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THE ROLE OF CRUCIAL EXPERIMENTS IN SCIENCE*

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Exactly how and what do we learn about scientific theories from experiment? The term 'crucial experiment' indicates that from some experiments we learn more than from others. Immense numbers of experiments are never recorded, and of those recorded and published most are forgotten and buried in the annals of science on dusty bookshelves possibly never to be looked at again.

From what class of experiments then do we learn? There are several competing theories of learning which define this class very differently.[1]

According to strict inductivists an experiment is basic (rather than 'crucial') if one can induce some important law of nature from it. Those logicians who recognized the invalidity of inductive generalizations saw the force of 'crucial' experiment in securing the truth of a scientific theory in a different way. Some thought it was possible to enumerate *a priori* all the possible rival theories, and regarded those experiments as 'crucial' which refuted $n-1$ rival theories and thereby proved the n-th. It is *reason* which conjectures and it is *experiment* which disproves and proves. But, as many skeptics pointed out, rival theories are always indefinitely many and therefore the *proving* power

of experiment vanishes. One cannot learn from experience about the truth of any scientific theory, only at best about its falsehood: *confirming instances have no epistemic value whatsoever.*

But are all unrefuted theories equally conjectural? According to some they are. According to others, some are more probable than others. Such probability values are calculated by modern inductive logicians by way of definitions of possible states of affairs which, of course, are bound to rely on strong assumptions. Once we agree to these assumptions, each confirming instance may assume some tiny epistemic relevance for the probability of theories.

Another suggested possibility was to withhold the status of proof from any theory, but to assess its verisimilitude by counting the number of its defeated serious rivals: one does not know how much we learn in this way about human imagination (in inventing new alternatives and designing crucial experiments) and how much about nature, for the most counter-intuitive characteristic of this learning theory is that it puts a tremendous premium on the invention of new false alternatives: degree of corroboration may be the hallmark of the perverse inventiveness of the human mind rather than of the theory's nature-dependent verisimilitude.

Can we then at least learn from experiment that some theories are false? I have shown elsewhere and shall argue again later that we cannot. Fries pointed out that no proposition can be proved from facts, but even if we were to *accept as true,* by methodological decision, certain factual propositions, the conventionalists' case for the

never-excludable possibility of rescue oper-
ations shows disproofs of specific theories to
be impossible. We cannot learn from experi-
ence the falsehood of any theory.

Two philosophers, Grünbaum and Pop-
per, tried to save falsificationism (and hence
falsificationist learning theory) from this
impasse. Grünbaum ([1969] and [1971])
withdrew considerably in the face of the
conventionalists' arguments that theories
cannot be conclusively refuted. His only
remaining claim seems to be that we can
learn about the high probability of the
falsehood of *some* scientific theories. And,
after enumerating some interesting exam-
ples where falsifications were ignored or dis-
cussed and revised, he leaves the question of
the empirical appraisal of *most* scientific the-
ories *completely open.* Learning from experi-
ence is then in disarray. Even, however, to
sustain his restricted claim—that *some* scien-
tific theories are falsifiable—he needs to
show: (1) that some factual ('basic') proposi-
tions are reliable; and (2) that some back-
ground knowledge can be so highly
probable as to be true 'beyond reasonable
doubt'. I do not see how he can do either.
Popper, on the other hand, offers a dif-
ferent, and, indeed, *general* solution. He
accepts that all scientific propositions, basic
or universal, are equally conjectural; he
then specifies a 'game of science' by which
we 'accept' some of them, 'reject' others.
Popper's game of science is governed pri-
marily by the moral maxim that one must
not stick to one's theories forever in the light
of unfavourable evidence; conventionalism
is morally wrong: we *must* learn from expe-
rience. But at the end of his classic *Logik der
Forschung* he offers a methodology without
an epistemology or learning theory, and
confesses explicitly that his methodology
may lead us epistemologically astray, and
implicitly, that *ad hoc* stratagems might lead
us to Truth.[2]

Popper's 'game of science' (or 'logic of
scientific discovery', or 'methodology', or
'system of appraisals', or 'demarcation crite-
rion', or 'definition of science'[3]) is a set of
standards for scientific theories.[4]

