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THE HERITAGE OF CONVENTIONALISM[1]

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In September 1904, i. e. around the *last* turn of a century, Henri POINCARÉ, the great French mathematician, gave a speech on the prospects of mathematical physics at the Congress for Art and Science in St. Louis.[2] He began with the questions:

"What is, at this time, the state of mathematical physics? Wherein consist the problems, which it has to deal with? ... Are we testifying a deep-going change? ..." His first "petit diagnostic" is: "There are hints for a serious crisis ..." (302)

He then discusses a set of endangered *principles*, among them that of the conservation of energy - seemingly questioned by the then newly detected radium, "ce grand révolutionaire des temps présents" (307, cf. 317) - and that of [special] relativity. Facing what he calls a "débâcle générale des principes" (318) he recommends not to give up the principles before a "loyal effort to rescue them" (319).

Thus I take as the prominent message of Poincaré's speech a certain *conservatism:* although he is open for new and even revolutionary developments in physics he encourages his colleagues to stick to old well-confirmed principles as long as possible. (One may trace this kind of conservatism back to NEWTON and his fourth *regula philosophandi.*)[3]

The observed conservatism is only *one* of two sides of *conventionalism,* the position which Poincaré is famous for: the

[1] Parts of this paper were given at UPR in March 2000. I have previously dealt with conventionalism in [74], [85], and [99].

[2] Poincaré [04]

[3] Newton inserted these rules in the 2nd ed. (1713) of his *Principia* at the beginning of Book III; cf. KOYRÉ [60]. For FEYERABEND's critique of the fourth rule cf. his [72].

principles have a special epistemological status: they are 'conventions' and thereby kind of *a priori*. The *other* side of conventionalism refers to the existence of *alternatives* to a given body of theoretical knowledge, most notably in geometry: an Euclidean description of the world may be replaced by a non-Euclidean, and *vice versa*. In other words: every geometric description comprises a certain 'conventional' part, which goes beyond the genuinely descriptive part. To separate the two parts, however, may be no easy enterprise.

The two sides - or rather directions? - of conventionalism are neatly expressed by NERLICH:

> Conventionalism ... wavers unhelpfully between a programme of reduction in which we *rid ourselves* of conventional structure ... and a programme of *retention* in which the conventional structure plays the indispensable role of making inchoate experience intelligible.[4]

Throughout Poincaré's writings we see both sides or directions of conventionalism at work. Certain principles of mathematics and theoretical physics are said to be conventional *because* they may be replaced by alternatives. And *because* there is a freedom of choice we may stick to well-confirmed and entrenched principles and alter other parts of the theory if necessary.[5]

In the following I'll first characterize the very heart of conventionalism: Poincaré's philosophy of geometry (sec. I). Then I'll discuss what logical empiricism has inherited from conventionalism (sec. II). Finally I'll comment on Quine's position toward conventionalism (mainly in its Duhemian version)[6] and related issues (sec. III).

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[4] Nerlich [76], 2nd ed., sec. 6.6, p. 155, cf. sec. 7.7, p. 178.

[5] Hugo DINGLER (1881-1954) endorsed only the conservative side of conventionalism (cf. my [74], sec. S.3). He consequently (and later also on political grounds) rejected EINSTEIN's theory of relativity; cf. e.g. his exchange with REICHENBACH in *Physikal. Zeitschr.* 1920f.

[6] I basically skip this type of conventionalism here, but cf. my other publications on the subject (n. 1).

I.

As is well known, Poincaré formulated conventionalist ideas first with respect to *geometry*. He did so as early as 1887 - though without using the term 'convention' - and most explicitly in an article published in 1891 which substantially became ch. III of his 1902 collection *La Science et l' Hypothèse*. The most essential passage in ch. III reads:

> *The geometrical axioms are* [...] *neither synthetic judgements* a priori *nor experimental facts.* - They are *conventions*; among all possible conventions our choice is *led* by experimental facts; but it remains *free* [...]. - In other words: *the axioms of geometry* (I do not speak of those of arithmetic) *are only disguised definitions.*[7]

