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The Intellectual Environment and Dialogue Partners of the Normative Theory of Science

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THE INTELLECTUAL ENVIRONMENT AND DIALOGUE PARTNERS OF THE NORMATIVE THEORY OF SCIENCE

Introduction: the demand for and the uses of various kinds of knowledge about science.

1. General typology of possible approaches in the study of science and the possible utility of these approaches; the contributions a normative theory of science hopes to be able to make.

2. The dialogue partners — polemical poles and authentic neighbours — constituting the intellectual environment of normative theory of science: "élitism" (historical relativism), "epistemological anarchism", and "demarcationism" — brief presentation and evaluation.

3. On the preconceptions of "system-oriented" theory of science — critical discussion of Popper's ontology.

4. Skeleton outline of a normative theory of science.

0. INTRODUCTION

00. The increasing interest in science and in the study of science is one of the characteristics of our age. To understand the current interest in the study of science we have to look at the u s e s of various kinds of knowledge a b o u t science. Since the uses to which scientific knowledge is being put will shape the destiny of mankind, the main impetus to the study of science has come from the need for knowledge about the production of scientific knowledge for the r a t i o n a l i z a t i o n o f s c i e n c e p o l i c y m a k i n g (in the dimension of efficiency). Obviously that sort of knowledge is one of the prerequisites of rational science policy making. Since science not only has changed the world around us but also our world view, and since an image of science is part of an image of man, knowledge about science is required also for improving the self-conception of man. The philosophical reflection which is t o i m p r o v e t h e i m a g e o f s c i e n c e needs the knowledge about science which only

the systematic study of science can provide. Before examining the various approaches in the study of science, we propose to glance at the two fields identified through these two uses of the knowledge about science.

01. The first field we would like to label e x t e r n a l   c r i t i q u e
o f   s c i e n c e: the application of criteria of s o c i a l   m e r i t to the
results of the application of scientific knowledge. These applications
produce changes not only in the material and social èco-system, but in
"human subjectivity" itself [1]. The great theoretical innovations influence
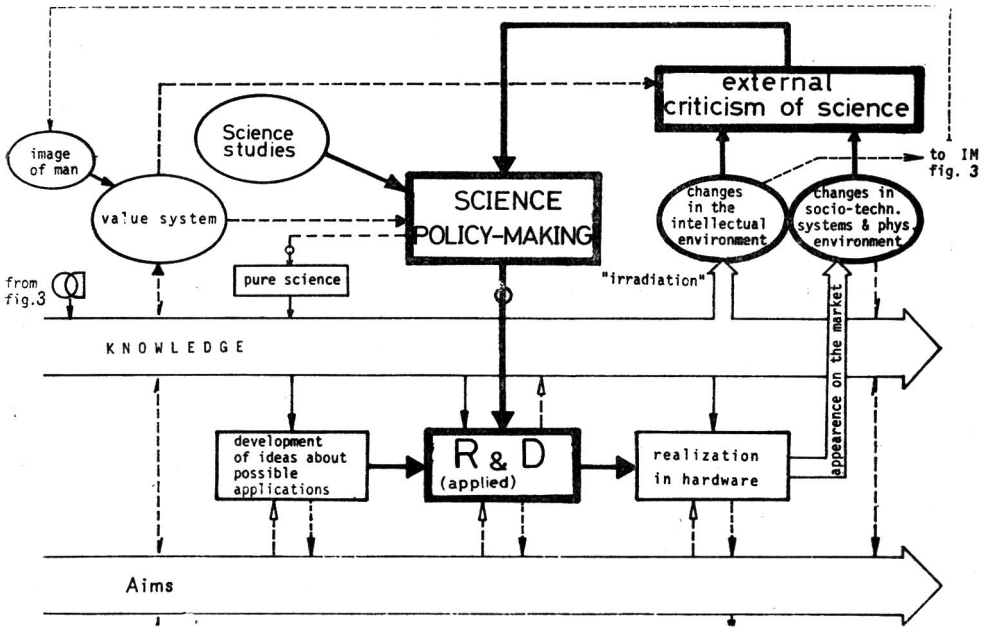

Fig. 1

the "intellectual climate", basic picture of the world, and self-understand-ing of men. These changes involve blessings as well as curses — the latter under the rubrics "biocide" (over-population and destruction of the envi-.ronment) and "menticide" (the scientistic reduction of the public-political practice of life), as well as the "human vacuum" [2]. One response to this state of affairs has been the emergence of an external critique of science as a part of the wider critique of modern civilization. This critique of science is a well-established discipline with a history moving from Nietzsche through Husserl and Ortega to Gehlen, Schelsky, Habermas, and Apel, and extends from the critically engaged social sciences to more strictly philosophical problems. Clearly, a fully developed understanding

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[1] Cf. Heelan, 1972a–c.
[2] Cf., e.g., Apel, 1973, I: 128–154; Brand, 1971; Senft, 1968.

of the phenomenon of science based on the work containing the full range of disciplines concerned with science is a pre-condition for a fully responsible critique of the place of science in the modern world. The external critique of science leads over to science p o l i c y m a k i n g. Politics is a preeminently p r a c t i c a l activity, essentially distinct from theoretical reflection, but (hopefully) guided by or mediated through such reflection. Laying the theoretical foundations for a rationalizing of science policy making necessarily involves the contributions of research about science and an external critique of science, mediated through the on-going public discussion of the aims of public life (*Lebenspraxis*).
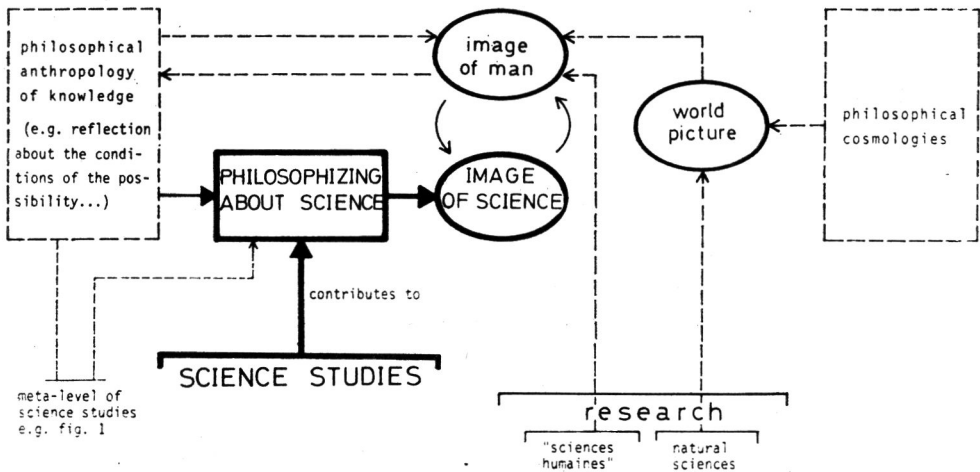

Fig. 2

Philosophical reflection on science leads in the direction of philosophical anthropology of knowledge, more specifically, to a reflection upon the conditions of possibility of science as a meaningful human activity. We mentioned above that research concerning science should contribute to improving our image of science. The elaboration and improvement of the image of science, utilizing the results of research concerning science, is the task of the p h i l o s o p h y o f s c i e n c e in the e t y m o l o g i c a l and t r a d i t i o n a l s e n s e of the word "philosophy". Utilizing the results of research concerning science and the foundational work of the philosophical anthropology of knowledge in order to integrate science rationally and harmoniously into the wider life-praxis of man is a central task of philosophy. Our image of science and our image of what it is to be human stand in the relation of the hermeneutic circle.

02. A glance at the recent history of the "philosophy of science" shows that the switches for the contemporary developments were already thrown in the middle of the thirties. At that time the foundations were

laid for a wide-focus multi-perspectivist, interdisciplinary approach to the study of science: Ossowska and Ossowski's programme essay published in I/1 of *Organon* [3]. About the same time a whole discipline representing a different, specialized, narrow-focus approach migrated to the U.S.A.: the logic of science of the Vienna Circle and of allied groups such as the Polish logicians and the so-called Berlin group. There it quickly gained a dominant position. Seldom has a style of thought been so closely linked with a geographic location. It reached its apogee in the fifties. Then the impact of criticism made itself increasingly felt, especially that coming from philosophers of physics; and this criticism has made the philosophy of science the liveliest part of the philosophical debate in the English--speaking world. Yet this criticism is not a recent phenomenon, at least not intellectually: in the middle thirties Popper had presented a most decisive critique of Logical Positivism. (With regard to this critique of its foundationalism he had of course, "predecessors", e.g. Whewell, Peirce, Duhem, and contemporaries (G. Bachélard). Yet it took about two decades before it began to have a broad impact, and before Popper's own alternative his evolutionary theory of the growth of knowledge began gathering increasingly many adherents. Associated with the Popperian critique has been the so-called new philosophy of science: Hanson, Polanyi, Kuhn, Toulmin *et al.* and, last but not least, Feyerabend and Lakatos, both starting from a Popperian platform. Also the wide-focus approach of Ossowska and Ossowski has its revival in the upsurge of interest in the sociology of science and "science studies" in general. But hitherto the integrating factor has been lacking for this wide-focus field of "science studies". And Popperian theory of research, which could potentially provide such a perspective, finds itself challenged by the historians of science, notably T. S. Kuhn and P. Feyerabend. Contemporary Logical Positivism applauds that critique since in its view it seems to show that the normative theory of research is a highly problematic endeavour — so that in the end perhaps only their own logic of science is a respectable philosophical approach. Seen from the standpoint of Logical Positivism, a theory of research which understands itself to be neither applied logic nor empirical investigation of science is fuel for a Humean bonfire. We would like to brave the flames and present a sketch of such an approach. However, as this approach to the study of science had been presented elsewhere (see References), we shall concentrate here on the relationships between our theory of research and other approaches to the study of science — empirical sociology of science, logic of science, history of science, and Popperian normative theory of science — with particular emphasis on the possible c o n t r i b u t i o n s, the possible u s e s of the knowledge provided by the various approaches. For this purpose we shift

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[3] Ossowska and Ossowski, 1936.

from a historical tour d'horizon to a t y p o l o g y o f p o s s i b l e a p-
p r o a c h e s to the systematic study of science (following in the footsteps
of Ossowska and Ossowski's wide-focus essay).

