, it was inconsistent with facts and highly confirmed physical theories. And *this* is how modern physics started: not as an observational enterprise *but as an unsupported speculation that was inconsistent with highly-confirmed laws.* For details and further references see my 'Realism and Instrumentalism', to appear in M. Bunge (ed.), *The Critical Approach to Science and Philosophy: Essays in Honor of Karl Popper* (New York: Free Press, <1964>).

[3] One might be inclined to add those who base their pronouncements upon an analysis of what they call 'ordinary language'. I do not think they deserve to be honoured by a criticism. Paraphrasing Galileo, one might say that they 'deserve not even that name, for they do not talk plainly and simply but are content to adore the shadows, philosophizing not with due circumspection but merely from having memorized a few ill-understood principles'.

I shall also try to give a positive methodology for the empirical sciences which no longer encourages dogmatic petrification in the name of experience. Put in a nutshell, the answer which this method gives to the question in the title is: you can be a good empiricist only if you are prepared to work with many alternative theories rather than with a single point of view and 'experience'. This plurality of theories must not be regarded as a preliminary stage of knowledge which will at some time in the future be replaced by the One True Theory. Theoretical pluralism is assumed to be an *essential feature* of all knowledge that claims to be objective. Nor can one rest content with a plurality which is merely abstract and which is created by denying now this and now that component of the dominant point of view. Alternatives must rather be developed in such detail that problems already 'solved' by the accepted theory can again be treated in a new and perhaps also more detailed manner. Such development will of course take time, and it will not be possible, for example, at once to construct alternatives to the present quantum theory which are comparable to its richness and sophistication. Still, it would be very unwise to bring the process to a standstill in the very beginning by the remark that some suggested new ideas are undeveloped, general, metaphysical. *It takes time to build a good theory* (a triviality that seems to have been forgotten by some defenders of the Copenhagen point of view of the quantum theory); and it also takes time to develop an alternative to a good theory. The *function* of such concrete alternatives is, however, this: they provide means of criticizing the accepted theory in a manner which goes *beyond* the criticism provided by a comparison of that theory 'with the facts': however closely a theory seems to reflect the facts, however universal its use, and however necessary its existence seems to be to those speaking the corresponding idiom, its factual adequacy can be asserted only *after* it has been confronted with alternatives *whose invention and detailed development must therefore precede any final assertion of practical success and factual adequacy*. This, then, is the methodological justification of a plurality of *theories*: such a plurality allows for a much sharper criticism of accepted ideas than does the comparison with a domain of 'facts' which are supposed to sit there independently of theoretical considerations. The function of unusual *metaphysical* ideas which are built up in a nondogmatic fashion and which are then developed in sufficient detail to give an (alternative) account even of the most common experimental and observational situations is defined accordingly: they play a decisive role in the criticism and in the development of what is generally believed and 'highly confirmed'; and they have therefore to be present at *any* stage of the development of our knowledge.[4] A science that is free from

[4] It is nowadays frequently assumed that '[i]f one considers the history of a special branch of science one gets the impression that non-scientific elements ... relatively frequently occur in the earlier stages of development, but that they gradually retrogress in later stages and even tend to disappear in such advanced stages which become ripe for a more or less thorough formalization'. (H. J. Groenewold, 'Non-Scientific Elements in the Development of Science', *Synthese*, vol. 10, 1957, p. 305). Our considerations in the text would seem to show that such a development is very undesirable and can only result in a well-formalized, precisely expressed, and completely petrified metaphysics.

metaphysics is on the best way to becoming a *dogmatic* metaphysical system. So far the summary of the method I shall explain, and defend, in the present paper.

It is clear that this method still retains an essential element of *empiricism*: the decision between alternative theories is based upon *crucial experiments*. At the same time it must *restrict* the range of such experiments. Crucial experiments work well with theories of a low degree of generality whose principles do not touch the principles on which the ontology of the chosen observation language is based. They work well if such theories are compared with respect to a much more general background theory which provides a stable meaning for the observation sentences. However, this background theory, like any other theory, is itself in need of criticism. Criticism must use alternatives. Alternatives will be the more efficient the more radically they differ from the point of view to be investigated. It is bound to happen, then, that the alternatives do not share a single statement with the theories they criticize. Clearly, a crucial experiment is now impossible. It is impossible, not because the experimental device is too complex, or because the calculations leading to the experimental prediction are too difficult; it is impossible because there is no statement capable of expressing what emerges from the observation. This consequence, which severely restricts the domain of empirical discussion, cannot be circumvented by any of the methods which are currently in use and which all try to work with relatively stable observation languages. It indicates that the attempt to make empiricism a universal basis of all our factual knowledge cannot be carried out. The discussion of this situation is beyond the scope of the present paper.

On the whole, the paper is a concise summary of results which I have explained in a more detailed fashion in the following essays: 'Explanation, Reduction, and Empiricism'; 'Problems of Microphysics'; 'Problems of Empiricism'; 'Linguistic Philosophy and the Mind-Body Problem'.[5] All the relevant acknowledgements can be found there. Let me only repeat here that my general outlook derives from the work of K. R. Popper (London) and David Bohm (London) and from my discussions with both. It was severely tested in discussion with my colleague, T. S. Kuhn (Berkeley). It

[5] These essays were published in Volume III of the *Minnesota Studies in the Philosophy of Science*; in Volumes I and II of the *Pittsburgh Studies in the Philosophy of Science*; and in *Problems of Philosophy, Essays in Honor of Herbert Feigl*, respectively. [In fact, the last paper referred to here never appeared. Feigl's *festschrift* is Feyerabend and Maxwell (1966). (Ed.)].

was the latter's skilful defence of a scientific conservatism which triggered two papers, including the present one. Criticisms by A. Naess (Oslo), D. Rynin (Berkeley), Roy Edgley (Bristol) and J. W. N. Watkins (London) have been responsible for certain changes I made in the final version.

2 TWO CONDITIONS OF CONTEMPORARY EMPIRICISM

In this section I intend to give an outline of some assumptions of contemporary empiricism which have been widely accepted. It will be shown in the sections to follow that these apparently harmless assumptions which have been explicitly formulated by some logical empiricists, but which also seem to guide the work of a good many physicists, are bound to lead to exactly the results I have outlined above: dogmatic petrification and the establishment, on so-called 'empirical grounds' of a rigid metaphysics.

