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PROGRESS AND RATIONALITY IN RESEARCH

(*Science from the Viewpoint of Popperian Methodology*)

PART I. THE IDEA OF A METHODOLOGY AND THE BACKGROUND OF POPPERIAN METHODOLOGY. THEORY APPRAISAL, METHODOLOGY APPRAISAL AND IDEAL OF SCIENCE

0. ON THE CONCEPT OF METHODOLOGY: THEORY APPRAISAL AND METHODOLOGY APPRAISAL

0.0. The Need for Methodology: Since Decision-making is an Ubiquitous Moment of Research There Cannot Be a Methodology-free Research

In the process of research the researcher finds himself time and again confronted with problems of decision making: to decide which of two alternative research programs should be followed, whether it is worthwhile to conduct a certain experiment, etc., etc. And just as often he is confronted with problems of *appraisal*, decision-making *ex post* so to speak: to decide whether a certain explanation is adequate, to appraise the comparative achievements of competing problem-solutions, i.e. to decide which is 'preferable', to decide above all whether a proposed new theory constitutes progress over its rivals, and so forth. *Ex ante* the researcher has to decide which course of action is 'rational', in the sense of purposive rationality (*Zweckrationalität*) – given his interpretation of his current research situation (which of course may be mistaken). This part of research activity resembles the similarly risky business of financial investment. *Ex post* he analyzes his past investment decisions, thereby attempting also to estimate opportunity costs: whether time and effort if invested in a rival theory program would with a certain likelihood have yielded better results. Again a delicate problem.

If, in any sort of activity, decision-making is complicated, the need is felt to systematize it. If this need is taken seriously, a special discipline will develop to meet it. As regards research, '*methodology*' is a suitable label for such a discipline. Some dispute whether methodology is possible; but a sceptical view on this issue already reflects a certain image of science, i.e. it is itself the result of a certain methodology which operates clandestinely. If

M. D. Grmek, R. S. Cohen, and G. Cimino (eds.), On Scientific Discovery, 43–102.

all research were like the unique acts of creativity in art, all the decision-making involved might be governed by 'tacit knowledge' in the sense of not being articulable. If so, a critique of decision-making in research would be like art criticism, since there would be no general criteria, no statute law. *Faute de mieux*, the critic would have to rely on his intuition, sensitivity, *Fingerspitzengefühl.* He would have to bring his personality into play. If on the other hand *all* of research were like a routinized procedure of 'problem solving', the methodology could be an algorithm. Clearly either view is a totalization – and patently false. Research obviously contains moments of both types and above all moments which in various degrees approximate to each of the above extreme types. *Insofar as research or an important section of it is a rational goal-directed activity, at least some part of the decision-making can be elucidated by a praxiological study.* ('Praxiology' is being used in Kotarbiński's sense: roughly as the theory of effective and efficient action.) If so – and we submit that this is a correct assumption – methodology is a viable project. Moreover, the researcher cannot avoid it since, whether he likes it or not, part of his time as researcher is spent in this sort of decision-making: *every researcher is his own part-time methodologist. There cannot be any methodology-free or methodology-neutral research.* (As little as there can be observation sentences or even communicable, hence formulable, perception reports free from theoretical ingredients.) The suspicion that methodology as such, i.e. as a discipline, might claim to be able to prescribe to the researcher what he should do is, as already hinted at above, based on a misunderstanding. The existence of certain misguided paternalistic methodologies which oversell themselves does not warrant such a generalization to the discipline as a whole. The researcher who does not recognize the interdependence of research and methodology will be a '*méthodologicien malgré lui*'. The methodological criteria and gambits he uses in his research activity will remain latent, and so long as they remain latent they cannot be criticized and hence there will be little chance of their being improved. We have belabored the obvious because in contemporary discussion the *raison d'être* and even the possibility of a methodology have been questioned.

0.1. What Contributions Can Methodology Make?

Most important of all is that it should contribute to the *refinement of our image of science.* This image is becoming increasingly important for our image of man. Questions such as 'Can scientific knowledge be rationally justified?' 'How does knowledge grow?' etc. have to be attacked if we are to refine our view about man's capacity of knowing, which is a central part of our self-conception as species. Moreover the image of science, in particular the prescriptive part of it, the ideal of science, is essential for the researcher's understanding of his activity, and hence eventually also for his success. The ideal of science also serves as a foil for the historiographer of science in his attempts to develop a descriptive picture of historically given science.

In addition to this general significance, methodology may, indirectly, help to increase the researcher's *efficiency*. For instance, through offering means of conceptualizing research situations and possible alternative developments, through critically analyzing and appraising the ways in which successful research enterprises have proceeded, through making explicit tacit presuppositions and more or less unnoticed dependencies on certain styles of thought, etc. All this with the view to increasing his freedom of decision – not with a view to prescribing to him how he should proceed.

0.2. What Sort of Discipline is Methodology?

If there is to be reasonable hope that it may fulfil the above-mentioned tasks, what sort of discipline would methodology have to be? First let us say what, in our opinion, it cannot be: let us distance ourselves from two popular reductivist views. (i) The view that conceives of methodology as an *empirical inquiry*, as another scientific discipline and equating it with social science *cum* historiography of science. This view is an example of *scientism*, and it is based upon an instance of the so-called naturalistic fallacy: the attempt to base good reasons for following a certain methodological recommendation or for opting for a certain ideal of science upon what one believes to be the facts, upon descriptive statements about how certain 'successful' research undertakings did in fact proceed. But to speak at all of 'success' one has to transcend the realm of description and explanation and enter that of appraisal. (ii) The view, contrary to the scientistic conception, that methodology should be *applied logic*. This view, *logicism*, totalizes one important aspect, logical moves in research and the logical aspects of the results of research, by holding that these aspects are all that matter in methodology. While the scientistic view rests on a fallacy, this view correctly covers a part of the truth while concealing others: of course logical aspects are very important, but, just as evidently, there are many other important features of research.

If methodology is to have a chance of fulfilling the above-mentioned demands (Section 0.1), it will have to be a discipline that develops a system of *recommendations about how to act in certain types of research situations in order to facilitate achieving the aim of this activity: scientific progress*; a discipline that articulates and criticizes such recommendations in order to improve them. Thus it cannot be identical with either sociology, psychology of science *cum* historiography of science nor with logic applied in the reconstruction of the results of research, although it will have to cooperate closely with both history of science and with logic. Methodology, if conceived as above, will have to identify types of research situations, and formulate recommendations stating what it would be rational to do if one were in a certain type of research situation. A methodological prescription or advice could be cast, e.g., as follows: "When you have to choose between two competing theories, it is rational to prefer the one that stands in relation R to its competitor – assuming always that your aim is to achieve cognitive progress". Then, of course, good reasons have to be brought forward why it would be rational to follow such and such advice. If two methodologies differ in their advice, e.g., in a concrete theory appraisal give different verdicts, then we are faced with the problem of *methodology appraisal*: appraising the comparative achievements of rival methodologies. *A methodological rule can be defended or criticized only argumentatively*: defended by giving *good reasons* for accepting the conjecture that it is 'better' than its rival in the sense of having greater potential for realizing the aim of the activity: scientific progress. Everything depends upon whether this can be plausibly argued. Strictly speaking two methodologies can be rivals only insofar as they have the same aim, i.e. to explicate the aim of research – scientific progress – in roughly the same way. To this problem we shall return later (Section 1).

In sum, methodology as a discipline is much like philosophical reflection; it produces prescriptions of the type of the so-called hypothetical imperative and good reasons to defend them or to criticize them. The system of recommendations articulates an idealized image of research. If research is conceived with Popper as basically an interplay of conjecture and criticism, of variation and selective retention, of making and matching, of innovative moments and moments of quality control, then we will distinguish two broad clusters of rules. One is rules of *quality control*, of the *ex post* appraisal of results and interim results of all sorts, but also appraisal of procedures, criteria, arguments, even of problems. To use a convenient *pars pro toto* label for this group, we propose to speak of rules of 'theory appraisal'. The other group are rules referring to the innovative moments. They could be called 'heuristic rules' – heuristic in a narrow sense since all the rules have an advice-giving function. A convenient umbrella word would be 'rules of *theory formation*'.

Popper has focussed on the theory appraisal. Rightly so, because theory formation always includes an essential element of conjecture, of creativity, which cannot be accounted for in a structural way. Hence there is no method of theory formation, and therefore this moment is not of the same interest to methodology as is quality control. But this does not mean that methodology may not have something to say about the structural characteristics of hypothesis formation. The often voiced reproach that Popperians have to abandon the study of hypothesis formation to psychology is unjustified. For instance, methodology studies the requirements of the output: e.g. the requirements that the tentative theory, which is the output of the hypothesis-generation moment, must be such that it is at least in principle capable of solving the problem at hand, that it must be falsifiable, that it should have as high a degree of testability as possible (say much), that, in case it is to function as a revised successor of a falsified theory, it must not be produced by an '*ad hoc*' adjustment of the theory which has met with experimental results contradicting it.

If the above programmatic definition is accepted, what sort of activity is methodology? It has obvious *similarities with a technology*. However, it would be too crude to propose that it may be viewed as 'the technology of scientific progress' because there are striking *negative aspects to the analogy between methodology and technology*. Technology is often conceived as law hypotheses in the context of solving concrete practical problems. We would prefer to define it as a system of prescriptions for how to use means to achieve certain pre-given goals.[1] The most reliable technologies are based on highly corroborated scientific theories. It need not of course be the best available theory. It suffices if it is sufficiently reliable since, in the context of application, the theory is being used only as an instrument of prediction, and moreover a cost-benefit analysis is always relevant. (Thus, e.g. in space flight Newton's theory is used rather than Einstein's, although in the context of basic science, the superseding of Newton's theory by Einstein's is one of the paradigmatic examples of scientific progress.) However, the theory to be used must be sufficiently well established. Assessing its degree of 'evidential support' – however this concept is explicated – is theory appraisal, a task that *ex definitione* only methodology can tackle. Hence *to attempt to base a methodology upon empirical science in the same way as a technology can thus be based would involve a vicious circle*. The relevant knowledge would involve also methodological appraisals, and methodological knowledge (however explicated) cannot be falsified or supported by empirical evidence in the same way in which scientific knowledge can. To suppose that it can is an instance of the scientific fallacy. This is the first important difference between methodology and technology.

Related to it is a second difference. There are methods or rules for accomplishing technological tasks such as producing certain sorts of steel. Methodology, however, can point out means for achieving cognitive progress only in the sense of facilitating its achievement, of facilitating the growth of knowledge. For instance, it can provide broad rules for theory appraisal and rules guiding the rational preference of one theory over its rivals. On the other hand, in connection with problems such as on which premiss to put the blame for a falsified prediction (Part II, Section 2.3), it can only give very broad global advice leaving the greatest part of the decision to the researcher's sensitivity; and in connection with hypothesis formation it can only give very general guidelines (Part II, Section 1.3), since creativity cannot be planned nor fully explained.

There is a third difference which is important. The goal of a technology can be stated independently of that technology. (E.g. if the goal is to produce steel of a certain specification, this specification can be given in the terms of physico-chemical properties, and it is not the task of the technology of steel production to provide these specifications – the goal is given from outside.) In the case of methodology the situation is different: *The specification of the aim* – facilitating scientific progress – *is itself one of the major tasks of methodology*. (For many *the* task of methodology.) Who else could explicate the idea of cognitive progress? Methodology has not only to explicate this idea, but also to criticize the explicata that have been proposed, a criticism that will lead to a comparative appraisal of the ideals of science which underlie different explicata. Methodologies will be rivals only insofar as they attempt to realize roughly the same aim. A methodology, whose proclaimed aim is to help achieve knowledge that is justified in the sense of having been shown to be true, and a methodology based on a non-justificationist view of human knowledge, cannot be appraised with respect to their comparative achievements without such comparative appraisal leading us to a critique of the ideal of science, the view of man's capacity of knowing underlying each of them. (Perhaps, although of course words do not matter, we could say that methodology is a 'quasi-technology' – a convenient label for epitomizing the above considerations of the positive and negative aspects of the analogy between methodology and technology.)

Thus this activity, which is neither empirical investigation nor mere applied logic, but rather argumentative, has affinity with philosophical reflection. Insofar as it attempts to find out what course of action is rational given a certain interpretation of the situation or attempts to develop assessments of good reasons, i.e. *ex post* to assess problem solutions, theories, procedures, decisions and so on, it may be said to develop what Popper calls a 'situational logic'. When it attempts to assess the efficiency of ways of proceeding, of actions, of research undertakings, etc., it qualifies as a praxiological study in Kotarbiński's sense; and insofar as it looks at research as processing a 'research program', i.e. a system of hypotheses (knowledge) – problems-instruments (techniques, calculi, etc.)-plans-etc. into a more refined system of that sort, it might be said to exemplify what nowadays is often labelled 'systems thinking'.[2] It constitutes a sub-field of philosophy rather than anything else, a sub-field that is secondary to some other fields as it is secondary to science itself in the sense that first there must be empirical research before a need for methodology can arise.

Methodology obviously does not and cannot deal with certain problem clusters of traditional philosophy. But nonetheless philosophers often reproach it for not attacking certain philosophical problems. Hence some procataleptic remarks about its limitations are called for. (i) Some philosophers (e.g. H. Spinner) accuse Popperian methodology of conventionalism, asserting that in spite of its being a non-justificationist approach it has not been able to come to grips with the problem of the so-called 'basic' sentences. So far as methodology is concerned Popperians hold that no type of sentence is to be accorded an epistemologically privileged status. In the empirical testing of hypotheses, data sentences are used, e.g. in physical research statements about material objects and processes but not about perceptual experiences. Perceptual reports form part of the good reasons for the conjecture that a certain 'basic' sentence or data sentence may be accepted *pro tempore*. When there appears no reasonable doubt concerning a data sentence (a matter to be decided by the researcher) it would be pointless for the methodologist to emphasize that it can always be questioned. For this reason the relationship between perceptual reports and a data sentence about physical objects is not analyzed by Popperian methodology. Such an analysis is regarded as a topic of general epistemology, and ontological analyses of acts of perceiving, etc. are left to philosophers. (E.g. 'epistemology as the ontology of the knowledge situation' as it is developed by Gustav Bergmann and his followers is a field that methodology cannot encompass.) (ii) Similarly Popper's three-world ontology is intended as means to an end: to provide a suitable ontological ground-plan for discussing certain methodological problems. It is not intended as an '*Aufbau der Welt*', as e.g. 'ontology as an argumentative structure upon a phenomenological base' by means of which to construct or reconstruct reality including our experience. This again is a task of 'first philosophy'. (iii) Popperian methodology presupposes that there exists a language and that researchers are capable of forming what is called 'a communication community'. For this reason some philosophers (e.g. K.-O. Apel) accuse Popperian methodology of a sort of abstractive fallacy. But such a reproach would be justified only if Popperians were to forget that research has such preconditions. This is acknowledged, but the analysis of the conditions of possibility of science (*sinnkonstitutive Bedingungen der Möglichkeit*) is left to philosophy proper, because this is a burden methodology cannot bear. All this is simply a practically necessary division of labor within philosophy. Of course it would be *hubris* if methodology were to aspire to encompass these subfields of philosophy. We have emphasized what may seem self-evident because in the literature there has been some misunderstanding about this situation.

1. METHODOLOGY APPRAISAL LEADING TO THE CRITIQUE OF SCIENCE; TASKS FOR METHODOLOGY FOLLOWING FROM THE IDEAL OF SCIENCE

1.0. Appraising methodologies includes consideration of two sorts of questions: Are the *solutions* a particular methodology proposes to the problems it has posed acceptable? Are the problems really those that matter? Will answering these *questions* help in facilitating scientific progress? The explication of the idea of progress will be governed by the ideal of science adopted: progress will, in general, mean coming closer to that ideal. In this way a comparison of two methodologies will involve comparing the reasonableness of their ideals. If two methodologies' fundamental ideals of science, although distinct from each other, can be shown to be alternative explicata for a common conception of scientific merit, then a comparative appraisal at least of these explicata is possible.