Popper's logic of discovery contains 'pro-
posals', 'conventions' about when a theory
should be taken seriously (when a crucial
experiment could, and indeed has been,
devised against it) and when a theory should
be rejected (when it has failed a test). In
Popper's logic of discovery—as in Pascal's,
Bernard's, or Grünbaum's—scientific theo-
ries are not based on, established, or 'proba-
bilified', by 'facts', but rather eliminated by
them. Progress consists of an incessant,
ruthless, revolutionary confrontation of
bold, speculative theories and repeatable
observations, and of the subsequent speedy
elimination of the defeated theories: 'The
method of trial and error is a *method of elim-
inating false theories* by observation state-
ments'.[5] 'Conjectures [are] boldly put
forward for trial, to be eliminated if they
clash with observations'.[6] Thus, the history
of science is seen as a series of duels between
theory and experiment, duels in which only
experiments can score decisive victories.
The theoretician proposes a scientific the-
ory; some basic statements contradict it; if
one of these becomes 'accepted',[7] the theory
is 'refuted' and must be rejected, and a new
one has to take its place. 'What ultimately
decides the fate of a theory is the result of a
test, *i.e.,* an agreement about basic state-
ments'.[8] Popper realizes, of course, that we
always test large systems of theories rather
than isolated ones. But he does not regard
this as an insurmountable difficulty: he sug-
gests that we should *agree* which part of such
a system is responsible for the refutation
(that is, which part is to be regarded as
false), perhaps helped by independent tests
of some portions of the system. Within
Popper's philosophy this kind of agreement
is absolutely indispensable: if one were
allowed to blame refutations upon the initial
conditions *all the time,* no major theory need
ever be rejected.[9] He is not content with tests
which are designed to test large systems: he
calls on the scientist to specify, beforehand,
those experiments which will, if their out-
come is negative, lead to the falsification of
the very heart of the system.[10] He demands
of the scientist that he specify in advance
under what experimental conditions he

would give up his *most basic* assumptions.[11] This moral demand, indeed, is the gist of Popper's 'demarcation criterion' or, to use another term, of his definition of science.[12]

Popper's definition of science can best be put in terms of 'conventions' or 'rules' governing the *'game of science'*.[13]

The opening move must be a *consistent, falsifiable hypothesis:* that is, a consistent hypothesis which has agreed-on potential falsifiers. A potential falsifier is a 'basic statement' whose truth-value is decidable with the help of the experimental techniques of the time. The scientific jury must agree unanimously that there is an experimental technique which will enable them to assign a truth-value to the 'basic statement'. (Unanimity can, of course, be reached by expelling the minority as pseudo-scientists or cranks.[14])

The next move is the repeated performance of the test in a controlled experiment,[15] and the second decision of the jury on what actual truth-value (truth or falsehood) to attribute to the potential falsifier. (If this second decision is not unanimous, there are two possible moves: either the potential falsifier must be withdrawn and, unless a replacement is found, the opening move cancelled; or, alternatively, the dissenting minority of the jury must be declared cranks and excluded from the jury.[16])

If the second verdict is *negative,* and the potential falsifier is rejected, then the hypothesis is declared 'corroborated', which only means that it invites further challenges. If the second verdict is *positive,* and the potential falsifier accepted, then the hypothesis is declared 'falsified', which means that it is *rejected,* 'overthrown', 'dropped', buried with military honours.[17] (In 1960 Popper introduced a new rule: military pomp can only be awarded to an eliminated hypothesis if, before it was falsified, it was at least once—in a different experiment—corroborated.[18])

After the burial a new hypothesis is invited. This new hypothesis must, however, explain the partial success, if any, of its predecessor, and also something more. A hypothesis, however novel in its intuitive aspects, will not be allowed to be proposed, unless it has novel empirical content in excess of its predecessor. If it has no such excess content, the referee will declare it *'ad hoc'* and make the proposer withdraw it. If the new hypothesis is not *ad hoc,* the standard procedure for falsifiable hypotheses, as described above, is followed for the new hypothesis.[19]

This 'scientific game', if properly played, will 'progress' in the sense that the theories subsequently proposed will have increasing generality (or 'empirical content'); they will pose ever deeper *questions* about the universe.[20]

Just as the rules of chess do not explain why some people should play the game and, indeed, devote their lives to it, the rules of science do not explain why some people should play the game of science and, indeed, devote their lives to it. The rules decide whether a particular *move* is 'proper' (or 'scientific') or not, but they remain silent about whether the *game as a whole* is 'proper' (or 'rational') or not. The rules say nothing either about the (psychological) motives of the players or about the (rational) purpose of the game. One can, of course, play the game as a genuine game and enjoy it for itself, without caring for its purpose or being aware of one's motives.

