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AFTER THE RECEIVED VIEW – Developments in the Theory of Science

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in memoriam
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Content

Introduction:
Developments in the Theory of Science

In the twentieth century the syntactic model in the theory and philosophy of science, the so called *Received View* – R. Carnap, C. G. Hempel, H. Reichenbach, E. Nagel –, is paradigmatic for answering questions on meaning, significance and validation of theoretical statements and of our scientific knowledge. Since the 1960's the *Received View* is challenged by the naturalistic (realistic), the sociological, the structuralistic and the constructive empiricist (representational) accounts of the "correct view" on our scientific knowledge. We take the *Received View* to frame the recent developments as an initial systematization. Today, there is a shift to a semantic account and the main interest is in transparadigmatic orientation. This is reflected by the ontologies of theory-languages, the theory of meaningful measurement and by analyzing the structure of experience, especially. It is there, we have to ask how ontology and empirical knowledge (beliefs) are interconnected with the issues of underdetermination. The description and systematization of theory change are the major topics here. Theory change is not just a problem of a dyachronous perspective but of our theorizing on the dynamics of beliefs – of our theory of decision and rationality. A lot of former scientific concepts, e.g. of Newton, Einstein, Duhem, Fisher are to be revisited then.

By shifting from object-level of empirical science to the meta-level of philosophy of science, we mention the means the scientist uses. But picking out the logico-linguistic distinction of 'use' and 'mention' as a central theme has to be supplemented and precisely defined by the instruments of classical logic. *Wilhelm K. Essler* – in the context of his suppositionalism and conditionalism – conceives of his logical operationalism as cognitive semantics. Following Tarski, he analyzes parts of the philosophy of science in terms of a level-ordered language. These levels are of different power in generating empirical knowledge. *Essler* gives an elementary exposition of the language of classification and comparison. This exposition conceptually clarifies our elementary perception and observation as well as parts of comparative experimental measurement. Application in detail is referred to epistemology. Leveling languages shows a way from the philosophy of science to "some metaphysics of experience" for physics as well as it shows for our empirical knowledge in general to be part of philosophy of science. Logical operationalism – different from a more or less "radical" structuralism in this respect – takes into account the metalogical research on antinomies. Consequentially, in systemizing empirical laws and their background assumptions Ockham's razor is applied. Choosing a theory-language and its ontology is economy-minded.

It is common to contemporary extensionalist approaches towards philosophy of mind, cognitive science and artificial intelligence to assume there is a distinction between pure syntax and semantic interpretation. The origin of the syntactic account and of the term 'pure syntax' is traced back to Carnap's distinction between pure and applied syntax and semantics, and to formalist analysis of mathematical systems as uninterpreted token manipulating games. Yet, sciences aim at developing empirically adequate theories resting on both the analysis of the semantical structure of scientific knowledge (theories/language) as well as the analysis of the concept of observable/empirical objects. *Gerhard Preyer* discusses the semantics of the syntactic account, i. e. the junctim of significance (confirmation) and meaning. References are made to the method of reduction – as of Carnap, C.I. Lewis, A. Pap – and to the structuralism of W. Stegmüller and J. D. Sneed. It is the sociological and historical account of scientific discovery going back – more or less – to Kuhn and the controversy on incommensurability und comparison of theories that dominates this discussion of the logico-semantical structure of scientific knowledge. Yet, it is to name in this context also N.R. Hanson, M. Fleck and M. Polany. In particular Fleck has firstly developed an account like Kuhn in his *The Structure of Scientific Revolution*. *Preyer* argues Popper can be defended against the critique of P. Kuhn, P. Feyerabend, I. Lakatos, H. Putnam. Hence, the Popper-Kuhn-controversy should be done with D. Davidson's critique of Kuhn's incommensurability-thesis which is directed against this third dogma of empiricism and its conceptual relativism rooted in a fundamental distinction of conceptual scheme (prediction, organizing, facing, or fitting) from neutral content (nature, reality, sensory promptings). Following Davidson here there is no way back to the dungeon of paradigms and the Kuhnian misconception of scientific activity and endeavor. Contrary to Kuhn: comparing theories is possible, even if there is a shift of meaning and reference. All validating scientific knowledge is taking place towards a background of beliefs which themselves can not be questioned all the time. These background-beliefs guarantee for any single belief's being true. An interpreter's actually translating beliefs embedded in (utterance-, sentence-) meanings proves the dichotomy of scheme and content implausible.