In our *intuitive ideas about what scientific knowledge should be* like and about the earmarks of progress we seem to have such a *common ground: there exists a common explicandum*. Probably – as John W. N. Watkins has argued[3] – all parties would accept the following naively stated desiderata. The aim of research is knowledge that is *genuine* knowledge, as *comprehensive* and as *deep* as possible: i.e. it should explain a lot and these explanations should help us better and better to understand the world and ourselves in this world. A successor theory's progress over a predecessor consists in its achieving more in at least one of these three aspects than the predecessor.

The two most prominent methodologies, which have placed the normative problem in the center, logical empiricism and Popperian methodology, have proposed different explicata for this intuitive ideal (as a common explicandum), and the Popperian ideal of science has arisen from criticizing the explicatum proposed by logical empiricism. We propose first to give some hints at the ideal as explicated by logical empiricism, then to move to the Popperian criticism of that ideal, and eventually to the alternative offered by Popper.

1.1. The guiding idea of the *ideal of science of logical empiricism*, of the foundationalist approach, is to give the desideratum *certainty, top priority*. As a result the most important *task of methodology* becomes that of formulating and legitimating a *rule of acceptance* in accordance with the basic conviction that a scientific proposition is ultimately acceptable only if it is true. To make such a role operational one needs a method of establishing in concrete cases whether a proposition is true or not, and the method must give us an *infallible* criterion of truth. This is the position of verificationism, the justificationist approach to rationality in science: a sentence is acceptable only if true and recognized as such. It is then weakened in probabilistic verificationism: a scientific proposition is acceptable only if it has been probabilified to a 'sufficient' degree. In this scheme experience plays a positive role (hence the tag 'positivism'): it serves as the final establishing arbiter. Verificationism, absolute or probabilistic, seems attractive only if one is willing to countenance a particular class of empirical propositions whose certainty does not need to be called in question: *a sure source* of knowledge about reality as an epistemological fundament on which to erect the edifice of science. Ideally, systems of propositions would be generated by deductive connections. This is a sort of 'proof-empiricism'. Again, in view of its obvious unattainability – since universal propositions infinitely transcend any finite set of singular sentences serving as a fundament – one lowers his demands and attempts to construct propositional systems with partial information-covering from a selection of 'basic' sentences assumed to be certain. Underlying this probabilistic verificationism seems to be a principle of hope that, as more and more evidence comes in, it may in the long run be possible at least asymptotically to approach that ideal state in which the evidence completely 'covers' the information of the complex sentences. Inductive logic or theory of confirmation is to provide the connections and to measure the degree of 'coverage'. Cognitive progress would then be defined in terms of better and better approximation to this ideal of science. Logical empiricist philosophy of science may be viewed as an articulation of various key aspects of this ideal of science.[4]

1.2. *Popper's critique* may, in accordance with what has been said above, be divided into two parts: the critique of the problem solutions offered, and the critique of the problems. (i) The critique of the *solutions* offered amounts roughly to pointing out that the various models (such as the various explicata offered for the concept of 'empirical significance', the models of explanation which promise an explication of the idea of causal explanation by stating not only necessary but also sufficient conditions, etc.) have not been able to help researchers approach certainty, the ideal's own centerpiece in measuring the results of research, not even when these results are highly stylized. This must be so because, firstly, the presupposed certain 'basis' does not exist, and, secondly, even if for the sake of argument such a 'basis' were conceded to exist, not only absolute verificationism but also probabilistic verificationism, inductivism, founders on Hume's criticism, since it follows from the probability calculus that the logical probability of a universal proposition on the basis of a finite set of evidential statements is zero. Even a partial retransmission of truth from verified conclusions to premisses is logically not possible. In addition, probabilistic verificationism, with its attempt to develop an inductive logic or similar method, will continue to lack the crucial deductive structure – which is one of the desiderata of the logical empiricists' own ideal of science. It must *ex definitione* introduce amplificatory logical moves. (ii) Popperian *criticism of the explicatum* of our intuitive ideal of science *proposed by logical empiricism* asserts that this explicatum is neither fruitful nor sufficiently similar to the reasonable part of the explicandum to be acceptable as the result of a successful explication attempt. The desideratum given top priority, certainty, is unattainable in principle – utopian. Since certainty and informative content are inversely proportional, and since certainty is given top priority, the value of 'high content of empirical information' must be sacrificed. However explicated, the desideratum of 'depth' – an important component of the intuitive ideal – is not only lost, but has also become anathema. (Another reason for the tag 'positivism'.) In sum, the price to be paid for the search for certainty is a total loss in all dimensions even of the ideal of science as explicated by the logical empiricists themselves: not only is certainty unattainable in principle, but in striving for it nonetheless, other desiderata of the logical empiricist's own ideal become unattainable. The very idea of knowledge in the sense of certain knowledge, which appears to be a secularization of the theologican's concept of revealed knowledge, can have no place in empirical inquiry.

From logical empiricism's explicatum of the intuitive ideal follow certain problems for methodology. Since the explicatum is mistaken, these problems are inappropriate. From the ideal it follows that the main *task* of methodology is to search for an *acceptance* rule, to formulate and legitimate such a rule. It conflicts with that idea of scientific progress which sees novelty as the essence of progress: insofar as we want theories to go much beyond 'background knowledge', to lead to new insights and thereby to new and deeper problems, it is unreasonable to hold that we are ultimately after theories whose information content will eventually asymptotically approach the state of being completely covered by the information carried by that 'complete' evidence. The justificationist, cumulative view of science is mistaken if only for that reason, and if, interpreted as a descriptive picture of historical science, it is refuted by the history of science because theories once regarded as certain, later were falsified and superseded by new theories. In short, the Popperian side shows that logical empiricism's commitment to foundationalism and inductivism has proved untenable – that the foundationalist approach founders like all *Begründungsphilosophie*. Scepticism would be a possible reaction to this insight, but it is not the only possibility.

1.3. *Popper offers an alternative* to the foundationalist approach and to scepticism. With his pioneer work of 1934, which has become a classic, he is the first to work out a deliberate non-foundationalist methodology and also a non-foundationalist, evolutionist theory of knowledge: a Copernican revolution in the philosophy of science. In the Popperian explicatum of the intuitive ideal the quest for the pivotal desideratum of the explicatum proposed by logical empiricism, certainty – the *Fata Morgana* of all foundationalist philosophy (*Begründungsphilosophie*) – is abandoned in favor of a *conjecturalist-fallibilist view of human knowledge*, at least for all knowledge about the empirical world. Such knowledge is in principle fallible, conjectural. But fallibilism preserves the idea of absolute truth as a regulative principle, especially in the comparative notion of a 'more (or less) accurate representation' (*mehr oder weniger zutreffende Darstellung*): we know what we mean by truth or truthlikeness in this sense even if there is no criterion of truth, i.e., even if in any concrete case we cannot with certainty tell whether a particular proposition is true or false.

The explicatum of our intuitive ideal of science must above all meet the meta-criterion of fruitfulness, implying *inter alia* such problems for methodology that a methodology which attacks these problems can hope to make the contributions mentioned above in Section 0.1. It must also meet the necessary condition of being 'sufficiently similar' to the intuitive ideal in those respects where the ideal is not utopian. Once it is recognized that certainty is unattainable in principle, the goal of research can be epitomized as representing more and more accurately (increasing in truthlikeness about) those aspects of reality whose comprehension (explanation) leads to new, fruitful perspectives and thus to new and deeper problems, so that we get a better and better understanding of the world and of mankind (contribution to the refinement of our world view). Science is concerned with developing theories further and with replacing theories by better ones, i.e. with cognitive progress. The question of acceptance has its place primarily when we ask whether or not we regard it as rational, as justified (given the practical situation at hand) to use a certain theory as an instrument of prognosis and to base technologies on it. Basic research is *ex definitione* concerned with cognitive progress, not with 'acceptance'; and if in the context of a methodological discussion Popperians speak of 'acceptance', this is short for saying that, since the theory in question has not (yet) been falsified and indeed has thus far stood up to all empirical tests, we propose to continue working on it and with it, i.e. to develop it further by *inter alia* subjecting it to new sorts of tests.

In sum Popper proposes an *alternative* to the quest for *certainty*, the search for a principle of induction, the 'new philosopher's stone', the search for *growth of knowledge*. Hence it is important to explicate the idea of growth of knowledge, of cognitive progress, and in this context the idea of one theory's being closer to the truth than its rival is likewise of great importance.

What tasks for methodology follow from the Popperian explicatum of the ideal of science? The *global tasks* will be: making explicit the various components of the ideal in more details, suggesting methodological rules supposed to facilitate the realization of the ideal as explicated, and supporting these rules by good reasons. If one accepts the Popperian explicatum of the intuitive ideal, then in the appraisal of the comparative achievement of rival methodological rules the key question will be for which of the competing rules it can plausibly be argued that it is of greater help than the other in realizing the ideal in the sense of Popper's explicatum.

What specific tasks for methodology follow from the above global tasks? The center of concern will be *preference* rules, i.e. to formulate such rules based on rules of *appraisal* and to find out what sort of good reasons might accompany the conjecture that one of a particular pair of competing problem solutions, theories, etc., should rationally be preferred over the other. The rules of comparative appraisal will have to be formulated not only for results such as theories and explanations, etc., but also for procedures and for past decisions – all this with a view to improving future decision-making. Here we consider only the issue of *theory comparison*. It is of course rational to prefer that theory which is 'better' than its rivals on all counts or on that count which matters in the particular comparison situation at hand. A theory T' is 'better' than T with respect to z to the extent r. Thus we will have to identify the relevant respects and explicate 'better' with respect to them. Evaluations of this kind are internal to science.

Apart from trivial desiderata such as internal consistency and empirical significance, a commonsensical desideratum is that the theory proposed should be pertinent to the problem at hand, a potential answer to the question raised. (Not even the rare cases of serendipity are without a guiding question even if it is concealed (M.D. Grmek).) A basic desideratum is that the successor theory T' should go beyond its rival (predecessor) T. If it contradicts the predecessor (and is successful in predicting), then this is an indicator that it has a greater 'depth' than the predecessor; and if so, it will be more fruitful, i.e. give rise to still 'deeper' problems. Another basic desideratum is that T' should say more than T, bring an increase in potential 'explanatory/predictive power', and that what it says should be correct, in particular its predictions successful.

Popper and even more so his followers have *focussed* on this dimension of '*truthlikeness*', where truthlikeness is conceived as a concept that solders content and truth. Content is not to be taken in an absolute sense. The problem of theory appraisal arises, typically, only when theories are competitors, when they attempt to solve the same set of problems. They are maximally competitive if they give incompatible answers to the same question(s). If the task of ascertaining which theory is closer to the truth were not thus limited, theory appraisal would presuppose a prior appraisal of the 'scientific interest' of the questions. 'Scientific interest' may be explicated objectively in terms of the contribution an answer to the question at hand is expected to make to cognitive progress in the discipline; it will be particularly high if the successor theory contradicts the theory that is the reigning champion. However, since an element of prognosis is involved, such an appraisal is very risky. Fortunately at least this problem need not trouble us here since we may presuppose that the theories under appraisal, T' and T, are competitors.

Intuitively everything appears fairly clear. The two basic distinguishing features of progress in the information-theoretical/epistemic dimension are: (a) that T' says more than T, i.e., in the area of the two theories, mutual concern; and (b) ideally, that what T' says is true, or, more realistically, what T' says is closer to the truth than what T says, again, with respect to the scientific problems at hand. It will be requested (i) that the empirical content of T' that goes *beyond* that of T has not been falsified although tested in tests of a certain severity (Part II, Section 2.1.0); (ii) that T' matches the past explanatory successes of the predecessor theory T, i.e. that the corroborated hypotheses thus far deduced from T (in the presence of auxiliary hypotheses) must also be deducible from T' (and auxiliary hypotheses) with at least the same degree of precision (i.e. with the same empirical content); and preferably T' should also refine and correct some of the original predictions. If a proposed successor theory fulfils at least the first two of these three requirements, then not only is T' closer to the truth than T but, so far as the scientific problems under consideration are concerned, T' also dominates T in content.

Of course, degree of corroboration (as the balance sheet of empirical criticism) together with preservation of explanatory successes provide but a fallible indicator on which to base the conjecture that T' is closer to the truth than T, since good performance of a theory to date no more guarantees high dividends in novel knowledge in the future than the good performance of a stock guarantees future profits. Nonetheless, the situation of the methodologist is better than that of the financial analyst relying on (inductivistic) extrapolation from charts, because the methodologist can rationally conjecture that the better corroborated theory is closer to the truth than the one with a lower degree of corroboration thus far. Of course he too may be proved wrong by future scientific developments.

Since a dramatic increase in 'truthlikeness' in the sense of content-cum-truth is possible only if the *successor theory is 'deeper' than the predecessor*, and makes possible 'deeper' explanations, the 'deeper' theory is to be regarded as the better one. The idea of depth will, of course, have to be clarified.

From the desideratum of increase in 'truthlikeness' as (fallibly) indicated by increase in degree of corroboration together with the method of falsification (which is thoroughly deductive), it follows that no amplificatory moves will be permitted in connection with theory testing. Certainty will thus be retained in the only area where it has a place. But since it follows from other desiderata, the requirement of *deductive* procedures need not be mentioned explicitly.

1.4. What Do the Above Considerations Mean in Terms of Specific Tasks for Methodology?

(1) From the desideratum of increase in 'truthlikeness' there follow two tasks: (1a) As much as it is necessary for the methodological problem of theory comparison, to clarify the concept ' . . . being closer to the truth than . . . '; (1b) to develop *indicators* by means of which we can produce argumentatively good grounds for the conjecture that one of an actual

pair of competing theories in fact comes closer to the truth than the other. The indicators will be fallible but must and can be objective.[5] (E.g. whether or not a prognosis about, say, an eclipse is falsified is objective in the sense that the fate of the prognosis is independent of human influence.) As is well known, the notion of degree of corroboration is offered as a fallible indicator that sets out the balance of attempted falsifications to date. The role of experience here is exclusively that of a critical arbiter. No 'founding' is sought after. To interpret degree of corroboration as designed to function in the long run much like degree of inductive support – as some philosophers have suggested (e.g. G. H. v. Wright) – has no grounds in Popperian methodology. (2) Since degree of testability is equivalent with content of empirical information, the more content a theory has, the greater is the risk of falsification and thus also its corroboration potential. From the desideratum of increase in degree of corroboration (as a fallible indicator of increase in truthlikeness) follows that of content increase (potential explanatory power). This in turn sets methodology the *task* of clarifying the concept of empirical content and of providing *instruments for making content comparison.* (3) A dramatic increase in content is possible only when the successor theory contradicts the predecessor, for only then does it really introduce new concepts and open new perspectives, thus leading to deeper explanations and to deeper and deeper problems. This sets methodology the *task* of *explicating the concept of depth* and of providing an *indicator of an increase in depth.* One indicator that functions similarly to a sufficient condition[6] is that the successor theory, in attempting to explain the predecessor theory, *corrects* it, i.e. from the successor theory (e.g. Newton's theory) a hypothesis is deduced (e.g. Newtonian versions of Galileo's law of free fall or of Keplerian laws of planetary motion) which, although contradicting the original explanandum, may be regarded as an improved successor to the original explanandum (e.g. Galileo's law of fall), or the explanandum mathematically is an approximation within a limited realm (in the example, when the height of fall is negligible in relation to the earth's radius) to the improved successor hypothesis. (The original explanandum could be derived if we make the false assumption that the earth's radius is infinite or the height of fall zero.) The successor theory (in our example, Newton's) introduces a new sort of concepts (causal concepts) which are not used in the predecessor theory (Galileo's and Kepler's law hypotheses do not involve any causal concepts); and it is plausible that it is these new concepts that enable us to look at the world in a new way, which in turn makes possible deeper explanations and gives rise to new problems of a greater level of depth. Therefore *the desideratum* *increase in depth appears to hold a key position.* In this way theory appraisal leads to appraising metaphysics or cosmological hypotheses (as Feyerabend prefers to call them), to appraising the comparative value, fruitfulness for research, of competing world-picture hypotheses. Scientists were once deservedly termed 'natural philosophers'. The best of them did and still do face up to the philosophical issues posed by their own work. As Joseph Agassi has argued,[7] giving top priority in the explicatum of progress to degree of testability carries an anti-metaphysical flavor; we would add that it is a hangover from positivism.