1. FROM WHAT POINTS OF VIEW CAN A RESEARCH UNDERTAKING BE CONSIDERED?

10. Instead of simply speaking of science, we would like to model
a single research undertaking. The model can be generalized: science is
then seen as an ensemble of research undertakings. In this schematic
approach science is viewed as a p r o d u c t i v e, i n n o v a t i v e "s y-
s t e m". The expression "system" indicates that we are — intuitively
and without technicalities — oriented toward "systems thinking". Con-
cepts taken from business administration are also useful in dealing with
"normal science" for there are many positive analogies with an industrial
enterprise. On the other hand, research in a situation of scientific revolu-
tion or extraordinary innovations (which Feyerabend calls "the great
moments of science") show a great similarity to artistic, creative acts.
We shall begin with the simplest case: a research undertaking in physics
on a small scale. For our current purposes, a naive-realistic attitude is to
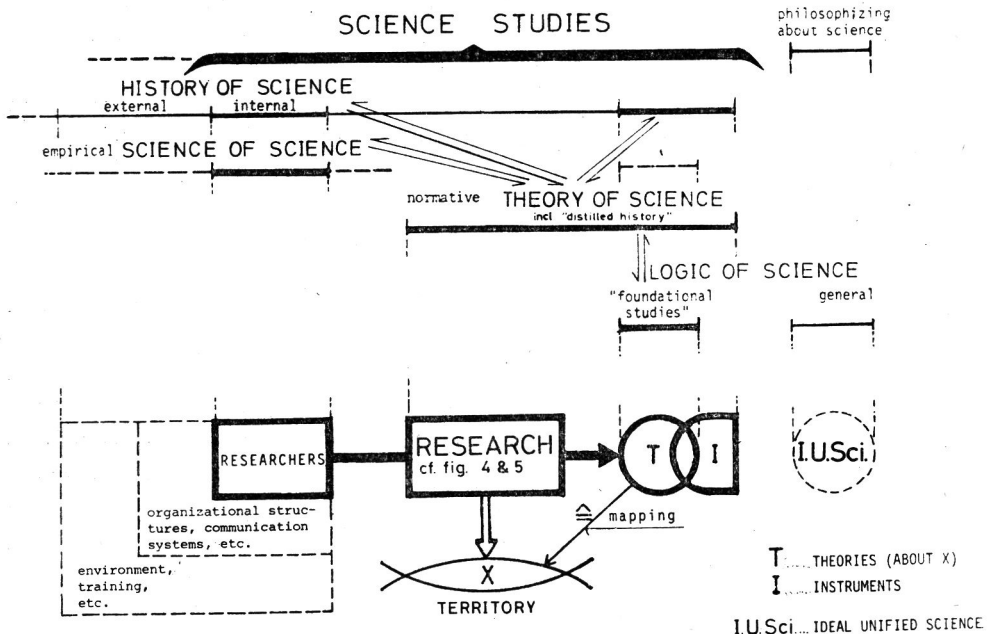be adopted.

Fig. 3

Treating research as a productive and innovative system, we make the following initial distinctions: producers, production, and products. Parallel and very useful for certain purposes is an "ontological" division into human components, processes, and abstract or "propositional" entities. Popper's ontology of three worlds — the material world (world-1), the world of consciousness (world-2), and the "third world" (world-3), the world of objective thought contents — offers a comfortable framework given our current interests. We shall later return to the framework itself. Thus, we group the components as follows:

(1) The so-called human components: first and foremost the research scientist as a producer of knowledge (producers and producer communities) and the interessees of these products (seen as consumers in a business administration model). We would also include those who mediate the exchange of knowledge, the distributors. The ontological status of the human component is relatively unproblematic, belonging primarily to Popper's world-2, since what is relevant here are above all their beliefs, interests, etc., rather than their bodies.

(2) The second component of processes and actions encompasses research in the widest sense as well as the "distribution" of results. Research — p r o d u c t i o n — consists of preparatory studies, of research in the strict sense: investigations (experimental and systematizing, theoretical labour), of internal and external c o m m u n i c a t i o n, of "s t e e r i n g" activities such as planning, internal criticism, self-reflection on results, diagnosis of difficulties, etc. Popper would place theese in the world-2. We will argue that they can be treated as world-3 as well as world-2 entities.

(3) The third group of abstract or "propositional" entities consists of objective thought contents or meanings (*Sinn*). Though they are available to us only through the mediation of world-1 sign-systems, they belong to world-3. These are the p r o d u c t s, the output of a research undertaking, consisting of k n o w l e d g e in the widest sense such as hypotheses or theories, as well as questions, objective problem situations and knowledge used as intellectual instruments (calculi; mathematical, statistical techniques; etc.). The concept of knowledge used here not only treats knowledge as i n p r i n c i p l e f a l l i b l e; we would also speak of "virtual knowledge", i.e., include under the heading of "knowledge" also alternative developments of hypotheses, theories, etc., independent from whether or not they have been the intention of an act of knowing. We call them "virtual" because if such a hypothesis, etc., would be "true", it could become the object of an act of knowing (and — to talk with Peirce — the "ideal community of investigators" would in the long run come to know it). Material instruments would also belong to the products of research enterprises (in the wide sense) in the natural sciences, and are naturally to be placed in world-1.

(4) A further component of the model is the s u b j e c t m a t t e r of the research in question — physical systems, biological phenomena, etc. Naively using the metaphor of a map, we can say that the scientist attempts to chart or to map certain aspects of the territory better and better. Of course the subject matter, the "object of study" is not just out-there-given, but influenced by the perspective, by the preconceptions of the perceiving subject, by the "theories" of the researcher. Nonetheless the metaphor of "mapping" or "charting" a territory offers a starting point, at least for the natural sciences.

Any research enterprise — and "science" itself as an ensemble of such undertakings — is embedded in a social-political milieu; finds itself in a certain intellectual climate, characterized among other things by a taste for certain concepts and perspectives and a "market" of intellectual resources: other research enterprises and research directions which provide relevant theories and intellectual tools, which suggest problems, and sometimes also function as paragons. To the factors impinging upon a research enterprise belong also the communication systems and organizational forms relevant in the contemporary scientific community, the way in which adepts are being trained and so on. We shall not dwell on these matters here.

110. The v a r i o u s w a y s i n w h i c h a r e s e a r c h u n d e r t a k i n g w i l l b e v i e w e d is dependent on the interest and preconceptions of researcher or research community, and these will be intimately connected with the training of the individuals in question. We would like to present three modes of viewing science in terms of their specific interest in (1) the human components (science of science), (2) results (logic of science), and (3) a combination of research processes and results (theory of research or theory of science).

111. S c i e n c e o f s c i e n c e. Persons trained in the social sciences [4], ranging from the psychoanalyst interested in motivation to the statistician interested in the number of publications in a specific discipline, will be primarily concerned with the human components, with producers and the effects of external circumstances on the processes and thus on the development of theories, disciplines, etc. The results of research will naturally play a role here, but the primary focus will be on the research processes — and the results will be seen as mental entities (e.g. hypotheses as beliefs). The concern is thus mainly — though by no means exclusively — with world-2.

What c o n t r i b u t i o n to what human endeavours can science of science make? In offering knowledge about the way in which extra--scientific factors influence research, about the political and organizational

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[4] Cf. e.g., Radnitzky, 1974b, Figs. 4–6; Radnitzky, 1974d, Figs. 2a–c.

conditions of improving knowledge, about communication systems, orga-
nizational systems, etc., science of science contributes to the task of mak-
ing research more e f f e c t i v e — especially with respect to normal
science. It seems that the sociologists of science regard a policy-making
and planning which is guided by sociology of science as the best means
to increase effectiveness. A presupposition for this result is, however,
that the sociologist must apply criteria of scientific merit and for this
he needs a sufficiently nuanced concept of progress; this concept cannot
be produced by science of science itself, but must be taken over from
research theory. While this is a thoroughly natural division of labour, the
requirement has yet to be adequately fulfilled, partly because the socio-
logists of science have not sufficiently recognized the problem, and partly
because the models of scientific progress which research theory has to
offer are not yet nuanced enough, and difficult to apply to concrete
current cases.

112. The h i s t o r y o f s c i e n c e — likewise one of the s c i e n c e s
h u m a i n e s — encompasses a wide spectrum from cultural history to
the genealogy of objective problem situations (Lakatos' "distilled history").
The latter treats results as members of world-3, and since it abstracts
from the human components (producers, etc.) it is not included in the
history of science as we understand the term here. The transition between
e x t e r n a l history focussing on influences upon the researchers and on
the impact of the products and i n t e r n a l history focussing on the
scientific community is a matter of degree; but it is at least clear that
even external history of science needs models of possible theory-develop-
ment and hence research theory. Even external history of science cannot
be a pure empirical social science: in order to understand the develop-
ment that actually did take place it must be able to see it in relation to
possible alternative theory-development; that means it cannot wholly
dispense with "distilled history". While research theory gets much of its
information about science from the history of science, history of science
(like sociology of science) cannot manage without theory of research
either: it is not "theory-independent".

What are the possible c o n t r i b u t i o n s of history of science? It
helps us to develop a historical consciousness — which is indispensible
for anyone wishing to get a deeper understanding of his own field of
study. The theory of research needs the history of science for testing
its models of knowledge production by applying them to historical research
enterprises: recently not only Logical Empiricism but also the Popperian
theory of science has been challenged by the history of science, notably
by T. S. Kuhn.

113. L o g i c a p p l i e d in the study of p a r t i c u l a r theories:
f o u n d a t i o n a l s t u d i e s. One whose scientific training is in the

area to which the research process in question belongs (e.g. a theoretical physicist with an interest in mathematical category theory) and is in addition a logician will tend to focus on those components which we have called "propositional entities". The so-called "foundational studies" (M. Bunge) are continuous with physical research, and can be seen as that last moment of a concrete research undertaking in which one attempts to improve the form of a relatively finished theory.

What is the possible c o n t r i b u t i o n of foundational studies? Since they help the researcher to i m p r o v e logical f o r m of his "f i n i s h e d" t h e o r i e s and to make their logical structure more t r a n s p a r e n t, they are d i r e c t l y relevant for the scientist — for the final stages of a research undertaking in those disciplines where theories can be given an axiomatized form (physics, mathematical biology, etc.). Moreover they make an essential contribution to what M. Bunge has called "scientific metaphysics": by means of axiomatizing techniques they articulate the key concepts of underlying a system of theories (e.g. the concepts of Space, Time, Causality underlying particular physical theories), i.e., they articulate the various "world pictures" underlying particular systems of theories culled from the history of science. Besides M. Bunge, H. Reichenbach and A. Grünbaum (who otherwise are to be regarded as representatives of the officially anti-metaphysical (!) Logical Empiricism) would be prominent representatives of this style of work.

114. G e n e r a l applied logic: l o g i c o f s c i e n c e — Logical Empiricism. One whose training is mainly in formal logic will likewise tend to focus on abstract, propositional entities, but not necessarily on the concrete products of historically given scientific research. The classical figures of this approach (e.g. Carnap) state explicitly that they are not concerned with "methodology". Rather than dealing with the results of concrete research processes, this direction investigates a "self--created" territory: the structures of the Ideal Unified Science. Dealing as it does with properties of the "final report" of science when all of the evidence is in, we can expect no direct contribution to science as an incomplete and on-going enterprise and its management.