The rules of the game are *conventions,* and can be formulated in terms of a *definition*.[21] How can one criticize a definition, in particular, if one interprets it nominalistically?[22] A definition is then a mere abbreviation, a tautology. What can one criticize about a tautology? Popper claims that his definition of science is 'fruitful': 'that a great many points can be clarified and explained with its help'. He quotes Menger: 'Definitions are dogmas; only the conclusions drawn from them can afford us any new insight'.[23] But how can a definition have explanatory power or afford new

insights? Popper's answer is this: 'It is only from the consequences of my definition of empirical science, and from the methodological decisions which depend upon this definition, that the scientist will be able to see how far it conforms to his intuitive idea of the goal of his endeavours'.[24]

This answer complies with Popper's general position that conventions can be criticized by discussing their 'suitability' relative to some purpose: 'As to the suitability of any convention opinions may differ; and a reasonable discussion of these questions is only possible between parties having some purpose in common. The choice of that purpose . . . goes beyond rational argument'.[25] Popper, in his *Logik der Forschung* never specifies a *purpose* of the game of science that would go beyond what is contained in its rules. The idea that the *aim* of science is *truth*, occurs in his writings for the first time in 1957.[26] In his *Logik der Forschung* the quest for truth may be a psychological *motive* of scientists—it is not a rational *purpose* of science.[27]

Even in Popper's later writings we find no suggestion of how to appraise one consistent set of rules (or demarcation criterion) as leading more successfully towards truth than another. Indeed, the thesis that any such argument connecting method and success is impossible has been a cornerstone of Popper's philosophy from 1920 to 1971. Thus I conclude that Popper never offered a theory of rational criticism of consistent conventions. He does not answer the question: '*Under what conditions would you give up your demarcation criterion*'?[28]

But the question can be answered. I shall give my answer in two stages: first a naive and then a more sophisticated answer. I start by recalling how Popper, according to his own account, had arrived at his criterion. He thought, like the best scientists of his time, that Newton's theory, although refuted, was a wonderful scientific achievement; that Einstein's theory was still better; and that astrology, Freudianism and twentieth-century Marxism were pseudo-scien-

tific. His problem was to find a definition of science from which these '*basic judgements*' concerning each of these theories followed; and he offered a novel solution. Now let us agree *provisionally* on the meta-criterion that a *a rationality theory—or demarcation criterion—is to be rejected if it is inconsistent with accepted 'basic value judgements' of the scientific community*.[29] Indeed, this metamethodological rule would seem to correspond to the falsificationist methodological rule that a scientific theory is to be rejected if it is inconsistent with an ('empirical') basic statement unanimously accepted by the scientific community. Popper's whole methodology rests on the contention that there exist (relatively) singular statements on whose truth-value scientists can reach unanimous agreement; without such agreement there would be a 'new Babel' and 'the soaring edifice of science would soon lie in ruins'.[30] Now even if there is agreement about 'basic' statements, but on the other hand no agreement whatsoever about how to appraise scientific achievement relative to this 'empirical basis', would not the soaring edifice of science equally soon lie in ruins? No doubt it would, Surprisingly, while there has been little agreement concerning a *universal* criterion of the scientific character of theories, there has been considerable agreement over the last two centuries concerning single achievements. While there has been no *general* agreement concerning a theory of scientific rationality, there has been considerable agreement concerning the rationality of a particular step in the game—was it scientific or crankish? A general definition of science thus must reconstruct the acknowledgedly best games and the most esteemed gambits as 'scientific'; if it fails to do so, it has to be rejected.[31]

However, if we apply this meta-criterion (*which I am going to reject later*), the falsificationist demarcation criterion has to be rejected.

The falsificationist demarcation criterion can indeed be easily 'falsified' by showing

that in the light of this meta-criterion the best scientific achievements were unscientific and that the best scientists, in their greatest moments, broke the falsificationist rules of science.

In Popper's version of falsificationism the basic rule is that *the scientist must specify in advance under what experimental conditions he will give up even his most basic assumptions:* Criteria of refutation have to be laid down beforehand: it must be agreed which observable situations, if actually observed, mean that the theory is refuted. But what kind of clinical responses would refute to the satisfaction of the analyst *not merely a particular clinical diagnosis but psychoanalysis itself?* And have such criteria even been discussed or agreed upon by analysts?[32] In the case of psychoanalysis Popper was right: no answer has been forthcoming. Freudians have been nonplussed by Popper's basic challenge concerning scientific honesty. They have refused to specify experimental conditions under which they would give up their basic assumptions. For Popper this is the hallmark of their intellectual dishonesty. But what if we put Popper's question to the Newtonian scientist: 'What kind of observation would refute to the satisfaction of the Newtonian, not merely a particular Newtonian explanation, but Newtonian dynamics and gravitational theory itself? And have such criteria even been discussed or agreed upon by Newtonians?' The Newtonian will, alas, scarcely be able to give a positive answer.[33] But then if psychoanalysts are to be condemned as dishonest by Popper's standards, must not Newtonians be similarly condemned?