Since 1941 Popper has drawn attention to the phenomenon that a deeper theory corrects the 'observational' law-hypothesis – independently of whether or not the latter has been falsified when the deduction is made – in the very process of explaining it.[8] It is important to notice the continuity in the empirical as well as in the mathematical aspects between the hypothesis corrected and the improved successor hypothesis deduced from the new theory, although the successor hypothesis contradicts the hypothesis that gets corrected. Thus, in spite of the break constituted by the new concepts introduced by the successor theory – the new perspectives it opens up and the new, deeper problems it poses – there still is an element of continuity in these two aspects.

1.5. In the Popperian ideal of science the essence of progress is seen in moving from problems to deeper problems. The Kantian-Popperian thesis of the propagation of problems claims that every solved problem generates new problems[9] (objectively, i.e. independently of the researcher's wishes, independently of whether or not he formulates them or even recognizes them). The deepening of problems is seen as a measure of progress.[10] "Science should be visualized as *progressing from problems to problems* – to problems of ever increasing depth."[11] When we confront this ideal of science with the picture of historically given science, we find that the history of science illustrates it well. Although this fact *per se* could not be used as a good reason for recommending the ideal of science, it nonetheless shows that the ideal is not utopian – as is the ideal of science of logical empiricism.

From the point of view of the history of the philosophy of science it is sweeping, but correct, to say that Popper is the chief critic of the methodology developed by logical empiricism and of the ideal of science at its root, an ideal which owes its decisive impulse to the philosophy of the early Wittgenstein. The polemic between Wittgenstein and Popper – which has remained implicit – in fact has a continuation: the most important contemporary critics of Popper are in turn indebted to the philosophy of the later Wittgenstein. This is true – in spite of all the differences between them – of Thomas Kuhn, Stephen Toulmin and Paul Feyerabend, to mention only the most well known. But in the long run they lose their grasp of the normative problem, and must lose it; and so Kuhn and Feyerabend see theoretical developments between which a 'scientific revolution' lies as incommensurable entities, similar to Wittgensteinian monadic forms of life which can only be evaluated from within and cannot be rank-ordered.[12]

1.6. Brief Comment on the Problem Situation as Reflected in the Literature

1.6.0. The problem situation arises from the attempts to clarify the ideal of science outlined above and to deal with the resulting methodological problems. There is a cluster of problems centering around explicating the concepts of content and truthlikeness. The approach generally adopted is to define logical content as the set of non-tautological consequences of a theory. But how could this be measured? Cardinal numbers obviously provide no viable measure. A measure of content has been defined only for certain very simple formalized language systems, using the concept of absolute logical probability as primitive. Aside from the fact that such model 'languages' have their value only as instruments for 'logical underpinning', this approach presupposes that one regards the concepts of absolute logical probability as at least as clear as that of content. (After all, one is the converse or complement of the other.)

Since in the most interesting cases of scientific progress the successor theory revises the predecessor theory, content comparisons have to be made between incompatible theories. Popper has pointed out that the successor theory has a greater content if the questions answered by the predecessor theory are a proper subset of those answered by the successor theory. (Of course, the questions that are of interest here are scientific questions, not just any questions.) D. Miller (1975) has questioned whether it is at all possible to ascertain that more accurate predictions are derivable from one theory as a whole than from its rival; Grünbaum has questioned whether it can be ascertained that one theory answers more questions than another.[13]

If one attempts to give an adequate explication for the idea of one theory's being closer to the truth than its rival, being a more accurate representation of the aspects of reality that interest us at the moment, the following approach seems natural: either the amount of information conveyed by the true consequences of the successor T' is larger than that conveyed by the true consequences of the predecessor T and its amount of false information not larger, or the amount of information conveyed by the false consequences of T' is smaller than that of T and its amount of true information not smaller. A set-theoretical interpretation of the comparative concept, called 'verisimilitude', takes the statement "the amount of information conveyed by the true consequences of T' is larger than that conveyed by the true consequences of T" to mean that the true consequences of T are a proper subset of the true consequences of T', and similarly, *mutatis mutandis* for the other parts of the definition. This interpretation proves inadequate if taken as an explicatum of our intuitive idea of 'more accurate representation than'. Miller (1974) showed that if one uses this set-theoretical interpretation of the definition of verisimilitude, the required subset relations can only obtain between axiomatizable theories if both are true. The next explicatum proposed hinges on the idea that the information conveyed by the true consequences of one theory minus the information conveyed by its false consequences must be larger than the information conveyed by the true consequences of the rival theory minus the information conveyed by its false consequences. This approach too has proved to be unfeasible. Andersson[14] has shown that some points of the criticism advanced by Miller and Tichý can be met, but concedes that there remains a fundamental difficulty which cannot be overcome even with finer measures of content and verisimilitude: it turns out that all false theories with the same measured content have the same degree of verisimilitude if this explicatum is used. This is an absurd consequence since, according to this explicatum, the verisimilitude of a false theory depends only on how much it says, not on what it says. Hence this explicatum does not meet the metacriterion of 'sufficient similarity' between explicatum and explicandum, a necessary condition for fruitfulness. Moreover the comparison of false theories is very important, since cognitive progress often consists in one falsified theory's being replaced by another which, although likewise falsified, is regarded as closer to the truth. (We need not to go to the history of science; a primitive example can illustrate this: the hypothesis 'The planets move in triangular orbits', although false, contains a kernel of truth (e.g. that the planets have closed orbits); the hypothesis 'The planets move in circles' is likewise false, but intuitively is closer to the truth than the first one.)

In the literature various positions can be discerned. Some writers go so far as to deny that our intuitive idea of one hypothesis being closer to the truth than its rival is fruitful. (For example, K. Hübner, A. J. Ayer and G. S. Robinson hold this view.) The rest would certainly agree that it is fruitful. Some of them have shown that the problem of explicating our intuitive idea via a concept introduced by a *formal* definition (i.e., a definition formulated in an idealized language schema, *IL*, based on standard logic) remains unsolved

(D. Miller, P. Tichý, G. Andersson). Undoubtedly this is true. If it should turn out to be unfeasible to make the intuitive idea more precise with this method, the absence of a formal definition will induce some to become sceptical about the value of the intutive idea for methodology (e.g. Miller), while others would question whether this sort of precision is necessary for the concept to be fruitful, and would still regard the intuitive idea as indispensable (e.g. H. Albert, G. Andersson).

1.6.1. In this situation it seems appropriate to recall the real issue here: our aim is to legitimate (in the sense of giving good reasons) a *preference rule*, which is based on a *rule of theory appraisal* – at the moment in the information-theoretical and epistemic dimensions. Hence the adequacy of the explicata proposed must be judged in terms of their fruitfulness for this methodological task. Throughout, of course, we must clearly distinguish between two sorts of tasks, that of explicating concepts and that of developing indicators.

1.6.2. *Some unorthodox reflections.*

1.6.2.0. Intuitively we distinguish between the information conveyed by a theory, i.e., the explicitly formulated theses, which constitute what the theory says, and the set of all the theory's consequences, which is independent of whether or not these consequences have been or ever will be formulated (or 'discovered'). In any case, the information of the theory, in the above sense, is so condensed that in practice it is difficult to test it directly. For this reason, in order to criticize a theory empirically we must derive testable consequences; we must extract from the information contained in the theory empirical information in small enough doses that it is technically possible to test it.

As is well known, in his classic exposition Popper defines the '*logical content* of a theory *T*' as the set of non-tautological consequences of *T*. Consequences whose truth rests on their logical form or on definitional conventions are, of course, irrelevant to our present task, since in this respect all theories are on a par. (For analogous reasons it would be pointless to compare the sets of consequences of inconsistent theories, since any sentence is deducible from an inconsistent theory. This demand for consistency thus remains, is necessary in principle, although P. Feyerabend is certainly right when he emphasizes that in research one deduces not x number of arbitrary consequences, but only those which represent a potential answer to a scientific problem, and that in the attempt to find an answer to a particular scientific problem an inconsistency between two of a theory's components need not always be relevant. On the other hand, when Feyerabend says that the demand for consistency would be unrealistic, since establishing consistency first requires axiomatization of the theory, which would take too long because in practice the theory would have already been superseded by then, this has to do only with methods of ascertainment, and not with the explication of concepts, which is what is here at stake.) At any rate, those consequences of a theory, which are generated by exploiting the peculiarities of the V-connective (as defined in standard logic), are quite irrelevant here, since they do not constitute possible answers to our scientific problems. In real science researchers do not make trivial deductions by joining to a deduced predictive hypothesis another hypothesis, connecting them with an 'or' – this would indeed be a 'philosophical joke'.

1.6.2.1. But, even admitting such qualifications, a *subdivision* is still needed within the set of consequences thus restricted. For in the context of theory comparison only a part of the synthetic consequences is relevant. These non-logical consequences are either metaphysical or empirical. The metaphysical portion of a scientific theory has metaphysical implications, implications for philosophical cosmology (Part II, Section 1.5). However, a theory will have such repercussions, such 'philosophical implications', only if it constitutes a major breakthrough, and a necessary condition for this is that the theory be regarded as closer to the truth than its predecessor. Hence it appears permissible to bracket the issue of a theory's philosophical implications. Given our aim (Section 1.5.), the class of relevant consequences can be limited even further. Since in the final analysis the issue is which of the competing theories is closer to the truth, what matters are the *testable* consequences. In some cases it will be possible to deduce testable consequences from the theory alone. *Given our aim, what matters are only such consequences as constitute potential answers to scientific problems* – either potential answers to our pressing scientific problems or potential answers to questions whose answers would eventually be of importance for improving our world-picture (cf. Part II, Section 1.5). Such consequences are as a rule not derivable from the theory alone. (For example, in order to be able to test Newton's theory of gravitation empirically, at least in astronomy, we need in addition, as auxiliary hypotheses, theories of optics.) As is well known, Popper defines the 'empirical content of a theory T' as the set of potential falsifiers of T. In the context of actual empirical criticism of a theory, what is relevant is the empirical content in the sense of the informative content of the set of potential falsifiers of T plus auxiliary hypotheses A (because normally such additional premisses are needed) i.e., the set of all conjunctions of appropriate statements of initial conditions and the negation of a hypothesis deducible from T in the presence of auxiliary hypotheses A, so that this conjunction contradicts the conjunction of T and A.

Thus it appears advisable to stipulate that the assertion that T' is better than T in the information-theoretical dimension is to *mean* that the information conveyed by the set of potential falsifiers for T' with the auxiliary hypotheses A necessary for deducing them is greater than the information in the potential falsifiers deducible from T and A. It is presupposed here that the hypotheses deduced concern scientific problems and that the theories are competitors, i.e., that they attempt to answer the same questions.

This yields a preference rule: *before* testing, prefer that theory for which there are good reasons to conjecture that its empirical content (in the presence of the necessary auxiliary hypotheses (Part II, Section 2.1.1) is greater than that of its competitor, that it is more falsifiable than its competitor.

1.6.2.2. All this has to do only with the issue of explicating concepts. The good reasons for such a conjecture hinge upon the use of some *indicator* (fallible but objective) of relative falsifiability. Such an indicator is, in principle, provided by the information conveyed by each of the sets of potential falsifiers *thus far* deduced as potential answers to our scientific problems. Thereby the proposed explicatum has plainly guided the production of indicators – as should be the case. In actual practice, of course, there is no usable measure of a unit of infoımation, and comparing two theories here seems feasible only if one theory entails the other. On the other hand, there is no point in content comparison anyway unless the theories are competitors, and ultimately the decisive question is whether what T' says in answer to the problems common to T and T' is a more accurate representation than what T says; what counts is the situation *after* testing. In brief, the problematic of content comparison seems, at least to this writer, to have received undue attention considering that the global aim is to legitimate a preference rule for the situation after testing.

In conclusion we can now return to the distinction initially mentioned. The information in a theory, the group of formulated theses constituting it, or, more accurately, constituting a particular version of it, is so condensed that in practice it is difficult to test it directly. The more general and the deeper a theory is, the more highly condensed the information will be. This is why the whole business of deducing testable consequences has to be gone through to make empirical criticism possible at all. Since every sentence (trivially) entails itself logically, the theses themselves are of course included in the set of the theory's consequences though not in the set of its testable consequences. However, although for our present aim only the content of the potential falsifier (or, in certain contexts, of the potentially falsifying hypotheses (Part II, Section 2.2.2)) matters, the two intuitive ideas – the content of a theory and the set of its consequences – are not identical. And it seems inadvisable to make them identical by definition. The hypothesis that a particular formulation of a theory has such-and-such content is again a conjecture for which good reasons can be provided, good reasons to be based upon our interpretation of the theory's theses. Making such a conjecture is a hermeneutic task. The conjecture may be criticized, *inter alia*, by confronting it with the information in the consequences including that in the 'metaphysical' component of the theory.

1.6.2.3. The above preference rule is applicable in the situation before testing. The simplest case would be that in which the two competitors give incompatible answers to the same scientific question. In this case, the theory that gives the correct answer or a more accurate answer than the rival does would be preferred. This consideration goes beyond the information-theoretical dimension. In the epistemic dimension, the assertion that T' is better than T is to *mean* (i.e., it is so explicated) that T' is closer to the truth than T. But this is just another way of saying: by 'truthlikeness' we *mean* that the theory is nearer to the truth whose answers to our scientific questions represent the relevant aspects of reality more accurately than those given by the competitor theory. The preference rule is: *after* testing, prefer the theory for which the conjecture that it is closer to the truth than its competitor is supported by good reasons. These good reasons will make reference to an indicator (fallible but objective): whether the potential falsifiers (or the potentially falsifying hypotheses (Part II, Section 2.2.2)) *thus far* deduced have or have not stood up to the empirical tests *thus far carried out*.

This is all that is needed to reach our aim. It is presupposed that in our language the idea of truth and the concomitant idea of truthlikeness (*mehr zutreffende Darstellung als*) function successfully. This may seem problematic. But it cannot be stressed too strongly: without the descriptive function (*Darstellungsfunktion*) there is no language in the full sense, no human language. Karl Bühler's work (1934) is highly pertinent here; Tarski's famous semantic definition of truth, *pace* Popper, is not.[15] It is, of course, a task of philosophy to clarify and explicate the idea of truth and its derivative concept of one hypothesis being closer to the truth than another. This is indeed a perennial task of *philosophia prima*. It cannot be a task for methodology (cf. Section 0.2, s.f. on the division of labor). Nor can methodology wait until 'first philosophy' has produced answers that are deeper and more relevant than those it has produced during the last two and a half thousand years. Hence to reject these concepts because we have not been able to provide (and perhaps never will be able to provide) an explication of them such that the explicatum is introduced via a formal definition couched in an idealized language schema *IL* based on standard logic would be pathetic, ineffectual, indeed even quixotic. Moreover it would be self-stultifying: the very argument for rejecting them would use the concepts of truth and truthlikeness and would presuppose their functioning – surely such an argument would claim to be true, correct, if it were to be taken seriously.

