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Conventionalism and its Impact on Logical Empiricism

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In my paper I want to do three things : first, to give an outline of the philosophical scenery into which the conventionalistic approach brought about a new perspective. Then I want to sketch the important themes of the Austrian empiricists and the impact of the French philosophers of science on the Viennese philosophers. Finally I shall try to evaluate the interplay between the French and Austrian philosophers and to point out the far-reaching change which it brought about in present day-philosophy. I am convinced that what we are involved in, namely in an attempt to understand the main line of thought of the conventionalistic thinkers, is not only interesting from a historical point of view, which it surely is, but much more so from a philosophical point of view. If it is really true what I think is true, namely that among those philosophical ideas which had the deepest influence on philosophy in general and especially on philosophy of science in this century, conventionalism and pragmatism were the most fruitful ones. Biased as I am in this regard, I do hope that this conference will lead to justify this opinion.

§1.

As Auguste Comte optimistically had foreseen, sociology and psychology were the latest among sciences which freed themselves from their old mother - philosophy. The fast development of the natural sciences in the 19th century was one of the reasons why materialistic ideas were flourishing. However, the main attraction were the different scientists and physicists like John F.W. Herschel, James Clerk Maxwell, Hermann von Helmholtz, and mathematicians like Augustin-Louis Cauchy or Carl Friedrich Gauß and Richard Dedekind, who are just mentioned here as examples of a rich and important concentration of the scientific community in the earlier decades of the last century. In the later part of it one of the most important revolutions, the „Darwinian Paradigm“ (M. Ruse), began to influence also the other sciences and philosophy. The crude materialistic explanations we find for instance in Ludwig Büchner, lost their significance because of the psycho-physics of Fechner and his followers. With these ideas the first steps of a scientific physiology were made in a field where also Mach started his early experiments. One has only to read the writings of Ernst Mach and Ludwig Boltzmann to realise the strong Darwinian influence on their general view of nature and of theories.

In the mainstream of this development we see also Franz Brentano, who even during his priesthood tried to present Comte as an important philosopher to the German Catholic audience. Few years later, when starting his career in Austria, he published the famous *Psychology from an Empirical Point of View*, a work announced as comprising six volumes, of which only the first one in 1874 was published according to the original plan. With this book started what Brentano had called the project of „descriptive psychology“ or „beschreibende Phänomenologie“. Descriptive psychology - contrary to genetic psychology which aims to explain the causes of the psychological phenomena - was thought

to describe the phenomena of the human consciousness, the elements of our psychological life. Its task therefore is, first of all, to provide a clear criterion to distinguish the psychological from the physical phenomena. As phenomena count only what can be object of our inner perception, that is objects like our thinking, our joy, our pain. They all show as their own character their directness towards (intentional) objects.

The large school of Brentano, with its outstanding members Anton Marty, Alexius Meinong, Kasimir Twardowski, Thomas G. Masaryk, Christian von Ehrenfels, Carl Stumpf, and Edmund Husserl, turned out to become the most influential one in the Habsburg empire where all just named, except Husserl, were renewing philosophy from the eighties of last century on. The rich topical variety and the method provided by Brentano himself gave enough ground to form a kind of scholastic, using the best of the neo-Aristotelianism of the 19th century and defending a scientific and objectivistic program of philosophy.

When Bertrand Russell published an article in 1924 in *The Dial* on the „Philosophy in the Twentieth Century“, he classified this „new philosophy“ by attributing to it two aims : „analysis as method and pluralism as a metaphysic“ ; to this characteristic he added that this philosophy is „not compatible with Kantian and Hegelian idealism because it rejects the logic upon which those systems are based“ [Russell 1924, 450-466]. As typical of this „new philosophy“ Russell mentioned that it conceives of all knowledge as scientific to be verified and proved by scientific methods. Another special concern of this new philosophy mentioned by Russell is the pre-occupation with logic and mathematics and under these especially with the logical analysis of propositions.

The very basis for this evaluation was Russell's profound knowledge of Meinong's philosophical work which confronted him with the idea that it is our narrow-minded concentration on the realm of the real which prevents us to accept the theses that also the non-existent and even the impossible objects must have some kind of being in order to be thought about in order to enter our judgements etc. While in the world of Meinong it is true that „the golden mountain is golden“ and that „the round square is round“ the robust sense of reality of Russell had to claim the non-existence of the golden mountain while the contradictory predications to an impossible object like the round square were nothing but logical howlers. Russell therefore had developed his theory of definite description in 1905 to reject Meinong's theory of non-existing objects. He thought of these object-theoretical propositions like the round square, which do not exist. They lead to absurdities which his theory could avoid. But he underlined that „absurdities are the experiments of the logician“ [*op. cit.* 462].