Poincaré focuses especially on the *axiom of parallels*. Note that Poincaré (at this time) does not use HILBERT's phrase 'implicit definition', but calls the axioms 'definitions in disguise' ('définitions déguisées'). Hilbert's use of the notion of implicit definition is *later* than Poincaré's 'definition in disguise'. Although later on Poincaré welcomed Hilbert's idea as vindicating his own[8] we must insist that, in the quoted passage, Poincaré's view of the epistemological status of geometric axioms is quite different from Hilbert's. This could hardly be otherwise because Poincaré - at least at this time - did *not* subscribe to Hilbert's sharp separation of mathematical and physical geometry. Since most of later philosophy of geometry takes this separation for granted, it is not easy for us to understand what exactly Poincaré meant when ascribing a conventional or definitional status to geometrical axioms.

The exact meaning of Poincaré's geometric conventionalism may be elucidated by tracing its roots back into 19th century (philosophy of) geometry. Certainly the development of non-Euclidean geometry by BOLYAI and LOBATCHEVSKY early in 19th century is the decisive first step.[9] But also very important is the proof by BELTRAMI (1868) and others of the *consistency* of non-Euclidean geometry.

This proof put the Bolyai/Lobatchevsky geometry, which so far had been only an exotic variant, mathematically on a par with Euclidean geometry: it is at least "as consistent as" the latter in the sense that *if*

[7] Poincaré [02], 75f; engl. 50

[8] cf. O'GORMAN [96]

[9] cf. TORRETTI [78], sec. 2.1

Euclidean geometry is consistent (which, at that time, no one doubted), *then* non-Euclidean geometry is as well.

Mathematical comparability of Euclidean and non-Euclidean geometry was considerably strengthened by RIEMANN's far more general notion of geometry - put forward in his Inaugural Lecture of 1854 (published in 1867) - according to which Euclidean geometry and the by then "classical" non-Euclidean geometries of Bolyai and Lobatchevsky are just two special cases of Riemannian geometry, namely the cases where the so-called curvature of space vanishes or has a constant (negative) value.

Finally I should mention the *group theoretical* approach to geometry by S. LIE and others whereby each geometry is characterized by its group (i.e. a certain algebraic structure); esp. this approach allows a stringent classification of geometries.[10] While, according to Poincaré, the choice of a specific group is conventional, the general concept of a group pre-exists in our understanding.[11]

It is evident from Poincaré's texts that he refers to *all* these developments. It is less evident which approach he might have preferred as a basis or framework for his conventionalism. (Of course, he also may have shifted his focus during his lifetime; here I do not go into these historical details.) It does not come with surprise, therefore, that various authors have put forward quite different interpretations of Poincaré's geometric conventionalism.

According to A. GRÜNBAUM's reading of Poincaré it is mainly the *metrical amorphousness* of space that Riemann suggested which serves as a basis for Poincaré's geometric conventionalism: whatever metric we ascribe to space *we* ascribe to space; *therefore* Euclidicity or non-Euclidicity is merely a matter of *convention.*[12]

I find Grünbaum's interpretation too one-sided, however. There are a lot of passages in Poincaré, which are pretty perfectly compatible with the view that, in fact, there is something *to find out* about the metrical

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[10] Cf. GIEDYMIN [77], 112 ff, and TORRETTI [78], subsec.s 4.4.4f; also my [74], p. 25, n.39 (a lengthy note on what Poincaré regards as the *object* of geometry).

[11] Poincaré [02], 93; engl. 70. - More recently FRIEDMAN [95] emphasizes the group-theoretical aspect of Poincaré's philosophy of geometry.

[12] Of Grünbaum's many writings on Poincaré cf. mainly his [63]. For his interpretation as well as his own philosophy of geometry cf. my [74], sec. P.5, esp. 37 ff. - Cf. also PUTNAM [74] on Grünbaum (and Quine) and my remarks on Putnam at the end of this paper.

structure of space.[13] Nevertheless the same structural facts may, according to Poincaré, be expressed either in an Euclidean or a non-Euclidean way.