1.6.2.4. Thus we can conclude this section by returning to our starting point, the typology of positions on the issue of truthlikeness. I would join H. Albert and G. Andersson, but would also conjecture that the attempts to explicate the idea '. . . is closer to the truth than . . .' in terms of an *IL* will carry with them a repetition of the degenerating problem shifts we have witnessed in the logical positivists' attempts to explicate the concept of 'empirical significance' by means of the *IL*: a host of problems will be induced by the very instruments introduced in order to solve the original explicatory problem.[16]

2. CRITICAL RATIONALISM: THE PHILOSOPHICAL FRAMEWORK OF POPPERIAN METHODOLOGY AND IDEAL OF SCIENCE

2.0. Methodology is not self-sufficient. Just a few examples. The material of its *Gedankenexperimente* it gets from the history of science. It needs the history of science in many ways. Even in theory appraisal it cannot do without it if only to clarify what exactly is to be appraised. For instance 'the Newtonian theory' refers ambiguously to a historical succession of various formulations and to different versions having been developed in one and the same period. Hence historical studies are an indispensable preparation for getting started. Methodology has to import some of its tools from studies about formalized language and other studies using formalized languages. It interacts with, e.g., the studies of creativity, which are an interface between history and psychology. And so forth.

Methodology is (as mentioned in Section 0.2) not independent of other subfields of philosophy either: every methodology is embedded in a philosophical *framework*. On the other hand methodology interacts with some of these other subfields: e.g., while it presupposes work in ontology, it makes contributions to the philosophical anthropology of knowledge.

2.1. Critical Rationalism Rests on Three Pillars: Realism, Fallibilism and Meliorism

2.1.1. *Realism*. We do not use the customary label 'critical realism' because naive realism, the view that the world is what it appears to be, has been discarded since antiquity. Realism is a necessary presupposition of methodology. Falsification presupposes the idea of error, hence that of truth and of the idea that one hypothesis may be a more or less accurate representation/description of certain aspects of reality. Whether or not a hypothesis is falsified depends on reality: the idea of an experiment is that reality 'gives an answer' which is independent of human influence.[17] (To take a trivial example, whether or not gold is heavier than iron is something that cannot be influenced by human beings.) Ontological realism as the thesis that material entities 'exist' in the full sense (are, *inter alia* because of their independence, given full ontological status) or as the thesis of the existence of the external world and of other minds is scarcely contested. Its main support is the unattractiveness of its denial since ontological idealism relentlessly leads to solipsism, a position, which, even if it may be consistent, is patently absurd. Also its corollary, the thesis that only what (physically, materially) exists can be the object of cognition for the natural sciences, appears scarcely controversial. Epistemological realism is the thesis that at least the properties of physical entities, physical processes and the 'reality' of a physical event are independent of any process of cognition, in particular of observation. This sort of independence is a precondition for the possibility of objective indicators of comparative truthlikeness.

One can combine *epistemological idealism* (the denial of epistemological realism) with ontological realism. This position implies an *instrumentalistic* view of scientific theories. It has become increasingly popular among quantum physicists. Popperians would not distinguish 'theory realism' as a special sort of realism since such a distinction is the result of the artificial distinction between 'theoretical language' and 'observation languages', of the two-language approach characteristic of positivistic-foundationalist philosophers who wish to give 'observation sentences' or 'observation predicates' a privileged epistemological status. For Popperians a theory – this holds good for theories of quantum mechanics no less than for Newton's gravitational hypothesis – talks about the world and makes truth claims which are in principle the same as those made on behalf of a data sentence deduced from the theory in presence of suitable additional premisses. The epistemological idealists hold that the theories of microphysics do not represent/describe aspects of the micro-world, but are, rather, nothing but instruments for deducing testable consequences; hence they regard the theoretical system consisting of the theory to be tested plus auxiliary hypotheses as an opaque instrument for transforming input information into (new) output information. They claim that it is impossible to draw a sharp line between observer and object: micro-object, apparatus, and the observer constitute a black box that cannot be analyzed. [18] The scatters are attributable to the entire box rather than to the micro-object – the more so if the micro-world is allotted a sort of 'reality' which is being 'created' by the observation itself.[19] The contemporary tendency towards epistemological idealism and the instrumentalist view of physical theory connected with it is exemplified by many famous scientists and philosophers of physics. Recently Paul Feyerabend has also joined the club. Thus some hold that quantum theory does not deal with (say) elementary particles and their properties as 'existents', but only with 'experimental arrangements' (Philipp Frank); C. F. von Weizsäcker speaks of the 'unobjectifiability' of microphysical attributes; Heisenberg writes, "The conception of the objective reality of elementary particles has . . . evaporated . . . into the transparent clarity of a mathematics that represents no longer the behavior of the elementary particles but rather our knowledge of this behavior".[20] If 'represents' were taken in the sense of expressing (*ausdrücken*), the point of the passage would evaporate and the passage would be trivialized. 'Represents' must therefore be construed in the sense of describing. The passage says that the mathematicized theories of quantum physics constitute knowledge not about the behavior of elementary particles but knowledge about 'our knowledge of this behavior'. This is its point. But then according to this dictum physics or at least that part of physics has become one of the *Geisteswissenschaften*. Its idealistic thrust (anti-realism) has become unmistakably clear. These philosopher-physicists claim that epistemological idealism follows from quantum mechanics. Mario Bunge has shown that this is not so, but that, e.g., for the empirical indeterminacy interpretation of Heisenberg's inequalities, its "only support is a positivist-philosophy popular in the 20's and 30's".[21]

Popperians point out that an instrumentalistic view of theories totalizes one ingredient in the testing of theories: deriving a potential falsifier from the theory, in the presence of suitable additional premisses,[22] and that if this totalization is made, the success of a prediction becomes totally mystical: why can it be that we deduce from the theory successful predictions if the theory does not more or less correctly represent some of the aspects of the reality about which the predictions are made?

I would surmise that, while epistemological idealism and the concomitant instrumentalist view does not follow from quantum theory, the intellectual motive underlying it stems from the historical situation of quantum theory. Elementary particles have properties which do not fit in with the world-picture, neither with that of common sense nor with that built out of the contributions of classical theories. Hence the question 'What sort of entities are they?' becomes disturbing. All difficulties are avoided if one holds that the statements about the behavior of elementary particles are not descriptive, that they are nothing but fictions to be used as instruments for predicting what will happen in the laboratory when a certain experiment is carried out; in the last resort they are predictions about the perceptual experiences of the experimenters. If this gambit is adopted, the question of the ontological status of these entities does not arise – they are but fictions and the theories nothing but black boxes used as instruments. If so, then the results of quantum-physical research have no repercussions on the level of world-picture. The task to examine the interaction between the results of quantum-physical research and our world view is eschewed. Hence instrumentalism is a lazy philosophy. (It may be convenient for experimental researchers if it is used only as a short-term moratorium on metaphysical questions in the hope later on to be better equipped to deal with them.)

For Critical Rationalism the ontological pillar, realism, is a philosophical presupposition of methodology. The defence of the realist posit cannot be the task of methodology because a methodologist embarking on this enterprise has *eo ipso* turned ontologist. Nor can methodology aspire to develop ontological analyses of, say, acts of perceiving, thinking, etc. The division of labor within philosophy requested in Section 0.2 and Section 1.5 is indeed indispensable. But methodology gives problems to ontology: e.g. Popper's precious insight that certain problems are literally discovered poses the problem, for ontology, of accounting for their partial independence (of Popper's world-3 entities), which problem is at the same time a test case for any ontological groundplan.

2.1.2. *Fallibilism.* While realism remains a posit and a philosophical (input) presupposition of Critical Rationalism, the conjecturalist approach, fallibilism, is the result of the thorough criticism of foundationalist philosophies. Hans Albert[23] has convincingly argued that the justificationist approach (inductivism, which has led to the problem of the justification of an Inductive Principle) leads to a trilemma: infinite regress, vicious circle or stopping the justification procedure at some epistemologically privileged sentences (such as the empiricists' sense data statements or observation sentences or the rationalists' various apriorisms, or the 'transcendental pragmatists' (K.-O. Apel) quasi-transcendental conditions of possibilities).[24] But this means a breaking off of the justification procedure at some juncture, regarded as absolutely certain, which runs counter to the very demand of 'founding' in a non-dogmatic manner. Fallibilism is moreover a contribution which Popperian methodology makes to the refinement of our image of man (man as a researcher) and hence to philosophical anthropology. The later Popper contributes also his evolutionary theory of knowledge, which is both a generalization of and a broader frame for his methodology.[25]

2.1.3. *Meliorism or cautious optimism, the means for achieving progress: critical methodology*. While realism is a metaphysical posit and fallibilism the result of the criticism of the foundationalist approach to science, the label 'meliorism' could be used to express the flavor of Popperian methodology. When the demand for absolute justification is consistently upheld, then – this is the lesson to be learned from Albert's trilemma – *prima facie* the only available position appears to be scepticism. Popper is the first to have worked out an alternative to the pendulous movement in the history of philosophy between unfulfillable demands such as the foundationalist demands and the reaction to them, wholesale scepticism. The Popperian alternative holds that empirical knowledge cannot be proved to be true, but it can be improved. We know at least roughly what we mean by 'scientific progress'; such progress is possible not only in principle, but is also exemplified in the history of science; although there is no guarantee that we will be successful in the future, there is a chance of it. With the help of Popperian methodology the chances of realizing progress in the sense of the ideal of science outlined above (Section 1) are better than with any other presently available methodology. This is the bold promise of Popper and his followers. Everything depends on whether or not we can plausibly argue for this conjecture. While realism is a presupposition and fallibilism a result, the critical methodology is the answer to the question 'Given realism and fallibilism, what is it *rational to do* in research?'; and *meliorism epitomizes the recommended attitude towards research and methodology*. The criticistic methodology is a general theory of rational (purposive-rational) action. It has been generalized from methodology in the narrow sense and it has research as its paradigmatic field of application. But it is claimed to be applicable in principle at least to *all* sorts of problems, not only to problems associated with knowledge production.

The core of the critical approach may best be expressed in Popper's own words: It is "the general idea of intersubjective criticism, or in other words, of the idea of mutual rational control by critical discussion."[26] This general idea can be explicated (in the etymological sense of that word) by several interrelated principles. In Figure 1 they have been sketched by a few major rules: the 'master rule' bans all immunization strategies; it functions much like a meta-rule stipulating that no rules may be used that would prevent discorroboration, falsification. It is the core of the so-called demarcation criterion. From it follows the 'basic operational rule', which prescribes 'severe' testing not only for conjectures, but also for falsifiers, for procedures, etc. Derived from it is a 'preference rule'. It has two aspects: Prefer theories with higher content to those with less content, and, after testing, prefer, *ceteris paribus*, that theory which has the higher degree of corroboration. From fallibilism follows moreover the rule indicated in Figure 1 as 'revision clause'. Since there is no epistemological rock bottom, it applies to all components of the scientific enterprise including data sentences, and as, therefore, falsification cannot be conclusive either (since one of the premisses in the argument is not conclusive), falsification must not be exempted from possible revision. These global rules are to give some guidance to the research process. In Part II we will attempt to spell out in some detail how, according to Popperian methodology, research should proceed in order to facilitate achieving cognitive progress in the sense of Popper's explicatum of our intuitive ideas of cognitive progress.

Before embarking upon this outline a final remark about the *realm of application of the critical method*: it is far wider than science. The critical method is used to distinguish rational from non-rational procedures: a procedure is *rational* if and only if it adopts the *critical policy*. If from a particular statement a consequence has been deduced that is 'unacceptable', then this statement has been criticized to that extent. This concept of criticism is applicable also to normative-evaluative issues (except 'ultimate values', which for the believer, are by definition exempted from criticism so that statements about them do not form part of purposive-rational activity).

Within the rational manners of proceeding we find a descriptive *distinction between science and non-science*. To draw this distinction is a problem of explication: for certain goals – and we shall shortly return to what these are – the intuitive idea of science is to be replaced by a concept of science that is a better instrument for these particular goals. The intuitive idea of science, our concept to be explicated, is partially defined by the goal of the activity, cognitive progress: Cognitive progress is the goal of empirical enquiry in general; scientific research is that empirical enquiry that can demonstrate at least a minimum amount of method. Since the goal of research is cognitive progress, i.e., improvement, expansion and deepening of our knowledge about empirical reality, within scientific research the critical method or policy must include *empirical* criticism as an essential component.

This has two consequences. (1) The demand that theories must be tested, are to be subjected to empirical criticism, makes sense only if the theories are falsifiable. That falsifiability thus constitutes a component of the explicated concept of science is therefore a corollary of the insight that in empirical research the critical method must essentially contain empirical criticism. (2) Insofar as science is primarily seen as an activity, as research, methods and strategies are more important than theories. Now it is possible to interpret most theories so that under this interpretation they are falsifiable. But a falsifiable theory can always be rescued from a falsification by adding *ad hoc* hypotheses. From this it follows that a general method, a policy, is scientific if and only if auxiliary hypotheses are not introduced *ad hoc* or, if such an introduction is expressly declared to be a temporary, purely heuristic measure, then the method is scientific if and only if these hypotheses are retained only if they lose their *ad hoc* character. For this reason the question, "When is introducing an auxiliary hypothesis *ad hoc* allowed, and when is it illegitimate to retain an additional hypothesis, which was originally introduced *ad hoc* as a temporary heuristic expedient?" is a topical problem for every methodology. Popper's answer can be summarized as follows: (i) Introducing an additional hypothesis *ad hoc* is illegitimate if this is done to preserve the theory from falsification and if the price to be paid for this is a decrease of the theory's empirical content, that is, of the information contained in the class of potential falsifiers. For this reason the scientific method dictates that a potential falsifier must be specified in advance: that one must be able to say what kind of experimental result or observation one would recognize as falsifying the theory. The as yet unsolved difficulty consists in defining '*ad hoc*' objectively – to speak of the intention of the researcher would be to lapse back into psychologism. (ii) But the salient point is whether an auxiliary hypothesis which was originally introduced *ad hoc* as a heuristic expedient but without reducing the empirical content of the theory is retained even if there is no reason to assume that it will be testable independently of the theory and will stand up to such a test. The circumstance alone that such an *ad hoc* auxiliary hypothesis is falsifiable is insufficient. And so the core of the scientific method as critical method à la Popper is the *prohibition of immunization strategies*. The demand for the falsifiability of scientific theories is only a corollary of the requirement of the method of empirical criticism. The critical method is for still another reason more important than falsifiability. A non-falsifiable hypothesis belongs to the realms of non-science. This assertion is descriptive, not evaluative. But if a hypothesis claims scientificality and is simultaneously immunized against falsification, then it is not only non-science but also pseudo-science. This is a negative evaluation, and this version of the solution of the demarcation problem has an important function in political debate and critique of ideologies.

This answer of Popperian methodology to the question of what scientific method is relieves us of the task of first having to indicate what is meant by 'science' before being able to reflect on 'scientific method'. The critical method including empirical criticism is the distinguishing feature of science, since it is the core of the 'scientific method'. The prohibition of immunization methods plays an important role in research. The demand that theories be falsifiable results as a precondition for the realizability of the method of empirical criticism. In the context of comparing theories, on the other hand, falsifiability hardly plays a role, since the researcher is almost never faced with the task of choosing one theory from a pair, one of which is unfalsifiable, that is, has no empirical content at all.

Outside methodology, it is very important *within non-science to separate out pseudo-science*, because this is indispensable in combatting the pollution of the intellectual environment by theories masquerading as science although not falsifiable, and it is still more important to unmask a policy that immunizes falsified theories against acknowledging their falsification. The importance of the demarcation criterion in the political context can scarcely be overrated, considering how rewarding it is for a propagandist if he succeeds in having an ideological, non-scientific doctrine illegitimately profit from the prestige of science. No wonder communists insist on the title of 'scientific socialism'. At the University of Moscow, there even exists a chair for 'scientific atheism', although, of course the theme of atheism forms part of theological inquiry no less than that of theism, and thus is in principle outside the realm of empirical inquiry. That such inquiry is non-science does not speak against it; only when it pretends to be empirical science – as 'scientific atheism' or 'scientific theism' – does it turn into pseudo-science.