One of the important features of philosophy in the Austrian Empire was that the strong relation to the empiristic tradition remained alive all the time at least since Theodor Gomperz, the father of Heinrich Gomperz, started the edition of J.St. Mill's works to be translated into German. And actually also Brentano himself was eager to get in personal contact with Mill. And it was him who directed also Meinong's interest to English philosophy, to study Hume's theory of relation to criticise Hume's theory of abstraction.¹ As it is known, the lively discussion between Meinong and Russell culminated such that fifty years after Meinong's death Gilbert Ryle could declare that Meinong's theory of objects is dead and will never be resurrected. Ryle had only overlooked that at the very time of his judgement the rise of the new born existence-free logic and of Meinongian semantics was already under way² However, this is another story.

There is no doubt that in present-day philosophy we have to acknowledge a strong interest in the philosophical theories of Brentano, Meinong and other members of this school, like Christian von Ehrenfels or Kasimir Twardowski. And this has to do with the fruitful research program which Brentano had to offer. According to Brentano, the source of knowing our inner world, the realm of the psychic phenomena is inner perception, the only kind of perception which should provide us with evident knowledge, because, according to Brentano, in all our psychic activities is included also the evident consciousness of these psychic phenomena. Thus Brentano is eager to provide a general theory of all kinds of objects of the inner world, an ontology and theory of categories.

The main division of two parts of the new science of psychology distinguishes between that theory which investigates the ultimate psychic parts which is such that out of them the totality of the psychic phenomena can be reconstructed. Brentano thinks that his procedure could lead to a kind of a *characteristica universalis*, as Barry Smith has called it. The other part, genetic psychology, should carry out the task to investigate the laws and provide the explanations for their coming and going, their causal relations. In a certain sense it is true that Brentano throughout his many philosophical changes remained an Aristotelian. This can easily be seen when we take a look at his readings of Poincaré and his different critical notes on Poincaré's philosophy of science. Especially concerning Poincaré's theory of space and his geometrical views, Brentano remains mainly negative. At different periods of his philosophical development Brentano dedicated much time and work to form a theory of space

1 Vide: A.Meinong, „Hume-Studien I: Zur Geschichte und Kritik des modernen Nominalismus“ (1877). In: Gesamtausgabe Bd.1, Abhandlungen zur Psychologie, Graz 1969

2 Vide: *Jenseits von Sein und Nichtsein*. Hg. R.Haller, Graz 1972

and time. One of the recurring questions was, where the presentation of space does come from. Following Aristotle, Brentano's central idea is the so-called nativistic idea that our presentation of space comes from the senses, seeing this presentation in every sensation. According to the Aristotelian theory, this is so because it is sensation which reveals to us spatially extended and shaped concreta, their movements involving the presentation of space. From this we are enabled to abstract also the spatial concepts. The contrary position to the nativistic, known under the label the empiristic one - denies that in sensations, for instance in sense-impressions of our eyes, we have any presentation of space at all [Brentano 1988, 138]. This anti Aristotelian position was alive for instance in J.St.Mill's theory and commonly held in English philosophy after Berkeley. It was also brought to acceptance in Germany by Johann Friedrich Herbart and Hermann von Helmholtz. Their theory sustains the view that the presentation of space is neither a priori in the Kantian sense nor is it provided by sensation but solely by the experiences. The third position - named „anoetic theories“ - denies the nativistic Aristotelian-Lockean one which Brentano himself holds as the true one, as well as the empiristic one, and this was Poincaré's.

Not only because of this is Brentano criticising Poincaré in different fragments of his Nachlass. That for instance Poincaré did not accept and use the traditional Aristotelian concept of axioms as something which is self-evident Brentano views as a crucial mistake in Poincaré's theory of space and geometry. That conventions and comfort should serve, where only evident knowledge was asked for, seemed to Brentano a completely unacceptable position. He calls it „a confusion of arbitrary stipulations of linguistic meanings with an arbitrary choice of hypotheses, which have to be tested.“³ There are other parts of Poincaré's theories which are criticised - like his concept of the continuum or his calculus of probability - but I think we need not go further into these because I do not see that Brentano's critique at that time did have any wider influence or acceptance in the scientific community. As arguments against the conventionalistic strategy they seem not at all very convincing.