Popper may certainly withdraw his celebrated challenge and demand falsifiability—and rejection on falsification—only for *systems* of theories, including initial conditions and all sorts of auxiliary and observational theories. This is a very considerable withdrawal, for it allows the imaginative scientist to save his pet theory by suitable lucky alterations in some odd corner of the theoretical maze. But even Popper's mitigated rule will make life impossible for the most brilliant scientist. For in large research programmes there are always known anomalies: normally the researcher puts them aside and follows the positive heuristic of the programme.[34] In general he rivets his attention on the positive heuristic rather than on the distracting anomalies, and hopes that the 'recalcitrant instances' will be turned into confirming instances as the programme progresses. On Popper's terms, even great scientists use forbidden gambits, *ad hoc* stratagems: instead of regarding Mercury's anomalous perihelion as a falsification of the Newtonian theory of our planetary system and thus as a reason for its rejection, most of them shelved it as a problematic instance to be solved at some later stage—or offered *ad hoc* solutions. This methodological attitude of treating as *anomalies* what Popper would regard as *counter examples* is commonly accepted by the best scientists. Some of the research programmes now held in highest esteem by the scientific community progressed in an ocean of anomalies.[35] Rejection of such work by Popper as irrational ('uncritical') implies—at least on our quasi-Polanyiite meta-criterion—a falsification of his definition.

Moreover, for Popper, an inconsistent system does not forbid any observable state of affairs and working on it must invariably be regarded as irrational: 'A self-contradictory system must be rejected . . . [because it] is uninformative . . . No statement is singled out . . . since all are derivable'.[36] But in such cases the best scientists' rule is frequently: '*Allez en avant et la foi vous viendra.*'[37] This anti-Popperian rule secured a sanctuary for the infinitesimal calculus hounded by Bishop Berkeley, and for naïve set theory in the period of the first paradoxes. Indeed, if the game of science had been played according to Popper's rule book, Bohr's 1913 paper would never have been published since it was inconsistently grafted on to Maxwell's theory, and Dirac's delta functions would have been suppressed until Schwartz.

In general, both Popper and Grünbaum stubbornly overestimate the immediate striking force of purely negative criticism, whether empirical or logical. 'Once a mistake, or a contradiction, is pinpointed, there can be no verbal evasion: it can be proved, and that is that'.[38] Grünbaum seems to think that the 'negative result' embodied in the Michelson-Morley experiment played a crucial logical role in the genesis of relativity theory.[39] But I have shown that prior to the emergence of relativity theory the Michelson-Morley experiment was in no 'logical' sense a 'negative result' for classical physics.[40]

This is how some of the 'basic' appraisals of the scientific élite 'falsify' the falsificationist definition of science and falsificationist morality.

I have tried to amend the falsificationist definition of science so that it no longer rules out essential gambits of actual science. I tried to bring about such an amendment, *primarily by shifting the problem of appraising theories to the problem of appraising historical series of theories, or, rather, of 'research programmes', and by changing the falsificationist rules of theory rejection.*

First, *one may 'accept' not only basic but also universal statements as conventions: indeed, this is the most important clue to the continuity of scientific growth.*[41] The basic unit of appraisal must be not an isolated theory or conjunction of theories but rather a *research programme*, with a conventionally accepted (and thus, by provisional decision, 'irrefutable') *hard core* and with a *positive heuristic* which defines problems, foresees anomalies and turns them victoriously into examples according to a preconceived plan. The scientist lists anomalies, but as long as his research programme sustains its momentum, he ignores them. *It is primarily the positive heuristic of his programme, not the anomalies, which dictate the choice of his problems.* Only when the driving force of the positive heuristic weakens, may more attention be given to anomalies. (The methodology of research programmes can explain in this way *the relative autonomy of theoretical science;* disconnected chains of conjectures and refutations cannot.)

(In my approach, then, we learn from experience primarily through a few verifying instances, but learning is a very complicated and theoretical process. For falsificationists learning comes only from negative instances. As Agassi put it in 1964: 'Learning from experience is learning from a refuting instance. The refuting instance then becomes a problematic instance' (p. 201). In 1969 Agassi again emphasizes that 'we learn from experience by refutations' (p. 169), and adds that one can learn *only* from refutation but not from corroboration (p. 167). But this is a very poor theory of learning from experience.[42] Feyerabend [1969] says that '*negative instances suffice in science*'.)