PART II. OUTLINE OF POPPERIAN METHODOLOGY

0. Among other things, I wish to make good the claim that Popperian methodology implies pluralism rather than pure falsificationism, hence that it is not a form of 'logicism'. Two diagrams will be used as a means of exposition.

Their function is twofold: first, to *portray* basic features of the methodology, and also to *identify problems* for the methodologist. The figures are, of course, dispensable, but are a convenient, space-saving device. Methodology, as conceived here (Part I), consists largely of hypothetical imperatives. According to the Popperian methodology, a research enterprise's chances of success are increased if the project is governed by these rules. The idea of success, cognitive progress, in the 'game of science' is explicated by filling out the ideal of science outlined in Part I. The 'cybernetic' models of research pictured by the two figures represent the gambits and moves recommended by the general rules (such as the master-rule of anti-conventionalism, the falsification rule and the preference rule) that follow from the explicatum of cognitive progress obtained from the ideal of science (N and NW parts of Figure 1). Since these methodological gambits and moves are intended to apply to any research enterprise worthy of the name, they must necessarily be schematic.[27] Applying them in a concrete research enterprise will involve taking the substantive preconceptions and presuppositions behind that enterprise into account. As in everything else, in any concrete research enterprise there are certain substantive presumptions. Of primary importance here are presuppositions about the general nature of the object(s) under investigation (cosmological hypotheses) and a programmatic definition stipulating how the discipline ought to look ideally, a program that arises from applying the general ideal of science to this particular discipline. (In Figure 1 the discipline investigating a realm of objects X has been labelled '$\hat{X}$', the 'X-ology' in question.) For this ensemble of cognitive (in part 'metaphysical') and prescriptive components we have proposed the umbrella title of 'internal steering factors' of the research enterprise.[28] (NW part of Figure 1, abbreviated as '*ISF*'.) If only by supplying criteria for appraising products,[29] the internal steering factors (which are close to a Kuhnian paradigm in one of its senses or the 'hard core' of a Lakatosian research program) give a group of various research enterprises a certain unity and direction, gather them together into a research direction, tradition, school, style of thought.

1. CONJECTURES AND CRITICISM (*'ENTWÜRFE UND ÜBERPRÜFUNGEN'*), VARIATION AND SELECTIVE RETENTION

1.0. According to Popper "science begins with problems".[30] Of course every question has its presuppositions, is askable, formulable only if some particular knowledge is presumed. Which comes first? This is the old question of the temporal priority of the chicken or the egg, or in philosophical parlance 'the hermeneutics of the question'. With respect to science, Popper sees successful movement as a progression from problems to deeper problems (cf. above Part I, Section 1.5). This process does not have any definite beginning nor end. It is an unending quest: there cannot be any internal limits to science since every solved problem creates new problems (the Kant-Popper thesis of problem propagation (Part I, Section 1.5)). Yet a problem may be suitably used to mark the beginning of a concrete research enterprise or even of a research tradition. In the literature Popperians are often accused of not considering the problem of where the problems come from because they concentrate only on criticism. We will argue that this reproach is unjustified. However, we want to point out immediately that the setting of priorities for sorts of problems (e.g. whether efforts should be concentrated on space research or molecular biology – just to take an example), on ranking *disciplines* for funding, etc., is a question of global research policy and not a problem of methodology. Methodological considerations presuppose that the task is cognitive progress in the discipline concerned.

1.1. Where Does the Problem Come From?

From where does one get the problem that sets a research enterprise in motion? There always arises need for an explanation: our 'pre-existing knowledge', no matter whether it consists of commonsensical background knowledge, which contains the precursors of scientific theories, or consists of scientific theories, is bound, sooner or later, to run into an observation it cannot explain. (In Fig. 1 the arrow from T_i to P_i.) This situation can become critical, namely when the observation contradicts the pre-existing knowledge or the pertinent theory. (In Fig. 1 this is represented by the arrow 'falsification' pointing at P_i.) The situation may be dramatic when the reigning theory encounters 'anomalies', when it either clashes manifestly with some experimental result or 'runs out of steam', as would be the case if, e.g., a set of 'observational' law hypotheses grows, which resists all attempts at explanation by means of the dominant theory, while an approach to explaining them by means of a newly conceived theory contradicts the reigning theory. An example might be spectroscopic laws before 1913.[31]

But our theories do not even have to have run into difficulties. Even if they are highly successful, there automatically arises a problem of explanation: to explain the key components in the explanans of the successful explanation, the theory by means of which we could achieve these explanations. This problem too is an objective problem: the researcher finds himself confronted with it or discovers it independently of his wishes and plans; it

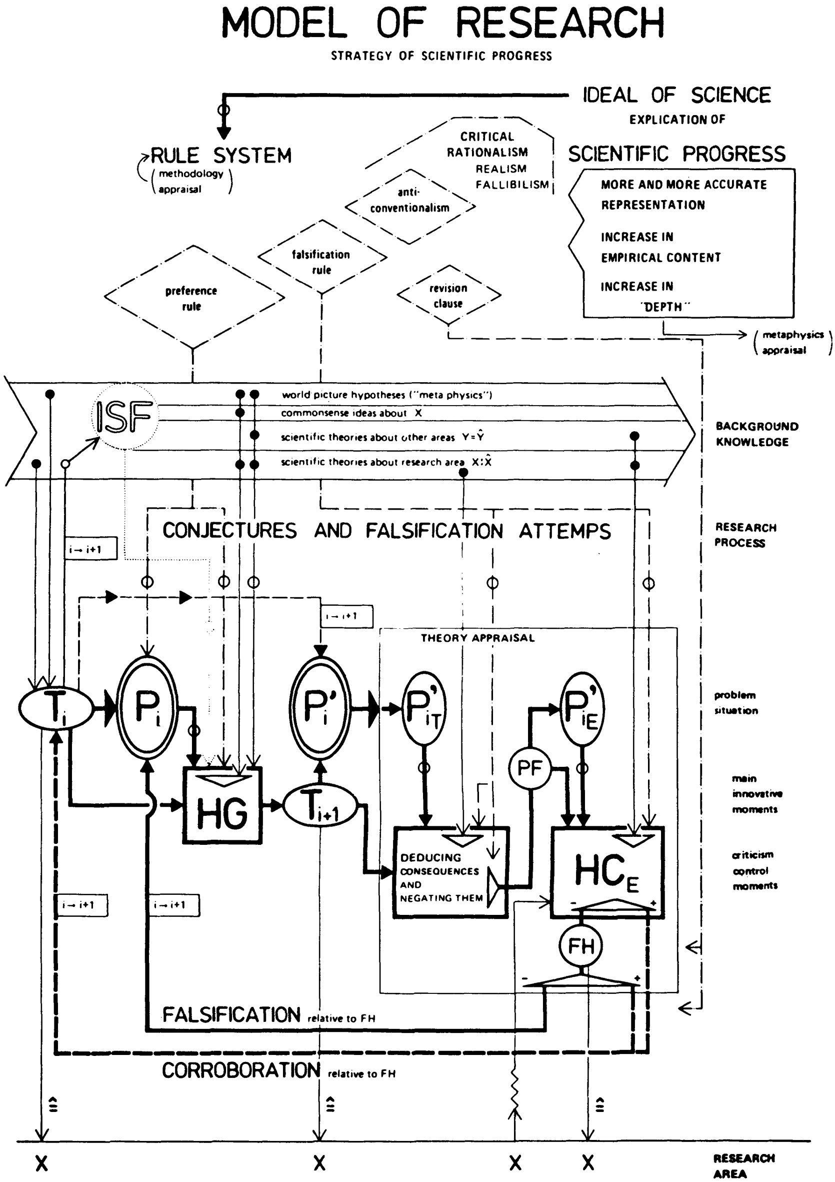

Fig. 1.
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arises as an unintended consequence of his research activity. This independence of the majority of problems is itself a compelling argument for attributing ontological status to them, as entities of Popper's world-3.[32]

In particular, one will attempt to extend a successful theory to new realms of application. And methodology also demands this. The 'preference rule' whose first part advises us before testing to prefer a theory with more content over rivals with less, urges us to generalize the successful theory. Sooner or later this will very likely have the result that the theory thus generalized runs into difficulties; it may get falsified, something that may eventually lead to an improvement of it, e.g. by making possible a more precise delineation of its range of application. (In Figure 1 this is represented by the arrow from the 'preference rule' to P_i.) Of course, the 'falsification rule' also demands that we continue to devise severe tests for the theory that hitherto has been successful – last but not least so that we can improve it, e.g. in the way just mentioned. These two 'rules' are intertwined since severe testing presupposes a high degree of testability, i.e. of empirical content. In sum, in Figure 1 the two arrows converging on P_i depict the two main sources of problems. Once a research enterprise has been embarked on in order to solve such a problem, a host of other problems arises on the way to its solution. Some of them are portrayed in Figures 1 and 2. These ensuing problems are obviously objective in that they come as unintended consequences of the decision to embark on that research enterprise.

1.2. Where Does the 'Predecessor Theory' Come From?

In Figure 1 only the two limiting ideal-typical cases have been portrayed: either the theory is one of the fairly well-developed scientific theories, at best the reigning champion theory of the discipline concerned (in Figure 1, $\hat{X}$); or it is embedded in the intuitive background knowledge, i.e., on the basis of preconceptions and presuppositions about the subject under investigation (in Figure 1, X), an attempt has been made to formulate an approach towards a theory, which in the beginning is naturally rudimentary. Thus in those cases where the field of study has just been opened up, in a hitherto unexplored realm of phenomena, 'metaphysics', cosmological hypotheses (in Figure 1, *ISF*) are structurally indispensable, and not merely indispensable as a part of psychological heuristics (world-2).[33]

1.3. Where Does the Successor Theory Come From?

In one sense, everything is permitted in hypothesis generation since the critical appraisal of the output comes afterwards, i.e. intermittently during the various stages of interim products. Only the structural characteristics of hypothesis generation are the concern of the methodologist. In Part I (Section 0.2) we mentioned that Popperians by no means treat the hypothesis generation as a black box whose study is to be left to the psychologist. Rather, psychological investigation or hypothesis formation needs the methodological study of structural characteristics (world-3) before getting off the ground.[34]

In Fig. 2 (p. 82) *h*ypothesis *g*eneration is represented by a box marked '*HG*'. This box has inputs from all levels of background knowledge. Hence its operation must include a selector. In Figure 2 this is symbolized by a frequency filter. The selection will be governed by the prior assumptions about the general nature of the phenomena studied. In Figure 2 this is indicated by the 'internal steering factors' *ISF* (whose operation has been mentioned in Section 1.0). When working within a research tradition, a selection criterion for both input assumptions and output hypotheses will be their coherence with the cosmological hypotheses of the tradition in question. When a dramatic shift in perspective is in the offing, the assumptions about the phenomena under investigation may conflict with the cosmological assumptions of the reigning tradition since an output theory (T_{i+1}) may conflict with the predecessor theory.

A possible source of input into the 'hypothesis-formation box' are theories about other sorts of phenomena, which may belong to the same or to another discipline. Hence in Figure 1 this input is shown as coming from $\hat{Y}$. ($\hat{X}$= abbreviation of the discipline studying the realm X, the X-ology in question, $\hat{Y}$= abbreviation for studies other than the one in which the theory under consideration is developed.) Example: Bohr conjectures a possible analogy between atoms (a realm about which no scientific knowledge existed at the time) and the planetary system (a realm about which full-fledged scientific theories were extant). Such a guess would belong to what is here labelled '*ISF*'. It governs input from the extant theories about planetary motion into *HG* to generate hypotheses about electrons, which are then subjected to falsification attempts. One expects that if there is an analogy there will also be negative aspects to it. Whether there are, and, if so, how far the analogy can be carried must be found out by empirical testing. In this way a basic conjecture of analogy provides a structural heuristic governing selection of input from neighboring scientific theories as well as experiments.

The overall operational characteristics of hypothesis formation (*HG* box) will be governed by methodological rules; in particular by the 'preference rule' (cf. Figure 1), because in order to make severe testing possible the output theory should have as much empirical content as possible: preferred are 'daring conjectures'.

We said above that in hypothesis formation 'everything goes' since criticism follows upon each interim product. (In actual research there is a continuous interplay between hypothesis formation and criticism – which cannot be mirrored by Figure 1.) However, inductive procedures will not be used – not even on the level of mental processes (world-2). This of course is a question for the psychologist to investigate, an interface between psychology of research and history of science. The methodological considerations strongly suggest that in mental processes a number of observed cases will only be recognizable as similar if at least an implicit, perhaps often subconscious conjecture has been made that they are entities that have something in common. That means that hypothesis formation as a mental process will never proceed in a way that would correspond to the algorithmic functioning of a Baconian inductive machine. (That is why Popper puts 'observational' law between shudder quotes; as there are no theory-independent descriptive statements, so there are no inductively, 'purely observationally-experimentally' produced law hypotheses.)

1.4. Appraisal of the Successor Theory

How is the output of the hypothesis-generation-station to be subjected to criticism, appraised? Most commentators regard this as the core of Popperian methodology. We hope to have made it apparent that the critical element is, however, comprehensible only as an integral part of the prescriptive picture of research. Obviously appraisal makes sense only in the light of a guiding ideal of science, an explicatum of progress. This ground has been covered in Part I. Figure 1 gives a skeleton diagram of research, Figure 2 a blow-up of the component theory appraisal.

The output of the hypothesis-generator automatically creates an objective problem: to examine the output critically, to appraise the successor theory. This criticism is basically a falsification attempt. It has two logical steps. To answer the question, 'What exactly does the theory say?', we have to deduce testable consequences, and in particular, consequences which enable us to test the theory severely. This stage is represented in Figure 1 by the left box within the box marked 'theory appraisal'. Then we have to answer the question, 'Is what T says true?', by ascertaining whether or not it stands up to the empirical tests made possible by the consequences deduced in step one. In Figure 1 the second stage is represented by the box marked '*HC*' (as short for '*h*ypothesis-*c*hecking/*c*ontrol'). The *HC*-box is provided on the output side with a decision triangle. The plus and the minus signs indicate the answer decided upon to the question, 'Did the attempts to falsify the potential falsifier (describing an individuated event) succeed or not?' If it has not succeeded then we have a falsifying hypothesis (describing a reproducible effect, which has a higher level of generality than the potential falsifier, which is a singular statement). In both figures the *f*alsifying *h*ypothesis is symbolized as '*FH*'. If we accept *pro tempore* the falsifying hypothesis, then there must be at least one false premiss in the argument for the consequences whose negation constituted the potential falsifier. This again automatically creates an objective problem; the so-called Duhem-problem: on which of the premisses to put the blame. In Figure 1 this is represented by a decision triangle at the output side of the theory-appraisal box. The minus and the plus signs indicate the possible answers to the question, 'Should the blame for the falsification be put on premisses other than the theory under test?' If this question is answered in the negative, the theory is falsified – relative to the falsifying hypothesis, that is, provided *FH* is not problematized. Since all empirical hypotheses are fallible, revision of *FH* is also in principle possible. Falsification cannot be conclusive since at least some of the premisses are not conclusive. Figure 1 renders this circumstance by means of an arrow from the 'revision clause'.

In Figure 1 two clear-cut cases are portrayed. In one, the falsification attempt has succeeded – which automatically produces a new objective problem: P_i has been transformed into P_{i+1}. With the new problem a new turn in the research enterprise begins, an iteration of the feed-back loop that has just been completed. But the new problem is more advanced, if only because one of the inputs into the *HG*-station is now the knowledge gained by the preceding investigation, including the falsification itself. This novel knowledge can help to refine the theory falsified, or, if you please, help to produce a refined version of it. In the second case, the falsification attempt has miscarried. Here the methodology advises us to regard T_{i+1} as corroborated to some degree – the degree of corroboration depending upon the severity of the testing – and to retain it until further notice, i.e., to continue to test it, to generalize it – but above all to regard it as preferable to the predecessor theory, at least in this respect.