It is interesting to note that Poincaré dismisses geometries with *varying* curvature as physically irrelevant because they do not exhibit free movability.[14] Already HELMHOLTZ had pointed out, even before the publication of Riemann's lecture,[15] that the *factual* free movability of solid bodies implies a constancy of curvature.[16]

What is, in my eyes, most important for Poincaré's conventionalist attitude toward geometry, is the fact that, to a certain extent, the different metrical descriptions of space are intertranslatable in the manner Beltrami and others, including Poincaré himself, have shown. And Poincaré connects this mathematical fact with semantic considerations:[17] The act of choosing one specific geometry - so Poincaré - amounts to choosing a specific *definition* of basic metrical terms. This could be done *post facto*: If, e.g., astronomical measurements would suggest a non-Euclidean structure of space - Poincaré imagined this "impossible" case well before Einstein's theories of relativity and respective eclipse experiments - , then we could (and would) *re-define* the term 'straight line' so as no longer referring to paths of light beams but to other lines. Given certain empirical findings there would always be a suitable way of re-definition which would result in the familiar Euclidean structure of space:

> ... what we call a straight line in astronomy is simply the path of a ray of light. If, therefore, we were to discover negative parallaxes, or to prove that all parallaxes are higher than a certain limit, we should have a choice between

[13] I am not the only one (s. previous note) criticizing Grünbaum's account: cf. Torretti [78], subsec. 2.2.5, n. 21 (to p. 84), and Nerlich [76], 2nd ed. 1994.

[14] Poincaré [02], ch. III (of 1891), p. 73; engl. 48.

[15] Helmholtz [66], also in his better known [68]: 'Über die Tatsachen, die der Geometrie zum Grunde liegen', the very title of which evidently refers to Riemann's lecture. For UEBERWEG anticipating Helmholtz cf. Torretti [78], subsec. 4.1.2. - Helmholtz maintained a kind of transcendental apriorism with respect to *topology*; cf. my [74], sec. P.3, p. 20.

[16] For a more thorough-going discussion of Helmholtz' philosophy of geometry cf. Torretti [78], sec. 3.1; in n.8 to p. 157 Torretti observes that - according to Riemann - only free movability of "perfectly flexible and inextensible strings" is required. - FREUDENTHAL [60], p. 10, too, indicated that one only needs quasi one-dimensional bodies.

[17] cf. also Poincaré [05], sec. III.1, and implicitly in [02], sec.V.4, too.

> two conclusions: we could give up Euclidean geometry, or modify the laws of optics, and suppose that light is not rigorously propagated in a straight line.

And without considering potential difficulties with the second way, he continues:

> It is needless to add that every one would look upon this solution as the more advantageous. Euclidean geometry, therefore, has nothing to fear from fresh experiments.[18]

With respect to HILBERT's 'implicit definitions' and (cf. below) REICHENBACH's 'co-ordinating definitions' one may want to call Poincaré's 'definitions in disguise' *implicit co-ordinating definitions*.[19]

Poincaré puts so much stress on such considerations of re-definability that I tend to take this as the heart of his conventionalism. In short, in my view Poincaré proposes a *definitional* or *semantical* conventionalism.[20] This is also quite evident in Poincaré's discussion of the metric of time and of the principles of mechanics. E.g. the basic "law" of Newtonian mechanics - $f = m.a$ - is read, by Poincaré, as a definition of force f.[21]

II.

Geometrical conventionalism gained new interest when, in 1916, EINSTEIN proposed his theory of *general relativity*. (*Special relativity*, as published by Einstein already in 1905, had raised a new conventionalist issue: that of simultaneity.)[22] In his general theory Einstein applied Riemann's abstractly developed conception of a curved space to the cosmos. The degree of curvature, according to this theory, depends on the distribution of matter in space. Where the density of matter is sufficiently high there should take place a measurable deviation from

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[18] Poincaré [02], ch. V, p. 95f, engl. ed. p. 73; cf. Poincaré [08], 2nd book, ch. V, sec. XII, and the "science fiction" worlds in [02], ch. IV.

[19] I have done so in my [74], sec. P.5, p. 41.

[20] Cf. also QUINE's reading of Poincaré, s. end of this paper.

