# Reichenbach's Concept of Logical Analysis of Science and his Lost Battle against Kant

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## 1. Reichenbach on Logical Analysis

Reichenbach was rather negligent in both the history of philosophy and the philosophical terminology he used. In particular, he introduced a very idiosyncratic use of the concept of "logical analysis" that has little to do with the way that concept was treated by analytic philosophers like Frege or Russell. In *The Rise of Scientific Philosophy*, for example, Reichenbach simply set it against "psychological analysis". While psychological analysis studies the "errors" of the "speculative philosophers", logical analysis makes a "rational reconstruction" of the scientific theories. To be more specific, it sets out the principles on which the results of sciences are *really* based, and not simply the way they were set out by their originators. "It endeavours to clarify the meaning of physical theories, independently of the interpretation by their authors, and is concerned with logical relationship alone." (Reichenbach 1949: 293) In other words, logical analysis investigates the "context of justification", not the "context of discovery", of this or that particular scientific theory.

Especially helpful for making logical analyses of science is Hilbert's axiomatic. In fact, Hilbert's axiomatic brings with itself the whole logic with the help of which Reichenbach made his "logical analysis" of science.

## 2. Metamorphoses of Kant's A priori

Reichenbach set out his program for logical analysis in *Theory of Relativity and A priori Knowledge* (1920). It accepted two fundamentally different principles of knowledge: principles of coordination and principles of connection. Principles of connection are empirical laws in the usual sense, involving terms and concepts that are already well defined. Principles of coordination, in contrast, are not empirical; rather, they must be first established in order to insure such adequate empirical definitions in the first place. In other words, principles of coordination are *constitutive of the object* of every particular scientific theory. In *this* sense (A) they are a priori. Of course, these a priori principles change with every new significant theory; they are not given once and for all. In *that* sense (B), the principles of coordination are empirical. They are the result of new observations and examinations.

For taking this position, Reichenbach was severely criticized by Moritz Schlick. The latter claimed that instead of a priori in a Kantian sense, we can interpret the principles of coordination as conventions in the sense of Poincaré. Schlick's criticism convinced the young Reichenbach: from 1922 onward, he adopted Poincaré's terminology of "convention". More especially, instead of principles of coordination he now spoke about definitions of coordination, underlining this way their conventional character. Unfortunately, this position totally neglected the first meaning of principles of coordination as being constitutive of the object of knowledge.

Assessing this turn in Reichenbach's philosophy, Michael Friedman has pointed out that Reichenbach "overhastily … acquiesces in the Schlick–Poincaré terminology". We fully agree with this judgment. We, however, cannot accept Friedman's claim that with its acceptance, "the most important element in [Reichenbach's] earlier conception of the relativized a priori is actually lost" (Friedman 1994: 26). In fact, Reichenbach never stopped believing that there are principles that connect the basic concepts of scientific theories with reality. This point is especially pronounced in respect of the so called principle of probability. Reichenbach already introduced it in his Dissertation (1916) and never stopped considering it necessary for any kind of knowledge (cf. Kamlah 1985: 162).

In this paper, however, we are going to track down another trace of Kantian apriorism in Reichenbach's "new philosophy". To be more specific, we argue that Reichenbach's program for a logical analysis of science, which was prominent in his works after 1920 until his death in 1951, was nothing but a transformation of the idea that science contains elements that are constitutive of their objects. In other words, despite the fact that in 1920 Reichenbach officially abandoned the idea that sciences contain a priori elements, he nevertheless continued to explore this in the form of a logical analysis of sciences. How this can be?

As well-known, Kant's position was that we can formulate all principles that make science possible once and for all through a logical deduction from pure reason: in fact, this was a task of solitary reflection. In contrast, Reichenbach believed that the definiteness of the coordination changes with every new scientific theory. Furthermore, the very idea that every significant scientific discovery brought with itself new principles of coordination posed a new task for philosophy. This was to explicate the new principles of coordination of all subsequent scientific discoveries. Among other things, this latest task led Reichenbach to set up the Berlin Group—a society for scientific philosophy with a clear interdisciplinary coloring. The Group (we shall return to it in § 4), with its most active members Kurt Grelling, Walter Dubislav and Alexander Herzberg, developed in a close relationship with the Vienna Circle.

## 3. Ambiguity in Reichenbach's Program

Hartmut Hecht was the first to draw our attention to the fact that Reichenbach's critic of Kant's a priori and the method of logical analysis of science are but two perspectives on the one problem of human knowledge (cf. Hecht 1994: 221). Moreover, despite the fact that they were different, it is impossible to conceive of them separately.

Indeed, on the one hand, Reichenbach criticized Kant's thesis that there is an ultimate table of the categories and principles of the scientific theoretical knowledge that is given once and for all; on the other hand, he claimed that sciences are only possible as long as they have coordination principles which are statements about the logical structure of sciences that change over time. It is exactly this way that the logical analysis of science and the criticism of the a priori made two sides of one point: the task now was not to *criticize* the *pure reason*

but to *logically analyze* the *sciences* in order to find out their specific principles of coordination.