In the tradition of the syntactic account there is a special problem called the "Goodman-Paradox" and "Goodman's theory of projectibility" of hypotheses – for Goodman entrenchment of predicates is only one feature of projectibility. *Schwarz* explores three issues concerning Goodman's so called "new riddle of induction". They are first: considerations of extensionality, second: limitations of the present theory of projection, and third: entrenchment and appeals to innate quality space. Schwarz argues the solution to the "new riddle" depends on psychological notions rather than syntactic or formal semantic properties.

The sociological and the historical account of scientific knowledge do not ask for trans-paradigmatically validating and justifying scientific knowledge. Answering this question on the other hand is "one" task in the theory and philosophy of science, i.e. science aims at supplying us with theories which are empirically adequate – through the change of scientific knowledge, as well. *Jeffrey E. Foss* shows in detail the error of both views the syntactic and sociological account, and – to take their place – he offers a new account, the information-economic account, which reveals the structure of science both at a time and through time. This regards the description and systematization of theory change by the syntactic account, i. e. thesis that new theories reduce older theories and the incommensurability thesis as part of the sociological account of theorizing. *Foss* constructs a five-fold unity of science for the ubiquitous exchange of information between the various scientific disciplines: 1. sociological, 2. logical, 3. ontological, 4. epistemological, and 5. temporal. Following this account, science grew both by elimination and accumulation.

Structuralism (W. Stegmüller, J. D. Sneed) carries on to the question, which language is used and to the consequential problem of immunizing theories against "recalcitrant experiences". The example here may be the rule of auto-determination, i. e. a theory defining the scope of its applications by itself. Therefore, at first, it is necessary to explicate what natural or physical law means and how these laws relate to scientific knowledge. *C. Ulises Moulines*, successor of W. Stegmüller at Munich University, takes (naive) set-theory not to be characteristic for the non-statement-view. He takes it just to be technically useful and the background-theory of structuralism to be a moderate (conceptual) holism. For *Moulines* the major difference between structuralism and different versions of operationalism – P.M. Bridgman, Carnap's method of reduction sentences in the 1930's et al. – consists in the semantic structure of scientific knowledge. The major question is: What is fixing the meaning of quantitative functions, e.g. of mass, distance? – or respectively: How is the extension of functions fixed in general? *Moulines* disputes the semantic analyses of quantitative functions that in the end come up with: the operation is the meaning. He votes for a moderate holistic picture of semantic concepts of scientific theories. This is contrary to P.

Duhm and W. Quine. *Moulines* takes clusters of concepts to be characteristic for scientific theories. Hence, identity of a theory does not consist in a single but in a specific group of concepts and its correlation. Systematic connection of concepts usually is called a law of a given theory, e.g. Newton's mechanics as a cluster of concepts of particle, time, distance, mass and force.

The building of empirically adequate theories is primarily concerned with theoretically and semantically analyzing quantitative concepts. It is commonly supposed that each and every observationally based quantifications represents a measurement. But this is erroneous. Many perfectly good quantities do not measure anything e.g. the number of 3's in the distance in kilometers between two cities. *Nicholas Rescher* clarifies what it is that makes a quantity a meaningful measurement. It is easy to list necessary conditions here e.g. effective determinability, reproducibility, robustness, validity, nomic involvement, predictive utility, and descriptive dimensionality. But unfortunately the issue of sufficiency is less easily handled and in fact has not really been resolved to date. Among the quantities whose claims to measuring something are distinctly questionable are quality of life and human intelligence (IQ). *Rescher* intends to conceptualize meaningful measurement by surveying so called fallacies of quantifications, e.g. what one can not quantify is not important, everything can be quantified, quantification and measurement are one of the same.

The problematic aspects of the concept of a physical law are well known. *R.I.G. Hughes* examines three of them in his contribution: the problem of modal force, the problem of accidental generalizations, and the problem of ceteris paribus clauses. It is argued that these problems are inescapable if laws are seen as a privileged class of empirical generalizations, but disappear if we consider laws as they appear in theories – provided that we view theories in the way that the representational account of theories suggests. This account is a version of the semantic view of theories proposed by van Fraassen and R. Giere. *Hughes* fixes the place of theoretical laws on the representational account, i. e. all theoretical laws are true in a model a theory defines. A model is a theoretical structure intended to represent parts of the world – in contrast to structuralism. Newton's law of motion provides a case study; and the representational account sheds light on the way they are deployed in Newton's "Principia", and so defuses the criticisms of Newton's procedure made by Poincaré. In this context, Einstein's approach of theorizing is informative. *James R. Brown* examines Einstein's distinction between constructive and principle theories and discusses several of the properties of principle theories in relation to visualization, thought experiments, and the question: Did Einstein change his mind and move of from a youthful positivism to a mature realism? Brown argues that understanding principle theories sheds much light on these matters. He interprets principle theories by linking them to what are called "phenomena".

