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内　容

日本ポパー哲学研究会
第６回年次研究大会および会員総会のお知らせ

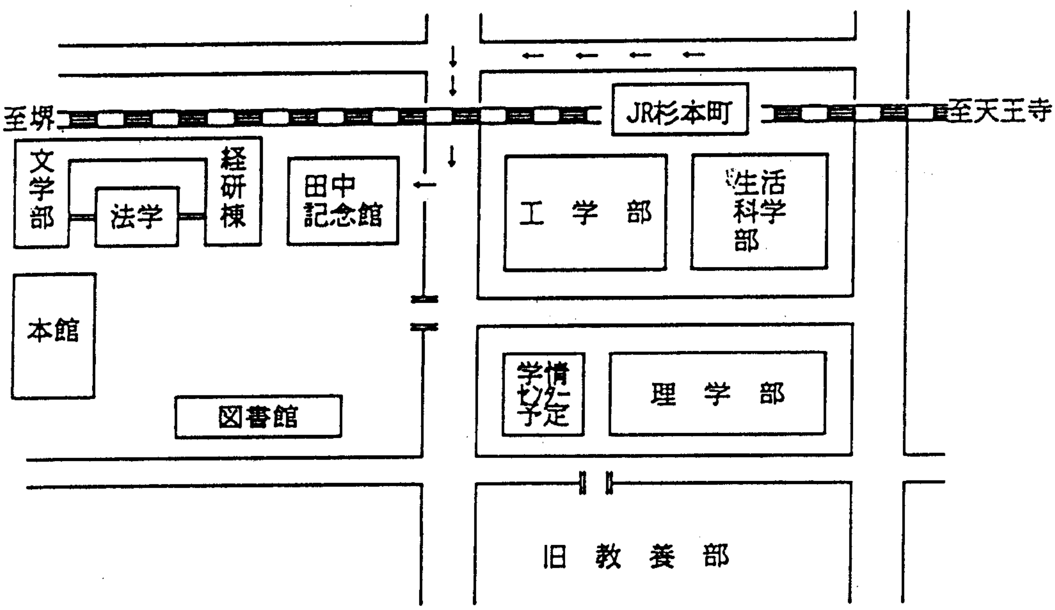
第６回年次研究大会および会員総会を下記の要領で開催することになりましたのでお知らせいたします。従来、やむをえぬ事情から東京を中心に大会が行われておりましたが、西日本の会員の皆さまにこれまでおかけしておりましたご不便を考慮して、今回は初めての試みとして、大阪市立大学で開催することとなりました。ご多用中とは存じますが、できるだけ多くの会員諸兄のご参加を賜りたく、ご案内申し上げます。

日時：　６月２４日（土曜日）10:00より
場所：　大阪市立大学　田中記念館　（地図参照）
会費：　参加費　1000円　　懇親会費　5000円程度

スケジュール：

10:00~12:00	Ian Jarvie (York Univ.) 講演	Popper's Republic of Science
	司会	小河原誠（鹿児島大学）
12:00~13:00	昼休み（運営委員会ミーティング）	
13:00~13:30	会員総会	
13:30~15:00	神野慧一郎（摂南大学）講演	「ポパーの実在論」
	司会	中才敏郎（大阪市立大学）
15:00~15:30	コーヒー・ブレーク（質問票整理）	
15:30~17:00	フロアー・ディスカッション	
17:00~	懇親会	

ＪＲ「天王寺」駅よりＪＲ阪和線（普通）で約13分「杉本町」駅下車。南東へ歩いて約５分。



POPPER'S REPUBLIC OF SCIENCE*

I.C. Jarvie
York University, Toronto

It might indeed be said that the majority of the problems of theoretical philosophy, and the most interesting ones, can be re-interpreted...as problems of method.
- Popper (*LScD*, p. 56)

Insoluble difficulties in the 'pure' logic of science led Popper to propose, in 1935, that we treat science as a social institution constituted by a set of rules, or methodology, that furthers certain aims. Popper's subsequent work is consistent with this 'social' reading, but he nowhere makes it explicit. Virtually all of his critics (and some of his followers) overlook this decisive shift from the logical to the social, and as a result simply fail to appreciate this most original feature of his thought.

Although I find a 'social turn' at the centre of Popper's thought, it needs stressing that his concerns were first and always philosophical. The epigraph of this paper makes a bold and ambitious philosophical claim. So far as I know, Popper never portrayed himself as having put forward a social view of science. This is fair enough. The social turn we shall find in his work was not followed up by empirical work. Popper never carried out empirical study of the workings of the institutions of science, nor did he write a scientific biography. Indeed, it would be fair to say that he has not even done any historical studies of science, at least ones which involved the study of primary documents. He reframes his problem as methodological, and methodology is a set of social conventions. Thus it also is to be stressed that Popper does not reduce science to being just another social institution. It is a social institution, but a very special one in both its aims (transcendent truth) and its results (universal scientific knowledge). There must be no confusion between Popper's 'sociologism' and that of "the Strong Programme in the Sociology of Knowledge". Nonetheless, the thesis of this chapter is that Popper is a major theorist of the sociology of science: *The Logic of Scientific Discovery* contains many profound and original ideas on the social character and constitution of science, and, more to our purpose, those social ideas on science contain the germ of his later, very influential ideas on society and politics. In other words, I shall argue that Popper did not philosophise about science and then turn to apply that philosophy to social and political thought. He was thinking socially from the beginning.

1. The Received View of Popper on Science

To begin with, it may be helpful to outline the main ideas of *The Logic of Scientific Discovery* as they are usually described. The work is in two parts. In Part I the ideas are sketched, in Part II they are worked out and defended in detail as a series of answers to objections.

The book focuses on two problems which are labelled 'the problem of induction' and 'the problem of demarcation'.

The problem of induction is that of how to gain general theoretical knowledge from experience. Experience always comes in particulars: we observe something about this, that or the other. Knowledge, by contrast, is general, even universal. Logic allows that no number of particular statements describing the experience of observing white swans shall permit the deduction of a general statement of scientific knowledge such as 'all swans are white'. How, then, do we get from particular experience to universal knowledge?

The problem of demarcation is that of how sharply to differentiate science from non-science, including pre-science, pseudo-science, folk wisdom, and metaphysics, as well as logic and mathematics.

Since, for Popper, knowledge presupposes language and must be formulated in statements, he is able to reformulate the two problems more crisply: the problem of induction becomes, how can we reach the general statements of scientific law when the statements reporting our experience are necessarily limited and particular; the problem of demarcation becomes, how can we demarcate scientific statements from other statements?

If we accept his autobiographical account, Popper had been engaged in thinking about these issues since 1919, when he was 17.[1] The first problem he says he solved fairly quickly: the relationship of scientific statements to statements of experience was deductive, not inductive. Logic allowed particular statements of experience to *refute* proposed universal scientific statements, and that, it turned out, was sufficient for the task at hand. Provided one did not expect science to consist of certain, or proved, knowledge, it was enough to have general statements that had survived attempts at refutation: they were not supported or recommended by anything other than the fact that they had so far survived whatever challenges experience had thrown at them. This made them the most qualified candidates for consideration as scientific knowledge.

Popper relates that it took him some years to realise that the two problems of induction and demarcation were connected, and that the demarcation problem was the deeper of the two: induction was one solution to the demarcation problem, deduction another. The solution to the demarcation problem that Popper poses and elaborates is that scientific statements be characterised as general statements which are falsifiable. Those already falsified belong to the history of science. Those not yet falsified are current science. Statements that are not falsifiable come in more than one type: there are the statements of logic and mathematics, which are tautologous truths; there are general statements that sound empirical but which nothing could falsify, these are metaphysical.

The reason science is best characterised as consisting of falsifiable general statements has to do with the manner in which science permits us to learn from experience. Puzzled by some logical conflict between our received ideas and statements capturing the evidence of experience, we seek a resolution. Three choices confront us. Either the report of experience is faulty;if not that, then the logical claim to detect a contradiction is faulty; if neither of those, then the received ideas are faulty. Thus is science the critical engagement of ideas with experience. It is the process of culling from among the ideas we have those that are worth criticising by the check[2] of experience. The check of experience is not easy to meet. We need to guard against formulating our ideas in a vague, weak or evasive manner, for these make empirical criticism difficult, even impossible. To foster the confrontation of ideas with experience Popper makes a revolutionary proposal.

Sir Francis Bacon, founder of the view that induction is what characterises scientific method, saw clearly that adoption of the correct method was not, in itself, sufficient. The impulse to find what we want, to observe what is not there, is overwhelming. The mind is filled with what he termed 'prejudices'. Bacon therefore proposed that scientists must first work on emptying their minds of all preconceptions and curbing the almost irrepressible desire to jump to conclusions. Thus, for Bacon, being a scientist involves a good deal of inner struggle to achieve a certain psychological condition, a condition where the mind is free from prejudice and thus open and receptive to the experience of Nature as it really is. Only in that purified state, Bacon thinks, will the methodological canons of inductive reasoning, patiently pursued, keep one clear of error.

There are two features of Bacon's account that are worth noting here for their contrast with what Popper proposes. One is that Bacon's account is individualistic - the budding scientist struggles alone against his prejudices. The other is that it is psychologistic: bad habits of mind are the trouble; science can only be attained after one has developed good habits of mind. Elsewhere, Bacon did envisage scientific institutions,[3] but not in his account of method.

2. Methodology and Social Practice

Popper's revolutionary move is not just to shift the methodological emphasis from induction to deduction - although that is a standard way to put it. Popper goes further: he also rejects Bacon's individualistic and psychologistic approach to readying oneself for science. Prejudices are not to be purged by some sixteenth century version of psychotherapy, but by confrontation with experience in a social setting. Science is a creative endeavour, it is the search for new knowledge, so Bacon's emphasis on the old - psychological state of the discoverer, on how ideas are obtained - is misplaced. What counts is how those ideas stand up to various checks we carry out on them, most importantly, the check of experience (*LScD*, § 2). Instead of working on the state of our minds, Popper argues for an institutional approach. As he sees it, the cooperative and social character of scientific research checks our Baconians impulses to see what we want to see, to jump to premature conclusions, and so on.[4] He refers to this as the friendly-hostile cooperation of scientists, and, more abstractly, as the intersubjective nature of the scientific enterprise (*LScD*, p. 44-48). Scientific ideas are formulated in publicly examinable ways, open to being checked by others as well as their proposers. Those others may be motivated in various ways: to find the truth, or by envy and hatred of the authors of the ideas. But the checking takes place within institutionalised methodological rules that are the best we can devise in an imperfect world. These rules do not ensure there will be no error, but they do create incentives to discover and expose error, rather than to evade it and cover it up.

