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Fact, Hypothesis and Convention in Poincaré and Duhem

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'Transformations of 'Conventionalism' in the Vienna Circle"

Abstract : The constitutive influence of Poincaré, Duhem and Rey on the philosophy of the Vienna Circle, long obscured, has become more widely recognized. Two aspects of the Viennese reception of these 'French Conventionalists' are explored here for the light they may throw on the Circle's own, still insufficiently understood conventionalism. First, what was the Viennese perception of what has recently been called (Poincaré's) 'structural realism' ? What if any part of that doctrine became assimilated into their theories ? Second, in the absence of a realist interpretation of the conventionnalists structuralism, how were the principles guiding theory construction and validation to be legitimated ? It will be suggested that the left Vienna Circle developed a decidedly constructivist version of conventionnalism in response.

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The aim of this paper is a limited one, forming but part of a larger project : the investigation of the transformations of conventionalism in the Vienna Circle.* The point of that project is to determine whether there is a version of the Circle's characteristic rejection of Kant's synthetic a priori that can survive Quine's celebrated criticisms of the dogmas of (logical) empiricism. If such "life after death" is possible after all, it will be only because the members of the left wing of the Circle attempted to effect a much more radical reorientation of philosophy than their critics ever credited them with — indeed an "abandonment" of traditional epistemology no less radical than Quine's own. Poincaré and Duhem played a pivotal role in that development : they laid out the force-field of conventionalism from which, as one of its inspirations, Vienna Circle thought developed. Since recent work has shown that the Circle's reception of French conventionalism was not only profound and varied, but also beset by a remarkable misunderstanding, it is imperative that the originals be reconsidered. Once we do so we find that the French conventionalists do not share an easily identifiable doctrine beyond the basic tenet that no abstract science can proceed without conventions of one sort or another.

I will concentrate on Henri Poincaré's and Pierre Duhem's main writings – those with which members of the Vienna Circle can be expected to be familiar – and neglect Abel Rey and Edouard Le Roy. After distinguishing the different senses in which Poincaré and Duhem speak of "convention" I will develop three theses. (These are distinct from a basic point, substantiated only in passing, that conventionalism does not mean idealism, but can accord with a general empiricist orientation). First, that engaging with the question of realism they reached conflicting answers ; second, that they differed on the viability of the distinction between theoretical fact and convention ; and third, that they provided a blueprint for the conception of scientific objectivity further developed in the Circle. Modest as they are, these theses are not without consequence. They indicate the problematic issues and answers that are crucial to the development of Vienna Circle conventionalism.

1. Conventions in Poincaré

For Poincaré, like for Mach, science is a system of classifying data and their regularities so as to facilitate predictions. In *Science and Hypothesis* Poincaré specified three types of conventions, over and above those of naming things and specifying units of measurement : the axioms of geometry ; the "principles" of mechanics, for instance, Newton's three laws ; and the methodological

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maxim of simplicity. These conventions shared this feature : "our choice among all possible conventions is guided by experimental facts ; but it remains *free* and is limited only by the necessity of avoiding all contradiction" [Poincaré 1902/1946, 65] ; (all italics in the original unless otherwise noted). In other words, our choice of the respective conventions is not determined by the subject matter under inquiry. Nevertheless, the three types were conventional for different reasons and their adoption follows different trains of reasoning.

For Poincaré, geometry was a special case among the mathematical sciences the foremost of which, arithmetic, was viewed as synthetic a priori. Geometry was treated differently from arithmetic — namely, conventionally — because in its case the choice between three alternative axiomatisations was left undetermined, as Friedman explains [1995]. (By contrast, the neopositivists, following Schlick, ascribed to Poincaré a general argument from underdetermination and discounted not only his reliance on the synthetic a priori, but also his presuppositions concerning the hierarchy of the sciences that were made untenable by relativity theory). Poincaré's argument for the conventionality of geometry depends on his use of the Helmholtz-Lie theorem of group theory according to which — given certain assumptions which need not interest us here — only three possibilities obtain : that space is Euclidean, has a constant negative or a constant positive curvature — only geometries of constant curvature are possible. The choice between these three possibilities is formally underdetermined. "The axioms of geometry are neither synthetic a priori judgements nor experimental facts. They are conventions. [...] *the axioms of geometry* (I do not speak of those of arithmetic) *are merely disguised definitions.* [...] One geometry cannot be more true than another ; it can only be *more convenient.*" [Poincaré 1902/1946, 65] The geometrical conventions allowed for the representation of facts which could also be rendered differently. Those differences, however, remained arbitrary from the point of view of abstract mathematics — and did not matter except to forbid the unique determinacy of the synthetic a priori. Choice between them was required for the representation of physical geometry, however, and this choice was also empirically underdetermined. How then was one of them adopted ? "[B]y natural selection our mind has *adapted* itself to the conditions of the external world, [...] has adopted the geometry *most advantageous* to the species : or in other words the *most convenient*" [*Ibid.*, 91 (added in 7th French ed.)]. Poincaré came to the view that the adoption of Euclidean axioms was to be explained by reference to our biological inheritance. The measure of convenience here would seem to be simplicity (I return to this below).

Of the conventions of the second type, the "principles" of mechanics, Poincaré gave the following example : "The principles of dynamics at first appeared to us as experimental truths ; but we have been obliged to use them as definitions. It is *by definition* that force is equal to the product of mass by acceleration ; here, then, is a principle which is henceforth beyond the reach of any further experiment. It is in the same way by definition that action is equal to reaction" [*Ibid.*, 101]. Like the axioms of geometry, then, the principles function as the basic definitions of a science. Possible discrepancies with empirical results were to be attributed to disturbing factors. Yet unlike the axioms of geometry, the principles have an empirical origin. Only the elevation of their epistemological status was conventional.