One of the cornerstones of contemporary empiricism is its *theory of explanation*. This theory is an elaboration of some simple and very plausible ideas first proposed by Popper[6] and it may be introduced as follows: let T and T′ be two different scientific theories, T′ the theory to be explained or the explanandum, T the explaining theory or the explanans. Explanation (of T′) consists in the *derivation* of T′ from T and initial conditions which specify the domain D′ in which T′ is applicable. Prima facie, this demand of derivability seems to be a very natural one to make for 'otherwise the explanans would not constitute adequate grounds for the explanation' (Hempel).[7] It implies two things: first, that the consequences of a satisfactory explanans, T, inside D′ must be compatible with the explanandum, T′; and secondly, that the main descriptive terms of these consequences must either coincide, with respect to their meanings, with the main descriptive terms of T′, or at least they must be related to them via an empirical hypothesis. The latter result can also be formulated by saying that the meaning of T′ must be unaffected by the explanation. 'It is of the utmost

[6] See K. R. Popper, *The Logic of Scientific Discovery* (London: Hutchinson, 1959), section 12. This is a translation of his *Logik der Forschung* published in 1935. The decisive feature of Popper's theory, a feature which was not at all made clear by earlier writers on the subject of explanation, is the emphasis he puts on the initial conditions and the implied possibility of two kinds of laws, viz. (1) laws concerning the temporal sequence of events; and (2) laws concerning the space of initial conditions. In the case of the quantum theory, the laws of the second kind provide very important information about the nature of the elementary particles and it is to *them* and *not* to the laws of motion that reference is made in the discussions concerning the interpretation of the uncertainty relations. In general relativity, the laws formulating the initial conditions concern the structure of the universe at large and only by overlooking them could it be believed that a purely relational account of space would be possible. For the last point, cf. E. L. Hill, 'Quantum Physics and the Relativity Theory', in H. Feigl and G. Maxwell (eds.), *Current Issues in the Philosophy of Science* (New York: Holt, Rinehart and Winston, 1961).

[7] C. G. Hempel, 'Studies in the Logic of Explanation', reprinted in H. Feigl and M. Brodbeck (eds.), *Readings in the Philosophy of Science* (New York: Appleton-Century-Crofts, 1953), p. 321.

importance', writes Professor Nagel,[8] emphasizing this point, 'that the expressions peculiar to a science will possess meanings that are fixed by its *own* procedures and are therefore intelligible in terms of its own rules of usage; whether or not the science has been, or will be [explained in terms of] the other discipline'.

Now if we take it for granted that more general theories are always introduced with the purpose of explaining the existent successful theories, then every new theory will have to satisfy the two conditions just mentioned. Or, to state it in a more explicit manner,]

> (1) only such theories are then admissible in a given domain which either *contain* the theories already used in this domain, or which are at least *consistent* with them inside the domain;[9] and
>
> (2) meanings will have to be invariant with respect to scientific progress; that is, all future theories will have to be phrased in such a manner that their use in explanations does not affect what is said by the theories, or factual reports to be explained.

These two conditions I shall call the *consistency condition* and the *condition of meaning invariance*, respectively.

Both conditions are *restrictive* conditions and therefore bound profoundly to influence the growth of knowledge. I shall soon show that the development of actual science very often violates them and that it violates them in exactly those places where one would be inclined to perceive a tremendous progress of knowledge. I shall also show that neither condition can be justified from the point of view of a tolerant empiricism. However, before doing so I would like to mention that both conditions have occasionally entered the domain of the sciences and have been used here in attacks against new developments and even in the process of theory construction itself. Especially today, they play a very important role in the construction as well as in the defence of certain points of view in microphysics.

Taking first an earlier example, we find that in his *Wärmelehre*, Ernst Mach[10] makes the following remark:

> Considering that there is, in a purely mechanical system of absolutely elastic atoms no real analogue for the *increase of entropy*, one can hardly suppress the idea that a violation of the second law ... should be possible if such a mechanical system were the *real* basis of thermodynamic processes.

And referring to the fact that the second law is a highly confirmed physical

[8] E. Nagel, 'The Meaning of Reduction in the Natural Sciences', reprinted in A. C. Danto and S. Morgenbesser (eds.), *Philosophy of Science* (New York: World Publishing, 1960), p. 301.

[9] It has been objected to this formulation that theories which are consistent with a given explanandum may still contradict each other. This is quite correct, but it does not invalidate my argument. For as soon as a single theory is regarded as sufficient for explaining all that is known (and represented by the other theories in question), it will have to be consistent with all these other theories.

[10] E. Mach, *Wärmelehre* (Leipzig, 1897), p. 364.

law, he insinuates (in his *Zwei Aufsätze*)[11] that for this reason the mechanical hypothesis must not be taken too seriously. There were many similar objections against the kinetic theory of heat.[12] More recently, Max Born has based his arguments against the possibility of a return to determinism upon the consistency condition and the assumption which we shall here take for granted, that wave mechanics is incompatible with determinism.

> If any future theory should be deterministic it cannot be a modification of the present one, but must be entirely different. How this should be possible without sacrificing a whole treasure of well established results [i.e., without contradicting highly confirmed physical laws and thereby violating the consistency condition] I leave the determinist to worry about.[13]

Most members of the so-called Copenhagen school of quantum theory would argue in a similar manner. For them the idea of complementarity and the formalism of quantization expressing this idea do not contain any hypothetical element as they are 'uniquely determined by the facts'.[14] Any theory which contradicts this idea is factually inadequate and must be removed. Conversely, an explanation of the idea of complementarity is acceptable only if it either contains this idea, or is at least consistent with it. This is how the consistency condition is used in arguments against theories such as those of Bohm, de Broglie and Vigier.[15]

The use of the consistency condition is not restricted to such general remarks, however, A decisive part of the existing quantum theory *itself*, viz., the projection postulate,[16] is the result of the attempt to give an account of the definiteness of macro objects and macro events that is in accordance with the consistency condition. The influence of the condition of meaning invariance goes even further.

> The Copenhagen-interpretation of the quantum theory [writes Heisenberg[17]] starts from paradox. Any experiment in physics, whether it refers to the phenomena of daily life or to atomic events, is to be described in the terms of classical physics ... *We cannot and should not replace these concepts by any others* [my italics]. Still the application of these concepts is limited by the relations of uncertainty. We must keep in mind this limited range of applicability of the classical concepts while using them, but we cannot and should not try to improve them.

This means that the meaning of the classical terms must remain invariant

[11] E. Mach, *Zwei Aufsätze* (Leipzig, 1912).

[12] For a discussion of these objections, <see> ter Haar's review article, 'Foundations of Statistical Mechanics', *Reviews of Modern Physics*, vol. 27, 1955, pp. 289–338.

[13] M. Born, *Natural Philosophy of Cause and Chance* (Oxford: Clarendon Press, 1949), p. 109.

[14] L. Rosenfeld, 'Misunderstandings about the Foundations of the Quantum Theory', in S. Körner (ed.), *Observation and Interpretation* (London: Butterworth, 1957), p. 42.

[15] <See> the discussions in Körner, *Observation and Interpretation*.

[16] For details and further literature, <see> Section 11 of my paper 'Problems of Microphysics'.

[17] W. Heisenberg, *Physics and Philosophy* (London: Allen and Unwin, 1958), p. 46.

with respect to any future explanation of microphenomena. Microtheories have to be formulated in such a manner that this invariance is guaranteed. The principle of correspondence and the formalism of quantization connected with it were explicitly devised for satisfying this demand. Altogether, the quantum theory seems to be the first theory after the downfall of the Aristotelian physics that has been quite explicitly constructed with an eye both on the consistency condition and the condition of (empirical) meaning invariance. In this respect it is very different indeed from, say, relativity which violates both consistency and meaning invariance with respect to earlier theories. Most of the arguments used for the defence of its customary interpretation also depend on the validity of these two conditions and they will collapse with their removal. An examination of these conditions is therefore very topical and bound deeply to affect present controversies in microphysics. I shall start this investigation by showing that some of the most interesting developments of physical theory in the past have violated both conditions.