How does Popper get from an inquiry firmly centred in the logic of science to an insight into aspects of scientific method that go beyond the logical into the social? Popper early in his discussion considers three objections to his view (*LScD*, § 6). The first objection is that we expect science to deliver positive information, so it is wrong-headed to characterise it in a negative way, by refutability. This objection Popper parries by asserting that he will later show that positive information has a logical connection to refutability (the promise is cashed in *LScD*, § § 31-46).The second objection is to the effect that, in the same way that inductions cannot be verified, no more can falsifications. In rebuttal Popper maintains that there is here an asymmetry in the logical relations. One contradictory evidence statement is sufficient to refute a generalisation; no number of noncontradicting evidence statements is sufficient to establish a generalisation. The third objection is said to be more serious: refutations can be handily evaded by conventionalist stratagems, such as the ad hoc introduction of auxiliary hypotheses or the redefinition of terms, or simply by refusing to look at the contrary evidence. Popper finds this objection logically insuperable as it stands. He writes:

> it is impossible to decide, by analysing its logical form, whether a system of statements is a conventional system of irrefutable implicit definitions, or whether it is a system which is empirical in my sense; that is, a refutable system... *Only with reference to the method applied* to a theoretical system is it at all possible to ask whether we are dealing with a conventionalist or an empirical theory. The only way to avoid conventionalism is by taking a *decision*: the decision not to apply its methods. (*LScD*, p. 82.)

Thus Popper accepts a decisive criticism of the very position often attributed to him - what I have termed the received interpretation. This is important because it is recognition of the problem created by the third objection that moves Popper into proposing what I call his social view of scientific method.

The conventionalist objection to the refutability criterion of science cannot be overcome on its substance. If someone chooses to indulge in *ad hoc* manoeuvres, or to ignore refuting evidence, they commit no logical or factual error. This does not mean their position is unobjectionable. What they do, Popper maintains, is to impoverish science by refusing to submit its claims to the tribunal of experience. A system sheltered in this way can easily and unnoticingly degenerate into metaphysics, that is, untestable statements. Popper thus shifts the discussion from logical matters to questions about choices, aims, policies and their consequences. For those concerned to keep science anchored in experience, Popper suggests adopting a supreme or meta-methodological rule not to avoid falsification (*LScD*, p.54).

Here, in the shift from purely logical criteria for science to methodology, we find the beginnings of Popper's social view of science. A methodology consists of methodological rules; each rule represents a decision, a choice to act in a certain way; we make these choices, in turn, in order to foster certain aims. They are thus open to discussion. Both the rationale of the choices and whether the choices will in fact foster the desired aims are matters on which there can be reasoned dispute.

Let me spell all this out in a little more detail. If we look closely at the opening pages of *The Logic of Scientific Discovery* we find that what is proposed is a *theory of method.* A scientist is stipulated to be someone who puts forward statements or systems of statements and tests them step by step. The task of a logic of scientific discovery, or a logic of knowledge, is to give a logical analysis of this procedure, "that is, to analyse the method of the empirical sciences" (*LScD*, p. 27). Popper proposes a method of deductive testing of theories ("deductivism") by deriving their consequences and checking these by various means: against each other (are they self-consistent?); by their logical form (are they empirical or tautological?); against other theories (are they consistent with them?); and finally against empirical experience (are they consistent with known and with newly discovered experimental facts?).

Discussing the problem of demarcation, Popper challenges the idea that science is a way of thinking (psychologism), or the method of induction, which is caught in a vicious infinite regress, or indeed that there exists any "natural" boundary to it. Instead, he suggests treating the refutability criterion as a proposal for a suitable agreement or convention to help us to "be able to say of a given system of statements whether or not its closer study is the concern of empirical science" (*LScD*, p.37). Reasonable discussion of the suitability of a convention is "only possible between parties having some purpose in common" (*LScD*, p. 37). Parties with some purpose in common refers, of course, to a social group of some sort.

The social group in question seeks knowledge of the world of our experience, the real world. How is its theoretical system to be distinguished from others? By the fact that it is submitted to tests and has stood up to tests. This is a methodological view of what constitutes experience. The two alternatives Popper is discussing are the inductive method and the deductive method. Deductivism, he proposes, admits to empirical science only those systems of statements that can be refuted by experience. However, this idea of falsifiability is insufficient (*LScD*, pp. 42, 50 and 54), because it is always possible to evade or deny a threatening falsification.

Does this mean that science is simply a subjective choice to proceed in a certain way? Not at all. But Bacon mistakenly identified the problem: objectivity has nothing to do with a psychological state of disinterestedness or of being free of prejudice. Instead, Popper draws on Kant's idea that "objective" refers to a statement that can in principle be understood and tested by anybody. This Popper calls the "inter-subjective testability" of scientific claims. In an important footnote added to the English translation he says "inter-subjective *testing* is merely a very important aspect of the more general idea of inter-subjective *criticism*, or in other words, of the idea of mutual rational control by critical discussion" (*LScD*, p. 44), making explicit reference to his own later works, *The Poverty of Historicism, The Open Society and Its Enemies* and *Postscript to the Logic of Scientific Discovery.*

This point about objectivity made, Popper resumes his only slightly veiled sociology. The theory of method, he announces, is concerned with the purely logical relations between scientific statements and with the choice of methods -

> decisions about the way in which scientific statements are to be dealt with. These decisions will of course depend in their turn upon the *aim* which we choose from among a number of possible aims. The decision here proposed for laying down suitable rules for what I call the 'empirical method' is closely connected with my criterion of demarcation: I propose to adopt such rules as will ensure the testability of scientific statements; which is to say, their falsifiability (*LScD*, p. 49).

What are these rules, why do we need them, can there be a theory of such rules? A methodology turns, he writes, on one's attitude to science. If one is interested in the advancement of science, in its constant revisions and corrections, one will be led to a very different answer than if one takes a narrowly logical and naturalistic view of science. Purely logical means offer no defence against metaphysics or anything else because of the possibility that refutation can be evaded. Instead we have to set up conventions for which we take responsibility: "by what we do with them and what we do to them. Thus I shall try to establish the rules, or if you will the norms, by which the scientist is guided" (*LScD*, p. 50).

That Popper is thinking institutionally could not, I think, be clearer; all the more puzzling, then, that it has been overlooked. Popper is here proposing that science is to be seen as an interested group that shares an aim and then legislates conventions for itself in order the better to pursue that aim. He does not explicitly say that his view is social, but he offers some analogies with the social institutions of games and of trial by jury, going so far as to refer to "the game of empirical science" (*LScD*, p. 53) and comparing its rules to the rules of chess. Certainly he seems to be arguing that science is constituted by its rules, as is chess. He also seems to be allowing that the rules of science can be debated, hence they are not immutable. Much the same goes for chess. The rules of chess have evolved and might evolve more. A rule revision would not necessarily make for a new game, especially if the rule was adopted by the International Federation. The fact that, in baseball, the American League permits the designated hitter to substitute for the pitcher and the National League does not, hardly raises serious questions about which league really plays baseball.

But the analogy to games has a flaw: games are relatively frivolous activities, engaged in for recreation and play. Science, by contrast, is the search for knowledge, a rather more weighty matter. Early scientists in the Age of Reason saw their activity as directed towards blowing away the cobwebs of error and superstition from the past, clearing the corridors that eventually led to the enlightenment of mankind. The very success of this project has led to mankind becoming seriously dependent on science and its applications for its economic livelihood and for preserving and extending its longevity. Thus the building of institutional norms or rules for science might better, I suggest, be compared not to a game like chess, but to the creation of dedicated social institutions - universities, for example, or learned societies. This might be especially apt as universities and some learned societies are subordinate institutions within the overall institutional creation we call science.

Popper does not pursue matters in this direction. Although methodology as he conceives of it is clearly institutional, he does not examine any actual scientific institutions and their workings, including the methodological and other norms prescribed. One of the very few other places where Popper brings to the surface the social nature of the view he is developing is the passage where he compares science to the institution of trial by jury (*LScD*, pp. 109-110).

Popper uses trial by jury to make a number of very important points about the theoretical context of inquiry and the way it directs our approach to the facts. Juries, he notes, decide issues of fact. The questions they are asked to decide will depend on the actual laws in force, and the procedures followed. He gives no examples. He has mind, I conjecture, the decisions built into a legal system that distinguish it from others: for example, 'minor child' may be defined differently in different places, restricting the charges that may be levelled at someone in one jurisdiction rather than the other; English-speaking readers will likely be familiar with a trial system that is adversarial, leading questions being permitted only in certain parts of the examination, and a jury that must sit mute, and not disclose its deliberations. What the jury does is come to a *decision* about a matter of fact. This Popper compares to scientists deciding to accept a basic statement. From this statement, together with statements about the law, consequences can be deduced (for example, that an accused has or has not committed an offence). Although the trial, and the conduct of the jury, are governed by rules, Popper is at pains to stress that the jury verdict never justifies or gives grounds for the truth of what it asserts it finds. By contrast, he notes, the judge's judgment is expected to be 'reasoned'. If the reasoning is unsound, that is a ground for challenge; no comparable challenge can be made to the substance of the verdict of the jury. Popper here seems to recommend a scientific parallel: the experimentalist experts decide the finding of fact, the basic statements; the presiding scientific community then tries to *judge* the implications of that finding. The finding of fact is far less frequently disputed than are the implications.

The analogies drawn with chess and jury trial serve to highlight on the one hand the constitutive nature of rules and on the other the embedding of crucial decisions in institutional procedures. The equivalent institution in science to the International Chess Federation, the jury and the judge, are not spelled out. Popper's focus remains on the logic of science as he endeavours to show that falsifiability is a viable criterion of scientific character once embodied in a methodology. In the course of defending the view in *LScD* he offers many further suggestions for methodological rules. What he does not engage in, as we shall see, is any discussion of the general picture: do the rules come as a set, or can we pick and choose and yet stay with the game of science?; how are decisions made when new rules are offered or modifications to old ones suggested?; is not submission to the rules constitutive of the institution of science broadly conceived?

3. Popper's Suggested Methodological Rules for Science

In the final section of Part I of *The Logic of Scientific Discovery* Popper discusses "methodological rules as conventions". He proposes a supreme or meta-rule which governs the later rules to be proposed. This reads:

(SR) "the other rules of scientific procedure must be designed in such a way that they do not protect any statement in science against falsification" (*LScD*, p. 54).

This meta-rule enshrines the falsifiability criterion of demarcation as a control for the whole system of rules. It takes care of the objection that falsification can always be avoided by mandating that it *will* not be avoided. Thus we see why I said that the received view of Popper is that he characterises science by falsifiability. In fact he finds falsifiability insufficient in itself. It is not self-justifying like a tautological truth. It has to be adopted, by decision, a decision governed by aims. We then impose it upon ourselves as a procedure, a methodology. Popper is no more a "naive falsificationist" than Bacon is a naive inductivist. Each was fully aware of the logical deficiencies of their alleged positions and did their level best to remedy them.

Although he writes of "proceeding systematically", Popper does not go on to write up a complete list of the rules which govern scientific method. He does not say so, but there are good reasons for not doing so. To expect this would be like expecting a legal commentator to specify the complete list of laws. No such list can exist. Law-making and law-reforming are on-going endeavours. Attempts are made from time to time to codify areas of the law, but never the system as a whole. All such codifications need constant maintenance and up-dating. In this respect, a methodology is not like the closed set of rules that constitute chess. Popper begins by proposing two examples, and we shall be able to extract others from later in the book. The first example is:

(R1) "The game of science is, in principle, without end. He who decides one day that scientific statements do not call for any further test, and that they can be regarded as finally verified, retires from the game" (*LScD*, p. 53).