The appraisal of large units like research programmes is in one sense much more liberal and in another much more strict than Popper's appraisal of theories. This new appraisal is *more tolerant* in the sense that it allows a research programme to outgrow infantile diseases, such as inconsistent foundations and occasional *ad hoc* moves. Anomalies, inconsistencies, *ad hoc* stratagems, even alleged negative 'crucial' experiments, can be consistent with the overall progress of a research programme. The old rationalist dream of a mechanical, semi-mechanical or at least fast-acting method for showing up falsehood, unprovenness, meaningless rubbish or even irrational choice has to be given up. But this new appraisal is also *more strict* in that it demands not only that a research programme should successfully predict novel facts, but also that the protective belt of its auxiliary hypotheses should be largely built according to a preconceived unifying idea, laid down in advance in the positive heuristic of the research programme.[43]

It is very difficult to decide, especially if one does not demand progress at each single step, when a research programme has degenerated hopelessly; or when one of two

rival programmes has achieved a decisive advantage over the other. In this sense there can be no 'instant rationality'. *Neither the logician's proof of inconsistency nor the experimental scientists's verdict of anomaly can defeat a research programme at one blow.* The falsificationist can be 'wise' only after the event if he wants to apply falsificationism to research programmes rather than to isolated theories. Nature may shout 'No' but human ingenuity—contrary to Weyl and Popper[44]—may always be able to shout louder. With sufficient brilliance, and some luck, any theory, even if it is false, can be defended 'progressively' for a long time. Grünbaum admits this now: but then what remains of his falsificationism?[45]

But when should a particular theory, or a whole research programme, be rejected? I claim, only if there is a better one to replace it.[46] Thus I separate Popperian 'falsification' and 'rejection', the conflation of which turned out to be the main weakness of his 'naive falsificationism'.[47] *One learns not by accepting or rejecting one single theory but by comparing one research programme with another for theoretical, empirical and heuristic progress.*[48]

My modification then presents a very different picture of the game of science from Popper's. The best opening gambit is not a falsifiable (and therefore consistent) hypothesis, but a research programme. Mere 'falsifications' (that is, anomalies) are recorded but need not be acted upon. *'Crucial experiments' in the falsificationist sense do not exist:* at best they are honorific titles conferred on certain anomalies *long after the event* when one programme has been defeated by another one. For the falsificationist a crucial experiment is described by an accepted basic statement which is inconsistent with a theory.[49] I, for one, hold that no accepted basic statement alone entitles us to reject a theory. Such a clash may present a problem (major or minor), but in no circumstance a 'victory'. No experiment is crucial at the time it is performed (except perhaps psychologically). The falsificationist

pattern of 'conjectures and refutations', that is, the pattern of trial-by-hypothesis followed by error-shown-by-experiment breaks down. A theory can only be eliminated by a *better* theory, that is, by one which has excess empirical content over the corroborated content of its predecessors, some of which is subsequently confirmed. And for this replacement of one theory by a better one, the first theory does not even have to be 'falsified' in the orthodox sense of the term.[50] Thus progress and learning are marked by instances verifying excess content rather than by falsifying instances,[51] and 'falsification' and 'rejection' become logically independent.[52] Popper says explicitly that 'before a theory has been refuted we can never know in what way it may have to be modified'.[53] In my view it is rather the opposite way round: before a theory has been modified we can never know in what way it has been 'refuted', and some of the most interesting modifications are motivated by the 'positive heuristic' of the research programme rather than by anomalies.[54]

Thus I offered a falsification of the falsificationist theory of 'crucial experiments'. But an opponent could claim that the falsification of my own new criterion is not much more difficult than Grünbaum's and Popper's. What about the immediate impact of great crucial experiments, like that of the falsification of the parity principle? Or the long, pedestrian, trial-and-error procedures which occasionally precede the announcement of a major research programme? Will not the judgment of the scientific élite go against my—or, indeed, against *any*—universal rules?

I should like to present my answer in two stages. First, I should like to amend slightly my previously announced provisional meta-criterion,[55] and then replace it altogether with a better one.

First, the slight amendment. If a universal rule clashes with a particular 'normative basic judgment', one should allow some time

for the scientific community to ponder about the clash: they may give up their particular judgment and submit to the general rule.[56] These 'second-order' falsifications must not be rushed.

Secondly, if we abandon negative crucial experiments in *method*, why stick to it in *metamethod*? We can easily have a second-order methodology of *methodological* (as opposed to scientific) research programmes: the methodology of research programmes self-applied.

While maintaining that a theory of rationality has to try to organize basic value judgments in universal, coherent frameworks, we do not have to reject such a framework immediately, merely because of some anomalies or other inconsistencies. On the other hand, a good rationality theory must anticipate further basic value judgments unexpected in the light of their predecessors or even lead to the revision of previously held basic value judgments. We reject a rationality theory only for a better one, for one which, in this quasi-empirical sense, represents a *progressive shift*. Thus this new—more lenient—metacriterion enables us to compare rival logics of discovery and discern growth in 'metascientific' knowledge.