3 THESE CONDITIONS NOT INVARIABLY ACCEPTED BY ACTUAL SCIENCE

The case of the consistency condition can be dealt with in a few words: it is well known (and has also been shown in great detail by Duhem)[18] that Newton's theory is inconsistent with Galileo's law of the free fall and with Kepler's laws; that statistical thermodynamics is inconsistent with the second law of the phenomenological theory; that wave optics is inconsistent with geometrical optics; and so on. Note that what is being asserted here is *logical* inconsistency; it may well be that the differences of prediction are too small to be detectable by experiment. Note also that what is being asserted is not the inconsistency of, say, Newton's theory and Galileo's law, but rather the inconsistency of *some consequences* of Newton's theory in the domain of validity of Galileo's law, and Galileo's law. In this last case the situation is especially clear. Galileo's law asserts that the acceleration of the free fall is a constant, whereas application of Newton's theory to the surface of the earth gives an acceleration that is not a constant but *decreases* (although imperceptibly) with the distance from the centre of the earth. Conclusion: if actual scientific procedure is to be the measure of method, then the consistency condition is inadequate.

The case of meaning invariance requires a little more argument, not because it is intrinsically more difficult, but because it seems to be much more closely connected with deep-rooted prejudices. Assume that an explanation is required, in terms of the special theory of relativity, of the

[18] P. Duhem, *La Théorie Physique: Son Objet, Sa Structure* (Paris, Marcel Rivière, 1914), chapters IX and X. See also K. R. Popper, 'The Aim of Science', *Ratio*, vol. 1, 1957.

classical conservation of mass in all reactions in a closed system S. If m', m'', m''', ..., m^i, ... are the masses of the parts P', P'', P''', ..., P^i ... of S, then what we want is an explanation of

$$\sum m^i = \text{const.} \tag{1}$$

for all reactions inside S. We see at once that the consistency condition cannot be fulfilled: according to special relativity $\sum m^i$ will vary with the velocities of the parts relative to the co-ordinate system in which the observations are carried out, and the total mass of S will also depend on the relative potential energies of the parts. However, if the velocities and the mutual forces are not too large, then the variation of $\sum m^i$ predicted by relativity will be so small as to be undetectable by experiment. Now let us turn to the *meanings* of the terms in the relativistic law and in the corresponding classical law. The first indication of a possible change of meaning may be seen in the fact that in the classical case the mass of an aggregate of parts equals the sum of the masses of the parts:

$$\mathrm{M}(\sum \mathrm{P}^i) = \sum \mathrm{M}(\mathrm{P}^i)$$

This is not valid in the case of relativity where the relative velocities and the relative potential energies contribute to the mass balance. That the relativistic concept and the classical concept of mass are very different indeed becomes clear if we also consider that the former is a *relation*, involving relative velocities, between an object and a co-ordinate system, whereas the latter is a *property* of the object itself and independent of its behaviour in co-ordinate systems. True, there have been attempts to give a relational analysis even of the classical concept (Mach). None of these attempts, however, leads to the relativistic idea with its velocity dependence on the co-ordinate system, which idea must therefore be added even to a *relational* account of classical mass. The attempt to identify the classical mass with the relativistic rest mass is of no avail either. For although both may have the same numerical value, the one is still dependent on the co-ordinate system chosen (in which it is at rest and has that specific value), whereas the other is not so dependent. We have to conclude, then, that $(m)_c$ and $(m)_r$ mean very different things and that $(\sum m^i)_c$ = const. and $(\sum m^i)_r$ = const. are very different assertions. This being the case, the derivation from relativity of either equation (1) or of a law that makes slightly different quantitative predictions with $\sum m^i$ used in the classical manner, will be possible only if a further premise is added which establishes a relation between the $(m)_c$ and the $(m)_r$. Such a 'bridge law' – and this is a major point in Nagel's theory of reduction – is a hypothesis

> according to which the occurrence of the properties designated by some expression in the premises of the [explanans] is a sufficient, or a necessary and sufficient condition for the occurrence of the properties designated by the expression of the [explanandum].[19]

Applied to the present case this would mean the following: under certain conditions the occurrence of relativistic mass of a given magnitude is accompanied by the occurrence of classical mass of a corresponding magnitude; this assertion is inconsistent with another part of the explanans, viz., the theory of relativity. After all, this theory asserts that there are no invariants which are directly connected with mass measurements and it thereby asserts that '$(m)_c$' does not express real features of physical systems. Thus we inevitably arrive at the conclusion that mass conservation cannot be explained in terms of relativity (or 'reduced' to relativity) without a violation of meaning invariance. And if one retorts, as has been done by some critics of the ideas expressed in the present paper,[20] that meaning invariance is an essential part of both reduction and explanation, then the answer will simply be that equation (1) can neither be explained by, nor reduced to relativity. Whatever the *words* used for describing the situation, the *fact* remains that actual science does not observe the requirement of meaning invariance.

This argument is quite general and is independent of whether the terms whose meaning is under investigation are observable or not. It is therefore stronger than may seem at first sight. There are some empiricists who would admit that the meaning of theoretical terms may be changed in the course of scientific progress. However, not many people are prepared to extend meaning *variance* to observational terms also. The idea motivating this attitude is, roughly, that the meaning of observational terms is uniquely determined by the procedures of observation such as looking, listening, and the like. These procedures remain unaffected by theoretical advance.[21] Hence, observational meanings, too, remain unaffected by theoretical advance. What is overlooked, here, is that the 'logic' of the observational terms is not exhausted by the procedures which are connected with their application 'on the basis of observation'. As will turn out later, it also depends on the more general ideas that determine the 'ontology' (in Quine's sense) of our discourse. These general ideas may change without any change of observational procedures being implied. For example, we may change our ideas about the nature, or the ontological status (property, relation, object, process, etc.) of the colour of a self-luminescent object

[19] E. Nagel, 'The Meaning of Reduction', p. 302.

[20] <See> Section 4.7 of M. Scriven's paper 'Explanations, Predictions and Laws', in *Minnesota Studies in the Philosophy of Science, volume III* (Minneapolis: University of Minnesota Press, 1962). Similar objections have been raised by Kraft (Vienna) and Rynin (Berkeley).

[21] For an exposition and criticism of this idea <see> my 'Attempt at a Realistic Interpretation of Experience', *Proceedings of the Aristotelian Society*, vol. 58, 1958, pp. 143–70.

without changing the methods of ascertaining that colour (looking, for example). Clearly, such a change is bound profoundly to influence the meanings of our observational terms.