This rule directly attacks those philosophers who, like Bacon, envisage science as eventually resulting in a body of finally verified truths. Such spokespeople are a rare breed today, sixty years into the Popperian age, but they are not yet extinct. It also raises a fundamental question against all those physicists, the most recent of whom is Steven Weinberg, who think science might culminate on one grand, final, Theory of Everything.

The second example is:
(R2) "Once a hypothesis has been proposed and tested, and has proved its mettle, it may not be allowed to drop out without 'good reason'" (*LScD*, p. 53-54).

Among the good reasons for a hypothesis being allowed to drop out of science are its replacement by one better testable, or the falsification of one of its consequences. This rule clearly addresses what is sometimes known as the 'stability of science'. The question is raised, if science is simply the set of unrefuted hypotheses, are we free to pick and choose among them? This rule suggests that no, once a hypotheses has achieved a certain status it can only lose that status in an orderly procedure. Although the content of science changes, sometimes quite frequently, science comes as a package; the institution treats picking and choosing as unscientific. At least, in principle it does; in practice things are less clear cut.

These examples of rules are nothing at all like the rules of inductive logic that other methodologists have sought for, and they can be debated and revised by the community of scientists, other interested parties, and the community at large, without special technical preparation. Consider the question of the plausibility of (R2). The analogy of law-making in the British Parliament may help. When a Bill passes all the stages and receives Royal Assent it gains a special status - namely Law. It gets printed in statute books, applied in the courts and cited in precedents. However, it is a fact that some laws simply fade away. They may stay on the statute books for centuries and yet not be enforced or even attended to. A good example is the laws against suicide. Does anything comparable happen in science? I suspect it does. That is, I suspect that hypotheses which once proved their mettle sometimes just fade out of sight, out of mind. Not because of the fallibility of memory (Morgan 1985), but because of an entirely new way of looking at things has arisen, and it is not worth bothering to refute each established result one by one - those in the know ignore them, those not in the know go on thinking that these are still current results. A good example from anthropology is the many, many stories from around the globe that Sir James Frazer collected in *The Golden Bough*. This best seller is still in print, but it would be hard to find anthropologists who could cite chapter and verse of where and when this or that report in it has been falsified. Almost all of them could be falsified in principle, but they may not have been in fact.

More problematic are cases advanced by Agassi and others where a scientist in good standing resists some scientific idea, perhaps even a dominant scientific idea, that appears to have proved its mettle. Sometimes the objection is described as metaphysical, sometimes aesthetic, sometimes it is inarticulate. Is such resistance unscientific? In so far as it motivates attempts to criticise and refute the idea, or to develop an alternative, the answer is no: motivation is irrelevant. In so far as it involves refusal to face facts, to suppress, or fail to acknowledge, then, yes.

The supreme rule (SR) and the two simple rules (R1 and R2) are offered by Popper early in the book as *examples* of the sorts of rules that can be used to govern science and ensure that its theories are connected to experience. The whole exercise Popper conceives of as his *theory of experience*. Without denying that characterisation, it is here being represented as a set of putative rules for the constitution and work of the Republic of Science. Nothing like a complete set of such rules exists in Popper's text. On the analogy with the law, mentioned above, this makes sense. But if the rules are to be the Constitution of Science, then they deserve to be spelled out so that prospective members know the dimensions of the regime they are agreeing to be governed by. But it is also possible to view the rules as a kind of common law - unwritten customs long established and only articulated and discussed at problematic moments. But Popper supplies no clear decision procedure in the case of dispute or infraction. Some of the epistemological and other sins Popper criticises and erects rules against are perpetrated by scientists in good standing, not just by philosophers. So how are complaints to be dealt with, how are infractions to be dealt with? It should be remembered that Popper is implicitly embedding the institution of science inside two larger entities. First in an open civil society governed by the rule of law. Thus general matters of criminal conduct and of dishonesty are already sanctioned. The second is that science is an activity for those with time and inclination to want *rerum cognoscere causas*.[5] Putting this another way round, Popper is taking it for granted that there are barriers to entry other than those erected by the necessity of subscribing to the rules he is articulating. Science is an institution or set of institutions embedded amongst other institutions, to some of which it is subordinate.

Popper concentrates mainly on rules of procedure - rules that guide us in how to proceed. But there are a few rules he mentions, like the SR, which in effect define the enterprise altogether. In the course of what follows we will occasionally find Popper offering meta-rules that are more like defining than procedural rules. But before continuing it is good to reemphasise that Popper's book is anything but a systematic treatment. The rules themselves are not always fully formulated, and are often couched as subjunctives. Nevertheless, let us proceed to extract the rest of the rules and thus to build them into a set that we can examine as a whole.

In discussing the metaphysical difficulties involved in ideas about causality, and the dubious standing of principles of causality (such as every event has a cause, no effect without a cause) Popper proposes to cut through them with a rule. The rule does not dogmatise about cause, but instead rather enjoins us.

(R3) "[We] are not to abandon the search for universal laws and for a coherent theoretical system, nor ever give up our attempts to explain causally any kind of event we can describe" (*LScD*, p. 61).

The resemblance of this rule to (SR) should be apparent. The implication is that those who do give up the search for universal laws or cease their attempts to explain events we can describe are opting to leave the republic of science; those who merely wish to do so are wishing to leave science. The normative value of this rule is considerable: it sets an overall aim for science (the search for universal laws, a coherent theoretical system and causal explanations), an aim that ties the group together. Science can then be seen as a special form of voluntary society. No-one insists you dedicate yourself to the search for universal laws and causal explanations, but if that is your concern then science is probably what you are doing (depending on your rules of procedure), and if that is not your concern you can aid in the fight against confusion by not calling yourself a scientist.[6]

Proceeding through the book, the next time Popper brings up a methodological rule is when he is considering the problem of how to maintain the empirical character of an axiom system, to prevent it from becoming conventional. There is, he argues, no natural solution, only a methodological decision will do the job. Accordingly:

(R4) "I shall...adopt a rule not to use undefined concepts as if they were implicitly defined" (*LScD*, p. 75).

This rule is a corollary, no more, of (SR) which enjoined citizens of the republic not to permit the evasion of falsification. Abruptly using empirical concepts as though they were conventions is a technique of protecting a system from empirical difficulty which Popper named the "conventionalist twist". (R4) functions specifically to help us avoid unnoticingly giving matters a conventionalist twist when dealing with formalised areas of science.

Generalising, Popper argues that

> it is impossible to decide, by analysing its logical form, whether a system of statements is a conventional system of irrefutable implicit definitions, or whether it is a system which is empirical in my sense; that is, a refutable system...my criterion of demarcation cannot be applied immediately to a *system of statements*...*Only with reference to the method applied* to a theoretical system is it at all possible to ask whether we are dealing with a conventionalist or an empirical theory. The only way to avoid conventionalism is by taking a *decision*: the decision not to apply its methods (*LScD*, p. 82).

Implicitly here is a general rule, not formulated, but which might be put as, "avoid conventionalist stratagems". Instead of such a general formulation, Popper went at it piecemeal. At the end of § 19 of *The Logic of Scientific Discovery* he pinpointed four conventionalist stratagems, and for each of them he devised a methodological rule. The four are: the introduction of auxiliary hypotheses *ad hoc*; the modification of the ostensive definitions; the raising of doubts about the reliability of the experimenter [and/or his apparatus][7]; the raising of doubts about the theoretician. In one of his few remarks about the social sciences in this book Popper comments that, compared to the physicist, the sociologist and the psychologist need constantly to guard against the temptation of these stratagems, and he singles out psycho-analysts as particularly prone (*LScD*, p.82).

The four rules Popper devises to meet the specific challenge of these four stratagems are as follows:

(R5) "[O]nly those [auxiliary hypotheses] are acceptable whose introduction does not diminish the degree of falsifiability or testability of the system in question but, on the contrary, increases it" (*LScD*, p. 83).

(R6) "We shall forbid surreptitious alterations of usage" (*LScD*, p. 84.)

(R7) "Inter-subjectively testable experiments are either to be accepted, or to be rejected in the light of counterexperiments" (*LScD*, p. 84).

(R8) "The bare appeal to logical derivations to be discovered in future can be disregarded" (*LScD*, p. 84).

The wording of (R7) is not wholly satisfactory - read carelessly, it seems to say experiments can either be accepted or rejected. What is plainly intended is a presumption that inter-subjectively testable experimental work be accepted. It should only be rejected in the face of counter-experiment. Thus in the debates about Pons and Fleischman's work on "cold fusion" these rules would prescribe attention to their inter-subjectivity, i.e. their repeatability. And, indeed, the scientific community did in the first instance try to repeat their work. Reiterated lack of success, as well as counter-experiment (thought experiments included), was what permitted challenges to come forward to the experimenters, their apparatus and their theoretical deficiencies.

Plainly, close adherence to these rules would be fairly devastating on much of the social sciences, where verbal sleight-of-hand, the introduction of *ad hoc* excuses and *ad hominem* indictments of the (class or other "interests" of the) investigators are almost standard.

Moving on, we come to what I call (R9), which is anomalous in the present discussion because it is not in the original text of Popper's book of 1935 but in a starred footnote, the star signalling its introduction in the translation of 1959. My reason for not excluding it is that while not stated in 1935 it was implicit and actually employed, as a careful reader will plainly see. It is a very general rule that has application well beyond science. Indeed, it might be termed a general methodological rule for the conduct of critical inquiry.

(R9) "[A]fter having produced some criticism of a rival theory, we should always make a serious attempt to apply this criticism to our own theory" (*LScD*, p. 85n).

An immediate consequence of this rule is that it should apply to the present endeavour. My theory that Popper is offering the skeleton of a constitution for the republic of science must itself be subjected to vigorous criticism, which I will undertake once the expository section of this chapter is laid out. An example of Popper utilising this rule is to be found in his discussion of what he terms the asymmetry between verifiability and falsifiability. The obvious objection to the demand that scientific statements be verifiable by experience is that it is impossible to satisfy. Science consists of universal theories and universal theories make assertions about infinitely large classes of objects. There is neither time nor opportunity to examine all those objects and verify the scientific claim. Also, many scientific statements refer to invisible (quarks, black holes), long disappeared (big bang), or entirely abstract objects (relations) and it is utterly unclear how they are to be verified at all.

Under the belief that Popper was proposing a simple substitution of verifiability for falsifiability, many philosophers made the criticism that falsification was in principle no more final or conclusive than verifiability was. A falsifying instance (this swan is black) presupposed verification (it is black and always will be black) if it was to falsify "all swans are white".

But Popper anticipated this objection and showed it can be answered. At issue was the logical asymmetry between verification and falsification. A single contradictory falsifies, a single verifier does not verify, except in the trivial sense that it does not falsify.[8] Conclusiveness was a red herring as far as the logical situation was concerned. But Popper recognised that formal demarcation criteria like verifiability and falsifiability were inadequate. Falsifiability is a logical property, but falsification is not. A statement is *declared* falsified, it is a matter for a decision. The necessity of such a decision is what sponsors their articulation into a set of rules, a methodology.