[21] [02], ch. VI (essentially from a paper given at the Paris 1900 International Congress for Philosophy), esp. 117 ff (engl. ed. 97 ff)

[22] cf. e.g. Reichenbach [24], § 7, and [28], § 19. For attempts to reduce this special kind of conventionality by slow clock transportation cf. ELLIS/BOWMAN [67] and GRÜNBAUM ET AL. [69], for more recent discussions MALAMENT [77], NORTON [92], and TORRETTI [99], subsec. 5.3.2.

Euclidean geometry. The sun, e.g., is heavy enough to let light beams passing the sun bend. Such a deflection of light was, in fact, observed during the sun's eclipse in 1919. In the eyes of various philosophers this challenged the privileged role of Euclidean geometry; among those philosophers were SCHLICK, CARNAP, and REICHENBACH, who later became well-known logical empiricists. Around 1920, however, they still adhered to a kind of neo-Kantian outlook on science - as did, of course, the neo-Kantian Ernst CASSIRER, e.g. in his *Zur EINSTEINschen Relativitätstheorie, erkenntnistheoretische Betrachtungen* (1920). Esp. telling are Reichenbach's first book, *Relativitätstheorie und Erkenntnis a priori* (1920), and Carnap's doctoral dissertation, *Der Raum*, one year later.

Reichenbach, in his 1920 book, ch. V, observes that '*a priori*' in Kant has two different meanings: on the one hand it would mean 'apodictically valid' ("apodiktisch gültig") or 'valid for all times' ("für alle Zeiten gültig"), on the other hand 'constituting the concept of an object' ("den Gegenstandsbegriff konstituierend"). Reichenbach then argues that '*a priori*' in the first sense is no more tenable, while it is still tenable, although historically changeable, in the second sense. More specifically, the concept of an object is constituted by a set of *Zuordnungsprinzipien*. (In the previous chapter Reichenbach had followed Schlick's *Erkenntnislehre* (1918) in that knowledge, in empirical science, comes about by co-ordination ("Zuordnung") of symbols (esp. equations) and reality, and that truth may be obtained only if some principles reduce the arbitrariness of co-ordination.) And against empiricism Reichenbach remarks: These principles, although, in the long run, they might be replaced by others, are not on a par with usual physical laws.[23]

When, in the 20ies, Schlick and others formed the Vienna Circle and a corresponding circle, with Reichenbach and others, was built in Berlin, the philosophical outlook on geometry of these groups became more and more *empiristic*. The mature outcome of this process was Reichenbach's *Philosophie der Raum-Zeit-Lehre* (1928).[24] In this work Reichenbach proposed what he called the *relativity of geometry*. Cast in terms of the Riemannian approach, the relativity of geometry amounts to the following. The metrical structure of space is given, according to Riemann, by the so-called metrical tensor g_{ik} . In principle, the six components of this tensor can be determined empirically, e.g. by

[23] Reichenbach [20], ch. VIII, p. 89

[24] Only in the fifties translated into English as *Philosophy of Space and Time*

means of measuring rods. If, thereby, these six components get certain definite values, the metrical structure of space is empirically determined as Euclidean; if not, as non-Euclidean. But the measuring rods could have been deformed by certain forces, say f_{ik} ; then the resulting geometry is not g_{ik} itself, but $g_{ik} + f_{ik} =: g'_{ik}$. In other words: if the empirically determined g_{ik} establish a non-Euclidean geometry, you could postulate such forces f_{ik} that the resulting geometry g'_{ik} is Euclidean, and *vice versa*. In this sense it is a matter of *convention* - although Reichenbach, in this argument, does not use this word - whether you ascribe to space an Euclidean or non-Euclidean structure.

Of course, matters are not quite as simple as that. It should be empirically detectable whether such distorting forces obtain or not. If, for instance, the measuring rods are distorted by thermal influence, a good physicist wouldn't take the measured lengths for granted, but would modify them in accordance with the well known laws of thermal expansion. However, if thermal expansion would be the same for all kinds of matter you could not detect such an influence of temperature. A force, which influences all kinds of matter in the same way, is principally undetectable. Reichenbach calls such forces *universal forces*. Hence the conventionalist strategy for blaming a force f_{ik} for resulting in a non-desired geometry is possible only with universal forces. But postulating such non-detectable forces is rightly criticized as *bad physics*. Reichenbach therefore suggests not to use such a spurious device. Given this methodological rule, geometry is *empirical*. Nevertheless it is important to note that it is logically possible to introduce universal forces and thereby arrive at the geometry you prefer. However, you may have to pay a high price: the resulting description of the world may turn out to be considerably more complex.