All this has a decisive bearing upon some contemporary ideas concerning the interpretation of scientific theories. According to these ideas, theoretical terms receive their meanings via correspondence rules which connect them with an observational language *that has been fixed in advance* and independently of the structure of the theory to be interpreted. Now, our above analysis would seem to show that *if we interpret scientific theories in the manner accepted by the scientific community*, then most of these correspondence rules will be either false, or nonsensical. They will be *false* if they *assert* the existence of entities denied by the theory; they will be *nonsensical* if they *presuppose* this existence. Turning the argument around, we can also say that the attempt to interpret the calculus of some theory that has been voided of the meaning assigned to it by the scientific community with the help of the double language system, will lead to a very different theory. Let us again take the theory of relativity as an example: it can be safely assumed that the physical thing language of Carnap, and any similar language that has been suggested as an observation language, is not Lorentz-invariant. The attempt to interpret the *calculus* of relativity on *its* basis therefore cannot lead to the *theory* of relativity as it was understood by Einstein. What we shall obtain will be at the very most *Lorentz's interpretation* with its inherent asymmetries. This undesirable result cannot be evaded by the *demand* to use a different and more adequate observation language. The double language system assumes that theories which are not connected with some observation language do not possess an interpretation. The demand assumes that they do, and asks to choose the observation language most suited to it. It reverses the relation between theory and experience that is characteristic for the double language method of interpretation, which means, it gives up this method. Contemporary empiricism, therefore, has not led to any satisfactory account of the meanings of scientific theories.[22]

What we have shown so far is that the two conditions of Section 2 are frequently violated in the course of scientific practice and especially at

[22] It must be admitted, however, that Einstein's original interpretation of the special theory of relativity is hardly ever used by contemporary physicists. For them the theory of relativity consists of two elements: (1) the Lorentz transformations; and (2) mass-energy equivalence. The Lorentz transformations are interpreted purely formally and are used to make a selection among possible equations. This interpretation does not allow <us> to distinguish between Lorentz's original point of view and the entirely different point of view of Einstein. According to it Einstein achieved a very minor *formal* advance [this is the basis of Whittaker's attempt to 'debunk' Einstein]. It is also very similar to what application of the double language model would yield. Still, an undesirable philosophical procedure is not improved by the support it gets from an undesirable procedure in physics. [The above comment on the contemporary attitude towards relativity was made by E. L. Hill in discussions at the Minnesota Center for the Philosophy of Science.]

periods of scientific revolution. This is not yet a very strong argument. True: there are empirically inclined philosophers who have derived some satisfaction from the assumption that they only make explicit what is implicitly contained in scientific practice. It is therefore quite important to show that scientific practice is not what it is supposed to be by them. Also, strict adherence to meaning invariance and consistency would have made impossible some very decisive advances in physical theory such as the advance from the physics of Aristotle to the physics of Galileo and Newton. However, how do we know (independently of the fact that they do exist, have a certain structure, and are very influential – a circumstance that will have great weight with opportunists only)[23] that the sciences are a desirable phenomenon, that they contribute to the advancement of knowledge, and that their analysis will therefore lead to reasonable methodological demands? And did it not emerge in the last section that meaning invariance and the consistency condition *are* adopted by some scientists? Actual scientific practice, therefore, cannot be our last authority. We have to find out whether consistency and meaning invariance are *desirable* conditions and this quite independently of who accepts and praises them and how many Nobel prizes have been won with their help.[24] Such an investigation will be carried out in the next sections.

4 INHERENT UNREASONABLENESS OF CONSISTENCY CONDITION

Prima facie, the case of the consistency condition can be dealt with in very few words. Consider for that purpose a theory T′ that successfully describes the situation in the domain D′. From this we can infer (a) that T′ agrees with a *finite* number of observations (let their class be F); and (b) that it agrees with these observations inside a margin M of error only.[25] Any alternative that contradicts T′ outside F and inside M is supported by

[23] In about 1925 philosophers of science were bold enough to stick to their theses even in those cases where they were inconsistent with actual science. They meant to be *reformers* of science, and not *imitators*. (This point was explicitly made by Mach in his controversy with Planck. Cf. again his *Zwei Aufsätze*.) In the meantime they have become rather tame (or beat) and are much more prepared to change their ideas in accordance with the latest discoveries of the historians, or the latest fashion of the contemporary scientific enterprise. This is very regrettable, indeed, for it considerably decreases the number of the rational critics of the scientific enterprise. And it also seems to give unwanted support to the Hegelian thesis (which is now implicitly held by many historians and philosophers of science) that what exists has a 'logic' of its own and is for that very reason reasonable.

[24] Even the most dogmatic enterprise allows for discoveries (cf. the 'discovery' of so-called 'white Jews' among German physicists during the Nazi period). Hence, before hailing a so-called discovery we must make sure that the system of thought which forms its background is not of a dogmatic kind.

[25] The indefinite character of all observations has been made very clear by Duhem, *La Théorie Physique*, Chap. IX. For an alternative way of dealing with this indefiniteness cf. S. Körner, *Conceptual Thinking* (New York, Dover Publications, 1959).

exactly the same observations and therefore <is> acceptable if T′ was acceptable (we shall assume that F are the only observations available). The consistency condition is much less tolerant. It eliminates a theory not because it is in disagreement with the *facts*; it eliminates it because it is in disagreement with *another theory*, with a theory, moreover, whose confirming instances it shares. *It thereby makes the as yet untested part of that theory a measure of validity*. The only difference between such a measure and a more recent theory is age and familiarity. Had the younger theory been there first, then the consistency condition would have worked in its favour. In this respect the effect of the consistency condition is rather similar to the effect of the more traditional methods of transcendental deduction, analysis of essences, phenomenological analysis, linguistic analysis. It contributes to the preservation of the old and familiar not because of any inherent advantage in it – for example, not because it has a better foundation in observation than has the newly suggested alternative, or because it is more elegant – but just because it is old and familiar. This is not the only instance where on closer inspection a rather surprising similarity emerges between modern empiricism and some of the school philosophies it attacks.

Now it seems to me that these brief considerations, although leading to an interesting *tactical* criticism of the consistency condition, do not yet go to the heart of the matter. They show that an alternative of the accepted point of view which shares its confirming instances cannot be *eliminated* by factual reasoning. They do not show that such an alternative is *acceptable*; and even less do they show that it *should be used*. It is bad enough, so a defender of the consistency condition might point out, that the accepted point of view does not possess full empirical support. Adding new theories *of an equally unsatisfactory character* will not improve the situation; nor is there much sense in trying to *replace* the accepted theories by some of their possible alternatives. Such replacement will be no easy matter. A new formalism may have to be learned and familiar problems may have to be calculated in a new way. Textbooks must be rewritten, university curricula readjusted, experimental results reinterpreted. And what will be the result of all the effort? Another theory which, from an empirical point of view, has no advantage whatever over and above the theory it replaces. The only real improvement, so the defender of the consistency condition will continue, derives from the *addition of new facts*. Such new facts will either support the current theories, or they will force us to modify them by indicating precisely where they go wrong. In both cases they will precipitate real progress and not only arbitrary change. The proper procedure must therefore consist in the confrontation of the accepted point of view with as many relevant facts as possible. The exclusion of alternatives is then required for reasons of expediency: their invention not only does not help, but it even hinders progress by absorbing time and manpower that could be devoted to better

things. And the function of the consistency condition lies precisely in this. It eliminates such fruitless discussion and it forces the scientist to concentrate on the facts which, after all, are the only acceptable judges of a theory. This is how the practising scientist will defend his concentration on a single theory to the exclusion of all empirically possible alternatives.[26]

It is worthwhile repeating the reasonable core of this argument: theories should not be changed unless there are pressing reasons for doing so. The only pressing reason for changing a theory is disagreement with facts. Discussion of incompatible facts will therefore lead to progress. Discussion of incompatible alternatives will not. Hence, it is sound procedure to increase the number of relevant facts. It is not sound procedure to increase the number of factually adequate, but incompatible alternatives. One might wish to add that formal improvements such as increase of elegance, simplicity, generality and coherence should not be excluded. But once these improvements have been carried out, the collection of facts for the purpose of test seems indeed to be the only thing left to the scientist.