For instance, the falsificationist theory of scientific rationality need not be seen as 'falsified' simply because it clashes with some actual basic judgments of leading scientists. On the contrary, on our new criterion it represents progress over its justificationist predecessors. For, contrary to these predecessors, it rehabilitated the scientific status of falsified theories like the phlogiston theory, thus reversing a *value judgement* (of inductivist historians) which expelled the latter from the history of science proper into the history of irrational beliefs. Likewise, it reversed the appraisal of the falling star of the 1920s: of the Bohr-Kramers-Slater theory.[57] In the light of most justificationist theories of rationality, the history of science is, at its best, a history of *pre*scientific preludes to some *future* history of science.[58]

Falsificationist methodology enabled the historian to interpret more of the actual value judgments (as seen at the time) in the history of science as rational: falsificationism constituted progress compared with inductivism.

On the other hand, I hope that my methodology will be seen, in turn—on the criterion I specified—as a further step forward. For it seems to offer a coherent account of *more* old, isolated basic value judgments as rational; indeed, it has led to new and, at least for the justificationist or naive falsificationist, *surprising* basic value judgments. For instance, for the falsificationist, it becomes *irrational* to approve of (and therefore retain and further elaborate) Newton's gravitational theory after the discovery of Mercury's anomalous perihelion; or it becomes *irrational* to approve of (and therefore boldly develop) Bohr's old quantum theory based on inconsistent foundations: it may even have been irrational to approve of Einstein's early relativity theory, at least without the shock of the Michelson-Morley experiment. From my point of view these were perfectly *rational* developments. According to my theory, unlike that of the falsificationists, Newtonians, Bohr and Einstein were right. Also, as seen from the point of view of my methodology, some rearguard skirmishes for defeated programmes were perfectly rational, and not signs of dogmatic behaviour; and thus it enables us to reverse those standard judgments of later historiography which led to the disappearance of many of these skirmishes from history of science textbooks.[59] Such rearguard skirmishes were previously deleted both by the inductivist and by the falsificationist party histories.

Progress in the theory of rationality happens to be marked by historical discoveries or rediscoveries: by the reconstruction of a growing bulk of value-impregnated history as rational.[60]

I, of course, can easily answer the question when I would give up my criterion of demarcation: when another one is proposed

which is better on my metacriterion.[61] (I have not yet answered the question under what circumstances I shall give up my metacriterion; but one must always stop somewhere.)

CONCLUSION

The problem of appraisal of scientific theories (of which the problem of demarcation is a zero-case) is one of the basic problems of the philosophy of science. Its solution determines the normative content of science-learning theory; the outline of our code of intellectual honesty; and also our historiographical outlook. (It also, by the way, determines a specific formulation of the problem of induction.)

There are three major approaches to the solution of this generalized demarcation problem:

(1) One may try to offer a universal demarcation criterion like the ones proposed by probabilists or falsificationists or by the methodology of scientific research programmes. This is Leibnitz's, Carnap's, Popper's, Grünbaum's (and my own) approach.

(2) One may agree that one anomaly may be more conclusive than another; one theory may be better than another; but there is and can be no universal demarcation criterion to decide. Each case has to be dealt with on its own merit and the judgment of authority (of the great scientists) adhered to. This is Polanyi's and Kuhn's approach.[62]

(3) One may deny that any theory is epistemically superior to any other approach; therefore, there are only competing beliefs, some of them called 'scientific'. This cultural relativism originating with ancient skepticism is widely spread now in contemporary anti-science movements; its most articulate expression is to be found in Feyerabend's recent 'epistemological anarchism'.

I view the third approach with horror: I view the second as abject philosophical surrender to authority. Unless we achieve progress in the solution of the generalized problem of demarcation, many branches of science may well degenerate into tribal specializations with standards uncheckable from the outside. This is where I see the most important challenge to the philosophy of science.

NOTES

1. 'Learning from experience' is a normative idea; all the different theories I am going to discuss have normative character. Moreover, all purely empirical learning theories miss the heart of the problem. Also *cf.* my [1970], 123, text to footnote 2.

2. These difficulties may only be 'solved' through a superimposition on this game of some—merely posited—'inductive principle' as I argued in [1968a] and [1971b]. On the epistemological level there has been no progress in the skeptic-dogmatist controversy since Pyrrho and Hume. In particular, Popper's contribution to the solution of the problem of induction, contrary to his own claim, is nil.

3. This profusion of synonyms has proved to be rather confusing.

4. Incidentally, this problem of standards is altogether alien to 'hermeneutics', so vigorously represented at this conference by Professor Apel.