I bring Reichenbach and Poincaré closer together than Reichenbach might have liked.[25] No doubt, Reichenbach is an empiricist, and Poincaré a conventionalist. But logical empiricism is a sophisticated empiricism. The emphasis on language made logical empiricists especially sensible to semantical questions. Before Reichenbach proposes his thesis of the relativity of geometry he emphasizes the need of defining the basic terms of physical science with respect to empirical findings, i.e., in Reichenbach's words, *coordinating definitions*. E.g. the

[25] Cf. FRIEDMAN [95] for a differing view.

term 'length' is supplied with empirical meaning by reference to some practical method of determining the length of something. And this includes that you decide whether you regard the measuring rod as distorted by universal forces or not. In this sense the resulting geometry is a matter of *definition*, just as in Poincaré - an *implicit coordinating definition*, as I called it above. Definitional conventionalism and logical empiricism are not incompatible, but complementary.

However, one should be careful when comparing conventionalism with logical empiricism. They are not on a par. Poincaré put forward his ideas well before the linguistic turn of 20th century philosophy. For Poincaré, if a principle is a convention or disguised definition, this does *not* mean that it is *analytic*; it is something "between" a synthetic *a priori* statement in the Kantian sense and an empirical statement. For logical empiricists, on the other side, a scientific statement should be either empirical or analytic; and if a statement is true by definition, it is bound to be analytic. Hence, in logical empiricism, the problem of conventionality surfaces in a transformed *gestalt;* it becomes the problem of exhibiting the definitional or, more general, analytical part of scientific theories.

One of the contexts where the transformed conventionality problem arose in logical empiricism is the following: In the 30ies logical empiricism began to accept that not all scientific statements may be translated into observational statements. This is already the case where dispositional terms like 'soluble' are involved. Such a term D cannot be defined by observational terms O_1 and O_2 in form of a biconditional

$$D \text{ iff (if } O_1 \text{ then } O_2\text{)},$$

but only in the form of conditionals, so-called 'reduction sentences', e.g. a pair

$$\text{If } O_1 \text{ then (if } D \text{ then } O_2\text{)} \quad \text{(necessary condition for } D\text{)}$$
$$\text{If } O_3 \text{ then (if } O_4 \text{ then } D\text{)} \quad \text{(sufficient condition for } D\text{)}$$

But such a pair is - at most - only a "partial definition" of D, and - worse than that - it isn't analytic either: it implies

$$\text{If } O_1\&O_3\&O_4 \text{ then } O_2, \textit{ or: } \text{not } (O_1\&O_3\&O_4 \;\&\; \text{not} O_2)$$

In general: If a theory contains non-observational terms - and all interesting theories do - there is no straightforward way to isolate the analytic statements from the empirical ones. Typical theoretical statements will always contain both components.

The late CARNAP, however, nevertheless proposed an ingenious method to isolate the analytic component of an aximatized theory: if '*T*' is the conjunction of the axioms (and corresponding postulates, i.e. those basic assumptions which connect theoretical and observational terms) of such a theory, its so-called Ramsey sentence '^{R}T' expresses its empirical content. Then the problem at hand takes the form: May '*T*' be decomposed into a conjunction of '^{R}T' and an analytic sentence '*A*', or: is there an '*A*' such that '*T*' is equivalent to '^{R}T&*A*'? Carnap's answer: Indeed 'if ^{R}T then *T*' may serve as such a sentence '*A*':

'*T*' is equivalent to '^{R}T&(if ^{R}T then *T*)',

and since the empirical content of '(if ^{R}T then *T*)' is '^{R} (if ^{R}T then *T*)' and this is equivalent to '(if ^{R}T then ^{R}T)', a tautology, '(if ^{R}T then *T*)' has no empirical content at all.