5 RELATIVE AUTONOMY OF FACTS

And this it is – provided these facts *exist, and are available independently of whether or not one considers alternatives to the theory to be tested.* This assumption on which the validity of the argument in the last section depends in a most decisive manner I shall call the assumption of the relative autonomy of facts, or the autonomy principle. It is not asserted by this principle that the discovery and description of facts is independent of *all* theorizing. But it *is* asserted that the facts which belong to the empirical content of some theory are available whether or not one considers alternatives to *this* theory. I am not aware that this very important assumption has ever been explicitly formulated as a separate postulate of the empirical method. However, it is clearly implied in almost all investigations which deal with questions of confirmation and test. All these investigations use a model in which a *single* theory is compared with a class of facts (or observation statements) which are assumed to be 'given' somehow. I submit that this is much too simple a picture of the actual situation. Facts and theories are much more intimately

[26] More detailed evidence for the existence of this attitude and for the way in which it influences the development of the sciences may be found in Kuhn's book *The Structure of Scientific Revolutions* (Chicago: University of Chicago Press, 1962). The attitude is extremely common in the contemporary quantum theory. 'Let us enjoy the successful theories we possess and let us not waste our time with contemplating what *would* happen if *other* theories were used' – this seems to be the motto of almost all contemporary physicists (<see> Heisenberg, *Physics and Philosophy*, pp. 56, 144) and philosophers (<see> N. R. Hanson, 'Five Cautions for the Copenhagen Interpretation's Critics', *Philosophy of Science*, vol. 26, 1959, pp. 325–37). It may be traced back to Newton's papers and letters (to Hooke, and Pardies) on the theory of colour. See also footnote 23, above.

connected than is admitted by the autonomy principle. Not only is the description of every single fact dependent on *some* theory (which may, of course, be very different from the theory to be tested). There exist also facts which cannot be unearthed except with the help of alternatives to the theory to be tested, and which become unavailable as soon as such alternatives are excluded. This suggests that the methodological unit to which we must refer when discussing questions of test and empirical content is constituted by a *whole set of partly overlapping, factually adequate, but mutually inconsistent theories*. In the present paper only the barest outlines will be given of such a test model. However, before doing this I want to discuss an example which shows very clearly the function of alternatives in the discovery of facts.

As is well known, the Brownian particle is a perpetual motion machine of the second kind and its existence refutes the phenomenological second law. It therefore belongs to the domain of relevant facts for this law. Now, could this relation between the law and the Brownian particle have been discovered in a *direct* manner, i.e., could it have been discovered by an investigation of the observational consequences of the phenomenological theory that did not make use of an alternative account of heat? This question is readily divided into two: (1) Could the *relevance* of the Brownian particle have been discovered in this manner? (2) Could it have been demonstrated that it actually *refutes* the second law? The answer to the first question is that we do not know. It is impossible to say what would have happened had the kinetic theory not been considered by some physicists. It is my guess, however, that in this case the Brownian particle would have been regarded as an oddity much in the same way in which some of the late Professor Ehrenhaft's astounding effects[27] are regarded as an oddity, and that it would not have been given the decisive position it assumes in contemporary theory. The answer to the second question is simply – No. Consider what the discovery of the inconsistency between the Brownian particle and the second law would have required! It would have required (a) measurement of the exact *motion* of the particle in order to ascertain the changes of 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 in order to ascertain that any loss occurring here was indeed compensated by the increase of the energy of the moving particle and the work done against the fluid. Such measurements are beyond experimental possibilities (<see> R. Fürth, 'Über einige Beziehungen zwischen klassicher Statistik und

[27] Having witnessed these effects under a great variety of conditions, I am much more reluctant to regard them as mere curiosities than is the scientific community of today. <See> also my edition of Ehrenhaft's lectures, *Einzelne Magnetische Nord- und Südpole und deren Auswirkung in den Naturwissenschaften* (Vienna, 1947).

Quantenmechanik', *Zeitschrift für Physik*, vol. 81, 1933, pp. 143–62). Neither is it possible to make precise measurements of the heat transfer; nor can the path of the particle be investigated with the desired precision. Hence a 'direct' refutation of the second law that considers only the phenomenological theory and the 'facts' of Brownian motion is impossible. And, as is well known, the actual refutation was brought about in a very different manner. It was brought about via the kinetic theory and Einstein's utilization of it in the calculation of the statistical properties of the Brownian motion.[28] In the course of this procedure the phenomenological theory (T′) was incorporated into the wider context of statistical physics (T) *in such a manner that the consistency condition was violated*; and *then* a crucial experiment was staged (investigations of Svedberg and Perrin).

It seems to me that this example is typical for the relation between fairly general theories, or points of view, and 'the facts'. Both the relevance and the refuting character of many very decisive facts can be established only with the help of other theories which, although factually adequate, are yet not in agreement with the view to be tested. This being the case, the production of such refuting facts may have to be preceded by the invention and articulation of alternatives to that view. Empiricism demands that the empirical content of whatever knowledge we possess be increased as much as possible. Hence *the invention of alternatives in addition to the view that stands in the centre of discussion constitutes an essential part of the empirical method.* Conversely, the fact that the consistency condition eliminates alternatives now shows it to be in disagreement with empiricism and not only with scientific practice. By excluding valuable tests it decreases the empirical content of the theories which are permitted to remain (and which, as we have indicated above, will usually be the theories which have been there first); and it especially decreases the number of those facts which could show their limitations. This last result of a determined application of the consistency condition is of very topical interest. It may well be that the

[28] For these investigations, <see> A. Einstein, *Investigations on the Theory of the Brownian Motion* (New York: Dover, 1956), which contains all the relevant papers by Einstein and an exhaustive bibliography by R. Fürth. For the experimental work, <see> J. Perrin, *Die Atome* (Leipzig, 1920). For the relation between the phenomenological theory and the kinetic theory, <see> also M. v. Smoluchowski, 'Experimentell nachwiesbare, der üblichen Thermodynamik widersprechende Molekularphanomene', *Physikalische Zeitschrift*, vol. 13, 1912, p. 1069, and K. R. Popper, 'Irreversibility; or, Entropy since 1905', *British Journal for the Philosophy of Science*, vol. 8, 1957, pp. 151–5. Despite Einstein's epoch-making discoveries and von Smoluchowski's splendid presentation of their effect (for the latter <see> also *Œuvres de Marie Smoluchowski, volume 2* (Cracovie, 1927), pp. 226 ff., 316 ff., 462 ff. and 530 ff.) the present situation in thermodynamics is extremely unclear, especially in view of the continued presence of the ideas of reduction which we criticized in the text above. To be more specific, it is frequently attempted to determine the entropy balance of a complex *statistical* process by reference to the (refuted) *phenomenological* law after which procedure fluctuations are superimposed in a most artificial fashion. For details cf. Popper, 'Irreversibility'.

refutation of the quantum-mechanical uncertainties presupposes just such an incorporation of the present theory into a wider context which is no longer in accordance with the idea of complementarity and which therefore suggests new and decisive experiments. And it may also be that the insistence, on the part of the majority of contemporary physicists, on the consistency condition will, if successful, forever protect these uncertainties from refutation. This is how modern empiricism may finally lead to a situation where a certain point of view petrifies into dogma by being, in the name of experience, completely removed from any conceivable criticism.