What this critique however does bring into the open is that at least in Brentano's inner circle the new French philosophy was taken seriously and could not be ignored. That Brentano's philosophical principles based on the evidence of inner perception did not permit to conceive of another kind of geometry than the Euclidean one, that *conventions* for the sake of convenience should be a surrogate for truth may have had too much the smell of an easy way out of serious difficulties, a way which could lead away from the true way of

3 F.Brentano, „Zu Poincarés Erkenntnislehre“ (1916), in: Versuch über die Erkenntnis. Hg. A.Kastil, erweitert und neu eingeleitet von F.Mayer-Hillebrand 1925-19702, p.207-236, p.222

philosophy back to idealism. Against this alternative also the strong empiristic wing of „the Austrians“, meaning here : the Brentanoists, were in opposition.

§ 2.

Hundred years ago Ernst Mach was asked to accept a call to the chair which originally was Brentano's, only that the description of the chair had been changed. It now included the additional clause „Philosophy : in consideration of the history and theory of the inductive sciences“ (unter Berücksichtigung der Geschichte und Theorie der induktiven Wissenschaften). The first course, and the only one he gave (because of the stroke which prevented further ones and led to his retirement in 1902) found its (revised) form in Mach's book *Erkenntnis und Irrtum*, a general theory of science. Three beliefs characteristic for Mach's philosophy of science appear at once to any reader of this work. The first is the Darwinian perspective which Mach very early adopted in order to understand the evolution of individual and collective knowledge. Mach stated, it is foremost the individual memory which serves as the valuable instrument to use experiences, and it is also the communicated memories of the ancestors which can be used and are to be used by the individual. Thus within the individual mind a world picture is formed in the most economic way concentrating the labour of many generations of many individuals in only one. This thesis is held in application of the principle of evolution and the principle of economy which is nothing but the application of the general evolutionary outlook to knowledge production. According to Mach's deepest conviction that „history has done everything so far, history may change everything...“, he is deeply convinced that science, like any other creation of man, is a product of history. But since the products of history are contingent, Mach does not believe in inductive necessity : „There is no inductive necessity other than a logical necessity“ [Mach 1986, 393]. He says what Hume has said and Wittgenstein will write a few decades later.

What comes to our mind when reading Mach, is also the interesting idea that our knowledge, and especially the scientific one, is the result of our combining the economic ability with imagination. The latter provides us with possibilities beyond the world of facts ; the economic twist reduces the variety of thoughts in order to fit the facts in question. If we remember how Mach has characterised the aim of science as the adoption of our thoughts to facts and one with another („untereinander“), we should be prevented from the crude judgement that his philosophy is just positivistic. Theory building is our best means to describe facts ; but within theory-building we are using idealisations, and they permit other conclusions. In his preface to the German translation of Pierre Duhem's „La théorie physique, son objet et sa structure“ Mach stresses - as Duhem did - the strong entanglement and inseparability of experiment and

theory, that means of the theory and observation. And he points out what others could learn from Duhem : that to any theoretical value countless experimental values may be assigned to. Actually Mach himself was of the same opinion.

I think that Einstein's collaborator in Zurich, the young Friedrich Adler, who had provided the translation of Duhem's book into German, will have had no problem to unite his Machian outlook with the holistic point of view stressed in Duhem's work. And this again seems to be true also of Mach himself. How could it have been otherwise in regard to the central idea of holism ? Mach surely shared with Duhem the belief that a continuous development of the natural science - and both refer in these contexts to physics - will lead to true descriptions of natural phenomena. The basic conviction Duhem shared with Mach was the common grounding of our beliefs by common sense : „The truths this common sense reveals to us are so clear and so certain that we can neither ignore them nor call them into doubt. Moreover, every scientific certainty and clarity is a reflection of their clarity and a prolongation of their certainty.“ (*La théorie physique*, 153f.) On the basis of this conviction Duhem was very clear concerning the systematisation with the aim in mind to achieve a natural classification of the phenomena. A proof that an order „established among the experimental laws reflects an order transcending experience“ cannot be given. This we can take as another confirmation of the importance of common sense in the interpretation of the theory of physics [Russell 1991]. The decisive step in the argumentation for the holistic interpretation of theories to be seen or detected in Duhem's argumentation is his refusal of the *experimentum crucis* in case of the conflicting hypothesis. It was mainly this Duhemian topic which in later time occupied a lot of discussions, starting with Adolf Grünbaum's harsh critique already in 1960, a discussion which in its main steps is documented in Sandra Harding's excellent collection in the Synthese Library : „Can Theories Be Refuted ? Essays on the Duhem-Quine Thesis“ [Harding 1976]. Duhem had claimed that if a phenomenon predicted by a theory „is not produced, not only is the questioned proposition put into doubt, but also the scaffolding used by the physicist“ [Duhem 1954, 185]. That at least is one version of a *holistic characterisation* of theories. The argument against the applicability of an experiment as a crucial one in order to decide between the conflicting hypotheses tells us, „When the experience disagrees with the physicist's prediction, he learns that at least one of the hypotheses that constitutes the set is erroneous, but the experiment does not indicate which“ [*ibid.*, 187].