What are the possible c o n t r i b u t i o n s of this g e n e r a l applied logic? Its main contribution is philosophical: a r t i c u l a t i n g a n i d e a l o f s c i e n c e which is based upon a certain substantive ideal of knowledge — a most ambitious foundationalism (*Ursprungsphilosophie* as H. Albert calls it). It has done so with a clarity and precision hitherto unprecedented in philosophizing and thereby set standards for others. Indirectly it has also provided means for improving the producers: training in applied logic for philosophers of science and researchers alike; and it has contributed to the development of logic. However all this had its dangers: if upon the ideal outlined a methodology is based (as some of

the epigones (not the masters!) have tried to do), the unrealistic ideal would in this way hamper progress, and, if taken seriously, bring science to a stand-still — as many critics from Popper to Feyerabend have convincingly argued. In short, its "logicism" has proved fruitful, its "foundationalism" a fundamental mistake. As we shall return to Logical Empiricism below we shall say no more here.

115. S y s t e m - o r i e n t e d   r e s e a r c h   t h e o r y. How, then, would one whose training is in business administration, the so-called "decision sciences", praxiology in Kotarbiński's sense or, more generally, in system-oriented styles of thought treat a concrete research undertaking? (We wish to avoid the expression "systems theory" because this indicates a formal technique, while we wish only to indicate a general mode of approach and perspective.) In the first place, he would treat research as a whole and attempt to develop models for this whole (which can be done concretely in terms of block and flow charts). In addition, he will also have a specific focus on the research with reference to its results. We call this approach "systems-oriented research theory", or simply "research theory". It includes that special type of internal history of science which Lakatos has called "distilled history of science". The specifically human components will be left out of this strict approach as much as possible. Research theory is thus differentiated from empirical science studies in that research as thought and communication processes, which science studies deal with as world-2 entities, are now treated as transformations of entities of world-3: as developments of objective problem situations, developments of theories, etc. The interest of the research theoretician is focussed on the quality control of products. The task is to develop a s u b s t a n t i v e l y   n o r m a t i v e   c o n c e p t   o f   t h e   p r o g r e s s   o f   k n o w l e d g e. Over against foundational studies, research theory does not attempt to improve the logical form of products, but rather, with reference to an objective problem situation, attempts to understand how a certain line of development proceeded, w h y it developed in that way, what v i r t u a l lines of development were in principle available, how this development is to be e v a l u a t e d, and how this evaluation is to be j u s t i f i e d. The treatment of the problematic of quality control should lead to c r i t e r i a   f o r   p r o d u c t s — not only for their logical form, but also for other aspects. On the basis of criteria for products one can develop c r i t e r i a   f o r   s t r a t e g i e s and planning, etc., in short, c r i t e r i a for the e f f i c i e n c y of the research process.

What is the possible c o n t r i b u t i o n of research theory?

1) It can help research to be more effective, but not by offering "methodologies". To offer such would be "overselling" because methodologies can at most be developed for certain partially routinized moments

of research (such as some partial moments of hypothesis-checking). In the "great moments" research is much too innovative, much too similar to artistic creation for a methodology not to impede progress. Research theory can improve the s e n s i t i v i t y of the scientist by aiding the more adequate conceptualization of the research situation, and thereby clarifying the manoeuvre space which is available. The stylized history of science produced with the aid of a model of knowledge production and progress can help the scientist develop a sense of his history, which is a necessary condition for fully understanding his present situation and problems. Finally, research theory can debunk the blind dependence on tradition, and thus improve self-understanding. All of these increase the freedom of the decision-maker, and help improve the quality of the decisions themselves. This general contribution to i n c r e a s i n g o u r u n d e r s t a n d i n g o f r e s e a r c h seems to us to be fundamental, for only when a certain level of knowledge about research-immanent factors and criteria for progress are available is it possible to investigate systematically the external factors of the organizational conditions of progress.

2) Using "research concerning science" as a general name for empirical science of science, history of science and research theory (whether or not foundational studies are to be included here or regarded as a part of physics (biology, etc.) will depend on the intention of the concrete specimen of "foundational studies" under consideration), we can see a further contribution of research theory in the *integration* of research concerning science. It can achieve this by: (1) providing a common conceptual framework such as the globally systems-oriented model, or models of a specific scientific tradition, and (2) by specifying tasks for the disciplines of research concerning science such as deriving hypotheses from the models of progress and scientific traditions which are to be tested by means of case studies. In this way one could have some assurance that the results of science studies are of maximum relevance for increasing the effectiveness of research through the control of external conditions. Reciprocally, one could guarantee that research theory, which obtains much essential information about its territory from the other disciplines, is of maximum relevance for these disciplines themselves.

3) The most important contribution of research theory (so it would appear to us) is an i d e o l o g y - c r i t i c a l one: knowledge about the progress of knowledge improves our concept of science and unmasks dogmatism. A) Dogmatism within science itself: there is always the danger that certain positions, paradigms, research directions, etc., become dogmatized and stand in the way of renewal ("paternalistic methodologies" present just this danger). Investigation of the ways in which particular paradigms and research-traditions can both reveal and conceal will help

reduce the scientist's dogmatic security and arouse an interest in alterna-
tives. This not only increases effectiveness, but leads generally to an
improvement of our i m a g e   o f   s c i e n c e. B) Dogmatically positing
science as the o n l y form of knowledge or rational activity: scientism.
By improving our concept of science, research theory also contributes to
the critique of scientism. At this point research theory makes its contri-
bution to the wider philosophical treatment of the full range of experience
and action and their integration into a rational and good life, both public
and private (see also "C" below). The recent work of Feyerabend has the
unmistakable aim of criticizing scientism, of achieving a sort of "enligh-
tenment". C) There is also the d o g m a t i c political labelling of certain
positions as emphatically being either scientific or unscientific. Thus,
I. Lakatos sees the political relevance of the demarcation problematic in
the ability to mark off science and pseudo-science. (However, the demar-
cation proved to be more difficult than one had thought. We will come
back to this later).

2. WHO ARE THE DIALOGUE PARTNERS AND "NEIGHBOURS" WHO CONSTITUTE THE INTELLECTUAL ENVIRONMENT OF "SYSTEM-ORIENTED" THEORY OF RESEARCH?

20. The place of a "system-theoretic" research theory in the intellectual
environment has already been partially specified with respect to other
models of viewing science. We would understand as neighbours, i.e.,
those whose work is of d i r e c t relevance to one's own concerns, those
from whom one can learn as well as those with whom one is obliged to
fight. These two groups overlap, especially in the case of the so–called
"new philosophy of science" in the U.S.A., which is attempting to bridge
the gap between history of science and logic of science, often under the
influence of the pragmatism of C. S. Peirce. The most important and
congenial neighbour in this area is the critical rationalism of the Popper
"school", of which we understand ourselves to be a branch. But before
sketching the general neighbouring positions in order to get a clearer
look at our agreements and differences, we will briefly review that style
of "meta-science" that until quite recently has dominated the scene:
L o g i c a l   P o s i t i v i s m. It constitutes one of the dialogue partners to
which we are p o l e m i c a l l y tied: which we think we must fight
in order to clear the way for our normative theory of research. The other
of these "negative" poles is the theory of science inspired by Wittgenstein
II, e.g., Toulmin's. This notwithstanding the fact that Wittgenstein II
(*Philosophical Investigations*) constitutes the antithesis of the theory of
language developed by Wittgenstein I in the *Tractatus*, to which Logical
Positivism is so heavily indebted.

21. If, with E. McMullin [5], one views the *Tractatus* as a thought experiment [6] telling us what theory of language and, by implication, what theory of physics w o u l d be adequate i f certain assumptions were true, one may apitomize the essence of Logical Empiricism as follows:

1) The field of legitimate study (for a Logical Empiricist) is d e l i- m i t e d thus: it is, of course, recognized that the relationship between sign and designatum is due to conventions; and it is presupposed that the language user, in particular the empirical scientist can already success- fully communicate with each other. That means that the problem of the conditions of the possibility of language communication is left to others (Ordinary-Language-Philosophy and transcendental pragmatics of lan- guage). This s e t t i n g a s i d e would be a justifiable division of labour, provided that Logical Empiricists wish only to develop a theory of natural science and provided that one keeps in mind that an a b s t r a c t i o n has been made from the "pragmatic" dimension, which must be reserved as soon as philosophical self-reflection sets in. (However hitherto the official position of Logical Empiricism has been that the aforesaid pro- blems are merely psychological — otherwise exceed 'the limits of what can be said.)

2) E x t e n s i o n a l i t y. If for the purpose of developing a theory of science, or designing and adequate ideal of science propositions other than declarative ones may be disregarded or else reduced to declarative ones — i.e., if all non-extensional contexts (such as contexts about human acts as meaningful acts and not just as behaviour) could be reduced to exten- sional ones (in other words if materialism would offer an adequate onto- logical ground-plan), then *PM*-ese would be *the* language to adopt (the only language which could be philosophically legitimated).

3) M a p p i n g t h e o r y o f l a n g u a g e ("picture theory"). If one has succeeded in constructing and adequate improved language, an "ideal language" *IL* (in which every expression has but one meaning, which is independent of the context and the situation of use), then for this *IL* would hold: the form of the signs would map the form of the world: the form of an atomic se۔ence would correspond to a state of affairs. If such a sentence is true it would map an atomic fact. (If false, yet well- -formed, a (logically) possible fact.) Thus there would be pre-theoretical correspondence rules.

4) F o u n d a t i o n a l i s m. If atomic sentences of *PM*-ese map atomic facts by means of theory-independent, "meaning-invariant" terms, and if we could find such sentences that are epistemi- cally unproblematic: certain — in no need of further validation,

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[5] McMullin, 1974.

[6] For the historical Wittgenstein the *Tractatus* was more than that: he then believed in this approach, believed that he had found t h e "transcendental" lan- guage scheme.

i.e., if there were an epistemic foundation, then verification of any sentence would essentially be either truth-functional testing (S is deducible in *IL* from a finite consistent set of such basic sentences) or estimating probabilities by means of inductive rules.

So far all this has been thought experiment. As soon as one dares to make the bold assumption that the antecedent clauses of the above hypotnetical statements are true, the thought experiment is turned into the most ambitious theory of natural science and metaphysics. The Vienna Circle took that step.

To 2): N o n - d e c l a r a t i v e propositions c a n *de facto* be so r e d u c e d without any loss that would matter for developing a theory of science. Hence *PM*-ese is the right sort of language; and, an atomic sentence formulated in *PM*-ese being true means [7] that it corresponds to a fact (that it maps the form of a fact).