1.5. Possible Repercussions on the 'Cosmological Hypotheses' on the World-Picture Hypotheses

Figure 1 indicates also that the replacement of the predecessor by the successor theory may have repercussions on our 'background knowledge.' It may have philosophical implications which feed back to the 'cosmological hypotheses'. In connection with a great scientific advance, where according to Popper the successor theory contradicts the predecessor, changes will have to be made in the assumptions at the level of the world-picture. Since the theory contradicts its predecessor it may turn out that in its construction the researchers presupposed (perhaps without being clearly aware of it) cosmological hypotheses that are incompatible with those underlying the predecessor theory, with parts of its *ISF*. In Part I it was suggested therefore that the 'scientific importance' of a question was maximal if successfully answering it would lead to a substantial change in our cosmological assumptions, in what hitherto we had taken for granted. If you prefer to use terms of the philosophic tradition, you may, instead of speaking of a feedback circle, say that this illustrates an aspect of the so-called hermeneutic circle or spiral,[35] a (non-vicious) 'circle' in the sense that the existence of certain presuppositions in its original, less adequate state was a prerequisite for developing the successor theory and therefore for refining, improving the preconceptions of the cosmological hypotheses, in particular by eliminating false items from them. These improved cosmological hypotheses will then form the 'internal steering factors' for the next turn in the scientific enterprise, in the unending quest. Thus even the 'metaphysical' components among the consequences of the successor theory or the cosmological presumptions underlying it are not immune to criticism emanating from empirical investigations, not totally immune to scientific developments, even if they are more resistant to change than the other components of the 'body' of knowledge at a given point in time. If this were not so, one would risk abandoning a set of 'internal steering factors' long before it had given a substantial part of the dividends in novel knowledge derivable from it.

2. THE COMPONENT OF THEORY APPRAISAL MORE CLOSELY EXAMINED

2.0. *On the Logical Structure of the Move of Falsification*

In the literature it is usually regarded as simply *modus tollens*. The sentence 'All swans are white' is indeed falsified by '*a* is a swan (statement of 'initial conditions *J*) and *a* is not-white'; '*J* and not-*P*' is a potential falsifier for 'All *J* are *P*'. (Incidentally, as Mario Bunge often has pointed out, of interest for methodology are scientific statements (e.g. Mendel's laws) but not 'Swans are white'. To use such sentences even for purposes of illustrating logical moves, in my opinion, carries with it a certain danger of 'model Platonism'.) For the hypothesis, 'The orbit of planet *M* is a circle', a potential falsifier may be a conjunction of four data sentences each describing an individuated event and based upon an observation of the planet's position. In this case the statement of initial conditions would be trivial, e.g. 'The heavenly body observed . . . is planet M'. From a logical point of view, to deduce not-T or not-A from $\vdash (T \,\&\, A) \rightarrow (J \rightarrow P)$ we need only J, P^*, and $\vdash P^* \rightarrow$ not-P. In actual research practice, however, the logical structure of the move of falsification often consists of two steps, a *modus ponens* part followed by a *modus tollens* part. (Its conclusion – this cannot be stressed too much – can never be the falsification of the theory tested, but only a disjunction.) The *modus ponens* part is portrayed (in Figure 2 in the left box up to but excluding the box 'negating'). Its structure is basically this. From the theory T and auxiliary hypotheses follows a testable consequence (in Figure 2, H_i).[36] The following empirical premisses are posited hypothetically: T, the theory to be tested, A, the conjunction of the auxiliary hypotheses we assume *pro tempore*, and J, the initial conditions we intend to produce in the experimental situation. The conclusion is P, a hypothesis describing a reproducible effect (*Vorgang*); it may then be particularized so as to describe a single event (in Figure 2, d_i).

In historical studies the empirical testing often concerns a singular sentence, and the use of the *modus ponens* part is explicit. Consider the following simple example. An archaeologist makes the conjecture that the house he is about to excavate was the house of a physician (J). How can he test J empirically? He may assume a theory about the way physicians' houses were built during the period concerned (T). T is not tested in the argument at hand, but used. From T and J in the presence of auxiliary hypotheses, a prediction, e.g. that the distance between two particular walls will be such-and-such (P). From P we derive, in the presence of singular statements about initial conditions (i), e.g. that we proceed with the excavation in a certain way and, eventually we derive a data sentence, e.g. that the archaeologist will find a particular structure of stones in a particular location (d). Then he looks at the fact, i.e. d is empirically tested.

The ensuing *modus tollens* part (right box in Figure 2) has basically the following structure. We cannot directly get the negation of the deduced consequences (not-P) as an experimental result; what we get is something positive, a description of a certain type of initial condition J together with a description of a certain type of reproducible effect P^*. The conjunction J and P^* has been experimentally established, with due regard for the principle of the fallibility of all empirical knowledge, of course. If P and P^* are incompatible, then from P^* not-P is deducible, hence not-P follows. From J and not-P not-(T and A) can be deduced; hence not-T or not-A, or both. This

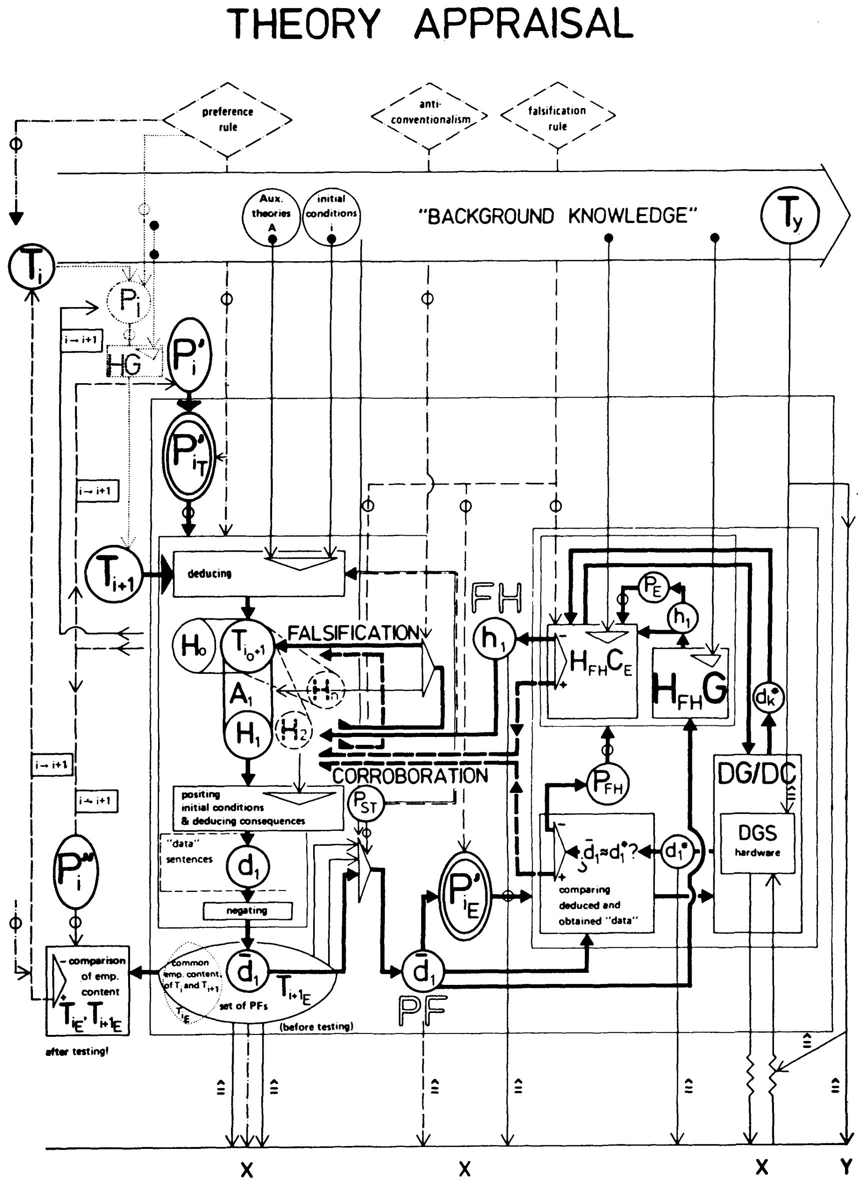

Fig. 2.

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leaves us with the options already mentioned above (in Section 1.4) as the so-called Duhem-problem. We will postpone discussing it until Section 2.3.

2.1. Close-up on the Information-Theoretical Part of Theory Appraisal

2.1.0. *Deducing testable consequences: to get a potential falsifier which will open the way for 'severe' testing.* The operational characteristics of the stage of obtaining a potential falsifier (*Falsifikationsmöglichkeit*) – in Figure 2, the left box – are governed by the 'principle of falsification or of severe testing'. In Figure 2 this is indicated by 'P_{ST}' as short for 'the problem of severe testing', the problem which regulates the selector at the output side of the theoretical component of appraisal. This oversimplifies the role of P_{ST}, but the diagram would be illegible otherwise. Of course, this problem already guides the deduction of testable consequences from the very start (in Figure 2, of the predictions $H_1, \ldots, H_n$). The point is to deduce consequences that constitute 'severe' testing, and for the sake of simplicity a triangle is used; there should really be a frequency filter ('*Schmalbandfilter*') here, since a test is a test only if its outcome is not known in advance. Hence deducing a hypothesis which already has evidential support independently of the argument in question, in a pragmatic way of speaking, 'an explanation', cannot constitute a test; nor can the repetition of the same test constitute a test in the full sense. In short, for an experiment to be a test, it must be about a deduced hypothesis, which contains information that goes beyond that carried by the 'background knowledge' or the predecessor theory. In Part I, Section 1.4 it was mentioned that, intuitively, a test is maximally severe if the potentially falsifying hypothesis, by contradicting one of the consequences deduced from the predecessor theory, conflicts with that theory. To clarify the idea of 'severe testing', the notion on which it hinges, the concept of 'background knowledge', has to be explicated.

2.1.1. *When can a successful prediction play a role in evaluating a theory?*[37]

On the problem of explicating 'background knowledge'. We here disregard the problem of determining when a prediction is successful and attempt to concentrate on the information-theoretical aspects of appraisal. What we want is a rule that tells us when it is rational to credit a theory with a successful prediction, and when not.[38] The rule must be such that it prevents the theory from being immunized against possible failure by the simple device of making deducible from it only such hypotheses as are already 'known'. i.e., which already have evidential support of their own before the deduction is made. Successful explanation is not a test of the theory. The idea of 'severity' is built into the idea of a test: to constitute a test of the theory, the information of the deduced hypothesis must go beyond what we claim to know on the basis of background knowledge.

The intuitive idea of background knowledge is roughly that it is all putative knowledge available at a certain time. In Figure 1 this is represented by the horizontal beam which is labelled in the right margin 'background knowledge'. In Figure 1 various inputs from this background knowledge are portrayed, with the corresponding selection mechanisms – for in a particular research enterprise only a small segment of 'what is known or assumed to be known' will be relevant. Moreover what is judged relevant will vary with the research tradition and of course with time, with the state of the discipline. This *maximal* interpretation of 'background knowledge' is seen to be problematic when we remember that one desideratum is 'boldness' of theories, i.e., that a theory should make predictions which are 'unlikely' in the light of our expectations based on what we think we know. If it is successful, we may say 'based on what we thought we knew'. Perhaps no more should be required than the following: background knowledge is the set of those theses which (are relevant for the scientific problem at hand and which) there is *at present no reason to doubt*. If more is required, viz. that we have reason not to doubt them, we seem to get entangled in circularity: if we were to require that they be theses that so far have a sufficiently high degree of corroboration, an explication of 'degree of corroboration' and hence of 'severity of testing' and hence of 'background knowledge' would already have to be at our disposal.

While the 'maximal' interpretation of 'background knowledge' appears problematic, the *minimal* interpretation is unproblematic. Testable consequences are deduced from the theory in the presence of auxiliary hypotheses and statements of initial conditions. The minimal construal of our intuitive idea of background knowledge explicates it as the information of the additional premisses, which at present we have no reason to doubt and are necessary for deducing testable consequences from the theory, i.e., for deducing consequences that constitute at least potentially an answer to our current scientific problems.[39] In Figures 1 and 2 background knowledge in this minimal interpretation is depicted as the output from the various selectors whose input comes from the beam labelled 'background knowledge'. The requirements that the consequences not be deducible from the additional premisses alone, i.e. that the theory's empirical information go beyond that of the additional premisses, is obviously needed since we want to test the theory, to extract information from it and not from the other premisses, which function in the argument somewhat like a catalyzer. However, it would be best to avoid the term 'background knowledge' here and to speak explicitly of the information conveyed by the additional premisses.

A hypothesis we have no reason to doubt at present (a 'fact') cannot count as a test for the theory if its information is necessary for constructing the theory, necessary in order to formulate T. (In Figure 1 this information is portrayed by the input into the box HG, hypothesis-generation.) Again the rationale behind this rule is that the outcome of such a 'test' would be known in advance. J. Worrall and E. Zahar have elaborated this 'heuristic' construal of background knowledge as the idea that 'a fact must not be used twice'. On the other hand, the genesis of the theory is irrelevant to its appraisal. If the theory contains more empirical information than that of the total input into the theory-generator (HG-box in Figure 1), then this new, additional information can certainly, if extracted in a suitable form, be the stuff for a severe test. In my opinion the above rule is justifiable only if the restriction is judiciously used: to prevent an allegedly new theory from merely summarizing the observations we have made, the 'data base' on hand.

However, since we are primarily interested in ascertaining whether or not the successor theory constitutes cognitive progress, it appears best to focus on the comparison of a successor and a predecessor theory, i.e. on the situation where there already exists a scientific predecessor theory and not just vague commonsensical knowledge. (In Figure 1 portrayed by the arrow from $\hat{X}$, the discipline in question, to T_i, the predecessor theory.) The foil against which the new theory, together with the additional premisses necessary for deducing a potential answer to one of our current scientific problems, operates is constituted by the system consisting of the rival theory and its auxiliary premisses. If the answers to this problem given by the two competing systems are incompatible, then the corresponding empirical test will be an *experimentum crucis*. In this situation we should be able to appraise the comparative achievement of the two competing *systems*, although of course not of the two competing theories alone. I. Lakatos and A. Musgrave have focussed on this situation. Like them I would place it in the center, but abstain from using the expression 'background knowledge' in this context.

2.1.2. *Deducing testable consequences: explanation vs. prediction.* The first step in the logical part of theory appraisal is deducing testable consequences, i.e., extracting information from the theory in chunks suitable for actual empirical testing. This is necessary because the theory itself condenses so much empirical information that it is not feasible to test it directly. In Figure

2 this stage is represented by the box marked 'deducing'. Its output is a growing system of deductive arguments such that the premisses of each argument have a common component: the theory (T_{i+1} in Figure 2). The conclusions are hypotheses that describe a reproducible effect (*Vorgang*). The arguments may be either explanatory or predictive. In Figure 2 one pattern T and $A \rightarrow H_0$,[40] portrays an explanation; the other patterns predictions ($H_1, \ldots, H_n$).