I quite early found it strange that especially in the discussion about the so-called Quine-Duhem thesis, which attributed a view to Quine that he apparently did not directly know from a study of Duhem, no one had the idea to try a closer look at the history. Arguments and propositions in the high days of analytic philosophy were taken in an almost clinical way, apart from the historical

context, such as anyone's ideas and theories were produced in an adamic language with no relation to any other source or text.

When I started to go somewhat deeper into the history of the Vienna Circle and especially into the vast corpus of Neurath's writings, I detected the link between the French conventionalists and Mach and the Machians in Vienna, which I had not heard mention before. I also became aware of the well-known fact that translations play an important role, which fact very often is neglected. At once I also found out in what regard the Austrians, actually the Viennese, were completely ajour and on terms with what was going on in French philosophy. When Duhem published his „Theory of Physics“ it immediately was to be translated into German. The same happened to Abel Rey's book „The Theory of Physics in Modern Philosophers“, which was translated by Rudolf Eisler, who otherwise is well known as editor and author of valuable philosophical dictionaries. The translation of Rey's book too was published very soon, just two years after it had appeared in France. And when Friedrich Adler had translated and published „Aim and Structure of Physics“ with the preface by Mach two years after the original first edition in French, Philipp Frank translated another of Duhem's booklet, „The Changes of Mechanic and of Mechanistic Explanations of Nature“, in 1912, the year he was starting his work in Prague as successor of Einstein.

These items show and make quite clear that there had been a strong interest in French philosophy of science at that period introducing some focuses of Mach's new positivism. As I have pointed out different times. It is important to take notice of the pre-history of the Vienna Circle and take into account the formation of an early group of young scientists (=philosophers) during the first decade of this century, a group which I have dubbed „the first Vienna Circle“. They all were Viennese, had studied mathematics and physics, or mathematics, history and economics. One name has been mentioned already, namely Philipp Frank, who, by the way, was the one asked by Ernst Mach to explain to him Minkowski and Einstein soon after Einstein's first publication on the relativity theory in 1905. Frank very soon adopted Einstein's theory, and Einstein himself trusted him early, a fact which explains that Einstein recommended Frank as his successor to the philosophical faculty of the University of Prague. And also later he regarded Frank's, Schlick's and Reichenbach's interpretations of his relativity theory as most adequate.

When Frank published his first article on the „Law of Causality“, he already presented himself as a close follower not only of Mach, but also of the French scientist-philosophers. Thus we find in the paper just mentioned the statement : „The law of causality, the fundament of all natural sciences, cannot be verified nor falsified by experience, but not because it would be an apriori truth necessarily true for all thinking, but only because it is a purely conventional stipulation. (Festsetzung)“

The two other members of this group were Hans Hahn and Otto Neurath. Unfortunately we do not know of the other members of this early circle who came together regularly on Thursday evenings, like the later famous Schlick Circle. It is however likely that also some women who had studied mathematics or other sciences, like Olga Hahn - who later married Neurath - were among them. Anyway this circle met more or less regularly, and we know from their writings that one of their targets was to unite their Machian outlook with the new French philosophy. It seems to me now that, in spite of their common interest and the general views they shared, there were enough differences among them, only that the differences were no principle ones. In the vast collection of Hahn's complete works there are no *philosophical* papers stemming from the early periods. From these later periods we, for instance, know that his understanding of the truth concept does not rely so much on the French conventionalists, but on the American pragmatists. Thus he compares the old metaphysical view of truth, which he refutes with the one he accepts. The old one tells us : „There is a reality, a world of true being, and a statement is true - according to this view - if it corresponds with what in this reality really is the case ; the statement of the law of gravitation for instance is true if in reality any two bodies are attracting each other, as the law states.“ (Mathematics and the Knowledge of Nature)

As opposed to this metaphysical picture, Hahn wants to keep to a minimum of presuppositions, and referring to John Dewey and William James he states, „Truth of a proposition consists in its confirmation (Bewährung).“ Surely, the truth predicate loses its timeless sense, but that is one of the fruits Hahn wanted to get. The applicability of the predicate remains secured, as long as we can predict successfully on the basis of it.