6 THE SELF-DECEPTION INVOLVED IN ALL UNIFORMITY

It is worthwhile to examine this apparently empirical defence of a dogmatic point of view in somewhat greater detail. Assume that physicists have adopted, either consciously or unconsciously, the idea of the uniqueness of complementarity and that they therefore elaborate the orthodox point of view and refuse to consider alternatives. In the beginning such a procedure may be quite harmless. After all, a man can do only so many things at a time and it is better when he pursues a theory in which he is interested rather than a theory he finds boring. Now assume that the pursuit of the theory he chose has led to successes and that the theory has explained in a satisfactory manner circumstances that had been unintelligible for quite some time. This gives empirical support to an idea which to start with seemed to possess only this advantage: it was interesting and intriguing. The concentration upon the theory will now be reinforced, the attitude towards alternatives will become less tolerant. Now if it is true, as has been argued in the last section, that many facts become available only with the help of such alternatives, then the refusal to consider them *will result in the elimination of potentially refuting facts*. More especially, it will eliminate facts whose discovery would show the complete and irreparable inadequacy of the theory.[29] Such facts having been made inaccessible, the theory will appear to be free from blemish and it will seem that 'all evidence points with merciless definiteness in the ... direction ... [that] all the processes involving ... unknown interactions conform to the fundamental quantum law' (Rosenfeld in Körner (ed.), *Observation and Interpretation*. This will

[29] The quantum theory can be adapted to a great many difficulties. It is an open theory in the sense that apparent inadequacies can be accounted for in an *ad hoc* manner, by *adding* suitable operators, or elements in the Hamiltonian, rather than by recasting the whole structure. A refutation of its basic formalism (i.e., of the formalism of quantization, and of non-commuting operators in a Hilbert space or a reasonable extension of it) would therefore demand proof to the effect that *there is no conceivable adjustment of the Hamiltonian, or of the operators used* which makes the theory conform to a given fact. It is clear that such a general statement can only be provided by an *alternative theory* which of course must be detailed enough to allow for independent and crucial tests.

further reinforce the belief in the uniqueness of the current theory and in the complete futility of any account that proceeds in a different manner. Being now very firmly convinced that there is only one good microphysics, the physicists will try to explain even adverse facts in its terms, and they will not mind when such explanations are sometimes a little clumsy. By now the success of the theory has become public news. Popular science books (and this includes a good many books on the philosophy of science) will spread the basic postulates of the theory; applications will be made in distant fields. More than ever the theory will appear to possess tremendous empirical support. The chances for the consideration of alternatives are now very slight indeed. The final success of the fundamental assumptions of the quantum theory and of the idea of complementarity will seem to be assured.

At the same time it is evident, on the basis of the considerations in the last section, that this appearance of success *cannot in the least be regarded as a sign of truth and correspondence with nature*. Quite the contrary, the suspicion arises that the absence of major difficulties is a result of the decrease of empirical content brought about by the elimination of alternatives, and of facts that can be discovered with the help of these alternatives only. In other words, *the suspicion arises that this alleged success is due to the fact that in the process of application to new domains the theory has been turned into a metaphysical system*. Such a system will of course be very 'successful' not, however, because it agrees so well with the facts, but because no facts have been specified that would constitute a test and because some such facts have even been removed. Its 'success' *is entirely man made*. It was decided to stick to some ideas and the result was, quite naturally, the survival of these ideas. If now the initial decision is forgotten, or made only implicitly, then the survival will seem to constitute independent support, it will reinforce the decision, or turn it into an explicit one, and in this way close the circle. This is how empirical 'evidence' may be *created* by a procedure which quotes as its justification the very same evidence it has produced in the first place.

At this point an 'empirical' theory of the kind described (and let us always remember that the basic principles of the present quantum theory and especially the idea of complementarity are uncomfortably close to forming such a theory) becomes almost indistinguishable from a myth. In order to realize this, we need only consider that on account of its all-pervasive character a myth such as the myth of witchcraft and of demonic possession will possess a high degree of confirmation on the basis of observation. Such a myth has been taught for a long time; its content is enforced by fear, prejudice and ignorance as well as by a jealous and cruel priesthood. It penetrates the most common idiom, infects all modes of thinking and many decisions which mean a great deal in human life. It

provides models for the explanation of any conceivable event, conceivable, that is, for those who have accepted it.[30] This being the case, its key terms will be fixed in an unambiguous manner and the idea (which may have led to such a procedure in the first place) that they are copies of unchanging entities and that change of meaning, if it should happen, is due to human mistake – this idea will now be very plausible. Such plausibility reinforces all the manoeuvres which are used for the preservation of the myth (elimination of opponents included). The conceptual apparatus of the theory and the emotions connected with its application having penetrated all means of communication, all actions, and indeed the whole life of the community, such methods as transcendental deduction, analysis of usage, phenomenological analysis which are means for further solidifying the myth will be extremely successful (which shows, by the way, that all these methods which have been the trademark of various philosophical schools old and new, have one thing in common: they tend to *preserve* the *status quo* of the intellectual life).[31] Observational results, too, will speak in favour of the theory as they are formulated in its terms. It will seem that at last the truth has been arrived at. At the same time it is evident that all contact with the world has been lost and that the stability achieved, the semblance of absolute truth, *is nothing but the result of an absolute conformism.*[32] For how can we possibly test, or improve upon, the truth of a theory if it is built in such a manner that any conceivable event can be described, and explained, in terms of its principles? The *only* way of investigating such all-embracing principles is to compare them with a different set of *equally all-embracing* principles – but this way has been excluded from the very beginning. The myth is therefore of no objective relevance, it continues to exist solely as the result of the effort of the community of believers and of their leaders, be these now priests or Nobel prize winners. *Its 'success' is entirely man made.* This, I think, is the most decisive argument against any method that encourages uniformity, be it now empirical or not. Any such method is in the last resort a method of deception. It enforces an unenlightened

[30] For a very detailed description of a once very influential myth, <see> H. C. Lea, *Materials Towards a History of Witchcraft*, 3 vols. (Philadelphia: University of Pennsylvania Press, 1939), as well as *Malleus Malleficarum* (London: John Rodker, 1928), translated by Montague Summers (who, by the way, counts it 'among the most important, wisest [sic!], and weightiest books of the world').