To 4): We a r e *de facto* justified in regarding a certain sort of sentences as epistemically unproblematic: w e p o s s e s s an unproblematic f o u n d a t i o n, *viz.* "observation sentences" or "protocol sentences". An atomic protocol sentence corresponds to sense data (better: to a phenomenal fact) — the tradition of Mach and Hume —, or corresponds to traits of a material object (better: maps a physical (perceptual) fact) — the tradition of physicalism (Carnap, etc.). Hence *PM*-ese can be tied *directly* to our experience of the world or to the world — in this case a world without mind: the world the natural sciences deal with. (In short, the world design offered by the *Tractatus* as a possibility i s the world, or at least the world of science).

Hence the following "foundationalist-inductivist" (McMullin's term) i d e a l o f s c i e n c e emerges: science should be (or, ideally, science would be) but enlarging the stock of observation sentences (broadening, enriching the secure base) and compounding them (either truth-functionally or by inductive rules and probabilities) to ever more complex sentences or sentence systems. P h i l o s o p h y o f s c i e n c e is then but the spelling out of the various aspects of this ideal science: constructing blueprint models of scientific explanation, of theories, etc.

The point of philosophizing by means of an *IL* is based on following idea: if one would possess an *IL* which correctly maps the form of the world, then one could learn about the world simply by studying the language and the patterns constructed by means of it. Analogously, if one would possess an adequate articulation of the ideal science couched in the *IL* (assuming, *per impossibile,* that the base is secure and that "all" the evidence, all the required observation sentences, is in), then o n e c o u l d l e a r n a b o u t s c i e n c e b y s t u d y i n g t h a t p a r t i c-

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[7] Notice the use of a non-extensional meta-language here, which however does not as such contradict the assumption.

u l a r i d e a l s c i e n c e. Any knowledge about the actual course of the history of science or about the practice of research or about the conditions of knowledge production would then by simply irrelevant for the task of learning more and more about what science should be like, of articulating the "internal" criteria of scientific merit.

Of course the thesis of the independence of normative theory of science c o u l d be correct even if the afore-said assumption of the existence of an adequate *IL* is counterfactual since there could be other ways of defending it. Moreover a declaration of independence — in particular one of partial independence — is not the same as a thesis of apartheid: we must try to sail between the Scylla of Apartheid and the Charybdis of the "naturalistic fallacy" (attempting to derive "ought" from "is"). Instead of starting from the misleading explicandum of foundationalist ideal of knowledge and science one might work on the explication of an idea of scientific progress (Ptolemy, Copernicus, Newton, Einstein, etc.) which is not prejudiced by a particular position in epistemology and ontology. That is what "demarcationists" (and we with them) wish to do.

22. THE CRITICS OF THE THEORY OF SCIENCE OF LOGICAL EMPIRICISM AND OF POPPERIAN CRITICAL RATIONALISM

220. Using the terminology suggested by I. Lakatos in his critique of S. Toulmin's *Human Understanding*, we shall label the three main types of critics in terms of their position with respect to the classical problem of demarcation: (1) "élitism" — as an umbrella word for the various positions which have in common the basic tenet that there are norms, standards of quality appraisal, but that all such standards are dependent on the judgement of a special group — an *élite*. (2) "Epistemological anarchism" — characterized by the tenet that there are n o universal methodological rules and that there is no reliance on the singular evaluations with respect to quality (scientific merit) made by prominent scientists in the course of the history of science. (3) "Demarcationism" — the tenet that the problem of quality control, as a generalized demarcation problem, can be solved w i t h o u t having recourse to any *élites*. Thus "demarcationism" wants to be a "democratic" position: if objective norms can be explicated, everybody can use them and thus everybody can make evaluations of scientific merit with their help.

To bring out clearer these three positions and to bring out our agreements and disagreements, we shall in turn examıne the a n s w e r s e a c h of them gives to the following s e t o f s t a n d a r d q u e s t i o n s in the theory of science: A) P r o b l e m s c o n c e r n i n g m e t h o d o - l o g i c a l r u l e s a n d c r i t e r i a o f s c i e n t i f i c m e r i t (quality): A.1) Are there u n i v e r s a l l y v a l i d m e t h o d o l o g i c a l r u l e s? A.2) If not, are there other types of methodological rules? A.3) Is the

problem of demarcation — or its generalized version, the problem of qual-
ity control — soluble? B) Problems concerning the p r o g r a m m a t i c
c o n c e p t i o n   o f   t h e   t h e o r y   o f   s c i e n c e: B.1) What is the
main task of theory of science? B.2) What sort of discipline should theory
of science therefore be? B.3) What possible contribution can it then
make? — and concretizing these possible contributions: C.1) What p r a c-
t i c a l   a d v i c e   can the theory of science give to the researcher, and
C.2) what advice to those concerned with science policy making?

221. "E l i t i s m" has many variants — T. S. Kuhn, S. Toulmin; M. Po-
lanyi, R. Merton, would fall into this group. The position is often clearly
influenced by the later Wittgenstein. Most discussed has been Kuhn's
position [8]: using a historical and sociological perspective, Kuhn, on the
basis of his case studies, comes to the conclusion that Popper's "falsifica-
tion model of the game of science" is unrealistic — theories always
already have negative evidence which is known; during period of "scienti-
fic revolution" there are no agreed methodological structures available
and hence a "revolution" cannot be described or evaluated in terms of
logic and experimental evidence alone. He emphasizes that science is the
work of a very special social group which has its specific group commit-
ments. Hence to understand what actually goes on in science, to under-
stand its history, we need more than a theory of science. All this is
plausible enough. Yet we think that the emphasis is too one-sided: To
understand science we surely need history and sociology of science, but
already to judge which developments were the important ones we a l s o
need a normative theory of science, and we need "distilled history of
science" to see the possible alternatives — and only against this back-
ground can we really understand the development which actually took
place. S. Toulmin extracts from the history of science an evolutionary
theory of the growth of knowledge: progress as essentially a continuing,
evolving transformation of individual concepts — conceptual changes, as
a means of meeting the challenge of unexpected experiences, exhibit the
rationality underlying the development of science.

What would representatives of "élitism" a n s w e r to the above-
-mentioned questions? There are no general methodological rules, n o
statute law. (A.1) All we have is the c a s e law of singular normative
judgements, singular appraisals of scientific merit. (A.2) On this ground
the problem of quality control can be dealt with (A.3), but o n l y by
an *élite* qualified to practice case law.

Thus the t a s k of the theory of science is the identification of the
relevant *élite* then the identification of the standards they actually use
in their practical singular evaluations, and — insofar as the standards do

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[8] Cf. Kisiel, 1974.

not belong to the "tacit dimension" in Polanyi's sense — to elucidate them. (B.1) Because the internal criticism of science (immanent to science) is thus similar to art criticism: there is no system of rules which might replace the *connaisseur,* theory of science will be much similar to analytic meta-aethetics or meta-ethics, which analyses the discourse of the art critic, except that in our case the critic and *connaisseur* is generally the producer himself. (B.2).

What are the concrete c o n t r i b u t i o n s of such an approach? It has no normative claims: following Wittgenstein's claim that "philosophy leaves everything as it is", a normative theory of science would be an illegitimate language game. (B.3) At most in elucidates *de facto* standards, but like ordinary-language philosophy it cannot take a critical stand. Its advice to researcher is "Do your master's thing!" "Follow the masters of the discipline of the research t r a d i t i o n (if they can be identified): follow the norms of the particular form of life!" If there is disagreement wait and see who wins. (C.1) (But how long should one wait?) Hence a "h a n d s - o f f" p o l i c y is the best thing in science policy making [9].

There is a s o u n d c o r e t o "e l i t i s m": Ordinary-language philosophy has made it very clear that rules, moral rules, etc., have "open texture", that no rule has a universal realm of *application*: there are always types of situation conceivable in which the rule has to be waived, be overruled by another rule. As soon as we do not have an algorithm, decision-making becomes a risky business, and in such circumstances some people — those with a special "sensitivity" (based on experience, imagination, "expertise", etc.) — will regularly do better than others: they constitute an *élite* in this sense.

On the other hand I. Lakatos correctly sees here a *degenerating problem shift*: a shift has occurred from the original problem of the quality control of products (world-3 entities) to the quality control of producers (world-3) since it is either lead relentlessly to a from of psychologism or sociologism (naturalistic fallacy!) or else is circular: its paradox is that it has secretly to use world-3 norms in order to identify the masterpieces and the masters in the first place. By reducing theory of science to the description, explanation or elucidation of the norms which are *de facto* used, it succumbs to the naturalistic fallacy, and looses the normative problem, the original problem of quality control. Its advice to the researcher is a sheer recommendation of conformism which would impede his vigour in making innovations. Since it holds that there are no objective internal criteria, it leaves science policy making without any arguments which might shield it from the danger of extreme forms

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[9] Polanyi has been the chief champion of the autonomy of "pure" science. In the famous debate about science policy making in *Minerva* in the thirties Polanyi and Bernal were the most important figures. For a critical survey of that debate cf. Radnitzky and Andersson, 1970.

of vulgarized "élitism" where outside forces try to impose their own standards — as standards of scientific merit — upon the scientific community. (Remember the case of "German" physics *vs* "Jewish" physics).

222. "E p i s t e m o l o g i c a l   a n a r c h i s m". P. Feyerabend has sharpened the élitist's historicism and relativism, and transformed it into a position which Lakatos labels "scepticism", and he himself "anarchism". For the Feyerabendian "anarchist" there can be (or should be) n o universally valid methodological rules because they would hamper scientific "progress" (and, perhaps, somewhat inconsistently for a conscientious anarchist or sceptic, he holds that there has been progress in science?). (A.1) Nor is there relying on normative s i n g u l a r judgements concerning examplary achievements in the history of physics. (A.2) Hence the problem of demarcation is unsolvable — a pseudo-problem. (A.3)

What positions in theory of science are the main target of Feyerabend's critique? He would no longer regard Logical Empirism as a worthy opponent: he has long since participated in demolishing its foundationalism, and he has found also its logistic approach to be unsuitable for theory of science. The only worthy opponent for Feyerabend is Popperian Critical Rationalism — Lakatos' version of which he already regards as "an anarchism in disguise".

An "epistemological anarchist" is in principle against all dogmas, rules, programmes. But if he happens to wish to tease the only opponent he judges worthy, Critical Rationalism, out of its dogmatic slumber, he will do so by producing arguments for doctrines that in the view of Critical Rationalism are "unreasonable". Thus he will argue than scientific progress — as Popperian normative theory of science understands it (he himself officially disclaims to make use of any such concepts!) — is best faciliated if one goes about in an anarchistic fashion, which is tantamount to arguing that a n y methodological rule would h a m p e r such progress. To support this thesis he will cull examples from the history of science (esp. physics) which make plausible the claim that for each and every methodological rule — however reasonable it may seem — there is some research situation in which the rule, if followed, would have hampered or stopped progress, and that, inversely, there is no methodological procedure — how "unreasonable" it may *prima facies* appear — for which there is not some research situation, in which, if followed, it would have led to success.