If Hahn had published these ideas as early as Frank had done, he too would have been included into Lenin's list of those reactionists who were condemned because of their denial of the objective lawfulness of nature.

The third one in this trio of adherents of conventionalistic or pragmatist views - or sins, as Lenin would have said, the third one was Neurath, who, historically-minded as he was from the beginning - seems to have been fascinated rather by the general approach of Duhem than by that of Poincaré. After all, he is - as far as I can see - the only one who frequently refers also to Abel Rey's outline of the new philosophy of science in France. Rey, a student of Boutroux and Poincaré, thus was granted something like the status of an official interpreter of the whole group labelled the French conventionalists.

When discovering for the first time how much Neurath had accepted or shared Duhem's ideas, I was pondering if the principle I called „the Neurath principle“ really deserved this name. But by considering Quine's wri-

tings and especially „Two Dogmas of Empiricism“ I thought it would be justified to honour Neurath in attaching his name to the principle stemming from Duhem. This principle to which I gave the name „Neurath-principle“ states that if a proposition is in conflict with the whole system, there are always two possible ways to achieve agreement : either change the proposition to be integrated in the system, or change the system. Propositions that are taken to be true and are incompatible with the system thus led to a change of the system, and with it also to their own revision in the context of the theory. This holistic interpretation clearly is also involved in the explanation of changing theories. Neurath as a historian and sociologist quite early understood very well that there always must be someone - some person or group of persons - to carry through a change, and thus it was clear to him that this can only be done by the members of the „republic of scientists“, a republic where all members have equal rights. Now you realise that where Th.S.Kuhn speaks of the scientific community, he is translating Neurath's term, keeping the same meaning.

One effect of Neurath's interpretation of theory change was also that the view Popper had defended in his „Logik der Forschung“ (1935), that if a universal proposition is falsified, it has to be crossed off, cannot and should not be accepted. Because, as we now know from Neurath and from Quine, every proposition can be sustained if we change the system, while it remains true what Neurath said, that there are no sacrosanct propositions at all.

In his reply to Moritz Schlick's *On the Foundations of Knowledge*, published in 1934 under the title *Radical Physicalism and the Real World*, Neurath stresses his belief that „science is ambiguous, and is so on each level...“ And Neurath underlines this in saying, „Poincaré, Duhem and others have adequately shown that even if we have agreed on the protocol statements, there is an unlimited number of equally applicable possible systems of hypotheses. We have extended this tenet of the uncertainty of systems of hypothesis to *all* statements including protocol statements that are alterable in principle“ [Neurath 1983, 105].

Despite the fact that Schlick does not agree with those who extend the space of conventions into an unlimited sphere, he too nevertheless is quite clear about the conventional character of language and also of the language of science. Therefore Schlick will agree with the defenders of conventionalism in general when he states, „The language in which we speak of physical relations must after all also have its own grammar and there is no doubt that it is determined by convention“⁴ But from the fact that the system of language is to be justified by the guiding conventions, or, as Wittgenstein has said and Schlick repeats, „by grammar it does not follow from this that the laws of nature will be just

4 M.Schlick, „Are Laws of Nature Conventions?“, in: *Philosophical Papers*, vol.II, p.438

conventions. As Schlick argues, the choice of a method of measurement of time or space will be free : „Theoretically it would be just as possible to consider the pulse beats of the Dalai Lama as marking measurement on them.“ And he notes that with that procedure „the laws of nature would take on an extremely complicated form. If we chose the rotation of earth as a measure of time, these laws appear in a very simple form, and indeed this is why we make that choice“ [*ibid.*].