Rather than summarizing or explaining historical scientific practice or attempting to justify individual beliefs, learning theory is concerned with the existence of inductive methods converging on the truth of increasing empirical data. *Kevin T. Kelly* and *Cory Juhl* illustrate the learning-theoretic approach in their paper by establishing necessary and sufficient conditions for "almost stable" identifications. Which requires that the method be guaranteed to stabilize the state producing only correct hypotheses drawn form a finite set. *Kelly* and *Juhl* show that the proof of the theorem involves the construction of a universal architecture for almost stable identification. This elaboration amounts to theorizing on the change (dynamics) of beliefs from old to new. R.A. Fisher is rightly credited with developing many statistical methods, yet paradoxically while statistical inference requires the use of probability, few appreciate the distinctness of Fisher's account of probability and its intuitive relationship to his statistical philosophy. *Harriott* recalls Fishers merits for introducing the discipline of statistics. He presents a sympathetic reading of Fisher's account of probability, his style, his account of statistical inference, and his interpreting on probability. And it is shown how this dovetails with his underlying philosophy of statistical inference.

Laboratory experiments have shown behavior in the ultimatum game which commentators have found anomalous from the point of view of rational choice paradigm (decision/game theory), e.g. economic theory (R. Thaler). So, we are confronted with the problem: "How could such behavior have evolved?" *Brian Skyrms* investigates the evolutionary dynamic of a symmetrized ultimatum game. There is one population and members of that population sometimes assume the role of ultimatum givers and sometimes the role of ultimatum receivers. *Skyrms*' study supplements the investigation of the ultimatum game between two populations by J. Gale, K. Binmore and L. Samuelson. In both cases there are (somewhat different) conditions under which the "anomalous" behavior can evolve. In the case investigated in his study, even when the anomalous behavior does not evolve, other weakly dominated strategies typically survive in a population polymorphism. Results call into question the descriptive adequacy of sequential rationality and subgame perfect equilibrium.

Quine's scientific realism means, we are ontologically committed to what a theory says there is. He formalizes ontic decisions of modern science and our scientific knowledge, i. e. the (modern) ontological commitments, such that properties (universals), concepts or forms are removed from the scope of science. *George N. Schlesinger* focuses on properties basing on "degrees of characterizations". A full-fledged predicate '$\prod$' which applies to an individual *i* has complete power of characterization through its capacity to set *i* apart from every other particular that lacks '$\prod$'. On the other end of the spectrum there are predicates with zero power of setting *i* apart from anything. In between, there are predicates with varying degrees of differentiating capacities. Special references are made to M. Black's critique of the principle of the identity of indiscernible and D. Amstrong's insistence that "being identical with ..." is a pseudo-property. Following illustrative examples, it is shown that his inquiry, which may have seemed to belong exclusively to the abstract study of ontological commitments has several down-to-earth implications. The issue shows that ontology and the scientific knowledge are to be seen interconnected.

B. van Fraasen disputes Quine's scientific realism and various issues in underdetermination of theories. In effect, van Fraasen's constructive empiricism rejects the syntactic account of theories and also in part, the structuralist account. The interconnection between ontology and scientific knowledge is the theme of *Carl A. Matheson's* critique of van Fraassen's constructive empiricism. His account is based on a selective scepticism concerning our knowledge of unobservable, which in turn is motivated by the underdetermination of theory by observation. He argues that the observable components of our theory are equally prone to underdetermination arguments; theories can be observationally adequate without making true claims about observables. *Thomas R. Grimes* completes Matheson's issue. He develops a new solution to the problem of underdetermination as it applies to the account of scientific realism and argues that the solution is an improvement over other proposals that have been offered for overcoming this problem. *Tang* scrutinizes scientific realism as eminent in Paul and Patricia Churchland's eliminative materialism, i. e. their version of identity-theory. They attempt to refute the argument from introspection – as used by the dualist to support his non-materialist position. They present several parallel arguments to show that the dualist's arguments are invalid. Tang examines all these arguments and argues that the Churchland's beg the question of the identity of mental states with brain states as much as the dualist begs the question of the non-identity of these states. This result is due to the underdetermination of the two positions. He then goes on to analyze why the Churchland's claim the identity of these two states and argues that they do so by blurring the "is of predication" with the "is of identity"; by confusing contingent identity with necessary identity; and by conflating two senses of scientific realism, which *Paul C.L. Tang* calls the "immanent sense" and the "metaphysical sense".