Discussing the difference between falsifiability, which is the logical property we wish a system of statements to have if it is to encounter experience and thus be classed as science, and falsification, Popper stresses that a decision accepting a basic statement which contradicts a theory is a necessary but not a sufficient condition of falsification, because non-reproducible single occurrences are of no significance to science. This is a rule-like assertion, but it is not formulated as one. Although on p. 86 Popper says, "Thus a few stray basic statements contradicting a theory will hardly induce us to reject it as falsified", he does not get round to a rule for this situation until p. 106.

(R10) "[W]e should not accept *stray basic statements* - i.e. logically disconnected ones - but ... we should accept basic statements in the course of testing *theories*; or raising searching questions about these theories, to be answered by the acceptance of basic statements" (*LScD*, p. 106).

A science, Popper holds, needs a point of view and theoretical problems. It is only in the context of investigations into those matters that interesting as opposed to stray basic statements are developed around reproducible effects.

Four more rules can be isolated from the text of Popper's book. They involve a reiteration, the exclusion of accidents, the specification of random samples and the equivalent of materialism. They have much to do with what sorts of theories and what sorts of estimates of theories are welcome in science. On p. 121 Popper brings in a rule not previously formulated about empirical content and shows its equivalence to another about severe tests. Arguing that he regards the comparison of the empirical content of two statements as equivalent to the comparison of their degrees of falsifiability:

(R11) "This makes our methodological rule that those theories should be given preference which can be most severely tested...equivalent to a rule favouring theories with the highest possible empirical content" (*LScD*, p. 121).

The dots replace a cross-reference to the place where rules (R5)- (R8) are formulated, (R5) clearly implying that severity of tests is a positive value.

Again as a corollary, this time to (R1), Popper wants to estop one of the commonest evasion devices in common practice, the appeal to accident:

(R12) "I propose that we take *the methodological decision never to explain physical effects, i.e. reproducible regularities, as accumulations of accidents*" (*LScD*, p. 199).

This rule is introduced during a technical discussion of probability in physics, where the issue is how to prevent probabilistic hypotheses from rendering the system of statements unfalsifiable. Yet it has broad implications, suggesting that in science we should refuse to be satisfied with explanations from accumulation of accidents. This sharply distinguishes science from technology, where an air crash, for example, will as a rule be explained by an accumulation of accidents.

In the same context of discussing the falsifiability of probability statements, a further rule is considered necessary:

(R13) "a rule...which might demand that the agreement between basic statements and the probability estimate should conform to some minimum standard. Thus the rule might draw some arbitrary line and decree that only reasonably representative segments (or reasonably 'fair samples') are 'permitted', while a-typical or non-representative segments are 'forbidden'" (*LScD*, p. 204).

A final example from this context of probability statements and their confinement is:

(R14) "[T]he rule that we should see whether we can simplify or generalise or unify our theories by employing explanatory hypotheses of the type mentioned (that is to say, hypotheses explaining observable effects as summations or integrations of micro events)" (*LScD*, p. 207).

Here the challenge was the claim that all observable events should be explained by micro events. Noting that this doctrine is similar to certain forms of materialism, Popper calls that a "metaphysical hypostatization of a methodological rule which is in itself quite unobjectionable".

4. Assessment

What then is to be made of this list of 15 rules ((SR) + (R1)-(R14))? First and foremost to reiterate: it is incomplete. A Constitution for the Republic of Science would need many more rules, and some specification of their institutional embodiment, including rules for dispute settlement. Popper makes no effort to organise the rules systematically and lay them out in a table so that they can be checked against one another and debated in relation to one another and the aims. This may explain why they are an aspect of his philosophy that is seldom discussed. The occasional challenge to one or another rule is seldom framed in a manner that suggests appreciation of the innovative brilliance of the idea. What seems to be at work is that both Popper and his critics suffer from a blindness to the institutional turn. Let us re-frame the rules by spelling this out.

What Popper's rules amount to is this. The demarcation between science and non-science cannot be stated in an abstract way, only in a practical way. Science is an activity carried on in a select community and this community is obedient to a set of rules which guide its activities. The community is both real and partial. Real because it consists of actual human beings, partial because their participation in this community is only one of the roles they play as social actors, and by and large their other roles are played in distinct, but overlapping communities. All scientists are, for example, citizens of nation states; virtually all scientists belong to families of orientation (their natal family), and the majority also to families of procreation. Most scientists belong to communities and groups in civil society (political, religious, voluntary, recreational). The vast majority of scientists belong to some one or other large institution that provides them with a livelihood: government departments, universities, laboratories, museums, business corporations, etc.

In none of these "outside" involvements is it necessarily the case that they conduct themselves by the same set of rules of procedure that Popper has been sketching out. Notoriously, there are critical and open-minded scientists who cleave to dogmatic religious beliefs and raise their children with a much stronger expectation they will also cleave to the religion than that they might in turn become scientists. The social institution of science and its constitutive rules are, then, a special niche in the overall social life of its members.

What kind of a social institution is it? Popper nowhere says anything about its internal organisation. The rules give us no guidance to the manner in which it is governed, or to whether there is established leadership. Indeed, although many rules are put forward for discussion, the issues of how these discussion are to be conducted and how decisions about amending the rules are to be made are not entered. Thus it is a stretch to see these rules as a Constitution for the Republic of Science or even as a proto-constitution. They are more like a proposed set of procedural rules for discussion by a body already in place.

Now of course science was at the time Popper wrote well and truly established in its place in society. It was in many ways unique among social institutions. Its acitivities tended to centre around the subinstitutions of the learned society, associated journals, universities, laboratories, and international conferences. Physics was understood by physicists to be a far-flung invisible college uniting colleagues from around the globe in common endeavours. But to point this out is also immediately to point out that there was a leadership. To be the senior professor at certain institutions, or the head of a learned society, was a concrete kind of leadership for which there was fierce competition. There was also intellectual leadership, independent of such posts, which turned solely on the esteem directed at the work of a particular person, regardless of formal post. And, without a doubt, there were some elements of charismatic leadership - scientists who could energise others with a vision, regardless of whether their own work was important.

Concrete leadership resided mostly in prestige institutions that themselves had constitutional means of selecting those position-holders. Election to professorships, elections in learned societies, the selection of journal editors, were all conducted by sets of rules bearing no relation to Popper's methodological or procedural rules. It is not at all clear that the weight of these offices and rules was brought to bear to move science in the direction Popper intended (especially if Kuhn is to be believed). Institutions and office-holders develop vested interests, some of an intellectual character. Thus, for example, the injunction to prefer the most severely tested theory, or always to apply a new criticism of the theory one opposes to one's own theory might not at all conform to institutional pressure and so be ignored.

Because his set of methodological rules is free-floating, not attached to any institutional framework, the task of defending them, and of debating them in a responsible way, is left up in the air. Popper has not provided a constitution for science, not even a declaration or charter of rights (and responsibilities) which the constitution is trying to embody in a system of governance. Perhaps that is as well. For the methodological rules Popper proposed are not intended for this or that university, this or that physics laboratory, nor are they intended for some kind of United Nations Organisation of physics. They are directed to science conceived of as a general and abstract republic. They enjoin: here, if you want to respect and advance these aims, is a set of proposed procedures. But "you" is unspecified, and what "you" are to do if you have what you think is a better idea is also unspecified. These proposals being published by a philosopher of science you thereby elect a kind of specialism of that name to debate these proposals. Yet since they are proposals to guide the practice of research in its most general aspect their testing and debate might seem to be most appropriately carried out by the community of scientists proper.

Another way to think of science is not as a series of concrete institutions but as an invisible college, an abstract institution rather like language. We might see Popper's rules as addressed to this wider community of science, one that has to do with self-identification and not with institutional gatekeepers. Popper's arguments and rules are then seen as directed to men and women of good will who want to advance the project of science. This could explain why individual scientists, in their autonomy, often pledge allegiance to Popper's vision of science, while the institutions to which they belong seldom do so. And parasitic institutions like those of the philosophy of science are even harder pressed to find any role for appreciative incorporation of Popper's ideas. Popper's lack of attention to the concrete institutional embodiments of science, and his efforts to formulate rules outside such constraints, betrays, I believe, a fundamental mistrust of existing institutions and practices. They were and are hierarchical and authoritarian and no longer welcoming to the man or woman of good will, as well as being subject to the corruptions of power and wealth. Later in his career, commenting on Thomas Kuhn's notion of "normal science", i.e. its concrete institutional embodiment, Popper
declared that:

'Normal' science, in Kuhn's sense, exists. It is the activity of the non-revolutionary, or more precisely, the not-too-

critical professional: of the science student who accepts the ruling dogma of the day; who does not wish to challenge it; and who accepts a revolutionary theory only if almost everybody else is ready to accept it -- if it becomes fashionable by a kind of bandwagon effect. To resist a new fashion needs perhaps as much courage as was needed to bring it about...
I admit that this kind of attitude exists; and it exists not only among engineers, but among people trained as scientists. I can only say that I see a very great danger in it and in the possibility of its becoming normal (just as I see a great danger in the increase of specialisation, which also is an undeniable historical fact): a danger to science and, indeed, to our civilization (Popper 1968, pp. 52-53).

In utilising this quotation I am leaping well out of my self-imposed restriction of period. But nothing in my present line of argument relies on this quotation. It simply is an explicit affirmation of what I think we will find, when we discuss Popper's ideas on society, history and politics, in the classical period that is my main concern: that his philosophy of science portended a radical critique of concrete institutions and a desire to hold them to normative standards that men and women of good will could agree upon, not those laid down by the entrenched experts.

1. According to the account that appears in *C&R*, p. 34 (a chapter first published in 1957) and elaborated in the autobiography of 1974/76.
2. The use of this term for the present purpose is first found in W. W. Bartley, III's *The Retreat to Commitment*, New York:Knopf 1962.
3. Cite *New Atlantis*.
4. Bacon identified four 'idols' as common dangers.
5. The second half of Virgil's line "Felix qui potuit rerum cognoscere causas" (*Geogics*, II, l. 490) is the motto of the London School of Economics, where Popper spent the latter half of his academic life.
6. The aim of science built into (R3) is not quite the same as that outlined in 'The Aim of Science' (1957) - 'satisfactory explanation'. Whether the difference is significant or represents a strengthening and deepening of it is an exegetical point I leave to one side. Agassi claims the 1957 paper portends a new philosophy of science.
7. The phrase in parentheses is mine but is clearly implicit in the text.
8. As to the objection that every falsifying basic statement verifies the contradictory of the statement being tested (a basic statement about a black swan could contradict "all swans are white" and verify "Some swans are not white"). this involves the same equivocation on the word 'verify'. Two contradictory statements cannot be true together. Any statement that does not contradict another can be said to verify it, even if it is false. The positivist demand for verification involved a stronger sense of 'verification' as a demand that statements be 'verified as true or probable', which required lots of verifying statements. A single one was of no interest. *Pace* Kuhn, a single falsifying statement is of great interest.