Reichenbach insisted that this is not merely a program for popularization of science. Rather, its pursue is exactly as complicated as the studies of science itself, in particular physics, are. To be sure, the philosophers must work hard in order to clarify the results that scientists achieve. This is a necessary work because the scientists themselves are more concentrated on making discoveries: "Scientific research does not leave a man time enough to do the work of logical analysis." (Reichenbach 1951: 123)

## 4. Reichenbach's Berlin Group and Leonard Nelson as Its Grandfather

To sum up, the task set out by Reichenbach before philosophy was a logical analysis of science: not only of physics but of *all* science. Moreover, he was deeply convinced that his program for "new philosophy" was radically anti-Kantian. In this section we are going to show the inaccuracy of Reichenbach latter claim with the help of a historical argument. To be more specific, we shall refer to the fact that a program close to that of Reichenbach was introduced, much earlier, by the Göttingen philosopher Leonard Nelson (1882–1927) who considered himself a Kantian.

In his philosophy, Nelson closely followed Jacob Friedrich Fries (1773–1843). Fries was Hegel's contemporary and also adversary and rival. He criticized Kant for his "rationalistic prejudice" that we can deduce all a priori concepts from one single principle and in one system. Fries opposed to it the program for analysing the a priori forms of knowledge by "self-observation". To be more specific, he claimed that while the subject of investigation of this program was still the a priori, the way we reach it was a posteriori, or empirical. It was a task of deduction of our a priori knowledge from our immediate knowledge, which, however, also included the scientific knowledge.

Fries' next claim was that metaphysical knowledge, which consists of a priori principles, grows; in other words, it *changes*. In particular, this is true of our knowledge of axioms of mathematics. Leonard Nelson was eager to point out that the growth of metaphysics was especially well demonstrated in its sub-discipline of philosophy of mathematics by the emergence of the non-Euclidean geometry. Indeed, it was discovered after Fries' death and introduced new axioms into it. Moreover, similarly to Reichenbach later, Fries and Nelson claimed that the task of the philosophy of mathematics is to reduce the number of the axioms to a minimum, retaining only those in it which are necessary for the logical *Aufbau* of the theory (cf. Nelson 1928: 110).

The most interesting point is that Reichenbach's two closest friends in the Berlin Group, Kurt Grelling and Walter Dubislav, were faithful followers of Nelson. Indeed, Grelling worked directly under Nelson for more than fifteen years, and while Dubislav had no direct contacts with this philosopher, he worked on Nelson and Fries for years (cf. Dubislav 1926). Apparently, this fact explains the strong theoretical integrity of the Berlin Group.

The main task of the Berlin Group was: an interdisciplinary work on sciences with the aim of establishing their specific principles of coordination. In the light of our analysis of Reichenbach's philosophy we made in §§ 2 and 3, it is clear that this program was nothing but a realization of Reichenbach's program for "logical analysis" of science. In this connection it should be pointed out that Leonard Nelson set up the Fries-Society that, in fact, was the forerunner of the Berlin Group, already before the First World War (in 1913). The Fries society was an interdisciplinary forum for discussions of philosophers, scientists and mathematicians which had its own theoretical organ: *Abhandlungen der Fries'sche Schule* (published between 1903 and 1937).

## 5. David Hilbert as Reichenbach's Critic

Especially intriguing is the fact that the interdisciplinary program of the neo-Kantian Leonard Nelson also inspired the top mathematician of the time David Hilbert of Göttingen—this to such a degree that the latter believed that he is a Kantian (cf. Majer 1994: 254). In particular, Hilbert claimed that mathematics is based on certain non-logical objects that are subject to our intuition. These are formal structures that have no content; Hilbert called them "ideal elements", or "implicit definitions" of thought.

This fact is puzzling for at least two reasons. (i) As already seen in § 1, Hilbert's axiomatic method played a central role in Reichenbach's program for logical analysis of science. (ii) In 1922, Schlick and Reichenbach were convinced that Hilbert's axiomatic method delivered an ultimate proof that there is no need for Kantian a priori intuition of perceptions in mathematics. How can this puzzle be explained?

Apparently, Schlick and Reichenbach treated Hilbert rather one-sidedly. Indeed, Hilbert's philosophy of mathematics can be interpreted not only as aprioristic, but also as conventionalist. What were the reasons for this oversight?

We have already noted that Reichenbach was a careless terminologist. In particular, he made a very free interpretation of the term "logical analysis". But Reichenbach's use of an inadequate terminology was even more clearly illustrated by his claim that he was an "empiricist". It seems that he had three main reasons for insisting on this point:

(i) It opposed Kant's claim that we can formulate the principles of science once and for all and in our reasoning. Instead, Reichenbach's position was that these principles change with every significant shift of science and so are a result of experience.

(ii) Further impulses to stick to this one-sided terminology came from Reichenbach's crusade in defense of Einstein's theory of relativity against idealistic philosophers of a quite different provenance, such like Hugo Dingler and Oskar Becker. Apparently, Reichenbach believed himself to be an "empiricist" because his opponents rejected empiricism.

(iii) A third reason for insisting on empiricism was the fact that "the opposition against Neo-Kantianism and other kinds of apriorism was a common bond which united logical empiricists and gave them a feeling of being part of a unique philosophical movement." (Kamlah 1985: 158)

Hilbert, who once sat in sessions of Nelson's Fries-Society, closely followed the development of the Berlin Group. Moreover, his assistant Paul Bernays actively participated in the life of the prominent offspring of the Berlin Group—"The Society for Empiric Philosophy". After the analysis we made in this section, it is no surprise that Hilbert criticized the naming of the society "empirical". Rei-

chenbach promptly reacted to this criticism, renaming it into "Society for Scientific Philosophy" (cf. Joergensen 1951: 48). Unfortunately, Reichenbach did not realise that he must also rename his philosophy. Indeed, it was "empiricist" in a very weak sense.

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