Thus Schlick wanted to underline that it does make a vital difference if we ascribe convention to the system of language or to laws of nature. Only if there is some correspondence with the facts we ascribe the meaning of propositions to the stakes of a law. If for instance „according to the conventionalists, the word ‘energy’ is defined by the condition of constancy and the condition of observability is relinquished, the word no longer designates what physicists and engineers mean by ‘energy’“ [Schlick, *ibid.*, 439]. According to Schlick, it makes no difference if we use the axioms of Euclidean geometry or those of Riemann : the case of physics remains the same : „Once the rules are fixed, i.e., an agreement is achieved concerning the grammar of the scientific language, there is no longer any choice about how to formulate any facts of nature... A natural law can then be represented in only one quite definite form and not in any other“

It seems to me that Schlick could not stick to this statement when he should have seriously considered what he had said about verification, the only form available to interpret the sense of a natural law : „The possibility of verification“, Schlick says, „does not rest on any ‘experiential truth’, on a law of nature or any other true general proposition, but is determined solely by our definitions, by the rules which have been fixed for our language, or which we can fix arbitrarily at any moment“ [Schlick, vol II, 468]. This more moderate formulation leaves room for a moderate interpretation of conventionalism, a form which Herbert Feigl called the „gemilderte Form des Konventionalismus“. This form sees theories as constructions which to some degree are arbitrary but restricts the arbitrariness by other points of view as for instance simplicity of the theory, and its explaining power. And in this context Feigl lists „Poincaré, Mach, Duhem, Enriques, Schlick (sic !), Reichenbach, Carnap“ under the label of moderate conventionalists [Feigl 1929, 110]. Even when this statement stems from an earlier time, it makes apparent what could have been realised already then : there are different conceptions and concepts of conventionalism in use and it is not easy to bring them down to only one. Syntactical difference and empirical equivalence will not do as sufficient criteria, in spite of the fact that Carnap’s principle of tolerance which tells that „everyone is at liberty to build up his own logic, i.e., his own form of language, as he wishes“ [Carnap 1937, 51]. If we take it bona fide : this principle of tolerance does not open the

door to an „Anything goes“ but is more or less restricted to the choice of a logical theory. However, its formulation left much more room for „bad humour“ too. When we take Neurath’s principle or Quine’s thesis, then there does not remain one kind of propositions which is immune to change : not the propositions concerning the empirical basis, not any kind of empirical propositions, not the so-called laws of nature nor the laws of logic.

What then keeps the body of our theories or, to speak in Neurath’s terminology, „the unified science“ in balance ? What is it that provides a barrier against confusion and breakdown ? I think if we are apt to answer such a question at all, we shall have to say : it is the practice of our life as well as the practice of scientific work which restricts our arbitrariness. None of the programs of the empiricism of our century does or can ignore the pragmatic point of view limiting something that logical and epistemic justification did not provide : a unique and clear procedure to decide between non-conflicting and conflicting, but arguable alternatives of scientific theories.

§ 3.

I could only point out some of the items which show the important role of the conventionalists’ ideas and arguments in the work and the theories of Austrian and especially of logical empiricists. It may be true what Don Howard has said, that „it is primarily through Quine’s writings that Duhem’s ideas have retained what currency they have in contemporary debates in the philosophy of science“ [Howard 1990, 363-384]. And Howard is right in pointing out that it would be wrong to underestimate the influence of Duhem on scientists like Einstein. It would be worthwhile going into this on a larger scale.

Before closing this paper, I want to say a few words to one line of criticism of the Duhemian argument, as it was put forward and stressed by Adolf Grünbaum. His criticism of Duhem seems to rest entirely on a very restricted interpretation of auxiliary assumptions on the one hand, and the fixation on the falsifiability of singular hypotheses. Duhem had said that „to seek to separate each of the hypotheses of theoretical physics from the other assumptions on which it rests in order to subject it in isolation to observational tests is to pursue a chimera“⁵

In the reconstruction of the Grünbaum argument this could be read as „No constituent hypothesis H of a wider theory can *ever* be sufficiently isola-

5 P. Duhem, *Aim and Structure of Physical Theory*, Princeton 1954, p.200: „The physicist can never subject an isolated hypothesis to experimental test, but only a whole group of hypotheses“ (*ibid.*, 187)

ted from some set or other of auxiliary assumptions so as to be separately falsifiable observationally“⁶ It is with this assumption that for some years philosophers have tried to find a conclusive answer. And as far as I can see, even before this happy situation was to be seen at the horizon of the future, the lively discussion stopped and disappeared from the journals.⁷

But the result was not that the Duhemian paradigm would have lost its attraction. On the contrary : its *return* in different forms made it very clear that it became the dominant model to interpret the aim and structure not only of theories of physics, but of all kinds of empirical theories.

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