* The text of this talk is taken from a book in progress. It was prepared for this meeting of the Japan Popper Society during a visit to Japan and, as a result, some of the references are incomplete.

The Rules of the Game: Comments on Professor Jarvie's Chapter

Joseph Agassi
Tel-Aviv University and York University, Toronto

Professor Jarvie always manages to open a simple project that to begin with seems quite proper but not too exciting and to turn it at once into an exciting and thought-provoking study. This time he began by stringing together the different passages in Karl Popper's classical book on method, so as to view it as a sociology of science proper. The result is most intriguing. The exercise is presented as a study of Popper's break away from tradition, his replacement of methodology-as-psychological with methodology-as-sociological. This is expressed by the shift of questions from the traditional, how do I learn/know? to, how do WE learn/know? Popper's shift was shared by Michael Polanyi, with less challenging results, however, as in the name of the scientific tradition Polanyi replaced the traditional republic of science with the scientific community and appealed to authority. Worse replacements followed. One famous philosopher, Larry Laudan by name, has claimed that the "real" question is, why should I believe my colleague the physicist when he tells me who is the leading physicist today? This is obscurantism.

Professor Jarvie approaches the social component of science in accord with his study of rationality. He and I rebelled against the division of rationality into rational thought and rational action. Instead, we took thought to be in part opinions, as parts of the conditions in which rational action occurs, and at times as thinking, which is rational action. (This move requires the admission of degrees of rationality, but I will not discuss this now.) Rather, let me show how all this can help solve an old problem. It is, does methodology prescribe or describe? Authors may say whether they describe or prescribe. This does not help, since matters are complex: prescriptions are of the possible, and since the procedures to be described are in fact prescribed in the republic of science. How then should we go about this problem?

Professor Jarvie's solution is quite unexpected: method emerges from the mere fact that scientific research is social and rational (it is a partly institutionalized rational action). For two reasons the problem is particularly fascinating for students of Popper's views. First, he demanded least and promised least: he promises no success and saw success in the development and testing of (possibly true) explanations. He observed that the game of science is fruitful, and, not wishing to guarantee that it will remain so, he did not try to explain this fruitfulness; as long as it is found fruitful, he said, people will play it. The second reason is more intriguing: Popper said, the rules of logic should suffice for scientific research. In what sense can this be true? in the sense of the logic of the situation. This makes Professor Jarvie's study exciting.

Is methodology descriptive or prescriptive, then? Professor Jarvie says, the logic of the situation of research mediates between the two options; it also allows for some explanation of the divergence between the institutionally prescribed and the institutionally practiced.

Popper's theory is then both prescriptive and descriptive of the better practices. Also, the logic that Popper said suffices for science is the logic of the situation, not logic in the narrow sense of the word. Any activity obeys some rules, and they are known as the logic of that activity. Similarly one speaks of the rules of the game, where the game is the institutionalized activity in question. Methodology is such a game. What are its rules and how binding are they and how much of the procedure do they prescribe?

This general question has no general answer, though for special cases special answers are available. Take rituals; in some systems ritual is more stringent than in others. In most religions there is a standard formula for prayers and even for the choice of which prayer to perform on which occasion -- sometimes details may be added to fit the occasion and sometimes private improvised prayers are allowed or encouraged. In the arts the rules are more liberal, especially in the West. But everywhere rules limit variation, and at times they are broken. In novel writing permitting the choice of commoners for heroes was revolutionary. Traditionally, some areas were closed even to research, such as theology and sexology, perhaps even the sociology of science. This, to the best of my knowledge, is ignored in the republic of science, especially by methodologists. I mention this not because I think that it is significant, I really do not know, but because as an anthropologist Professor Jarvie takes it for granted that the rules described and the rules followed need not be the same. Researchers want such variances explained and reformers want them changed, and he is both.

This is contrary to the latest fashion in the sociology of science, which is fashionable because it repeatedly presents researchers as having no intention to follow the canons of proper conduct, that to the extent that they follow the rules, they do so only out of fear of discredit. In other words, it presents as the chief interest of researchers not the satisfaction of curiosity but the sale of their wares. Such people do exist; they are pretenders, though. Pretenders appear in different field, and their conduct is of little interest. Professor Jarvie's point is much more interesting: his interest is in the variance between rules described and rules followed by honest researchers. What are the rules regarding research and how closely are they followed by the better and more honest researchers and why? Honest discussion of this may reduce pretence such as that of the fashionable sociologists of science.

Rules of research first appeared around 1600, during the scientific revolution. Galileo said then, researchers must know mathematics, as the Book of Nature is written in mathematical symbols. Bacon said then, researchers must believe only their own eyes. Today these rules sound strange, because of the great influence of William Whewell of the mid-nineteenth century. He said, researchers develop explanatory ideas, test them empirically, and accept the results of the tests: refutations lead to their rejection and confirmations render them scientific. He viewed mathematics as essential for science for reasons different from those of Galileo. Popper closed this chapter when he said, mathematics helps furnish testable theories. Bacon's rules are still popular though Whewell proved them impossible when taken strictly and useless otherwise.

Whewell and Popper differed significantly: the former recommended to stop testing confirmed theories and the latter recommended the opposite. Both recommendations are problematic, since there is no rule as to when to stop tests, yet they must be stopped to avoid stagnation. Also Whewell recommended and Popper rejected Bacon's rule of "the ladder of Axioms": Bacon assumed the existence of degrees of abstractness of theories, and he demanded that moves be made from one degree to the next, without skipping a step. Popper disagreed: the more abstract is more testable, he said, and he recommended to go for the highest degree of testability. For my part I am wary of all this: I do not know what the steps in the ladder of axioms are; I do not know if and how degrees of abstraction can be assessed. The same goes for degrees of testability. All we know is, in any valid inference the conclusion is not more testable than the premise. Does this holds for abstraction too?

As I moved from Whewell to Popper, I skipped the conventionalist school of Duhem and Poincare. They suggested completely new rules: do not consider any theory as final, but no scientific hypothesis is ever to be removed, even when a more sophisticated alternative to it is available, since for some purpose the less sophisticated one is the better means, namely, the simpler. Also, when devising an alternative to a theory, deviate from it minimally. I think the notions of small deviation and of simplicity are mere metaphors here.

This is the literature on the rules of the game. I did not mention Polanyi and Kuhn, as they say, reject all explicit rules; worse still, they say, the scientific tradition recommends the appointment of scientific leaders to prescribe scientific public opinion. This is true to some extent, especially after World War II, but it is not science, and I have no good word for it. Perhaps we better not speak of science at all but of the search for the truth instead. This will at once render conventionalism a non-option, as the chief aim that its fans ascribe to science is practical, not theoretical.

The overturning of Newton's mechanics refuted the views of Bacon and of Whewell. The great gap between it and its heirs refuted conventionalism too. Some conventionalist historians, notably Whittaker, have declared the gap small. This is incredible. The situation then looks like a blind alley. In response great historian of science A.N. Meldrum suggested to replace the logic of science with a psychology of science. Kuhn endorsed this, even though it clashes with Polanyi's sociologism that he endorses too. Popper stuck to his sociologism and presented methodology as a part of logic. Even were he utterly mistaken, this would make him one of the greatest. Philosophers who ignore him, for good reasons or bad, are plainly unphilosophical and irresponsible.

There are all sorts of games, and they have their rules, their logics; to abide by logic, to be logical proper, the moves allowed and/or prescribed by any game must be allowed by logic. It is very important to notice that these are very limited to two items. First, contradictions must be deemed false. Hence their negations must be deemed true. This follows from the basic demand of logic: in order to be valid, an inference must comply with the rules of transmission of truth. This rule is very easy to follow as it does not hold for surmise: when an inference is found invalid, it may be still upheld, but as a mere surmise. Alternatively, it may be rescued by the claim that some premises were left unstated but understood in context.

Consider the game of the axiomatization of a given system, then. It allows neither surmise nor the omission of premise: it demands of all axioms to be stated explicitly, without omission or repetition, and without contradiction, and to entail all the theorems of the system in strictly valid inferences. The logic of axiomatization is more strict than logic proper, but is not in any way opposed to it, of course. Is the logic of science of the same ilk?

Logic permits inductive inferences on the condition that they are viewed as mere surmises. This was stressed by Bertrand Russell, for example. He wanted more: he looked for some further rules to give some inductive inferences the status of more than mere surmise. This extra bit required by Russell, said Popper, is neither possible nor necessary. Suffice it to require, he said, that surmises should be put to empirical test. Can all surmises be tested? No. Only the more promising ones should. What do tests tell? Logic says that successful tests attest that the surmise tested are false and unsuccessful test attest that if the surmise is an error this test does not evince this (though another test might). Russell said, this is not enough. Popper said, it is enough for research. What should researchers do when an error is found? Popper said, admit the reports of the empirical information as true so that thereby the surmise is declared false so that dogmatism is avoided. Now clearly dogmatism does often clash with the aim of the search for the truth, and then -- but only then -- it is not allowed. Is the requirement to endorse information a necessary and sufficient condition for the search for the truth? The methodical study of the logic of the situation invites the study of this question.

The reason Russell was dissatisfied with Popper's suggestion is that the two were not sufficiently clear about the logic of the situation. Russell considered the aim of science both the search for the truth and the guidance for scientific technology. Popper did not discuss guidance; his social and political philosophy allows for such a discussion, however. This too is required by the logic of research: research as an activity may be motivated by different kinds of aims. So there is no need to insist that science is the search for the truth; what Popper presented as the logic of science may be taken to be a part of it, namely, the logic of the search for the truth. What conventionalism-instrumentalism presented as the logic of science is likewise incomplete, and may be taken as the logic of the search for utility, i.e., the logic of technology. This will show that conventionalism-instrumentalism is in error even about utility, since, as Bacon said, the search for the truth may be more useful than the search for usefulness. Bacon's claim is not always true: engineers may find it more useful to center on technology rather than wait for some handy scientific progress; socially, however, Bacon's claim is admitted by all. So in a sense even Bacon, the father of modern methodology and the father of the psychologistic trend in it, he too had in mind research as social not as psychological. Except that he was not so clear about matters as we ought to be.

Memorial Celebration
Professor Sir Karl Popper

Monday December 12 1994

About 150 invited guests attended the Memorial Celebration of the life & work of Karl Popper in the Founders' Room at the London School of Economics & Political Science [LSE]. Among those present were past students, colleagues, & friends of Sir Karl's from France, Germany, Denmark, Austria, Italy, Spain, Greece, the Czech Republic, Hungary, the USA, & Hong Kong. The meeting was not publicly advertised.

Sir Karl was born on July 28 1902, and died on September 17 1994 at the age of 92, following a brief illness. After his stay in New Zealand from 1937-1945, he had spent his entire academic career in Britain at the LSE, where he was Professor of Logic & Scientific Method from 1949 to 1969.