5. Popper [1963], 56; his own italics.

6. Popper [1963], 46.

7. For the conditions of acceptance of basic statements, *cf.* Popper [1935], section 22, and my [1970], 107–8.

8. Popper [1935], section 30.

9. Grünbaum, I am sure, abhors this Popperian conventionalism. This is why he wishes—to my mind, unsuccessfully—to assign high epistemic value to both basic statements *and* to background knowledge.

10. For references *cf.* footnotes 33 and 47.

11. *Cf.* text to footnote 32. Also *cf.* my [1970], 107.

12. *Cf.* my [1970], 109. For an interesting discussion *cf.* also Musgrave [1968].

13. Popper [1935], section 11 and also 85. The first paragraph in section 11 explains why he gave the title *The Logic of Scientific Discovery* to his book and is worth quoting:

Methodological rules are here regarded as *conventions*. They might be described as the rules of the game of empirical science. They differ from the rules of pure logic rather as do the rules of chess, which few would regard as part of *pure* logic; seeing that the rules of pure logic govern transformations of linguistic formulae, the result of an inquiry into the rules of chess could perhaps be entitled *The Logic of Chess,* but hardly *Logic* pure and simple. (Similarly, the result of an inquiry into the rules of the game of science— that is, of scientific discovery—may be entitled *The Logic of Scientific Discovery.*)

14. I am afraid Popper did not spell out this implication; although he mentions, as if it were a matter of fact, that cranks do not 'seriously disturb the working of various social institutions which have been designed to further scientific objectivity . . .' (Popper [1945], II, 218). Then he goes on: 'Only political power . . . can impair (their) functioning. . . '. (Also *cf.* his [1957*a*], 32.) I wonder.

15. For the concept of 'controlled experiment', *cf.* my [1970], III, footnote 6.

16. *Cf.* footnote 14.

17. Popper [1935], sections 3 and 4.

18. *Cf.* Popper [1963], 242–5.

19. Following Popper's new rule referred to in the previous footnote, the anti-adh cness rules may also be tightened; and we have to distinguish between *ad hoc*$_1$ and *ad hoc*$_2$; *cf.* my [1968*a*], 375–90, especially 389, footnote 1.

20. Popper [1935], section 85, last sentence.

21. *Cf.* Popper [1935], sections 4 and 11.

22. For an excellent discussion of the distinction between nominalism and realism (or, as Popper prefers to call it, 'essentialism') in the theory of definitions, *cf.* Popper [1945], Chapter 11, and [1963], 20.

23. Popper [1935], section 11.

24. *Ibid.*

25. Popper [1935], section 4.

26. Popper [1957*b*].

27. Popper, in 1935, called the search for truth 'the strongest (unscientific) motive' ([1935], section 85).

28. This flaw is the more serious since Popper himself has expressed qualifications about his criterion. For instance, in his [1963] he describes 'dogmatism', that is, treating anomalies as a kind of 'background noise', as something that is 'to some extent necessary' (p. 49). But on the next page he identifies this 'dogmatism' with 'pseudoscience'. Is then pseudoscience 'to some extent necessary?' Also, *cf.* my [1970], 177, footnote 3.

29. 'Basic value judgments' sounds better in German: '*normative Basissätze*'.

30. Popper [1935], section 29.

31. This approach, of course, does not mean that we *believe* that the scientists 'basic judgments' are unfailingly rational; it only means that we *accept* them in order to criticize universal definitions of science. (If we add that no such *universal* criterion has been found and no such *universal* criterion will ever be found, the stage is set for Polanyi's conception of the lawless closed autocracy of science.)

The idea of this meta-criterion may be seen as a 'quasi-empirical' self-application of Popperian falsificationism. I had introduced this 'quasi-empiricalness' earlier in the context of mathematical philosophy. We may abstract from *what* flows in the logical channels of a deductive system, whether it is something certain or something fallible, whether it is truth and falsehood or probability and improbability, or even moral or scientific desirability and undesirability: it is the *how* of the flow which decides whether the system is negativist, 'quasi-empirical', dominated by *modus tollens* or whether it is justificationist, 'quasi-Euclidean', dominated by *modus ponens.* (*Cf.* my [1967]). This 'quasi-empirical' approach may be applied to *any* kind of normative knowledge like ethical or aesthetic, as has already been done by Watkins in his [1963] and [1967]. But now I prefer another approach.

32. Popper [1963], 38, footnote 3; my italics. This, of course, is equivalent to his celebrated 'demarcation criterion' between science and pseudo-science—or, as he put it, 'metaphysics'. (For this point, also *cf.* Agassi [1964], section VI.)

33. *Cf.* my [1970], 100–1.