Feyerabend is guided by an ideology-critical aim: the unmasking of false images of science, the inductivist image, the image patterned upon the ideal articulated by Logical Empirism, the Popperian image of science, etc — it seems indeed, that they are all in principle false. Such a criticism is the main t a s k   o f theory of science à la Feyerabend. (B.1) Thereby theory of science is tacitly reduced to case studies in the h i s t o r y

of science, and comments on the various stylization of that history. (B.2) Through such studies hopefully the f r e e d o m of the researcher as decision-maker in opaque and risky situations will be increased: by liberating him from the influence of false images of science, of false methodologies based thereon, etc. (B.3).

What is Feyerabend's a d v i c e to the scientist? "Do your own thing! — for everything is possible". (Or: "Do what you feel like, for possibility itself holds no particular promise!") Since critical Critical Rationalism (not only naive Critical Rationalism) is dead, everything is permitted. (C.1) However, if one looks closer one finds that there are some very broad recommendations: The "p r i n c i p l e   o f   t e n a c i t y": stick to your preconceptions and research programme as long as possible! — because only in this way you will get out of it most of that can be got. He holds that in most situations it will pay to adopt an attitude of "t h e o r y   p l u r a l i s m" working with several alternatives — yet "pluralism" is not recommended as a general policy in all situations. The revival of old, discarded theories and points of view often pays off (because, I thing, there are but a few root metaphors [10]). In terms of science policy, the advice is to adopt a "p r i n c i p l e   o f   p r o l i f e r a-t i o n", pluralism of approaches, of research directions, etc., for the discipline as a whole. (C.2).

Feyerabendian "anarchism" is — like scepticism — difficult to refute. According to it there can be change, but in principle no progress. This is the paradox of historism — historical relativism — in this theory and history of s c i e n c e: precisely the history of physics offers the examplary, paradigmatic example of progress! While certainly no rule has a universal realm applicability, this is not the same as there being no general rules. Thus, once again, the normative problem has been lost — the baby thrown out with the bath water. We have been liberated from false images of science, but not possessing an articulated or even articulatable concept of scientific progress, scientific merit, it seems that the ideology--critical intention remains unfulfilled: we, once more, have no weapons to guard against usurpatory impositions from outside. Feyerabend holds that defending a theory involves — inevitably in some situations — the use of "political" propaganda: this is the way it has always been and the way it must be. One wonders whether something like a naturalistic fallacy is not lurking in the background here. The researcher is left to his own devices since the recommendations are too broad to be of much assistence in his practical daily work. The science policy maker might find that at least in fields where research is very expensive, the principle of proliferation is impracticable. On the other hand, the principle will help to counterabalance or to forestall some of the negative consequences of the monopolizing tendencies of research traditions which have been

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[10] Cf. Radnitzky, 1974a, § 22.

successful for a longer period of time [11]. And Feyerabend's intention —
increasing the freedom and sensitivity of the researcher — dovetails with
ours. The *pars destruens* of his studies is impressive, and it has done
a lot to enlighten all of us.

223. "D e m a r c a t i o n i s m". Lakatos labels those who place the
quality-control of products at the centre of theory of science — as
a generalized demarcation problem — "demarcationists". He holds that
there a r e general criteria, at least that of comparative "content in-
crease". (A.1) In the s i n g u l a r c o m p a r a t i v e j u d g e m e n t s
on the p a r a d i g m a t i c cases of the history of physics of the last
two centuries (case law) he finds a sufficiently clear and stable *expli-
candum* of the concept of s c i e n t i f i c p r o g r e s s. (A.2) Thus the
demarcation problem is solvable. (A.3)

This may be expressed by applying Lakatos' own terminology reflex-
ively ("on the meta-level") to his research programme in the theory of
science: the "hard core" of his research programme are preconceptions to
the effect that the results of research belong to the world-3 (Popper's
ontology being presupposed), that there are — in world-3 — universal,
normative standards, that we can "find" them, and that if we articulate
them, thereby explicating our intuitive idea of scientific progress, we
can continually clarify and improve this intuitive idea (our *explicandum*).

Thus the chief t a s k of the theory of science is q u a l i t y c o n t r o l
o f p r o d u c t s: to articulate and legitimize internal criteria: criteria
that should seperate better from less good knowledge, and thus define
progress and degeneration. (B.1) Thus theory of science deals with world-3
phenomena. Hence there is a sharp distinction to be made between norma-
tive theory of science on the one hand and sociology of science and
history of science (historiography of the "actual" course of history) on
the other. Because it is a corrolary of the "hard core" of the programme
that quality control of products is completely seperated from the quality
control of producers, the history of actual genesis and that of the impact
it has made — whether is has been or is generally accepted or not —, all
this is irrelevant for the normative task of quality control. To mix the
two types of problems would be similar to the genetic fallacy in aesthe-
tics (a derivative of the naturalistic fallacy): the genesis of a work of
art is irrelevant of its evaluation *qua* aesthetic object. Thus theory of
science itself is conceived as a n o r m a t i v e - h e r m e n e u t i c discip-
line: a typical *Geisteswissenschaft*: it is to help us improve our under-
standing of the Rationality immanent in the exemplary achievements of
the history of science, and to use this understanding in the evaluation of
current research programmes. (B.2) The task of explicating the intuitive
idea of scientific progress, scientific merit, and — doing this in black-

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[11] Radnitzky, 1973b, § V.

-and-white — to demarcate, to reject the claims of "scientificity" made by pseudo-science, is important, not least because of its political relevance: an adequate demarcation criterion will help to stop external groups who try to impose their private standards on science. (B.3) Like Feyerabend, he hopes to increase the freedom of the individual researcher; "distilled history of science" will help the researcher to conceptualize better the possibilities inherent in a concrete research situation. Theory of science makes also a contribution to the history of science: stylizing its gross structure in terms of a sequence of research programmes and providing standards of evaluation. Thus Lakatos emphasizes also the contribution the theory of science can make to the history of science, while Feyerabend, Toulmin and Kuhn rather stress the importance of history of science for theory of science.

The a d v i c e Lakatos would give to the scientist: "Do your own things (as Feyerabend says), but o n l y s o l o n g a s y o u m a k e c l e a r t o u s a n d t o y o u r s e l f precisely h o w i t s t a n d s w i t h t h e r e s u l t s of your research programme i n c o m p a r i s o n w i t h o t h e r p r o g r a m m e s: the keeping the score must be explicit in order to make the competition between programmes honest and open. (C.1) This, of course, is less a practical advice concerning the choice of lines of research than a code of honesty, a point made clear by Feyerabend. Parallel with Feyerabend's principle of tenacity are two recommendations: to protect the "hard core" (otherwise the programme cannot show what it is worth) and to give a newly conceived theory a breathing space (otherwise it would not even have a chance to develop into a theory that may constitute a serious competitor to the veteran theory). The science policy maker would be — implicitly — advised first to make an evaluation of the past performance of competing research programmes and then use this information for estimates of future performance. (C.2). (One wonders whether this last-mentioned advice might not bring us from the frying pan of epistemological anarchism ("anarchism in disguise" as Feyerabend calls it) into the fire of inductivism (thought to have been overcome).)

Lakatos' position clearly comes closest to our own: constitutes our closest authentic neighbour. Its difficulties are many: we still do not possess operative criteria required for making the comparisons of "empirical content" required to operationalize the concept of "content increase". The meta-criterion which Lakatos proposes for demarcation criteria: criterion A is better than criterion B if A allows us to reconstruct more of the exemplary history of physics than B, this criterion appears to us to dilute the *explicandum* concept. The requirement that the *explicatum* be sufficiently similar to the *explicandum* (otherwise we would not be sure that the improved concept proposed really explicates the intuitive idea (or less clear and less fruitful idea) it is supposed to replace

or to have processed), merely requires that the clear-cut positive/negative
instances of the application of the *explicandum*-term must be likewise
clear-cut positive/negative instances of the application of the *explicatum*-
-term. To require more — i.e. the "more" in Lakatos' definition of the
meta-criterion — simply dilutes the *explicandum*: when one procedes
from the paradigmatic cases to further cases, one must eventually come
to more and more problematic cases, borderline cases. (These can hope-
fully be dealt with by the means of the improved concept, the *explicatum*,
but cannot serve as part of the *explicandum*.) In short, it appears to us
that the criterion of demarcation/quality control must be made stronger
than its self-application on the meta-level. It has to be legitimized by
arguments that make it plausible that by adopting it we will facilitate
progress, the growth of knowledge. Any appeal to the history — other-
wise than clarifying the *explicandum* — brings with it the danger of the
naturalistic fallacy.

As mentioned, not even Lakatos has any concrete advice to be given
to the research worker. He is anxious to have some advice to the science
policy maker. Yet the extrapolation from the past performance of a rese-
arch programme (as compared with that of its rival competitors) to esti-
mating future performance is extremely risky. On the hand, perhaps,
evaluation *ex post* together with taking into account as much of the
research situation as we can, combined with „rational betting" is the
best possible policy in such a risky business as science policy making in
particular with respect to "pure" science. While the principle of giving
a breathing space is intended primarily for interim products rather than
for relatively finished products, there will be no convincing arguments
against sticking to a research programme whose performance in the recent
past was poor (it might improve — who knows for sure that it will not?).
The issue is pressing because a science policy not guided by adequate
criteria cannot even get off the ground.

3. ON SOME OF THE PRECONCEPTIONS OF "SYSTEM-ORIENTED" THEORY OF SCIENCE: CRITICAL DISCUSSION OF POPPER'S ONTOLOGY

Our chief adversary is "élitism", especially its historicism and its pro-
grammatic conception of the theory of science: analysing the standards
of scientists is a n e c e s s a r y task, but it is important mainly as
a preliminary to the main question: which standards can be legitimated?

Our a u t h e n t i c neighbours, leaving aside Feyerabend (from whom
we can learn a great deal, though his statements seem to be deliberately
exaggerated for polemical purposes), are Popper and Lakatos. Termino-
logically speaking, since we place the general problematic of quality
control, of criteria, at the centre, and regard demarcation as a special

case of the application of criteria — eliminating those theories or re-- search programmes [in Lakatos's sense] which make claim to "scientificity" but make such a poor showing with respect to the relevant set of criteria of scientific merit that we would not even grant them the title of "science": quality control in black and white — so to speak —, we would not accept "demarcationism" as a descriptive title. We think that the problem of internal criteria should be treated in a much wider context: it is not the case that every context of evaluation places purely epistemic considerations ("degree of truth-content", etc.) in the centre of interest (as Lakatos seems to assume). For instance with respect to a single hypothesis (law) in the context of an investigation carried out in a period of "Normal Science", there are besides the dimension of epistemic criticism other dimensions of criticism such as criticism concerning logical and semantic properties, coherence with the basic categories of the veteran theory, relevance to the problem at hand, and so on [12]. But more importantly, it seems to us that the criteria problematic cannot be dealt with in a frontal assault, that a solution to that problematic is rather a s p i n- -o f f of a satisfactory model of knowledge progress. Thus, the initial task is the development of useful (normative) models of knowledge production for different kinds of knowledge and knowledge production. However, before going into evaluation of such models, it is perhaps necessary to a r g u e that we do in fact belong to the Popper tradition. In particular, we must remove the suspicion that we are suspectible of a psycho- or sociologism. In order to deal with this issue, we must take a closer look at Popper's o n t o l o g y, which we have naively made use of up to this point. Recently this ontology has been attracting considerable attention in the periodical literature [13].