Normally, we hold scientific knowledge to be intrinsically rational and science traditionally is taken to be a model of rationality. But there are different interpretations of scientific rationality and conduct. *David Resnik* explores the limitations of the instrumentalist model of scientific rationality and discuss alternative models: the dynamic accounts "conventionalism", "reflective equilibrium" and "naturalism". He does not suggest that we abandon the instrumentalist model, since it still has many useful applications. However, we need to recognize its limitations and be open to alternative approaches to scientific rationality that can successfully account for the phenomena that this traditional view cannot accommodate. The instrumentalist model most clearly applies to local decisions relating to specific epistemic goals and methods, but another model should be invoked in order to understand global decisions concerning general epistemic goals and methods. He argues that an dynamic and naturalistic model of scientific rationality provides us with the most promising approach to understanding global decisions in science. *Aldo Montesano* examines the different meanings of the term rationality in economics and proposes a framework where they can be placed. He introduces a distinction among the rationality of theory, i. e. economic theory is deductive, the rationality of action, i.e. agents actions are intentional, and the rationality of agents' preferences on actions, which implies three different sub-rationalities: the rationality of preferences on consequences, the rationality of expectations and the rationality of the function which determines agents' preferences on actions. Further on, he introduces a formal representation of these rationalities and their role in economics commented upon.

For Popper scientific practice is intrinsically rational. His view of science as a social phenomenon and of scientific theories as refutable renders science much less ideal than the traditional account of it as proof. *Joseph Agassi* argues nevertheless, pluralism requires further relaxation of Popper's account. Viewing science as the result of our filtering from the world what we value, renders it much less ideal, since what we value may and usually does come in a mixed bag. *Agassi* shows that Popper's view of normal science as dangerous, then, is an exaggeration, since it is a social phenomenon; we may judge its output post hoc and decide what part of it we value.

Although metaphors are not well esteemed as part of scientific language, we realize they can not be secluded. So, they seem to be functional. *Michael Bradie* demonstrates that the heuristic dimension of metaphors in science involves the use of metaphors in the construction and development of new theories or new formulations of the old based upon perceived or created analogies or similarities extrapolated from prior theories or from analogies or similarities extrapolated from experiential realm – i. e. not from any formal theory. The cognitive or epistemic dimension of the role of metaphors in science involves the application of theories to reality. *Bradie* argues that the cognitive or epistemic role of metaphors in science is more significant than is often realized. When theories are applied to the world, the application of the theory results in a metaphorical redescription of the phenomena to be understood. In normal science, the range of application of theory is extended via exemplars – worked out examples which, when successful, are shown to fit the data. *Bradie* presents some illustrations from various disciplines and concludes with a brief consideration of some objections and some remarks on the implications of his analysis for some central epistemological and metaphysical issues of our scientific knowledge.

As others have remarked, discussion of the values relevant to work is rare in the science literature, in spite of the great and growing importance of such issues e.g. Putnam. *David Gruender* briefly explores possible reasons for this, and revisits the fact-value-distinction. Above all he examines major ethical theories for their possible bearing on value issues in the sciences. References are made to the semantics von value-statements of logical empiricism, the emotivists (A. J. Ayer, C.L. Stevenson), the naturalistic fallacy (G.E. Moore), to R. M. Hare's approach and J. Dewey's application of value theory to the sciences. A version of the theory identifying values as existing or as potential facts important to human beings is sketched showing its application to individuals and social groups. Some consequent global implications are sketched, as well. Anyway, we should remember: Wertfreiheit remains an unquestionable constraint on object-theories of scientific conduct. Wertfreiheit guarantees for objectivity, rationality and acceptance of our scientific knowledge. That does not exclude scientists from being committed meta-theoretically to a demanding ethics they use during their common scientific affairs. But they do not have to.

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