Dr John Ashworth, the Director of the LSE, welcomed the guests. The audience heard spoken tributes from Sir Hermann Bondi FRS, Mr Bryan Magee, Professor John Watkins, Professor Dr Günter Wächtershäuser, Mr David Miller, Professor Julien Musafia, and Professor Pedro Schwartz OBE. Musical tributes were paid by Sir Claus

Moser & Mr Gordon Kirkwood, who played the slow movement from Mozart's piano sonata K448, and by the cellist Julius Berger and pianist Julien Musafia, who played a number of pieces, culminating in the Catalan *Song of the Birds* [*Cant de ocells*], a favourite of Casals's, & the last piece of live music that Sir Karl heard. There was also a piano performance by Mr Musafia and Lory Wallfisch of Sir Karl's youthful fugue in F♯ for organ, here transcribed for four hands. (This fugue was first played in public on the organ in St George's Chapel, Windsor in 1991 by Gillian Weir.)

BONDI spoke on **Sir Karl's impact on the science community**, and stressed how important Popper's thesis of the power of imaginative thought in science, promulgated especially by Peter Medawar, had been for the proper understanding amongst scientists of how science works. MAGEE recorded his pleasure & astonishment that, on the day after Popper's death, three of the four serious Sunday newspapers in England [*The Independent on Sunday*, *The Sunday Telegraph*, *The Sunday Times*], had described him as the greatest philosopher of the century; for although he had long believed this himself, he had usually found his opinion questioned by professional philosophers, if not ridiculed. Magee expressed the hope encapsulated in his title, **Sir Karl Popper – a philosopher for tomorrow**, that

Popper's philosophy would eventually be received with the seriousness & respect it deserved. WATKINS, who had been Popper's colleague at the LSE for many years, recounted in **Sir Karl Popper at LSE 1946-1969** some stories of his time there, especially of his famous seminar.

One of Sir Karl's profoundest preoccupations in the last years of his life was the new surface-metabolism theory of the origin of life propounded by the German patent attorney Günter Wächtershäuser. In his moving talk on **Sir Karl Popper, mentor of science – a personal view** WÄCHTERS HÄUSER reported how the inductivist attitude pervading chemistry had discouraged him from further scientific work after his PhD, turning him to the law. Problems in the objective sense play an important role in German patent law, and a search for some discussion of their status led him to his first meeting with Popper, on a bookshelf, later to a real meeting at Alpbach, & then to a reunion with real science. Popper's enthusiastic support at every level in the development of his new theory was vividly & appreciatively described.

In his talk entitled **Sir Karl Popper on Logic & Scientific Method** MILLER interspersed personal reminiscences of his time as Popper's research assistant in the 1960s, & of his later collaboration with Popper on various problems of probability theory, with a defence of the fundamental doctrine of critical rationalism that *logic is the organon of criticism, not of proof.* MUSAFIA (**Sir Karl Popper & music**) spoke of Popper's intellectual love for music and introduced the fugue that he had written in his early twenties. Finally SCHWARTZ spoke on **Sir Karl Popper - the man**, telling of the warmth & generosity of the lovable person behind the sometimes combative & forbidding philosopher.

A booklet containing texts of these speeches is to be published at a later date by the LSE. Plans were announced also for an annual or biennial lectureship on Sir Karl's philosophical ideas, to be instituted at the School with the support of the newly created SIR KARL POPPER MEMORIAL FUND. Details of the Fund may be obtained from The LSE Foundation, Room H810, PO Box 3, LONDON WC2A 2AL.

Department of Philosophy
University of Warwick
COVENTRY CV4 7AL
e-mail: d.w.miller@csv.warwick.ac.uk

ポパー哲学と教育の思想

青木　英実

I

カール・ポパーの哲学が教育の思想や理論、教育実践に与える示唆については、わが国では必ずしも十分に検討されて来ていない。周知の如くポパーは、教員志望の学生としてその知的歩みを始めたのであった。「とてつもなく退屈」で「驚くべき時間の空費」をしている中等学校から＜落ちこぼれた＞ポパーの学生時代の夢は「若者が退屈せずに学べ、問題を提起し討論するよう励まされ、問いかけもしない問題に対する欲しもしない答えを聞かされなくてもよい学校、試験に通るために勉強するのではない学校」をつくることであった[1]。

ポパー思想と、ポパーが知的活動を始めるうえで重要な環境を与えたオーストリアの教育改革運動との関連性については、すでにW．W．バートリーのまとまった先行研究がある[2]。またJ．A．アルトは、ポパーの心理学的・教育学的初期著作における諸テーゼがポパー哲学の認識論に関連性を持つことをかなり詳細に跡付けている[3]。

オーストリアのみならず、十九世紀末から二十世紀初頭にかけてヨーロッパ各国、アメリカ、やや遅れて日本において、一般に「新教育」（アメリカでは「進歩主義」、日本では「大正自由教育」、ドイツでは「改革教育学」とも）と総称される教育改革の理論と運動が展開される。

これらの理論と運動が、－国や、主唱者によつて、さまざまなニュアンスをともなつてはいるが－共通に掲げたスローガンのひとつは、児童の自発性、児童生徒の興味と関心を重視する＜自己活動＞（Selbst--tätigkcit）であった。バートリーも指摘しているように、今世紀初頭の（O．グレッケルの率いる社民党政権下）オーストリア、ウィーンはこのような教育改革運動の一つの中心、メッカでもあったのだ[4]。

「新教育」が教育の方法原理上対抗し、また対抗せざるえなかつた当時の支配的理論が、ヘルバルトおよびヘルバルト主義の教授学である。ヘルバルト主義の心理学はよく知られているように人間の精神世界を要素的な「表象」からなるものととらえる。要素的な「表象」の連合によって体系的な知識を習得させようとする「実質陶冶」の思想がその基本にあった。普通義務制を根幹とする組織的かつ系統的な近代学校教育は十九世紀に形成されてくるが、この近代学校教育において大量に必要とされた教師の教育方法として、要素主義的な心理学に立ち、それゆえ機械的かつ段階的に組織化され、形式化されたヘルバルト主義の教授学はきわめて有効に思われたのである。ヘルバルト主義は、衰え始めたペスタロッチ主義にかわって米・欧・日の学校を席捲することとなる。（ヘルバルト主義の残滓は、今なお気づかれないままに、たとえば初等中等教育での授業の指導案における＜導入・展開・整理＞といった段階構成として見出される。）

II

ヘルバルト的な要素主義心理学に対してウィーン教育研究所でポパーが学んだK．ビューラーは、このような要素主義、連合心理学を批判しており、自伝にあるようにポパーもまた連合主義の誤りに明確に気づくこととなった[5]。連合主義、要素主義は機械的な、あるいは暗記中心の＜教え込み型＞教育と結びつかざるを得ない。一方、ポパーの認識発達の理論、学習の理論は次のように要約できよう。

つまり、我々は「行為と選択によって経験から学ぶ」[6]。そして経験による学習とは「我々の期待や理論そして我々の行為プログラムを修正することからなる。それは修正と選択の過程であり、とりわけ我々の期待の反駁によるものである。有機体はこの見方によれば、それがアクティブで目的あるいは選好を持ち、期待を作り出すかぎりでのみ、経験から学ぶことができる」[7]。

このような認識発達の、また学習の理論は、「新教育」の活動主義的な教育論と密接な内的関連性があると見ることができよう。しかるに、晩年のポパーは、自己の思想形成とも大きな関わりのあった、教育改革の「自由を求める理想主義的運動」がイデオロギーと化し、学力水準、礼儀や自己規律の低下をもたらしたと考えているようである[8]。ポパーはこの原因を「自由と民主主義」に対する誤解にあると見ている。

教育論的に、そしてポパー思想の立場に即していえば、自由を求める教育改革の思想は、教師や大人や学校や国家の権威にかえて、「子ども」を権威にまつり上げ、教育活動を方向づける一切の認識の源泉とみ

なしたにすぎなかった。「子ども」を権威にまつり上げれば、子どもの「代理人」を自称する者が集団的な圧力を行使することも可能になる。パーキンスンがいうように、これは権威主義的思考、あるいは「正当化主義」（justificationism）という点ではいささかも変わらない[9]。

ポパーは、学習の理論を進化のプロセスに結びつける中で、膨大で複雑な構造的達成物を維持し、守り、模倣を含んで直接的に伝達する＜教え＞（instruction ）と、創造的・変革的な過程としての＜選択＞（selection ）の二つの過程を挙げている。有機体の進化でも、知的進歩でも、この二つの、つまり「保守的な」傾向性と「変革的な」傾向性とが、相互交渉し、協同している、と[10]。「こども」をまつり上げることで、教育における「保守的」な力が失われたこと、これが欧米の教育システムが「破壊」された一因であろう。だとすれば、我々にとって教育システムにおけるこの二つの傾向性を、いかにco-operateさせるかが、ポパーの日本人への「遺言」に応える道だといえよう。

＜注＞

(1)K. R. Popper, *Autobiography*, in *The Philosophy of Karl Popper*, ed. by P. A. Schilpp, Open Court, 1974. p. 31.

(2)W. W. Bartley, Die östereichische Scul--reform als die Wiege der moderne Philosophie, in Club Voltaire; Jahrbuch für kritische Aufklärug Ⅳ, hrsg. von G. Szczesny, Rowohlt, 1970.（原文はTheor*y of Language and Philosophy of Science as Instruments of Educational Reform ;Wittgenstein and Popper as Austrian Schoolteacher, in Methodological and Historical Essays in the Natural and Social Sciences, ed by R.*S. Cohen and M. W. Warofsky, Reidel, 1974.）

(3)J. A. Alt, *Die Früschriften Poppers-Der Weg Poppers von der Pädagogik und der Psychology zur Spätphilosophie, Peter Lang 198*4.

(4)W. W. Bartley. ibid., S. 354.

(5)K. R. Popper, ibid., p. 60.

(6)K. R. Popper/J. C. Eccles, *The Self and Its Brain*, Springer, 1977, p. 132.

(7)K. R. Popper/J. C. Eccles, ibid., p. 132.

(8)カール・ポパー（長尾龍一訳）、「日本から学ぶもの」、長尾龍一・河上倫逸編、『開かれた社会の哲学ーカール・ポパーと現代』、未来社、53頁。

(9)H. J. Perkinson, *Against Learning*, in *H.* J. Perkinson, R. M. Swartz, S. G. Edgerton, Know*ledge and Fallibilism, New* York University Press, 1980, p. 29.

(10)K. R. Popper/J. C. Eccles, ibid., p. 134.