[31] Quite clearly, analysis of usage, to take only one example, presupposes certain regularities concerning this usage. The more people differ in their fundamental ideas, the more difficult it will be to uncover such regularities. Hence, analysis of usage will work best in a closed society that is firmly held together by a powerful myth such as was the philosophy in the Oxford of about ten years ago.

[32] Schizophrenics very often hold beliefs which are as rigid, all-pervasive, and unconnected with reality, as are the best dogmatic philosophies. Only such beliefs come to them naturally whereas a professor may sometimes spend his whole life in attempting to find arguments which create a similar state of mind.

conformism, and speaks of truth; it leads to a deterioration of intellectual capabilities, of the power of imagination, and speaks of deep insight; it destroys the most precious gift of the young, their tremendous power of imagination, and speaks of education.

To sum up: *Unanimity of opinion may be fitting for a church, for the frightened victims of some (ancient, or modern) myth, or for the weak and willing followers of some tyrant; variety of opinion is a feature necessary for objective knowledge; and a method that encourages variety is also the only method that is compatible with a humanitarian outlook.* To the extent to which the consistency condition (and, as will emerge, the condition of meaning invariance) delimits variety, it contains a theological element (which lies, of course, in the worship of 'facts' so characteristic for nearly all empiricism).

7 INHERENT UNREASONABLENESS OF MEANING INVARIANCE

What we have achieved so far has immediate application to the question whether the meaning of certain key terms should be kept unchanged in the course of the development and improvement of our knowledge. After all, the meaning of every term we use depends upon the theoretical context in which it occurs. Hence, if we consider two contexts with basic principles which either contradict each other, or which lead to inconsistent consequences in certain domains, it is to be expected that some terms of the first context will not occur in the second context with exactly the same meaning. Moreover, if our methodology demands the use of mutually inconsistent, partly overlapping, and empirically adequate theories, then it thereby also demands the use of conceptual systems which are mutually *irreducible* (their primitives cannot be connected by bridge laws which are meaningful *and* factually correct) and it demands that meanings of terms be left elastic and that no binding commitment be made to a certain set of concepts.

It is very important to realize that such a tolerant attitude towards meanings, or such a change of meaning in cases where one of the competing conceptual systems has to be abandoned need not be the result of directly accessible observational difficulties. The law of inertia of the so-called *impetus theory* of the later Middle Ages[33] and Newton's own law of inertia are in perfect quantitative agreement: both assert that an object that is not under the influence of any outer force will proceed along a straight line with constant speed. Yet despite this fact, the adoption of Newton's theory entails a conceptual revision that forces us to abandon the inertial law of the impetus theory, not because it is quantitatively incorrect but *because it achieves the correct predictions with the help of inadequate concepts.* The law

[33] For details and further references, <see> Section 6 of my 'Explanation, Reduction, and Empiricism'.

asserts that the *impetus* of an object that is beyond the reach of outer forces remains constant.[34] The impetus is interpreted as an inner *force* which pushes the object along. Within the impetus theory such a force is quite conceivable as it is assumed here that forces determine *velocities* rather than accelerations. The concept of impetus is therefore formed in accordance with a law (forces determine velocities) and this law is inconsistent with the laws of Newton's theory and must be abandoned as soon as the latter is adopted. This is how the progress of our knowledge may lead to conceptual revisions for which no direct observational reasons are available. The occurrence of such changes quite obviously refutes the contention of some philosophers that the invariance of *usage* in the trivial and uninteresting contexts of the private lives of not too intelligent and inquisitive people indicates invariance of *meaning* and the superficiality of all scientific changes. It is also a very decisive objection against any crudely operationalistic account of both observable terms and theoretical terms.

What we have said applies even to singular statements of observation. Statements which are empirically adequate, and which are the result of observation (such as 'here is a table') may have to be reinterpreted, not because it has been found that they do not adequately express what is seen, heard, felt, but because of some changes in sometimes very remote parts of the conceptual scheme to which they belong. Witchcraft is again a very good example. Numerous eyewitnesses claim that they have actually *seen* the devil or *experienced* demonic influence. There is no reason to suspect that they were lying. Nor is there any reason to assume that they were sloppy observers, for the phenomena leading to the belief in demonic influence are so obvious that a mistake is hardly possible (possession; split personality; loss of personality; hearing voices; etc.). These phenomena are well known today.[35] In the conceptual scheme that was the one generally accepted in the fifteenth and sixteenth centuries, the only way of describing them, or at least the way that seemed to express them most adequately, was by reference to demonic influences. Large parts of this conceptual scheme were changed for philosophical reasons and also under the influence of the evidence accumulated by the sciences. Descartes's materialism played a very decisive role in discrediting the belief in spatially localizable spirits. The language of demonic influences was no part of the new conceptual scheme that was created in this manner. It was for this reason that a reformulation was needed, and a reinterpretation of even the most

[34] We assume here that a dynamical rather than a kinematic characterization of motion has been adopted. For a more detailed analysis <see> again the paper referred to in the previous footnote.

[35] For very vivid examples, <see> K. Jaspers, *Allgemeine Psychopathologie* (Berlin: Springer-Verlag, 1959), pp. 75–123.

common 'observational' statements. Combining this example with the remarks at the beginning of the present section, we now realize that according to the method of classes of alternative theories a lenient attitude must be taken with respect to the meanings of all the terms we use. We must not attach too great an importance to 'what we mean' by a phrase, and we must be prepared to change whatever little we have said concerning this meaning as soon as the need arises. Too great concern with meanings can only lead to dogmatism and sterility. Flexibility, and even sloppiness in semantical matters is a prerequisite of scientific progress.[36]

8 SOME CONSEQUENCES

Three consequences of the results so far obtained deserve a more detailed discussion. The first consequence is an evaluation of *metaphysics* which differs significantly from the standard empirical attitude. As is well known, there are empiricists who demand that science start from observable facts and proceed by generalization, and who refuse the admittance of metaphysical ideas at any point of this procedure. For them, only a system of thought that has been built up in a purely inductive fashion can claim to be genuine knowledge. Theories which are partly metaphysical, or 'hypothetical', are suspect, and are best not used at all. This attitude has been formulated most clearly by Newton[37] in his reply to Pardies' second letter concerning the theory of colours:

> if the possibility of hypotheses is to be the test of truth and reality of things, I see not how certainty can be obtained in any science; since numerous hypotheses may be devised, which shall seem to overcome new difficulties.

This radical position, which clearly depends on the demand for a theoretical monism, is no longer as popular as it used to be. It is now granted that metaphysical considerations may be of importance when the task is to *invent* a new physical theory; such invention, so it is admitted, is a more or less irrational act containing the most diverse components. Some of these components are, and perhaps must be, metaphysical ideas. However, it is also pointed out that as soon as the theory has been developed in a formally satisfactory fashion and has received sufficient confirmation to be regarded as empirically successful, it is pointed out that in the very same moment it can *and must* forget its metaphysical past; metaphysical speculation must *now* be replaced by empirical argument.

[36] Mae West is by far preferable to the precisionists: 'I ain't afraid of pushin' grammar around so long as it sounds good' (*Goodness Had Nothing to do With It* (New York, 1959), p. 19).