Any conception of science stands on a more or less determinate image of man and world, and this image will rest on a more or less articulated ontological foundation. "Ontology" here signifies a taxonomy of the basic k i n d s of entities, and the assigment of their ontological status. What, then, are the criteria for evaluating an ontology (rules of the "game of ontology")? They are derived from two basic requirements which stand in a certain tension to one another: the demand for parsimony, i.e., as few different kinds of entities as possible (*Entia non sunt multiplicanda praeter necessitatem*), and the demand of applicability, i.e., as many different kinds of entities as are n e c e s s a r y for an analysis of "reality" (*Entia non sunt expurganda praeter necessitatem*). These requirements will be weighed differently depending on the context within which one constructs ontological taxonomies and makes ontological assays. A "pure" p h i l o s o p h e r, interested in the "structure of the world" (*Aufbau der Welt*), may hold fast to the principle of parsimony (from

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[12] Cf. e.g., Radnitzky, 1974d, pp. 68–95.
[13] For instance, Dolby, 1974; Meynell, 1974; Wojick, 1974.

Ockham's razor to Quine's "preference for desert landscapes"). This stance can be carried to an extreme, and if it is rigidly applied in the theory of science it leads to what M. Bunge has aptly called "theory demolishing techniques". As he has noted, if the s c i e n t i s t rigidly applied such standards, science itself would be brought to a standstill. Thus, the student of the theory of science will tend to hold the second principle to be more important, for he is interested in an ontology which can serve as a foundation for scientific theories and theorizing about them. The danger in this tendency is that of overpopulation, as well as a loss of transparency, consistency and clarity in comparison with the work of professional ontologists (such as e.g., G. Bergmann).

P o p p e r, as a philosopher of science, belongs to the second group. He needs an ontology which fits his theory of science. He offers a p l u - r a l i s t i c o n t o l o g y, adding to the dualism of the material world and consciousness world-3, to which he seems to grant likewise full status: "existence". Therefore some call it a "hyper-realism". This world-3 is the world of sense and meaning: of objective problem-situations, theories and arguments as such, etc. A large part of this meaning, this knowledge in the objective sense, is v i r t u a l, i.e., it is not required that this knowledge (problems, theories, etc.) have actually been the intention of some empirical consciousness, that somebody has thought about them, imagined them, etc., or that somebody will ever do so: it suffices that they c o u l d become the intention of such intentional acts. (Someone thinking on C. Peirce's lines might say: they would become the intention of the ideal (indefinite) community of investigations in the long run). Of course not only "true" hypotheses, theories, etc., belong to world-3 but also *"false"* one.

The thesis that n o n e of the three worlds or realms (or groups of sorts of entities) is reducible to any other, hinges upon the status alloted to the world-3. The first dualism in the realm of "existents", that between what most ontologists would call "perceptual (physical or material)" entities and "phenomenal" entities (mental entities forming a sub-group) is — like the distinction between "things" (perceptual or phenomenal) and its mirror image, phenomenalism, which claims that *only* consciousthemselves "realists" and denied only by two extreme positions: metaphysical materialism, which asserts that consciousness does not "exist", and its mirror image, phenomenalism, which clamis that *only* consciousness exists in the full sense, and which relentlessly leads to solipsism. If the position of "realism" is not thematized, problematized — for which there is no reason here —, the decisive point becomes *the status of the entities of Popper's world-3.* (Ontologists like Gustav Bergmann, who works as an *IL*-philosopher, and who ontologizes e.g., what the logical connectives stand for: universality, transitivity, etc., allots these types of entities an ontological status "lower" than "existence". But these are

technical matters which we here can leave aside.) W h a t   r e a s o n s
d o e s   P o p p e r   o f f e r   f o r   o n t o l o g i z i n g   w o r l d - 3,   f o r
g i v i n g   i t   a   s e p a r a t e   s t a t u s (whatever category of ontological
status is allotted to it)? His main argument is the partial i n d e p e n -
d e n c e,   a u t o n o m y of world-3. A further reason is the circumstance
that world-3, mediated through world-2, *interacts* with world-1. (These
two belong indeed to the main "patterns" in any "ontological game"). Al-
though partially created by humans — and due to the recognition of
this fact by Popper, his ontology cannot be labelled "Platonistic" — the
world-3 entities have a partial autonomy. This can be seen in the circum-
stance that many world-3 properties are unintentional results of creative
acts, and in the circumstance that these entities have a life of their own:
the truths of the third world are discovered and not created. (For exam-
ple, the series of natural numbers is a human construction, but the distin-
ction between even and uneven numbers, prime numbers, etc., are unin-
tentional consequences of this construction.) Popper draws here attention
to a striking phenomenon: t h a t   o n c e   w e   h a v e   s t a r t e d,   w e
b e c o m e   e n t a n g l e d   i n   c e r t a i n   p r o b l e m s   w h e t h e r   w e
w i s h   i t   o r   n o t. For instance we give an axiom system a certain in-
terpretation; but the number of deductions is practically infinite since
the solutions are conclusions which we obtain with certain "initial" condi-
tions (*Randbedingungen*). Thus we know the meaning of the axiom sys-
tem always only partially, and only successively do we come to know
more and more of the meaning of the key terms. The autonomy has also
another aspect: although the "creation" of world-3 entities is performed
by men, it yet always presupposes other world-3 entities: the so-called
hermeneutics of the question. (As Popper emphasizes, we always start
from a high level of pre-understanding.) As we have said, this ontology
is quite comfortable for research theory.

Now a brief glance at some of the p h i l o s o p h i c a l   d i f f i c u l -
t i e s. Popper's ontology is very similar to Frege's. Frege's ontology is
a "thing" ontology and should be classified as an "objective idealism"
(rather than as a "hyper-realism"): objective because it does not — like
"subjective idealism" — assert that o n l y the psychic has full onto-
logical status; idealistic, because the "connection" between meaning and
referent remains subjective, i.e. mediated through consciousness. The
main objection against any kind of "objective idealism" is that it is absurd
to assert that consciousness can generate non-psychic entities. Although
the creation is only partial, always utilizing building stones from world-3,
the difficulty remains fundamental. Ontologists charge that Frege hypo-
statizes the contents of consciousness into something "non-consciousness",
non-mental, non-phenomenal. The same charge can be levelled against
Popper's ontology.

Frege's main motive for this move was anti-psychologism (to fore-

stall logic being reduced to "laws of thought", etc.) Popper, similarly,
wants to preclude the possibility of reducing the theory of science to
the psychology and sociology of research. As absurd as such a reduction
may appear, it continually reappears. Another of Frege's motives was
the problem of intentionality [14]. G. Bergmann has put forward powerful
reasons for not granting the highest ontological status ("existence") to
Frege's Meaning (*Sinn*) [15]; and these strictures would apply to Popper's
world-3 as well. But in agreeing to this criticism, we should not forget
that Bergmann is a professional ontologist interested in the *Aufbau der
Welt*, viewing philosophy as "a dialectical structure on a phenomeno-
logical base". On the other hand, Popper is looking for an ontology which
makes sense of and supports the task of the theory of science. It seems
to us that one can v i e w P o p p e r's o n t o l o g y a s a w o r k i n g
h y p o t h e s i s, and see here a division of labour: while the theory of
science and research indirectly make certain contributions to ontology,
the detailed working out of an ontology must be left to the "pure" philos-
opher (not to philosophy o f...). If one doesn't dare to carry on until all
of the philosophical difficulties have been cleared up, then one must
resign oneself to working on ontology for the rest of one's life. Our partic-
ular difficulties with Popper's ontology lie in another direction than
those of the professional ontologist. We shall presently turn to them, but
before doing so mention a criticism coming from different quarters.

    While an ontologist who, like G. Bergmann, stands in the tradition
of *IL*-philosophy will agree with Popper that o n t o l o g y is the *philo-
sophia prima*, a philosopher who, like K. O. Apel, stands in the tradition
of "transcendental" philosophy will disagree with this and point out that
in this way one looses sight of the "subjective" conditions of the possibil-
ity and validity of knowing, that one looses the "transcendental subject"
and thus falls back behind Kant and even Descartes. Apel would attempt
to mediate between the position which regards ontology as *philosophia
prima* and idealism which regards e p i s t e m o l o g y as *philosophia
prima* by means of his "t r a n s c e n d e n t a l p r a g m a t i c s of l a n-
g u a g e" which reflects on the rules which precede all conventions, which
make them possible — universal pragmatic principles which make speech
acts and successful communication possible. (The term "transcendental
subject" may be interpreted as the system of such principles.) On these
lines the above-mentioned thesis of the independence of world-3 would
be interpreted as the thesis of the non-identity of the author's meaning
(*Autorensinn*) and the meaning of the text (*Textsinn*). The latter is in the
Peircean way of expressing it: the meaning the text w o u l d (counter-
factually!) have for the "ideal, unlimited, communication community" in
the long run. At any rate the objection appears to be well-taken that

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[14] Radnitzky, 1974c, p. 153.
[15] Cf. Bergmann, 1964, esp. p. 135 and fn. 11.

Popper's ontology cuts short reflection on the conditions of the possibility of language communication, and hence of knowing: that it looses sight of a dimension of traditional philosophizing.

The difference between the two traditions comes out clearly when one takes a glance at the treatment of the p r o b l e m o f T r u t h: to Popper's ontology fits only the c o r r e s p o n d e n c e theory of truth, which got its hitherto clearest elaboration in Tarski's famous semantic definition of truth (essentially based on a mapping relation between a physical fact, the sign (world-1) and the form of a state of affairs (whether phenomenal (world-2) or physical (world-1) state of affairs). To Apel's transcendental pragmatics of language fits only a c o n s e n s u s theory of truth: true are those judgements (not expressions — true f o r human beings as we are acquainted with them) with respect to which the "i d e a l communication community" w o u l d, in the long run, reach consensus. (Of course the consensus of all and any empirical subjects is irrelevant to the definition of truth, although it may have to do with estimates of the justifiability of applying the term "true" in the above sense to concrete cases.) We have made this excursion into philosophy proper to caution those readers with primarily a philosophical interest, that our commending of Popper's ontology for the theory of science need not imply that we are insensitive to the risk that our p h i l o s o p h i- c a l friends are apt to accuse us of *naiveté*.