However, if I would decompose any "real life" theory '*T*' in this manner, the result would probably look rather artificial! I would hardly say that I thereby have gotten valuable "insight" into the epistemological structure of the theory.

III.

This is about the end of this story. QUINE, as you know, has vividly opposed the very idea of analyticity itself since the early fifties. Much later, namely in a 1975 paper, 'On Empirically Equivalent Systems', he discusses the thesis that "natural science is empirically under-determined":

> If all observable events can be accounted for in one comprehensive scientific theory - one system of the world, to echo Duhem's echo of Newton - then we may expect that they can all be accounted for equally in another, conflicting systems of the world. [...] Surely there are alternative hypothetical substructures that would surface in the same observable ways"[26]

[26] QUINE [75], p. 313

This sounds rather conventionalist, although conventionalism isn't even mentioned in the paper. However, Quine refers to both, DUHEM and POINCARÉ, firstly - by way of "digression" (315) - to Duhem's holism (313-315) - I skip that here - , secondly to alternative metrizability à la Poincaré:

> Here we have one formulation of cosmology that represents space as infinite, and another formulation that represents space as finite but depicts all objects as shrinking in proportion as they move away from center. The two formulations [...] are empirically equivalent. But [...] the example is disappointing as an example of under-determination, because [...] we can bring the two formulations into coincidence by reconstruing the predicates. [...] The two formulations are formulations [...] of a single theory.[27]

The "*reconstrual of predicates*" which Quine refers to here may best be illustrated by a simpler example, and is done so by Quine (319 ff): The switch of two theoretical (i.e. non-observational) terms like `electron´ and `molecule´ in a theory(-formulation). By such a switch all observational consequences remain the same. Poincaré's example, according to Quine, is principally of the same sort, though more sophisticated. In the simpler case we would readily admit that switching the two terms would not result in a new theory; rather, again Quine, the old and the new set of sentences are only two different "*formulations*" of the same theory, just as we would say that - say - an English translation of Newton's *Principia* is still the same theory. Technically speaking, Quine proposes that *theories* are classes of theory formulations, which pairwise are logically equivalent or can be turned into logically equivalent formulations by way of *predicate reconstrual*. This is a neat way, I would say, to settle the question whether the choice between alternatives, as regarded by conventionalism, really is a choice between *different* accounts of a subject matter or merely amounts to something like a choice of *language*.

Most people, when considering questions of conventionality, feel that such questions often are on the verge toward *triviality*. After all, Poincaré himself compares the choice between geometries to that between the system of feet and inches and the metric system, or between cartesian and polar coordinates. Nevertheless we tend to find the choice between geometries more interesting and profound. But principally all these choices should be regarded as on a par, as questions

[27] *loc. cit..*, p. 322

of the choice of words or, in Quine's terms, of *reconstrual of predicates*. - Quine admits, however, that it may be rather difficult or even impossible, to find out whether two given formulations can be transformed into each other by reconstrual of predicates, i.e. whether they are formulations of the same theory. Maybe Schrödinger's and Heisenberg's different "formulations" of quantum mechanics is a nice case in question.

However, I am not yet convinced that reconstrual of predicates, or the like, is the whole story of conventionalism. Poincaré, despite his sometimes sloppy remarks on the choice of co-ordinates and the like, would probably oppose to such a view. He simply was too little language-orientated to be read in terms of analytical philosophy. Nevertheless I find it useful to reconsider conventionalism in these terms. It may, e.g., be profitably asked whether conventionalism wasn't after the same deeper question as Quine, namely after the question whether our theories in natural science are empirically underdetermined - although classical conventionalists like Poincaré and Duhem couldn't have expressed their ideas that way. And this question of under-determination is far from trivial - and left open by Quine.[28]

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[28] Sometimes matters of conventionality were raised to a kind of meta-level: H. PUTNAM [74] detects in Quine's related arguments for the impossibility of 'radical translations' a *conventionalist ploy*. In this Quine would meet with Grünbaum's account of geometric conventionalism à la Poincaré and Reichenbach. At the same time Putnam defends Quine's critique of analyticity and Reichenbach's (in his eyes) empiricist philosophy of space and time. Cf. my critical remarks on Putnam in [85], pt. II.

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