ポパーと熱狂的演繹主義　（Ⅱ）
Popper and the Deductive Chauvinism

熊谷　陽一（信州大学）

５．科学的説明の問題

科学的説明に関する哲学的分析は、ヘンペルの理論の登場以来、現代科学哲学の中心的活動の一つとなっている。説明をめぐって、どのようなことが問題となるのか。この点について少し述べることから、後半部を始めたい（＊）。

われわれは、自分たちの世界で起こる様々な事象について、「なぜ」という疑問を発して、それに対する答えを求めるときがある。なぜといった問いへの答えは、通常、説明と呼ばれる。しかし、説明は種々ある。一つの事象には、宗教的説明が与えられることもあれば、科学的説明が与えられることもあり、常識による説明が与えられることもある。では、いかなる条件を満たすとき、説明は科学的なものとなって、それ以外のものから区別されるのか。この点を解明するというのが、科学的説明の理論の主要な課題である。この課題に対してはこれまで、科学的説明の一般的パターンを示すモデルを与えるという仕方で哲学的分析がなされる場合が多かった。たとえば、ヘンペルのＤＮモデルとＩＳモデル、初期のサモンのＳＲモデルが、そうである。

科学的説明の問題をおもに扱っている現代の科学哲学者の一人であるサモンによると、科学的説明の理論の歴史を振り返るとき、大きく分けて三つの考え方が主張されてきたという。その第一は、認識的な（epistemic）考え方である。これは、論証や、疑問と解答、情報の付与といった認識に関係する概念によって、説明を分析しようとする立場である。第二は、様相的な（modal）考え方であって、偶然に起こったようにみえる事象が実は必然的であったことを示すというのが、説明の目的であるとする立場である。第三は、存在的な（ontic）考え方である。これは、或る事象の生起の説明が果たされるのは、実在の側で成立している構造（一般的パターン）にその事象が適合していることを示すことによってであるとする立場である。これら三つの考え方は、古典力学の場合にみられるような決定論的世界観の下では、実質的な区別がつかない。しかし、物理的世界が統計的確率論的法則に支配された非決定論的な質のものだとすると、三つの考え方には、大きな相違が生じ、存在的な考え方以外のものは維持できない。こうサモンは主張する（Salmon [1984] chap.4）。

まず、様相的な考え方に困難が伴うことは容易に理解される。非決定論的世界では、まったく同一の条件でも、一定の事象が生起したりしなかったりするわけであるから、事象の生起の必然性を語ることの意味が判然としないのである。

次に、認識的な考え方には、説明関連の問題が生じ、その結果、認識的相対性を主張するはめに陥る場合がある。たとえば、論証を説明のための必要十分条件とみなすヘンペル理論は、〈帰納的統計的（ＩＳ）モデル〉を提出することによって、非決定論的世界における個別的事象の生起について科学的説明が可能であることを示したかのように思えるが知識状況に依存する説明を容認するに至ってしまっている。

ヘンペルのいうＩＳモデルとは、個別的事象の生起を統計的法則に包摂する論証によって構成されているものをいう。ただし、演繹的論証のもつような確実性ではなくて、高い帰納的確率でもって被説明項を帰納的に導出する論証が考えられている。ＩＳモデルの事例としてヘンペルが挙げるものの一つは、連鎖状球菌症を患った（Ｓ）ジョーンズ（ａ）がペニシリン投与をうけて（Ｐ）回復した（Ｒ）という場合である。この事例では、統計的言明　「p（Ｒ／Ｓ＆Ｐ）　＝　ｒ　（ｒは１に近い確率値）」が説明項の一要素をなす。この事例は、ＩＳモデルにつきまとう一つの困

難を示すのに用いることができる。いま次のようなことが起こっていたとしよう。すなわち、ある種の連鎖状球菌はペニシリンに対する耐性をもっている(J)。 そして、ジョーンズの病気がこのペニシリン耐性変種の菌によるものであった。こうだとしてみよう。このとき、ＩＳモデルに従うなら、説明を構成するのは、事実とは違うことを言明する被説明項（～Ｒａ）をもつような帰納的論証であって、ジョーンズの病気からの回復は説明されないことになってしまう。というのは、ペニシリン耐性変種の菌による発病はそうでない菌による発病と較べれば希なことだからである（Hempel［1965］訳書78頁）。

この事例は、ヘンペルがＩＳモデルの〈曖昧性(ambiguity)〉と呼んだ困難を例証している。この困難は、コッファが指摘したように、母集合の選択から来ている。前者の事例では、母集合として、連鎖状球菌症を患っており（Ｓ）、かつペニシリン投与をうけている（Ｐ）ものという集合が選択されている。そして、後者の例では、母集合として、ペニシリン耐性変種の菌による（Ｊ）というさらなる分割を受けた集合が選択されている。そして、ここには、どちらの母集合を説明に関連するものとみなしうるか、という問題が生じているのである（Coffa［1974］）。

サモンによれば、ＩＳモデルなるものは、不充分な情報の下での不完全な演繹的法則論的（ＤＮ）説明でしかない。そして、演繹的法則論的説明とは、説明項が与えられたときに被説明項が生起することが決定されていることを示すことに他ならない。したがって、ヘンペルの説明理論は結局のところ、決定論的世界観を前提としているとみなさざるをえない（Salmon［1984］p.53）。

サモンは、ポパーが科学的説明の理論をもっているとしたら、それは、ヘンペル理論に同じく認識的な考え方にたつものであり、しかも、ポパーが帰納的論証を拒否する演繹主義を守る限りは、演繹的法則論的説明の理論にとどまる、とみなす。そして、サモンは、こうした立場を、説明に関する熱狂的演繹主義と名づけ、非決定論的世界観の下では擁護できないものだと主張したのである（本稿前半部（Ｉ）参照）。

確かに、サモンの理解に賛成したくなるような発言がポパーの著述には見受けられる。たとえば、『発見の論理』第３章「理論」の第12節とか、『客観的知識』第５章「科学の目的」の冒頭部がそうである。しかし『果てしなき探求』「形而上学的研究プログラム」の次の箇所を読むとき、科学的説明に関するポパーの考えは、それほど単純なものではないと推測される。

━私は［『発見の論理』で］自分が実在論者であることを告白したが、これは信念の告白にすぎないと考えていた。それゆえ、私は自分の実在論的議論について、この議論は「われわれの世界には規則性が存在するという形而上学的信念……を表明しているものである」と書いた。 ……『補遺』の最終章で私は……非決定論、実在論、客観主義について論じ、これら三つの形而上学的理論が互いに共存できることを論証しようとし、その共存可能性を一種のモデルによって示すために、……性向およびとりわけ傾向性の実在性の推測をもちだした（Popper［1976］訳書215頁）。

この発言に筆者が注目するのは、ここには確率を傾向（propensity）と捉えることが含意されおり、このことは、サモンのいう存在的な考え方を採用することであり、説明を知識状況に依存させずにすむので、科学的説明の理論にとって重要だからである。 以下では、まずポパーによる傾向性説を概観し、次いで、この説に合致する説明の理論が演繹主義の立場で可能かどうか、そして可能だとして、それは果して熱狂主義に陥るかどうか、検討して行く。

６．傾向性としての確率

前章の引用で言われている傾向性説をまとまった形で観取するのに好都合な著述に、論

文「傾向性の世界」（Popper［1990］）がある。

この論文で、彼はまず、この論文で扱う中心問題が、因果性についてと、われわれの世界観の変化についてであると述べ、次いで、1927年頃に物理学に生じた大きな変化に触れながら、確率に関して現在まで自分が考えてきたことに言及している。

―デカルト以来、物理学者は、世界が巨大だが高度に精密な時計仕掛けであると考えてきた。が、ハイゼンベルクが登場するに至ると、微細過程が時計仕掛けを不精密なものにすると認めて、物理理論に確率概念を持ち込まざるをえなくなった。多くの物理学者は、確率は知識の欠如に関係せねばならないという主観説を採用したが、ポパー自身は、これとは反対に、客観説を採用することにした。そして、当初は相対頻度説を受け容れていたが、数十年に及ぶ検討の結果、彼が最終的に到達したのは、自らが〈確率の傾向性解釈〉と呼ぶものであった。

ポパーによれば、ラプラス理論にみられるような古典的確率論は、「或る一つの事象の確率とは、その事象に有利な可能性の数を、すべての等しい可能性の数で割ったものをいう」とする確率の定義から出発して強力な体系を構築したが、単なる可能性を扱うだけであった。　たとえば、イカサマの骰の場合には、等しい可能性がないので、古典的数値という意味では確率について語ることができない。しかし、等しくないことを見積ることができるなら、可能性に重みをつけることができることになる。重みがつけられた可能性という考えが、確率のより一般的な理論の基礎をなす。こう認めるとき、生じる問題は、そうした可能性に数値を付与する方法があるのか、ということである。

ポパーは、そうした方法として統計的方法を考える。つまり、彼は、問題とされる事象を産み出す一定の状況（situation）を繰り返すことができるなら、可能性に重みをつけることができる、と理解するのである。事象の生起の頻度が、その生起の可能性の測度なのである。

こうした理解は、相対頻度説にとどまるのではない。こう考えるポパーは、傾向性説を含意する以下のような四つのコメントを加えている。

（1）いま仮に、或るイカサマの骰を投げることで、２の目が出る可能性の重みをつけるとしよう。もしそれが 0.1666（＝1/6）ではなくて 0.15 でしかないことを見出したとすれば、それは、この骰を投げるということの構造に、２の目が出るという事象を現実化する傾向性が内在することを意味する。

―「したがって、私の第一の論点は、或る一つの事象を現実化する傾き（tendency）ないし傾向性が一般には、それぞれの可能性に内在し、しかも投げるというそれぞれの単独事象に内在するということ、および何度も投げることで実際に現実化したことの相対頻度に訴えることで、この傾きないし傾向性に値をつけることができるということである」。

（2）或る単独事象が生起する可能性について語るのではなくて、繰り返して行なうことにより一定の統計的平均を産み出す内在的傾向性について語ることのほうが、より正確な語り方になる。

（3）繰り返しをさらに続けるとき、つまり繰り返しの繰り返しをするとき、これに関連するすべての条件が安定し続けるならば、安定性への或る一つの傾きが出て来る。

（4）或る骰を投げる一連の運動が安定な統計的頻度を産み出す傾向性は、物理的状況の不変な（invariant）三つの側面によって、つまり、(a) その骰の内部構造、(b) 地球がそれに及ぼす力から成っている見えざる場、(c) 摩擦、等々によって、説明される。

以上のようにコメントを加えた上で、ポパーは次のように主張する。　―条件が安定し続けておれば、統計的平均も安定し続けるという傾きは、われわれの宇宙の最も注目すべき特徴の一つである。これを説明できるのは、傾向性説によってだけである（Popper［19

90] pp.11-12）。

ここには、単独事象（single event）について確率を云々するとき、確率を見積るための事象系列は、単なる系列ではなくて、一定の組の生起条件（a set of generating conditions）によって特定されている系列でなければならないのであり、　こう認めることは、傾向性説を採用することに等しい、という主張（Popper [1959] pp.34-5; [1983] pp.355）が込められている。

ポパーによる確率の傾向性説を説明の理論に持ち込むことができるなら、その結果、説明の理論は存在的な考え方のものとなることは、これまでに概観したことだけからも、容易に想像されることであろう。問題は、そのような理論が、演繹主義と折り合いをつけうるものとなるのか、ということであろう。

7．説明に関する演繹主義

サモンは、当初、低確率の個別的事象についての説明があるという観点から、ヘンベルのＩＳモデルを批判し、これに代わるものとして〈統計的関連（ＳＲ）モデルの説明理論〉を唱えた。この頃のサモンによれば、個別的事象の科学的説明とは、その事象に統計的に関連する個別的因子と一般的因子との寄せ集め(assemblage)をいうのであって、論証から構成されるものではない。その後、サモンは、ＳＲモデルでも説明関連の問題が生じることを認めるに至り、新たな見解を抱くようになったが、個別的事象の科学的説明は論証ではないという見解を依然として保持している。