We now turn to our own d i f f i c u l t i e s with Popper's ontology — not as ("pure") philosophers or ontologists but simply as students of t h e o r y o f r e s e a r c h. It seems to us that Popper's attempts to guard the theory of science against psycho- and sociologism have thrown the baby out with the bath water in seeing no possibilities of treating entities which primarily belong to world-2 such as actions, production processes, creative acts, etc., in a manner other than that of empirical psychology. Popper thinks that we can learn more about production processes by studying products, that we can learn about products by study- ing production processes [16]. If one adds to these the further assumption that production can o n l y be treated as a world-2 entity, factual (em- pirical) mental process, then we have the following theses:

1) That knowledge about world-3 can clarify world-2 matters. We have already asserted this in a somewhat stronger form in saying that the history and sociology of science cannot be pursued without a model of scientific progress; already because without criteria of scientific merit one does not even know which developments are the important ones.

2) That konwledge about world-2 tells us nothing essential about world-3, i.e., nothing of interest for theory of science. This might be taken as roughly corresponding to our above assertion that the genesis

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[16] Cf. Popper, 1972, p. 114.

of products as well as their general acceptance or rejection is irrelevant to their quality control, and to the extent that this simply means that the empirical psychology of research is irrelevant to the problematic of quality control, we would of course agree. Yet, Popper's claim goes beyond this in claiming that production can o n l y be treated from a psychological point of view, and this assumption seems to us to be unjustified: we feel that one can treat production as belongings to a l l t h r e e worlds or realms, and that in research theory we m u s t do so. If we are interested in increasing the efficiency of production then it is naturally to the point to investigate the production process itself, in particular certain aspects of the production — treated as world-3 phenomena — which are not available in a study of products, and which can only be studied by "participant observation". Just why should it be impossible to treat actions, especially linguistic actions, to which thinking in the widest sense belongs, other than exclusively as world-2 p s y c h o- l o g i c a l entities? Popper himself comes close to our position when he says that language belongs to all three worlds. Only because they embody a meaning can certain things or facts (shapes) of the material world (world-1) become symbols, i.e., things which stand for something else: something absent, something thought, remembered, imagined, or even something false: for *Sinn* (world-3). And while the relation between a specific material sign and the meaning which it carries is largely conventional or arbitrary, the relation between the meaning and the act of judging is hardly merely accidental in this sense. A Husserlian would find it ironic that Popper lists Husserl's *Logical Investigations* in the bibliography to "Knowledge without a Knowing Subject", but in the article itself never mentions the man who attempted to demonstrate the essential relations between, e.g., act of judging and judgement in such a way that the act of judging is not seen to have a merely empirically psychological sense. In other words, phenomenology makes claims to being a transcendental philosophy (the real sense of which the Popper school has yet to deal with) and the judgement is preserved in its ideality (read "world-3 status" if one insists on a, perhaps, prematurely ontological statement) without being thereby "Platonised" either. In order to understand thoroughly the essential interconnections between production and product and thus to come to an adequate understanding of e i t h e r pole, we need to investigate the different ways in which each pole affects and is relevant to the other pole. On the basis of these considerations too, it appears important that a system-oriented research theory be developed that investigates research (not only products, but also strategies, criteria, etc.), and yet avoids the psychologistic-sociologistic reductionism. We would like to sketch a first step toward such a theory in the last section, with special attention to our affinities and differences vis-à-vis the Popper tradition.

4. SKELETON OUTLINE OF THE "SYSTEM-ORIENTED" THEORY OF RESEARCH

Since our programme has been presented in some detail elsewhere, we can be satisfied with a brief sketch here [17]. Our research theory focusses primarily on the component "research with its current interim results". Aside from such d i a c h r o n i c moments of research such as preparation, prospecting, the editing and formulation of relatively determinate results, etc., we also need s y n c h r o n i c   d i s t i n c t i o n s such as i n v e s t i g a t i o n in the strict sense, interwoven with other moments such as c o m m u n i c a t i o n and "s t e e r i n g" moments. It seems to us that the actions which constitute the research process (primarily all thought processes) can be dealt with under normative, evaluative aspect and in this context be conceptualized as consisting of world-3 entities. This is precisely the attempt of research theory: it abstracts as much as possible from the factual human components, from thoughts and ideas as psychological entities (world-2). Thus there is already here an essential difference over against the empirical psychological and sociology of research and science. The research process is grasped as a transformation of knowledge, problem situations, etc. It is in principle irrelevant whether these entities are historically exemplified. A research theoretical investigation can stylize the history of science (rational reconstruction); but it can also take v i r t u a l investigations — possible lines of development — as its object. In short, r e s e a r c h is grasped a s a t r a n s f o r m a t i o n of complexes which consist of k n o w l e d g e, p r o b l e m s, i n t e l l e c t u a l   i n s t r u m e n t s, p l a n s, e t c., i n t o i m p r o v e d complexes. The mark which distinguishes our enterprise from Popper's at this point is that Popper speaks of this complex globally as „knowledge", while we find it important to see various s u b - d i v i-s i o n s in "knowledge" in Popper's sense.

The first component of the knowledge-problem-instrument complex consists of k n o w l e d g e in the narrower sense: h y p o t h e s e s, e x-p l a n a t i o n s, t h e o r i e s, etc., as the interim r e s u l t s of research, and including p o t e n t i a l knowledge. Naturally, knowledge is seen to be essentially fallible. However well-founded it may be, it is to be accepted only *pro tempore,* which is a characteristic of empirical knowledge. An important evaluation criterion for this component is the degree of corroboration and perhaps also the degree of support through evidence as an indicator for the degree of "Verisimilitude".

The second component of p r o b l e m s consists of "knowledge", of propositions, which are expressly provided with a question mark. They are "objective" in the sense that the actions of the scientist cannot change this problem situation into something unproblematic (without precisely

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[17] See References.

a n s w e r i n g the question, thereby responding to the problem-situation
i n i t s o w n t e r m s) or simply ignoring it. In the physical sciences
these problem situations depend primarily on the properties of the world-1
territory, in the human or social sciences on world-1 and world-2 entities,
and in research theory on the properties of the world-3.

The third component consists of objective knowledge which is applied
in the form of i n s t r u m e n t s such as calculi, strategies, plans, etc.
Especially in the natural sciences there are world-1 instruments (hard-
ware) important, but these instruments can only be created and function
a s instruments, as components of a research process, thanks to world-3
entities in the form of the theories on which they are based and accord-
ing to which they are built.

As a further difference vis-à-vis Popper, we find additional distinc-
tions within the component "knowledge" in the strict sense: d i f f e r e n t
k i n d s o f k n o w l e d g e, and therefore d i f f e r e n t k i n d s o f
k n o w l e d g e - p r o d u c t i o n, of "transformation stations" for know-
ledge-problem-instrument-plans-etc. complexes. These include "stations"
for h y p o t h e s e s f o r m a t i o n - a n d - c h e c k i n g, for f o r m a-
t i o n o f e x p l a n a t i o n s, systems of explanations, f o r m a t i o n o f
t h e o r i e s, systems of theories, etc. "D a t a" may be treated here as
a kind of raw material for the production of knowledge (as evidence or
clues) since after their use they play no further role, i.e. they are not
built into a theory. These "transformation stations" are to be investigated
in a series of detailed models, all of which belong to the general model of
the progress of knowledge mentioned above.

At this point we can begin to make good on our promise to say
something about the evaluation of such models. The evaluation is guided,
by two criteria: (1) The pre-understanding which the model expresses
must be sufficiently realistic. Whether or not this is in fact the case can
only be determined when we attempt to apply the model in case
studies; and in this process one attempts to improve the models. (2) The
model must be fruitful for research theory itself, i.e. it must aid in the
identification of tasks which are essential for the development of re-
search theory. In other words, the distinctions which are introduced
concerning the relations between the components of the models ought to
identify problems whose solution contributes important new knowledge
about the progress of knowledge. This itself can only be determined in
a p p l i c a t i o n in concrete research-theoretical investigations.

We spoke earlier of the component of research which we named
"i n v e s t i g a t i o n". In concrete research processes this component is
interwoven with other moments such as "communication" and "steering"
(planning, internal criticism, self-reflection, diagnosis of difficulties, etc.).
The stategies that underly the "steering", the criteria, etc., can likewise
be treated under normative aspect, conceptualized as world-3 antities and

considered in their own right, quite aside from the question whether they are made the intention of any intentional acts of an empirical subject. The moment of internal communication between scientists must by and large be relegated to world-2. Still, it must be included in research--theoretical investigations concerning a c t u a l historical undertakings. Therefore, a system-oriented research theory is obliged to w o r k v e r y c l o s e l y w i t h e m p i r i c a l s t u d i e s, which can be done without falling into psychologism and sociologism. [The relevant p h i l o s o p h i-c a l problem here: to develop a philosophical theory about the interaction of the three worlds.]

When we inquire into v i r t u a l knowledge-problem-instrument developments, i.e., when we attempt to determine the "manoeuvre space" of a specific historical research undertaking, then we can ask just what it is that gives the line of development its d i r e c t i o n. From the many p o s s i b l e lines of development, only one (at most a few) was realized historically. (There may, of course, be two or more lines of development originating from one situation, though generally m a j o r competing lines of development will differ markedly with respect to what is accepted as "knowledge", which then conditions ,what is taken to be problematic.) We can thus investigate the factors which underly the task-determination preparatory to concrete work ("prospecting") as well as the running criticism of the interim results. In this model of s t e e r-i n g f a c t o r s, [18] it is argued that the most important of these factors is the p r e - u n d e r s t a n d i n g with respect to the general properties o f t h e t e r r i t o r y (especially on the quasi-ontological level of world--picture hypotheses) as well as the p r o g r a m m a t i c c o n c e p t i o n o f o n e's d i s c i p l i n e based upon this pre-understanding. "Pre-understanding" sounds psychological, or culturological, but what is meant are hypotheses (they may not even be seen as such by the scientist himself), and thus world-3 entities. Since the model presents and aids in the investigation of two groups of meanings (*Sinn*) — preconceptions (hypotheses) and ideals (criteria) —, thought experiments become not only useful but necessary: e.g., which assumptions on the level of world-picture m u s t b e p o s t u l a t e d as steering factors for a determinate, though perhaps virtual, development of knowledge-problems-instruments? [19] The model of steering factors should demonstrate which factors determine the mode of approach, influence the criteria, and thereby guide the movement of a research enterprise. The model must then be applied to the history of science, and controlled in detailed case studies. Here we find another difference with respect to Popper: the system-oriented research theory required very d e t a i l e d case studies, it cannot rest content

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[18] Cf., e.g., Radnitzky, 1974b, Fig. 2.