[37] I. B. Cohen (ed.), *Isaac Newton's Papers and Letters on Natural Philosophy* (Cambridge, MA: Harvard University Press, 1958), p. 106.

> On the one side I would like to emphasize [writes Ernst Mach on this point][38] that *every and any* idea is admissible as a means for research, provided it is helpful; still, it must be pointed out, on the other side, that it is very necessary from time to time to free the presentation of the *results* of research from all inessential additions.

This means that empirical considerations are still given the upper hand over metaphysical reasoning. Especially in the case of an inconsistency between metaphysics and some highly confirmed empirical theory it will be decided, *as a matter of course*, that the theory or the result of observation must stay, and that the metaphysical system must go. A very simple example is the way in which materialism is being judged by some of its opponents. For a materialist the world consists of material particles moving in space, of collections of such particles. Sensations, as introspected by human beings, do not look like collections of particles and their observed existence is therefore assumed to refute and thereby to remove the metaphysical doctrine of materialism. Another example which I have analysed in 'Problems of Microphysics' is the attempt to eliminate certain very general ideas concerning the nature of micro-entities on the basis of the remark that they are inconsistent 'with an immense body of experience' and that 'to object to a lesson of experience by appealing to metaphysical preconceptions is unscientific'.[39]

The methodology developed in the present paper leads to a very different evaluation of metaphysics. Metaphysical systems are scientific theories in their most primitive stage. If they *contradict* a well-confirmed point of view, then this indicates their usefulness as an alternative to this point of view. Alternatives are needed for the purpose of criticism. Hence, metaphysical systems which contradict observational results or well-confirmed theories *are most welcome* starting points of such criticism. Far from being misfired attempts at anticipating, or circumventing, empirical research which were deservedly exposed by a reference to experience, they are the only means at our disposal for examining those parts of our knowledge which have already become observational and which are therefore inaccessible to a criticism 'on the basis of observation'.

A second consequence is that a new attitude has to be adopted with respect to the *problem of induction*. This problem consists in the question of what justification there is for asserting the truth of a statement S given the truth of another statement, S′, whose content is smaller than the content of S. It may be taken for granted that those who want to justify the truth of S also assume that after the justification the truth of S will be *known*. Knowledge to the effect that S implies the *stability* of S (we must not

[38] 'Der Gegensatz zwischen der mechanischen und der phänomenologischen Physik', *Wärmelehre* (Leipzig, 1896), pp. 362 f.

[39] L. Rosenfeld, 'Misunderstandings', p. 42.

change, remove, criticize, what we know to be true). The method we are discussing at the present moment cannot allow such stability. It follows that the problem of induction, at least in some of its formulations, is a problem whose solution leads to undesirable results. It may therefore be properly termed a pseudo problem.

The third consequence, which is more specific, is that *arguments from synonymy* (or from co-extensionality), far from being that measure of adequacy as which they are usually introduced, are liable severely to impede the progress of knowledge. Arguments from synonymy judge a theory or a point of view not by its capability to mimic the world but rather by its capability to mimic the descriptive terms of another point of view which for some reason is received favourably. Thus for example, the attempt to give a materialistic, or else a purely physiological, account of human beings is criticized on the grounds that materialism, or physiology, cannot provide synonyms for 'mind', 'pain', 'seeing red', 'thinking of Vienna', in the sense in which these terms are used either in ordinary English (provided there is a well-established usage concerning these terms, a matter which I doubt) or in some more esoteric mentalistic idiom. Clearly, such criticism silently assumes the principle of meaning invariance, that is, it assumes that the meanings of at least some fundamental terms must remain unchanged in the course of the progress of our knowledge. It cannot therefore be accepted as valid.[40]

However, we can, and must, go still further. The ideas which we have developed above are strong enough not only to *reject* the demand for synonymy, wherever it is raised, but also to *support* the demand for irreducibility (in the sense in which this notion was used at the beginning of Section 7). The reason is that irreducibility is a presupposition of high critical ability on the part of the point of view shown to be irreducible. An outer indication of such irreducibility which is quite striking in the case of an attack upon commonly accepted ideas is the feeling of *absurdity*: we deem absurd what goes counter to well-established linguistic habits. The absence, from a newly introduced set of ideas of synonymy relations connecting it with parts of the accepted point of view; the feeling of absurdity therefore indicates that the new ideas are fit for the purpose of criticism, i.e., that they are fit for either leading to a strong *confirmation* of the earlier theories, or else to a very revolutionary *discovery*: absence of synonymy, clash of meanings, absurdity are desirable. Presence of synonymy, intuitive appeal, agreement with customary modes of speech, far from being *the* philosophical virtue, indicates that not much progress has

[40] For details concerning the mind–body problem, <see> my 'Materialism and the Mind–Body Problem', *The Review of Metaphysics*, vol. 17, 1963.

been made and that the business of investigating what is commonly accepted *has not even started.*

9 HOW TO BE A GOOD EMPIRICIST

The final reply to the question put in the title is therefore as follows. A good empiricist will not rest content with the theory that is in the centre of attention and with those tests of the theory which can be carried out in a direct manner. Knowing that the most fundamental and the most general criticism is the criticism produced with the help of alternatives, he will try to invent such alternatives.[41] It is, of course, impossible at once to produce a theory that is formally comparable to the main point of view and that leads to equally many predictions. His first step will therefore be the formulation of fairly general assumptions which are not yet directly connected with observations; this means that his first step will be the invention of a new *metaphysics*. This metaphysics must then be elaborated in sufficient detail in order to be able to compete with the theory to be investigated as regards generality, details of prediction, precision of formulation.[42] We may sum up both activities by saying that a good empiricist must be a critical metaphysician. Elimination of all metaphysics, far from increasing the empirical content of the remaining theories, is liable to turn these theories into dogmas. The consideration of alternatives together with the attempt to criticize each of them in the light of experience also leads to an attitude where meanings do not play a very important role and where arguments are based upon assumptions of fact rather than analysis of (archaic, although perhaps very precise) meanings. The effect of such an attitude upon the development of human capabilities should not be underestimated either. Where speculation and invention of alternatives is encouraged, bright ideas are liable to occur in great number and such ideas may then lead to a change of even the most 'fundamental' parts of our knowledge, i.e., they may lead to a change of assumptions which either are so close to observation that their truth seems to be dictated by 'the facts', or which are so close to common prejudice that they seem to be 'obvious', and their negation 'absurd'. In such a situation it will be realized that neither 'facts' nor abstract ideas can ever be used for defending certain principles come what may. Wherever facts play a role in such a dogmatic defence, we shall have to suspect foul play (see the opening quotation) – the foul play of those who try to turn good science into bad, because unchangeable, metaphysics. In the last resort, therefore, being a good empiricist means

[41] In my paper 'Realism and Instrumentalism' I have tried to show that this is precisely the method which has brought about such spectacular advances of knowledge as the Copernican Revolution, the transition to relativity and to quantum theory.

[42] <See> Section 13 of my 'Realism and Instrumentalism'.

being critical, and basing one's criticism not just on an abstract principle of scepticism but upon *concrete suggestions* which indicate in every single case how the accepted point of view might be further tested and further investigated and which thereby prepare the next step in the development of our knowledge.