彼が新たな見解を全面的に押し出しのは、『科学的説明と世界の因果的構造』（Salmon [1984]）においてである。サモンの新しい理論では、個別的事象の科学的説明は、事象間の統計的関連を示す部分と、それらの事象を因果的に結びつける部分とからなる。彼は、自分の新しい説明理論が、後者の部分において、非認識的概念としての説明を唱える存在的な考え方に立っていると主張した。

サモンの新たな主張によれば、或る個別的事象を科学的に説明するとは、確率論的過程を含めた世界の一般的パターンのなかでその事象の位置を定めることをいうのであり、このことは、ヘンベルによる説明の理論に同じく、法則への包摂という考え方に沿っているものである。ただし、サモンは自分の新たな理論がヘンベルの理論と違うところがあるとして、次のように主張する。 ──ヘンベルによる理論は、包摂がなんらかの論証によると理解する認識的な考え方、しかも、論証を説明の必須要素とみなす〈推論的な(inferential)考え方〉に立っている。しかし、自分新しい理論では、包摂は事象が実在的な一般的パターンに合致することによるという意味しかもたない。両者で包摂の意味が違う。

そして、前記の著作の刊行に先立っては、彼は次のように主張していたのである。
──「推論的な考え方は法則への包摂という関係を誤解している。われわれは統計的関連が説明されるべき事実に関わることがいかにしてなされるかをみたとき、科学的説明についてのカヴァー法則の考え方をもつことが説明を論証とみなすことなく可能なことを発見する」（Salmon [1979] p.421）。

筆者の理解では、存在的な考え方に立つサモンの新しい説明理論はヘンベルのそれにみられない利点を有している。というのは、前者では、法則的言明と実在の普遍的構造との対応ないし一致ということを要請することにより、第５章で述べたような説明関連の問題が生じないからである。存在的な考え方に従えば、真な統計的言明であっても、実在の普遍的構造に対応するところに欠けるならば、説明に用いられてはならないのである。

しかし、このことは、説明において言及される確率は、単に統計的関連を述べているのではなくて、世界の構造の傾向性を述べていると解することによって、存在的な考え方を推論的な考え方に付加するならば、同じく言えるのである。しかも、包摂の意味を変更することなしに、そう言えるのである。推論的な考え方を採用すれば必然的に認識的な考え

方をしていることになるとは限らないのである。ところが、この論点は、いまのところ筆者にはその理由が分からないのだが、サモンの新しい理論では、真剣な考慮の対象になっていない。

この世界には或る普遍的なものがあって、これを探求することが科学活動であると考えられる。サモンもそうした理解に立っているのではないだろうか。 そうだからこそ、彼は、一般的パターンと自らが言う因果的構造に対応するような統計的言明だけを説明項に含めることを、科学的説明の必要条件にしているのであろう。 そうだからこそ、たとえば、次のようにはみなさいのであろう。個別的事象〈Ｇａ〉の生起の説明として「 Ｇａ なのは Ｆａ だからだ」と言われる場合の「だから」がそのままで実在の因果的構造に対応しているのであって、単称確率言明「 p(Ga／Fa) = r 」を説明項の必須の要素とすることで、科学的説明は成立する、などとは。

このことを少し哲学的な言い方で言えば、「 Ｇａ なのは Ｆａ だからだ」と言われる場合、これが科学的説明になるとみなされるときには、「事象のタイプＦとＧとの間には、世界の普遍的構造を反映するなんらかの一般的関係がこの世界に成立していて、それが、個別的な事象の間になんらかの仕方で事例化されている」ことを前提にしている。こう言えるのではないだろうか。だとすると、一般的関係の事例化を明示することが、科学的説明には、求められるであろう。そして、この明示は演繹的論証で形で与えられる。こう考えることは自然なことであろう。（熊谷 [1993] pp.32-4）

「 Ｇａ なのは Ｆａ だからだ」は、その「だから」に、事実の法則への包摂を示す演繹的論証が込められてはじめて、科学的説明になっていると言える。 こうした理解の下で、ＤＮモデルに適う説明をすることには、なんの問題も生じない。というのは、たとえば、普遍言明「∀ｘ［Ｆｘ→Ｇｘ］」と単称言明「 Fa 」とによる単称言明「 Ga 」の包摂が演繹的論証によってなされ（「→」は含意）、かつ、その普遍言明が実在の普遍的構造を反映するとみなされるときには、このことは、そのままで、世界に成立するとされる一般的関係が個別的な Ｆａ という事象と同じく個別的な事象 Ｇａ の間に事例化されていることを明示しているとみなされることになるからである。

しかし、ＩＳモデルによってヘンペルが扱おうとしたような事柄については、話はそううまくは行かない。というのは、本稿での理解では、一般的関係の事例化を明示するのは演繹的論証の形でなければならないのだが、統計的言明「p(G/F) = r （rは1に近い確率値である必要はない）」が実在の普遍的構造を反映するとみなされたとしても、この言明と「 Ｆａ 」とによる「 Ｇａ 」の包摂は、演繹的論証によっては果たされないからである。

演繹主義がこうした事態を困難として抱えるのは、論証を説明のための必要十分条件とみなす熱狂主義の立場をとるときである。論証を説明のための必要条件に過ぎないという見解を認めるならば、たとえばレイルトンが〈確率論的説明の演繹的－法則論的モデル〉と名づけたものが可能となって、困難は生じない。

レイルトンはこのモデルを例証するものとして、ミクロなレベルでの個別的事象であるU_{238}原子のアルファ崩壊という事例を持ち出した（Railton [1978] ）。 この原子の放射性崩壊の半減期は、理論的説明により、6.5×10^9 年なので、U_{238}原子が一定の短時間内に自然崩壊を起こす確率は極めて小さいが、ゼロではない。この確率をｐとすると、これも、物理定数の一つであるU_{238}原子の崩壊定数を用いて理論的に与えられる。仮にU_{238}原子からのみなるサンプルについて或る短期間に一つの崩壊が観測されたとしよう。この崩壊した原子の核をｕとすると、ｕが自然崩壊する確率がｐであることを結論

として得るような一つの演繹的論証が構成できる。　この論証で、レイルトンは、確率には、U_{238}原子核が基底状態にある（すなわちU_{238}からのみなる物質が外からなんの影響も被っていない）場合にその原子核が粒子を短期間に放出する傾向性（しかも単独事例（single-case）の傾向性）という解釈が与えられている、と理解している。　したがって、この論証の前提になっている統計的言明が単なる統計的関連を述べただけの言明ではなくて、実在の非決定論的構造を反映するものとみなされた法則的言明と理解している。つまり、サモンに同じく、レイルトンは存在的な考え方に立っている。さて、この論証だけで、原子核ｕが短期間に崩壊を起こしたという個別的事象の科学的説明になっているように思われるかもしれないが、実はそうでない。レイルトンによれば、演繹的論証で示されたのは、二つの個別的事象の間の傾向性でしかなく、個別的事象そのものの生起ではない。これから、原子核ｕが短期間に崩壊を起こしたという個別的事象の説明をうるには、被説明項そのものを述べ立てる主旨をもつ〈挿入的追加項(parenthetical addendum)〉を必要とする（Railton [1978] p.217）。

つまり、レイルトンの理解では、非決定論的構造をもつと考えられるミクロなレベルでの個別的事象の科学的説明は、演繹的論証で尽きるのではない。というのは、挿入的追加項を先の演繹的論証の前提に付け加わるものとするなら、それによってできる演繹的論証は自己同一律による当たり前の論証になって経験的内容を欠いてしまうからである。したがって、演繹論証に挿入的追加項を単に付加するのは、被説明項が述べ立てる個別的事象が、説明項が述べ立てる個別的事象との間にしかじかの傾向性（しかも先の例では極めて低い確率）をもつ関係のなかで生起した、と言わんとしてのことである。

レイルンの以上のような議論には、ヘン見られないところがある。それは、演繹的論証を説明の必要条件としている、というところである。演繹的論証を説明の必要条件とすることは、すでに述べたように、世界に成立する一般的関係の事例化を演繹的の形で明示することであり、科学的説明の理論において真剣な検討の対象にせねばならない事柄である。説明に関する演繹主義は、この点を踏まえておればよいのであって、これより強い主張をする必要はない。筆者のみるところ、科学的説明に関するポパーの言説は、いま述べたところの演繹主義と整合的であり、熱狂的演繹主義にならずにすむのである。

世界の普遍的構造を探ることが科学活動であると考えられる以上は、この構造を反映するとみなされる一般的関係の事例化を演繹的論証の形で明示することは、科学的説明には必須の事柄である。　もっとも、科学的説明は、場合によっては、挿入的追加項という決断（decision）を述べるものを含むことがある。筆者が科学的説明の理論として可能と考えるこうした考え方は、理論の受容可能性の問題において方法論的決断の役割を強調するポパーの考え方と、相通ずるものがあろう。確かに、ポパーは、非決定論の立場をとっているが、世界の非決定論的構造が反映していると思われる事象の生起の説明がいかなるものかについて、解明をほとんど行なっていない。しかし、ポパーが詳細にこの解明を展開したとしたら、それは、サモンの主張するような〈説明に関する熱狂的演繹主義〉に陥るとは限らないのである。

（おわり）

(*) 本稿の（Ⅱ）は、本会の昨年度研究大会のシンポジウムで「科学的説明という観点からポパーの「傾向性の世界」を読む」と題して発表した事柄と、拙論「科学的説明の演繹主義的理論へ向けて」の一部とを結合させて出来上がったものである。

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『ポパーレター』前号の訂正とお詫び

本研究会の顧問である団藤重光先生からポパーへの追悼文を寄稿していただき,前号の『ポパーレター』に掲載させていただきました。発行後,先生より誤植のご指摘を受けました。先生からいただいた原稿は誤植一つない完全原稿でしたが,私のワープロへの入力ミスのため,ご迷惑をお掛けしました。ここにお詫びし,訂正させていただきます。お手数ですが,ご訂正をよろしくお願い申し上げます。(編集部:立花)

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13頁,31行	
可謬的(誤)	可謬論的(正)
13頁,36行	
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16頁,11行	
には(誤)	のは(正)

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第６回年次研究大会は，大阪市立大学を中心とした関西の会員の皆様のご協力により，初めて東京を離れて，開催することができることになりました。今回はヨーク大学（トロント）のジャーヴィ教授をお招きして，ご講演をしていただくことになりました。また本会会員の神野先生にもご講演をしていただけることになりました。今回はこの２人の先生のご講演だけに的を絞り，その後に続く議論を深めてまいりたいと存じます。尚，紙上討論という形ですが，アガシ教授も議論に参加されます。当日，会員の皆様によって活発な議論が展開され，この大会が実り豊かなものになることを祈念しております。（立花記）

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慶応義塾大学商学部　渡部研究室

☎　03-3453-4511 Ex. 3137

Fax　03-3798-7480

編集部　〠010　秋田市手形学園町1-1

秋田大学教育学部　倫理学研究室

（立花希一）

☎　0188-33-5261 Ex. 2608

Fax　0188-36-6738