34. *Cf.* my [1970], especially 135ff.

35. *Ibid.*, 138ff.

36. *Cf.* Popper [1935], section 24.

37. *Cf.* my [1970], especially 140ff.

38. Popper [1959], 394. He adds: 'Frege did

not try evasive manoeuvres when he received Russell's criticism'. But, of course, he did. (*Cf.* Frege's *Postscript* to the second edition of his *Grundgesetze.*) This historiographical mistake may also be related to Popper's earlier overconfidence in the unambiguity of mathematical reasoning. Also *cf.* my [1968*a*], 357, footnote 2.

39. E.g. Grünbaum [1963], ch. 12.

40. *Cf.* my [1970], 159–65. [*Added in press*]: *Cf.* also Zahar [1973], and the discussion that followed it in the *British Journal for the Philosophy of Science* in 1974.

41. Popper does not permit this:
There is a vast difference between my views and conventionalism. I hold that what characterizes the empirical method is just this: our conventions determine the acceptance of the *singular,* not of *universal* statements. (Popper [1935], section 30.)
Grünbaum, too, rejects the idea of treating theories as conventions.

42. *Cf.* my [1970], 121, footnote 1, and 123.

43. In my [1970] I called patched-up developments which did not meet such criteria *ad hoc*$_3$ strategems. Planck's first correction of the Lummer-Pringsheim formula was *ad hoc* in *this* sense. A particularly good example is Meehl's anomaly (*cf.* my [1970], 175, footnote 3, and 176, footnote 1). This conceptions of '*ad hoc*$_3$' is partly anticipated by Grünbaum [1964], 1411.

44. Popper [1935], section 85.

45. Grunbaum [1971], 126–7.

46. *Cf.* my [1968*a*], 383–6, my [1968*b*], 162–7, and my [1970], 116ff. and 155ff.

47. *One important consequence is the difference between Popper's and Grünbaum's discussions of the 'Duhem-Quine argument' and mine; cf.* on the one hand Popper [1935], last paragraph of section 18 and section 19, footnote 1; Popper [1957*a*], 131–3; Popper [1963], 112, footnote 26, 238–9 and 243; and Grünbaum, [1960], [1969], and [1971]; and on the other hand, *cf.* my [1970], 184–9.

48. *Cf.* my [1970], 132–8. [*Added in press*]: Also *cf.* Zahar [1973], 99–104.

49. As a consequence of my criticism, Popper withdrew from this position. *Now* he says that only *important* '*real*' falsifiers should make us reject a theory. As he recently put it: 'The first *real* discrepancy can refute [a theory]'. But when is an accepted basic statement, inconsistent with a theory, a *real* falsifier? Obviously this is a matter for the scientific *élite* to decide. For instance, according to Popper, Mercury's anomalous perihelion was *not* a 'real' discrepancy. A planet moving in a square would be a 'real' one.

By 1970 Popper had to choose: will he go on searching for a better universal demarcation criterion and accept the methodology of scientific research programmes, or will he become a Polanyiite. He chose the latter. (*Cf.* Popper [1971], 9.) Also *cf.* footnote 62.

50. Popper occasionally—and Feyerabend systematically—stressed the *catalytic* role of alternative theories in devising so-called 'crucial experiments'. But alternatives are not merely catalysts, which can be later removed in the rationed reconstruction, but are *necessary* parts of the falsifying process. (*Cf.* my [1970], 121, footnote 4.)

51. *Cf.* especially my [1970], 120–1.

52. *Cf.* especially my [1968*a*], 385 and my [1970], 121.

53. Popper [1963], 51.

54. *Cf.* especially my [1970], 135–8.

55. *Cf.* above, 364.

56. There is a certain analogy between this pattern and the occasional appeals procedure of the theoretical scientist against the verdict of the experimental jury; *cf.* my [1970], 127–31.

57. Van der Waerden thought that the Bohr–Kramers–Slater theory was bad: Popper's theory showed it to be good. *Cf.* Van der Waerden [1967], 13 and Popper [1963], 242ff.; for a critical discussion, *cf.* my [1970], 168, footnote 4 and 169, footnote 1.

58. The attitude of some modern logicians to the history of mathematics is a typical example; *cf.* my [1963–4], 3.

59. *Cf.* my [1970], section 3(c).

60. There is nothing necessary about this process. I need not say that no rationality theory can or should explain *all* history of science as rational: even the greatest scientists make wrong steps and fail in their judgment.

61. [*Added in press*]: Since this paper was prepared, such a methodology has indeed been proposed: *cf.* Zahar [1973], 99–104.

62. For Popper's recent conversion to Polanyi's position above, *cf.* footnote 49.

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