In an explanatory argument the conclusion is 'known', i.e. it has evidential support independent of this argument. An interesting case is that in which, although the pattern is still an explanatory one, some new knowledge is generated because the original explanandum gets corrected by the explanation. Popper has emphasized this most significant phenomenon of the improvement of an 'observational' law in explaining it with a more universal law or theory since at least 1941,[41] and has also pointed out its close link to the idea of depth: "... whenever ... a new theory of a higher level of universality successfully explains some older theory *by correcting it*, then this is a sure sign that the new theory has penetrated deeper than the old ones".[42]

This improvement by explanation is best dealt with by means of a simple example. Let the explanandum E by Galileo's law of free fall, i.e. his conjecture based on experiments and on many thought experiments that the acceleration of a freely falling body in a vacuum on earth is constant. Let H_0 in Figure 2 portray the Newtonian version of Galileo's law. H_0 contradicts E. But within a certain realm, for falls where the height above the surface of the earth is negligible in comparison with the earth's radius, the *numerical* values of E are so close to those of H_0 that this constitutes a good reason for considering E *mathematically* (and empirically) as an approximation of H_0, the hypothesis deduced by means of Newton's theroy, or H_0 as an improved successor of E. Although we say that Newton's theory explains Galileo's, what is 'assimilated' into Newton's theory as one of its consequences can only be the improved successor hypothesis of E. This is still more obvious if we consider Einstein's theory and Newton's. For a certain realm the consequences deduced by means of Newton's theory may be considered *mathematically* (and empirically) as approximations of the consequences deduced by means of Einstein's. But this relation of approximation holds only for certain *deduced consequences* of both theories. It would be inappropriate to take this as a good reason for considering Newton's theory, its basic theses, as an approximation to Einstein's since the latter introduces new concepts, new ways of looking at the world that are incompatible with the Newtonian perspective (Part I, Section 1.4).

The hypotheses $H_1, \ldots, H_n$ differ from H_0 only in that when the deductions are made they do not possess any evidential support. They constitute potential knowledge, so to speak. The respective deductive arguments yield virtual predictions: if T_{i+1} is a conjecture that has not yet been tested, the consequences deduced by means of it cannot profit from the theory's degree of corroboration. But in the present context the predictions function primarily to *test* the theory and we are only secondarily interested in the pieces of novel knowledge they may convey. Assuming that we do not have reasons now to problematize the additional premisses, the negation of H_i, viz. J and not P constitutes a *potentially falsifying hypothesis* for T_{i+1}.

How can we find out whether the negation of H can be generalized to a falsifying hypothesis describing a reproducible effect? Even H_1, describing a reproducible effect, condenses too much empirical information to permit testing it directly. Thus again from H_1 in the presence of statements of initial conditions (singular sentences) consequences are deduced, hypotheses of such a low degree of generality that we can directly confront them with the 'facts'. We can call them 'data' sentences; they have singular form and describe an individuated event. Such a data sentence is not a report of a perceptual experience. Reports about perceptual experience (e.g. about my own or NN's perceiving a certain pointer reading) do, as Popper long ago pointed out, form part of our reasons for accepting a certain data sentence, i.e. for seeing no point in questioning its correctness for the time being. Other parts needed in these good reasons will involve a theory of the hardware instruments used, of their functioning and the causal link between object of study and measuring apparatus, a theory of perception, and a theory of communication, since more than one observer is involved and the findings have to be formulated in language to be communicable at all. We shall return to this presently.

At this point the process of deduction ends. By negating the deduced data sentence and combining it with the statement of initial conditions (J & $\neg P$) we get a *potential falsifier* (*Falsifikationsmöglichkeit*) for H. The potential falsifier describes an individuated instance of a kind of state of affairs (*Zustand*) or process outlawed by the law hypothesis H. If even a single individual belonging to its realm of application exemplifies that state of affairs or effect, then the law hypothesis is falsified unless the blame is put on a different premiss (or premisses). Although this is so in logic, in actual research more is needed. Before returning to this we wish to make two comments.

We speak of a theory, e.g. T_{i+1} in Figure 2, when we assert that the successor theory is a common component in all the explanantia of the set of arguments which are outputs of the deductive operation. However, the successor theory is conceived as a theory with a career dimension, or if you please, a sequence of successive versions of a theory. In the course of testing the theory and of producing new knowledge by means of it, the theory itself is often processed, refined. Thus in terms of Figure 2 the theory in the pattern (T and $A \rightarrow H_0$) is really T_0: in the history of science it is typically a step towards a new theory, e.g. a central component of the later theory. T_k could stand for a full-fledged theory, which has grown out of T_0 or for a well-developed version of the original thoery. T_n may well stand for a so-called incorrigible theory (Heisenberg's term is '*abgeschlossene Theorie*'), i.e., a theory that cannot be substantially improved, processed into a 'better version', but can only be superseded by another theory in the sense in which Newton's theory was 'finished' quite early in its career but was eventually superseded by Einstein's. Of course the degree of corroboration of the theory also changes over its history: T has to be evaluated in terms of its career. Imre Lakatos has placed this in the center and Ernan McMullin is very likely justified when he views a Lakatosian research program as a theory over its history rather than a series of theories; but already in Popper's classic of 1934 he clearly emphasizes this career dimension – with the idea of appraising a theory in terms of past performance and the focus on the idea of improving a theory through testing it.

The predicted consequences (in Figure 2 H_1 to H_n) can function as potential corroborators of T; the explained hypothesis (H_0) cannot. This is clear from what has been said in Section 2.1.1 in connection with 'background knowledge'. In the measure in which the predicted hypotheses constitute severe tests and prove to be correct, the theory gets credit in terms of degree of corroboration – although always revocable credit. In this process the pattern ($T \,\&\, A \rightarrow H_0$), which was but a potential explanation as long as T had not been tested at all, turns into an explanation proper. Although neither H_0 nor the original explanandum E in our example can function as corroborators of T, T should get some credit for this achievement, i.e. for the explanation itself – apart from whether or not the hypothesis explained gets corrected in attempting to explain it. In Part I we mentioned that one requirement on the successor theory was that it should match the past explanatory successes of the predecessor theory; it should explain everything the predecessor theory explains with at least the same precision. This is usually regarded as a necessary condition for considering the change to be cognitive progress and nothing more is said about it. But should not the successor theory get some explicit credit also for this (explanatory power), since it is an achievement in itself? Having been able to match the past explanatory successes of its rival could serve as an indicator for 'conservation of corroborable content of consequences deducible from the predecessor theory at least in the mathematical and empirical aspects of that content'. For instance, Einstein's theory has but few corroborators in relation to Newton's but also matches all the latter's explanatory successes. We think that, apart from the fact that it may explain these hypotheses with greater precision, his achievement should also be taken into account in addition to the degree of corroboration, when good reasons are produced for the assertion that Einstein's theory comes 'closer to the truth' than Newton's. In any case, we would like to bring this matter up for discussion.

2.2. *The Empirical Component in Theory Appraisal* (right box in Figure 2)

2.2.0. *Comparing the negation of the deduced data sentence with the experimentally obtained data sentence; the role of technical data-generating systems.* The logical structure of this component has been dealt with in Section 2.0.

From the logical point of view a singular data sentence, incompatible with a data sentence deduced from H_1 in the presence of statements of initial conditions, J, if assumed true, permits the deduction of either not-H_1 or not-J. However, in actual research more must be requested. First, of course, the potential falsifier, the conjunction of the *negation* of the deduced data sentence and the statement of initial conditions, has to be empirically tested. As mentioned in Section 2.0, an experiment will yield a *positive* result, e.g., J and P^*, a statement of initial conditions together with a statement describing the observed event; and if P^* logically entails not-P (if $\vdash P^* \rightarrow \neg P$), P is falsified unless we go back to question the correctness of the statement of initial conditions. Thus basically the test of the potential falsifier consists in an attempt to falsify it by comparing it with an observed, an experimentally established data sentence about the type of event in question. In Figure 2 the *SW* box within the right box (empirical testing) depicts this comparison of the negation of the deduced data sentence with the experimentally obtained data sentence (where J is not problematized for the moment). The input into this box is the result of our experiment (in Figure 2 d^*). To get an experimental result one needs hardware instruments, *technical data-generating systems*. In Figure 2 these are represented by the box *DGS*. A technical data-generating system is based upon some physical theory (T_y in Figure 2). Hence in an obvious sense all data are 'theory-laden' to some extent.[43] But the data are thereby made dependent not upon the theory under test, but on the various theories *used* (not in this context tested) in this theory appraisal. The theories used are theories underlying the construction and manipulation of the hardware instruments as well as theories used as software, e.g., logico-mathematical techniques (used mainly in the information-theoretical part of the appraisal) or statistical theories used in connection with data-generation.[44] If only because the theories underlying the data-generation are in principle fallible, data sentences that could not be produced without these theories must themselves be fallible in principle. There can be no epistemically privileged 'basic' sentences.

2.2.1. At the output side of the 'comparison station' there is a decision point: the decision concerns the answer to be given to the question, "*Has the attempt to falsify the potential falsifier* (J & $-P$) *succeeded?*" If the answer is positive (in Figure 2 indicated by a plus-sign at one end of the decision-triangle), i.e., if the negation of the deduced data sentence has been falsified (J being considered to be fulfilled), this potential falsifier is to be rejected and the theory has received corroboration. If, on the other hand, the potential falsifier (in Figure 2, not-d_1) has withstood testing, then the researcher automatically has the new problem of finding out whether or not the potential falsifier, thus established, can be processed into a falsifying hypothesis (in Figure 2, P_{FH}).

Before continuing, a remark may be in place about the accusation of conventionalism often voiced in the literature with respect to the procedures just outlined. The decisions involved are neither based merely on conventions nor do they give rise to conventions. They are based on good reasons. In principle any of the data sentences may be questioned any time. But it would be stultifying to do so without good reason. For instance, one may decide to stop re-checking a particular statement about initial conditions or re-checking the experimental apparatus simply because there are at the moment no reasons for supposing the statement to be incorrect or the apparatus to be working improperly. This may turn out to be wrong – and may get corrected. But here a convention has been established only in a Pickwickian sense. The decisions are based on good reasons; they are controllable and corrigible and if necessary often get corrected by the objective method of falsification. (As was pointed out in Part I, this correction is objective since the outcome of a properly conducted experiment cannot be changed by human intervention.) Decisions are unavoidable in research. But if the moments of decision were considered to be all that mattered in research, then this methodological view would indeed merit the label of 'conventionalism'. It is as much a totalization as the opposite pole, 'logicism', the view that the logical moves are all that really matter in research. Conventionalism would imply an instrumentalist view of theories. In the Popperian schema the moment of decision is kept in balance by theory realism: by the view that whether or not a hypothesis gets corroborated in an empirical test is 'decided upon' by reality itself so to speak, ascertainable in an perfectly objective way. Fallibilism by no means excludes the objectivity of indicators and methods.

2.2.2. Let us return to the *problem* we left: *Can the potential falsifier be processed into a falsifying hypothesis?* (P_{FH} in Figure 2.) First it is conjectured that J together with the experimentally obtained data sentence, which conflicted with the data sentence deduced from H_1, describes an instance of a reproducible effect. This is again a move that can be labelled 'hypothesis generation' (in Figure 2, the box $H_{FH}G$) whose output is more general than a data sentence but less general than the hypothesis H_1. It describes a reproducible effect; it claims that for all occasions the conditional ($J \rightarrow P^*$) obtains. It is a *potentially* falsifying hypothesis for H_1 and A. (In Figure 2, h_1 in the upper middle of the right box.) In order to get it corroborated, if possible, we have to test the conjecture, attempt to falsify it. In Figure 2 the testing process is portrayed in the box $H_{FH}C_E$. For the purpose of testing, further, additional data are requested from the data-generating system (arrow from $H_{FH}C_E$ to DGS). The data obtained constitute one of the inputs into the testing station. At the output side is again a decision point. The minus sign at one end of the triangle represents the case where the attempt to falsify h_1 has miscarried. We have now a falsifying hypothesis proper, not only a potentially falsifying hypothesis. This process can also be described in sociological terms or, better, it has its counterpart in Popper's world-2: To secure the objective status of the falsifying experiment, to secure intersubjectivity, other researchers must be able to repeat the experiment with the same result. Otherwise the scientific community will not accept the hypothesis as a falsifying hypothesis. But when a hypothesis has been accepted (*pro tempore*) as a falsifying hypothesis, the severity of any future repetition of the same empirical test will be zero.

A law hypothesis may get corrected in attempting to explain it if the conclusion of the explanatory argument can be considered an improved successor of the original explanandum. Analogously for the falsification of a predicted hypothesis: it may give rise to a refinement or, if you please, to an improved successor of the predicted bypothesis, which was falsified. Again a very simple example may save words. If H_1 stands for a rudimentary so-called gas-law (whose core is the formula pv = constant, at constant temperature), a certain experiment will falsify it, and the falsification leads directly to a more refined version, a somewhat more precise successor hypothesis H_1' stating that the relationship holds only for temperatures within certain specified limits. Successful attempts to falsify H_1' in turn to give rise to further refinements, further specification of the realm of validity of the law-hypothesis.

2.3. Deciding Which Premiss is to Blame for the Prediction Failure: The So-called Duhem-Problem

We ended the outline of the logical structure of the falsification move (Section 2.0) by drawing attention to the options left by the conclusion either not-T or not-A or both. Let us have a closer look at the two[45] options, to save the theory or to retain the auxiliary hypothesis. (Assuming that there are good reasons for not questioning the reliability of the experiment or the correctness of the deduction of P, for not deciding to ignore the contradiction or for postponing judgment etc.)

Option A: change the auxiliary hypothesis, i.e. replace A by A′ in order to save T. A' must meet certain requirements: it must be incompatible with A and the experimentally established hypothesis P^* must be deducible from T and A', i.e. T and A' must provide a potential explanation of P^*.[46] Potential because to begin with the explanatory pattern is *ad hoc* and will remain *ad hoc* until A' has been corroborated, until there is independent evidence for A', i.e. evidence apart from the experimental result (J and P^*). Hence this *potential explanation of the experimental results does not by itself* provide a good reason for deciding to replace A by A' and to retain T.

Option (B): Replace T by T while retaining A.* T^* must meet the following requirements: T^* must contradict T since T^* must enable us to deduce a sentence that contradicts P, and T^* in the presence of the old additional premisses A must provide a potential explanation of our experimental or observed result (J & P^*). However, even *if a theory becomes available that fulfils both requirements*, this does *not by itself* entitle us to regard option (B) as obligatory, since A does not follow from the theory's fulfilling both requirements, and hence it does not follow that T is assuredly false. These two negative rules are already some contribution the methodologist can make to the Duhem-problem.

Examples will substantiate the above considerations. Let T stand for the Newtonian theory of gravitation, A for auxiliary hypotheses including a description of the planetary system without Neptune, J for statements of initial conditions such as some initial position and momentum of Uranus at some specific point of time; let P stand for the false description of the orbit of Uranus deducible from these premisses, and P^* for the hypothesis that correctly describes the observed orbit of Uranus. If one uses option (A) in this example, then A' differs from A in that it contains a different description of the planetary system, viz. it includes an existential hypothesis about the planet 'Neptune' (not contained in A).

Example No. 2 (used by Elie Zahar): T stands again for Newton's theory, A for a conjunction of ordinary auxiliary hypotheses including a description of the solar system which includes a singular statement assuming that the sun can be treated as a point-mass, J for the statements such as some initial position and momentum of Mercury at some specified point of time, and P the false hypothesis that the orbit of Mercury is a stable ellipse. P^* stands for a hypothesis describing (correctly) Mercury's perihelion's precession. (In Figure 2 it would be portrayed by the falsifying hypothesis h_1 which appears in the middle between the two large boxes.) T^* stands for General Relativity Theory. A' differs from A in that in it the above assumption about the sun is replaced by the hypothesis that the sun's density is not evenly distributed (a singular statement, although of a higher generality than one describing an individuated event).