[19] Illustrations culled from the history of physics are provided in: **Radnitzky, 1974a**, esp. pp. 31 ff., **42 ff.**

with sketches. Of course the case studies are never an end in themselves; they have a ladder function: to extract philosophical, research-theoretical conclusions. This is masterfully done in Popper's work.

The research enterprise with its (internal) steering factors is embedded in an intellectual milieu as well as in a social and political environment. At this point research theory develops models for investigating the ways in which the "intellectual climate" and the "market" for intellectual resources influence the research enterprise and are in turn influenced by the results of research. Here again interest is focussed on propositional systems, on knowledge which is enriched and changed by other knowledge. Thus, here too research theory remains distinct from science studies, though a close co-operation is absolutely necessary for b o t h enterprises. This fringe area is, perhaps, that part of research theory as it is programmatically conceived here which is the most distant from Popper's concept of the theory of science.

So long as the output of research enterprises embedded in a research direction or tradition is satisfactory — a steady production of new knowledge, of innovations albeit of an ordinary magnitude, there is no reason for the researchers to question the "internal steering factors" underlying them (their "paradigm"). H e n c e the tradition slowly evolves: there are p e r i o d s approximating the ideal N o r m a l   S c i e n c e   in Kuhn's sense, plateaus.

B u t   w h y   d o   s h i f t s   i n   p e r s p e c t i v e ,   i n   t h e   p r e c o n-c e p t i o n s ,   o c c u r? To answer this, we have to study the *dynamics* of the research process. The transformation of knowledge-problem-instrument complexes is more closely specified by a break-down into "empirical" moments in the narrow sense (e.g. experimental-physical research processes) and "theoretical" or "systematizing" moments (such as the construction of explanations or theories, as well as the production of new knowledge by means of derivations from theories). They are interlocked — even if in advanced disciplines there is a division of labour which reflects the distinction (experimental and theoretical physics, e.g.) — but the distinction helps to understand the co-agency. "E m p i r i c a l" work consists chiefly of h y p o t h e s i s   f o r m a t i o n/t e s t i n g, hence essentially involves d a t a, and hence needs hardware instruments (data-generating technical systems). Therefore among its auxiliaries are (besides statistical techniques, etc.) the theories underlying the instruments. "T h e o-r e t i c a l" work is chiefly p a t t e r n   c o n s t r u c t i o n: explanatory patterns, theories (e.g. Newton's gravitational theory), systems of theories (e.g. classical mechanics); and hence it essentially involves m o d e l   m a k-i n g. Its main auxiliary is mathematics (as tool of model construction and information selection). The c o - a g e n c y, of "theoretical" and "empirical" moments is viewed as a flow of information, problems, conceptual frameworks. The model of the co-agency, like the others, have normative

implications (e.g. in reference to the timing of the interaction of empirical and theoretical moments — which can be illustrated by case studies from the history of physics). On the basis of these models one can construct a typology of objective research situations as well as of the possible manners of interaction among the various moments, all of which can be presented by flow-charts.

A further elaboration deals with the more detailed structures in the above models: thus, w i t h i n the "e m p i r i c a l" moment in the narrow sense one builds models for hypothesis construction, hypothesis checking, data generation, and w i t h i n the "t h e o r e t i c a l" moment one builds models for the construction of explanations, theories, etc. Here we can only give a glimpse: sketching a sort of co-agency which is easily illustrated by the history of physics.

One type of this t a c k i n g between the two levels is one originating in pieces of knowledge which are produced in empirical work and which are residua with respect to the veteran: whenever "empirical" work produces a corroborated hypothesis a law (description of observed regularities in nature), this a u t o m a t i c a l l y poses a problem (world-3 phenomenon!) for the "theoreticians": to explain the law, to incorporate it into the existing body of accepted knowledge (of theories accepted for the time being). If they succeed in deriving the law, in incorporating it into the body of knowledge, they have shown that the claim to novelty was spurious and achieved a consolidation of both law and veteran theory. If the law cannot be explained, they will try to strengthen the veteran theory such that the law is derivable from the improved version. If the new piece of knowledge resists all attempts at explanation (a residual that grows (e.g. the spectroscopic knowledge before 1913)) or if it manifestly clashes with the veteran theory (e.g. Michelson's findings), the f l o w between "empirical" and "theoretical" moments i s d i s t u r b e d. To restore the balance which is characteristic of Normal Science, the "theoretician" has to make a b o l d "c o n j e c t u r e" (e.g. Bohr's old quantum hypothesis). This conjecture may form the c o m m o n c o r e of the premises of a growing system of deductive patterns such that one of the patterns constitutes an e x p l a n a t i o n of the residual piece of knowledge, while the conclusions of the o t h e r s make n e w statements about the real systems investigated. Or more exactly, after having given them "empirical import" (which is necessary since originally the conclusions refer to the model of the real system studied) the hypotheses derived by means of the conjecture contain virtual n e w knowledge about the real system investigated. If these hypotheses ("predictions") are corroborated by empirical tests, this i n d i r e c t l y gives s u p p o r t to the c o n j e c t u r e: a flow of information, this time from the "empirical" level to the "theoretical". In significant cases the extraordinary innovation on the "theoretical" level which has restored the balance in

the flow between the levels involves a s h i f t   i n   t h e   p r e c o n c e p-
t i o n, in the "internal steering factors". Often this perspectival shift
is first implicit and only later recognized. (The sequence sketched corres-
ponds roughly to the Kuhnian type of "scientific revolution". However
for stylizing the history of science with its "fusions", "branching off",
etc., one needs more than the Kuhnian dichotomy [20].) Since the model
designed to map certain aspects of specific real systems always has
also some negative analogy — already because model making in-
volves abstracting, simplifying, idealizing, etc. — there   m u s t   sooner
or later arise such a   c r i s i s   situation which may lead to a "scientif-
ic revolution".

When a perspectival shift has occured one will ask "d o e s   i t   c o n-
s t i t u t e   p r o g r e s s?" To answer this, one needs an *explicatum* of the
concept of progress, and, if comparative evaluation of theories or of
"internal steering factors" is to become possible, criteria of scientific
value. Against the prevailing trends of relativism in the theory of science
(e.g. Feyerabend's "epistemological anarchism") or the élitism of various
sorts (Kuhn, Toulmin, Polanyi) it is held that there are   t y p e s   o f
c r i t e r i a   t h a t   o v e r - a r c h   r e s e a r c h   t r a d i t i o n s. Even if
they have no universal application, i.e. in concrete research situations
a compromise may be required, when one wishes to do science one is
bound to honor them — or be unsuccessful. Thus research theory contains
explicitly   n o r m a t i v e   moments: it develops models for the criteria
of different types of evaluation of different kinds of products, and of
different kinds of knowledge such as single hypotheses, data, models of
explanation, etc. Taking the degree of corboration of a hypothesis as
a (fallible) index of its degree of "truth content", may be regarded as
an attempt to produce a connection between the theory of hypothesis-
-checking or -control and philosophy proper (here especially ontology).
"System-oriented" research theory attempts to articulate and legitimize
criteria not only for the evaluation of products, of the output of research
anterprises, but also to devise criteria for strategies, etc. Thus not only
quality control of products but an analogue to cost-benefit-analysis is
sought for: also the efficiency control of processes is a desideratum.

Insofar as research theory is research, its models of knowledge produc-
tion should be applicable to itself. The "steering factors" underlying re-
search theory have been hinted at. The "theoretical" moments in research
theory centre around model construction (models such as those outlined
above) and specified models of hypothesis formation/checking, etc [21]. The
"empirical" moments are historical case studies and participant observa-
tion of on-going research.

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[20] Cf. Heelan, 1971; Heelan, 1972b.
[21] Cf. Radnitzky, 1974d, pp. 96–102.

The distinctions between the Popperian approach and this type of "system-oriented" research theory appear (1) in the e m p h a s i s on the fact that there are d i f f e r e n t kinds of knowledge, which leads to more d e t a i l e d models, which are to be controlled by means of detailed case studies. (2) A further difference is the view that not only historical case studies, but a l s o o n - g o i n g research processes should provide material, since (as Feyerabend has also argued) information concerning certain aspects of research can only be obtained by "participant observation". (3) Finally, the fact that we require a c l o s e c o o p e r a t i o n with not only the history of science but also the e m p i r i c a l "science of science" makes us much less autonomous than the Popperian orientation; and (4) that we trust that in spite of such an attitude it will be possible to avoid falling into the traps of psychologism and sociologism. (5) A further difference we mention only in passing: a different view concerning the relationship between natural sciences and the *sciences humaines* (in particular the hermeneutic/critically-reconstructive social sciences and *Geisteswissenschaften*). All these disciplines must have something in common — otherwise we would not be entitled to speak of "research" in all cases — e.g., certain aspects of hypothesis-checking. On the other hand, it appears to us that, e.g. the hermeneutic/critically--reconstructive social sciences, although they contain the nomological social sciences as one essential moment they also have very special methodological problems of their own in addition to those common to all research. As we would deny the usefulness of the distinction between pure and applied research with respect to the typical human sciences (or to these moments which are most characteristic of them), we submit that the problematics of scientific merit or intellectual quality cannot, with respect to these group of disciplines, be separated from that of social merit (from *Praxisbezug*). That, as soon as, in the relevant sense, a "unified science" thesis is not presupposed, one may no longer transfer an *explicatum* of Scientificity or Scientific Progress that is adequate for the natural sciences to the *sciences humaines*.

The Popperian "school" appears to be interested only in the nomological social sciences. We would even doubt that the typical hermeneutic//critically-reconstructive moments of the *sciences humaines* can be adequately dealt with by a "system-oriented" approach. Hence for these disciplines we feel that the theory of research hinted at above has to be complemented by a special theory of research tailor-made for the *sciences humaines* [22]. On the other hand, we have great hopes of entering into a dialogue with the "Popperians": through the self-reflection of the Popperian orientation upon its own way of procedure [23]. It is clear that

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[22] Cf. Apel, 1973; Kockelmans, 1969; Radnitzky, 1968–1973, Part II.
[23] Cf. Apel, 1973, II: 112, 222, 225, 248, 251.

Lakatos' theory of science is a typical hermeneutic-normative discipline — and hence illustrates that producing knowledge a b o u t knowledge production in physics is a very different activity from physics, from producing knowledge about nature, that the theory of physics is one of the *sciences humaines.* Even that it is more like philosophical reflection than like research.

It may well be that the picture of the research theory we have presented — and which still is in a largely programmatic stage — looks a great deal less original than one might have initially thought. It is obvious that we are in many respects the disciples of Popper. But for a theory of research originality seems to us to be a great deal less important than usefulness. In an age in which science and the rationalization of science policy making is a central concern, we feel that this broadening of the original Popperian framework may, and the last section may be rather speculative — but as Popperian theory of science tells us it is creditable to make bold conjectures.

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