Since in none of these situations can logic alone tell us which option is rational, *what good reasons can be adduced for opting for one rather than for the other*? Everything hinges upon whether or not the potential explanation can be processed into an authentic explanation, i.e., *whether or not the new component originally introduced* ad hoc *into the explanans REMAINS* ad hoc. Only if it gets corroborated, gets evidential support independent of this explanation, is option (A) the rational choice. A methodology, whose rules must *ex definitione* remain schematic, since they are to cover all kinds of research enterprises, cannot give the researcher more advice – he must decide everything else for himself, case by case.[47]

In the Uranus example the singular statement about the existence of an additional planet gets straightforward independent support when the postulated planet has actually been observed. Holding that the blame for the falsification of the hypothesis deduced from Newton's theory can be put on one of the singular premisses of the argument enables the alleged falsification to be turned into a triumph for the theory. In the Mercury example there is good reason, sufficient for rational action, for rejecting the option to change the additional premisses while retaining T if it appears impossible to provide independent evidence for the assumption about the uneven distribution of mass in the sun. This good reason becomes almost compelling if, in addition to this, the successor T^* has been corroborated, has gotten independent evidential support by making possible the deduction of novel knowledge. But this development is not a necessary condition for having good reasons to abandon T, i.e., for rejecting the rescue operation, which introduces A'. If A' does remain *ad hoc*, then, even without any available successor theory, T would have to be considered falsified, i.e., to have some falsity content. But since it has so many other successes to its credit it would still not be irrational to continue to work with T or on refining T.

In Figure 2 the large triangle between the two boxes represents the decision point described by the Duhem-problem. Where to put the blame for a falsification of a testable consequence is something only the researcher himself can decide. But methodology does have an uncompromising prohibition: "If you decide to put the blame on the theory, you must *never* attempt to repair the situation by *reducing* the empirical content of the theory. This is forbidden by the master rule, the 'anti-conventionalist' rule which outlaws any strategies that would immunize a theory against criticism." In Figure 2 the arrow from this rule to the 'Duhem-triangle' marks this guiding precept. In addition, methodology tells the researcher that in constructing good reasons for his decision everything depends upon whether or not the component introduced in response to the challenge of the falsification – be it A' or T^* – remains *ad hoc*. If A' or T^* has just been invented and lacks independent evidential support, the *ex ante* hypothesis that it will or will not remain *ad hoc* is a risky conjecture. The researcher himself is in the best position to make such a conjecture.

2.4. The Situation after It Has Been Decided Whether or Not the Successor Theory is Falsified, at Least in Its Present Form

If the successor theory is falsified, then this situation automatically creates a new problem, a return to the problem that was the starting point of the research enterprise in Figure 1, but now on a higher level. In Figure 2 this is portrayed by the arrow which runs in the left margin from T_{i_0+1} to P_{i+1}. This turn brings us back to Figure 1, although P_i is now replaced by P_{i+1}. It is needless to point out once again that falsification cannot be conclusive since some of the premisses in the argument cannot be conclusive. It is fallible, like all our empirical knowledge, yet it is perfectly objective.

If the falsification attempt has failed, the successor theory has thereby been corroborated to the extent corresponding to the severity of the test involved. Then the problem automatically arises of appraising the comparative achievements of the predecessor and the successor theory in other areas. In Figure 2 the arrow that runs downward in the left margin depicts this move and this brings us back to the themes dealt with in Part I.

If T has been replaced by T^* or A by A', this move automatically creates the urgent problem of testing the new component in order to find out whether our provisional faith in it is indeed justified. Truly an unending quest.

EPILOGUE

From the Quest for Certainty to the Search for Cognitive Progress: The Non-Foundationalist, Meliorist View of Human Knowledge

With a brief glance backwards into the history of ideas, one can see in Popper a Copernican revolution not only in methodology but also in our view of human knowledge: the change from a justificationist to a non-justificationist, fallibilist view of human knowledge. Impressed by the advent of the *scienza nuova* and by the triumph of Newtonian physics, philosophers committed to a foundationalist view of human knowledge saw their task in proving scientific knowledge to be true. This quest was basically a theologoumenon: the idea of scientific knowledge was modelled upon the idea of revealed knowledge, which for the believer is infallible. This is reflected in the ordinary-language use of 'to know' and is particularly clear in scientism. If a foundationalist program is to be attractive, one needs an infallible source of knowledge. The philosophers sought this either in an empiricist basis (e.g. Bacon) or in an intellectualist foundation (e.g. Descartes).

Hume's insight that, even if a secure foundation were granted, since the evidential basis is always finite, the justification of a general statement would need an amplificatory move discredited the whole approach. A vain search ensued for a Principle of Induction that might bridge the gap, and the strict foundationalist was left with either infinite regress or resort to apriorism. This intellectual experience opened the gates to irrationalism on a large scale, not only in epistemology but also in political thinking – e.g. the disastrous influence of Rousseau.

Kant attempted to rescue a place for rationality by means of apriorism, an apriorism which in a way combined elements of both the above-mentioned foundationalist scheme, with his synthetic *a priori* truth. But his examples lost their status, some of them in consequence of scientific developments.

The logical empiricists *a limine* rejected the idea of a synthetic *a priori*. They wanted to construct an inductive logic that could answer Hume's objections by yielding a measure of degree of inductive support applicable also to general statements from a finite evidential base. They were more impressed by the way the crisis in mathematics had been resolved than by the crisis in physics; and they took metamathematics as their model for philosophy of science. They even flirted with the idea of finding a secure foundation in observation sentences or observational predicates; and even if this foundationalism has by and large been abandoned nowadays, their ideal of science is clearly within the foundationalist line (cf. Part I, Section 1.1, 1.2).

Popper's work signified a Copernican revolution both in methodology and in epistemology: he produced the first worked-out non-foundationalist view of human knowledge. His criticism of both the solutions offered and the problems raised by logical empiricism has been dealt with in Part I, Section 1.3. A critique of Kant's rescue attempt on Popperian lines would run: that the synthetic *a priori* truths which for Kant were paradigmatic examples have dissolved;[48] that the question of whether there is a synthetic *a priori* needs first a more precise distinction between *a priori* and *a posteriori*, which must partly take recourse to psychology, and second a precise interpretation of an alleged synthetic *a priori* sentence so that we could examine its status. But the most decisive question is: is the conceptual structure one gets by crossing 'analytic/synthetic' with '*a priori/a posteriori*' fruitful for methodology? That does not appear to be the case – at least to this writer. Are the sentences which Kant viewed as synthetic *a priori* truth 'conditions of the possibility' of 'experience'? To answer this query we would also have to clarify Kant's concept of experience. However, we would transform the question into: Are there conditions of the possibility of *cognitive progress*, and if so, which? A Popperian would answer this last question in the affirmative, and would in this sense be a Kantian. Our capacity for conjecturing creatively, for proliferating proposals, as well as our capacity to learn from our errors– in sum the critical attitude and the criticist method – he would see as *conditions of the possibility of cognitive progress*, although not a guarantee of such progress. After the foundering in principle of all foundationalist philosophizing,[49] how can rationality be restored to its rightful place? By the critical method. Rationality can also be given a place in political activity by the method of criticizing and of testing proposals.[50] This is of course a rejection of utopian schemes referring to 'the totality' and ending in totalitarianism.

NOTES

1 Cf. Radnitzky (1978b), Section 1.2, and Radnitzky (1979b).

2 Radnitzky (1972) and Radnitzky (1974b) attempt to spell out this way of looking at research.

[3] Cf. Watkins (1978) in Radnitzky and Andersson (1978); see also Radnitzky (1980).
[4] This is the main thesis of Radnitzky (1968/1970, Vol. I); (see Radnitzky, 1972).
[5] Since normative-evaluative utterances can be questioned in a way in which descriptive statements cannot, this alone is a sufficient reason for the distinction between facts and values, between 'Is' and 'Ought'. This is spelled out in Radnitzky (1979c).
[6] Cf., e.g., Popper (1972), pp. 196–204, esp. p. 197.
[7] Cf. Agassi (1975).
[8] Cf., e.g., Popper (1972), pp. 204f; Popper (1963), p. 222; Popper (1975), p. 75.
[9] Cf. Kant (1783), Section 57, in the edition Kant (1911), p. 352.
[10] Cf., e.g., Popper (1963), p. 222; Popper (1972), esp. p. 118; Popper (1975), p. 75.
[11] Cf. Popper (1963), p. 222.
[12] This is spelled out in Radnitzky (1977), Section 6.
[13] Cf., e.g., Grünbaum (1976) and Grünbaum (1978).
[14] Cf. Andersson (1978) and comments on it in Radnitzky (1977), Section 1.34.
[15] Cf. Radnitzky (1977), Section 1.34, on Popper's use of Tarski and also Section 1.2 on the good reasons for working with formalized languages.
[16] Cf., e.g., Radnitzky (1968/1970), the chapter on Empirical Significance.
[17] It cannot be influenced in the same way in which, say, a norm expressed by a statement such as, e.g., 'It is forbidden to . . . ' can be revised by human action. The connection with the distinction between 'Is' and 'Ought' has already been mentioned. The importance of this distinction for liberal democracy, its role in the political philosophy of Critical Rationalism, is examined in Radnitzky (1979c).
[18] In Figure 2, *X, DGS*, together with the observer, all merge into a black box. For a penetrating examination of this idea of the 'participation' of the observer in the 'creation' of reality cf. Jammer (1977).
[19] For a brief critical examination of this trend the reader is referred to Jammer (1977).
[20] Cf. Heisenberg (1958).
[21] Cf. Bunge (1977a), p. 151; see also Bunge (1973) and Bunge (1977b).
[22] The left part of the box 'falsification attempts' in Figure 2.
[23] Cf., e.g., Albert (1968/1975), pp. 11, 13, 15, 24, 27, 31, 35, 56, 61, 72, 170.
[24] Cf. Hans Albert's critique: Albert (1975).
[25] Cf., e.g., Popper (1972), and Popper and Eccles (1977).
[26] Cf. Popper (1934–1959), p. 44 n. 1, and Popper (1934), p. 18.
[27] Cf., e.g., Radnitzky (1974b), pp. 7f. The claim that the rules apply to all research should not be taken to imply any form of a 'unity of science' thesis. We only wish to imply that the *sciences humaines* – a very heterogeneous group – also contain certain facets (explanation and criticism of explanations and descriptions) to which the model of Figure 1 and 2 may be adapted. Again, to claim this is not to deny that the typical humanities, the *Geisteswissenschaften*, e.g. philology, may have additional methodological problems of their own not covered by these models: there is a great difference between the task of 'giving an explanation of understanding' and the task of 'understanding explanation'. One such peculiarity is the phenomenon of self-fulfilling (self-stultifying) prophecy. Whenever a prediction refers to a process which may be influenced by the action of those whose future behavior is predicted, communicating the prediction to these people may influence the outcome. Hence, if it cannot be excluded that the predicted effect may be due to a *self-fulfilling prophecy*, the prediction *cannot* constitute a *test* of the

theory by means of which the prediction was made, and hence a successful prediction cannot corroborate the theory. The difference between, e.g., whether forecasting and economic forecasting (if people believe the prediction of a bullish tendency on the stock market, there will be a bullish tendency!) is a facet of the distinction between '*Is*' and '*Ought*' (cf. here too Radnitzky, 1979c. Section 2.1).

[28] E.g. in Radnitzky (1972), (1974a), (1974b).

[29] For example, if you have made the assumption that the real system studied sufficiently approximates an isolated system, then you will suppose that a deterministic system can be used to model it and it will be reasonable to require of your law hypotheses that they be universal in form; if you regard it as an 'open system', you may be willing to accept statistical laws. Such a 'metaphysical' posit will influence your programmatic definition of your discipline. E.g., your conception of what a human being is will influence your view on whether the discipline of psychology should be much like a natural science or should be a *Geisteswissenschaft* or a *Handlungswissenschaft*.

[30] E.g., Popper (1972), p. 144.

[31] Cf., e.g., Radnitzky (1972), Section 223, and Radnitzky (1974a), p. 86.

[32] Cf., e.g., Popper (1972), pp. 160, 118f, 147.

[33] Cf., e.g., Popper (1972), pp. 160, 118f, 147.

[34] Popper's 'transference law', e.g., Popper (1972), p. 114.

[35] Popper would be able to agree with this; e.g. (1972), p. 259 "growth of our knowledge . . . as consisting throughout of corrections and modifications of previous knowledge", or (the growth of knowledge) "is largely dominated by a tendency towards increasing integration towards unified theories" (1972, p. 262), or p. 71 "the growth of all knowledge consists in the modification of previous knowledge"; cf. also (1959, p. 276).

[36] Usually rendered by the meta-sentence: $\vdash [T \,\&\, A \rightarrow (J \rightarrow P)]$.

[37] This problem is dealt with in details in, e.g., Radnitzky (1979a), Section 2.2.

[38] The inductivist's answer is 'always'. Because he wants to be able to stylize conditionals with the connective for material implication, for 'All swans are white' everything that is not a non-white swan functions as a potential satisfier although intuitively cases of non-swans are completely irrelevant for the 'credibility' or estimated truthlikeness of the conditional. Among other things, the inductivist's answer leads him to make the paradoxical claim that the richer theory is easier to probabilify than the less rich theory. Cf. Musgrave (1978).

[39] Of course it would be pointless trivially to proliferate consequences by making use of the peculiarities of the v-connective.

[40] Expressed in the symbolism used in Section 2.0. H_0 might be $(J_0 \rightarrow P_0)$.

[41] Cf., e.g., Popper (1972), pp. 204f (first published in 1957).

[42] Cf., e.g., Popper (1972), p. 202 or Popper (1975), p. 97.

[43] Popper (1934/1959), p. 107, n. 2; Radnitzky (1974a), Section 2212 (p. 80) and n. 75 and Radnitzky (1974b), p. 24.

[44] These 'auxiliary' theories or even the hardware itself can take the lead and research can become 'governed' by the instruments instead of being oriented toward the original scientific question. Such a problem shift, if temporary, may constitute an interlude with a high growth rate of new knowledge, in which new hardware techniques and instruments make possible the production of new knowledge, or new software, e.g. mathematical techniques, make the deduction of novel conclusions possible. However, if it is

'totalized', i.e. if the instruments, hardware or software, take the lead for some time, the ensuing problem shift deteriorates into a 'pathology' of the discipline concerned.

45 From a logical point of view there are three options; but in praxis the third option is not interesting.

46 $\vdash A' \rightarrow \neg A$, $\vdash [(T \,\&\, A') \rightarrow (J \rightarrow P^*)]$.

47 It can take quite a long time before such an issue can be decided. E.g., in the case of the clash between Miller's experimental results about an alleged 'ether-effect' and a system whose key component was Einstein's theory, it took about 25 years to find out that one of the additional premisses used in Miller's falsification argument was falsified in empirical testing (temperature variations in the apparatus).

48 As regards arithmetic, both logicist and formalist conceptions contradict Kant's view; in physical geometry Euclid's system has lost its monopoly position; as regards the propositions of *'reine Naturwissenschaft'* (e.g. the principle of causality) the only reasonable construal appears to be to interpret them as methodological advice.

49 In today's German philosophical scene the foundationalist approach takes various forms. The so-called Erlangen School manifests the whole syndrome: a constructivistic approach leading to the 'ortholanguage' (*Orthosprache*), 'protophysics' and a 'pragmatic grounding' (*pragmatische Begründung*) of sentences. According to K.-O. Apel's own interpretation of his philosophy, it is a continuation of Kant in the pragmatics of language and it attempts to provide a 'transcendental-pragmatic foundation' from the conditions of the possibility of our ability to speak with each other. J. Habermas attempts to reach the same goal with a 'quasi-transcendental/quasi-empirical' foundation, with 'compelling arguments' which could guarantee the sameness of opinions in the long run. Thereby the idea of truth is played down and it is replaced by a consensus conception of 'truth'. They all appear to commit the same basic error: discourses are taken to be the forum for establishing truth and value (*wertschöpfende Instanz*) – but discourses can only be the forum for *examining* claims to truth or value (*wertprüfende Instanz*). This would be the gist of a Popperian criticism of these contemporary trends in German philosophy, which are again steeped in the foundationalist approach.

50 Cf., e.g., Radnitzky (1977).

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