> This certainty we ourselves have bestowed upon [a principle] voluntarily, so to speak, by looking upon it as a convention. Are the law of acceleration, the rule of the composition of forces then only arbitrary conventions ? Conventions yes ; arbitrary, no. They would be if we lost sight of the experiments which led the creators of science to adopt them, and which, imperfect as they may be, suffice to justify them. [*Ibid.*, 106]

The definitional conventions represented factual assertions that had become elevated by collective fiat to framework-constitutive principles immune from testing. The gain derived from this type of convention was stability. Convenience here meant usefulness as near-enough approximation, as fecundity for prediction and for extensions of applications and the unification of theories [Psillos 1996].

The convention of the third type, finally, the methodological maxim of simplicity, is conventional because it finds no equivalent in non-metaphysical assumptions about reality that would ground them, as it were, by giving the maxims something they can correspond to. As Poincaré notes, normally, every law is held to be simple until the contrary is proved. Justifying this custom, however, is no easy matter. "Sometimes simplicity hides under complex appearances ; sometimes it is the simplicity which is apparent, and which disguises extremely complicated realities" [Poincaré 1902/1946, 130]. Still, the case remains that "[w]e must stop somewhere, and [so] that science may be possible we must stop when we have found simplicity" [*ibid.*, 131]. Poincaré concluded :

> If the simplicity were real and essential, it would resist the increasing precision of our means of measure. If then we believe nature to be essentially simple, we must, from a simplicity that is approximate, infer a simplicity that is rigorous. This is what was done formerly ; and this is what we have no longer a right to do. The simplicity of Kepler's laws, for example, is only apparent. This does not prevent their being applicable, very nearly, to all systems analogous to the solar system ; but it does prevent their being rigorously exact. [*Ibid.*, 133]

Simplicity is a constitutive convention of science : without it, science would hardly be possible, but we cannot say that the world corresponds to these assumptions.

While this reference to the practical exigencies of inquiry seems to suggest a pragmatic reason for the adoption of methodological maxims, Poincaré also spoke pointedly of an "instinct of simplicity" [*ibid*, 139]. This formulation suggests an evolutionary basis for our adoption of these maxims. This suggestion finds further support in a passage from *Science and Method* which discusses Mach's economy principle. There Poincaré remarked that it was "at the same time a source of beauty and a practical advantage" :

> Whence comes this concordance ? [...] is there here a play of evolution and natural selection ? Have those peoples whose ideal most conformed to their highest interest exterminated the others and taken their place ? All pursued their ideals without reference to consequences, but while this led some to destruction, to others it gave empire. One is tempted to believe it. If the Greeks triumphed over the barbarians and if Europe, heir of Greek thought, dominates the world, it is because the savages love loud colours and the clamorous tones of the drum which occupied only the senses, while the Greeks loved intellectual beauty which hides beneath sensuous beauty, and that this intellectual beauty it is which makes intelligence sure and strong. [Poincaré 1909/1946, p. 367-8]

Here, clearly, the Machian sense of economy was understood as an inherited intellectual proclivity — one moreover which was differentially distributed between the human races. Poincaré's pragmatic reasons for simplicity come close to represent rationalisations *post factum.*

We may wonder whether unity, like simplicity, is a convention of the third type. "Every generalisation", Poincaré noted, "implies in some measure the belief in the unity and simplicity of nature". While the former belief, like the latter, in effect functions as a constitutive methodological maxim in theorising, Poincaré denied that it is merely conventional in nature and origin. "If the different parts of the universe were not like the members of one body, they would not act on one another, they would know nothing of one another ; and we in particular would know only one of these parts. We do not ask then, if nature is one, but how it is one." [1902/1946, 130] The unity of nature, Poincaré held, grounds the goal of a unified theory of physical reality. This goal, he believed, physical science was approaching [*ibid.*, 154].

I should note that there is also a type of hypotheses that could be mistaken for conventions. These are the "indifferent" or "neutral" hypotheses, which are distinct from genuine inductive generalisations and postulations of symmetries and idealisations enabling mathematical physics. Poincaré's early example is the hypotheses that matter is composed of atoms or is continuous (he came to change his mind on the status of this particular hypotheses later). A scientist "might have made the opposite assumption without changing his results. He would only have had more trouble to obtain them ; that is all." [*Ibid.*, 135] These hypotheses might be thought conventional because what they seemed to assert lay in fact beyond all possibility of testing. "These neutral hypotheses are never dangerous, if only their character is not misunderstood. They may be useful, either as devices for computation, or to aid our understanding by concrete images, to fix our ideas as the saying is. There is, then, no occasion to exclude them" [*Ibid.*]. Though conventional in a sense — belief in these hypotheses could not answer to fact — they differed from true conventions in the role they played within scientific theories. Indifferent hypotheses represented what later rational reconstructionists would call the "cognitively negligible" components of theories. Unlike the types of convention considered so far they were not of constitutive importance (we might call them "pseudo-conventions").

2. Conventions in Duhem

Duhem did not pronounce on the axioms of geometry (even his [1912] only discussed number theory), but his views on the so-called principles and methodological maxims are well documented. Concerning the status of the so-called "principles of science", Duhem disagreed with Poincaré. Since no part of physical theory could be tested in isolation, he argued, the general principles of a science were as much part of the complex of statements tested as other assertions. The principles were thus not immune from testing. In fact, it seemed to him more misleading than helpful to think of laws being turned into definitions. On the whole Duhem took the relevant conventions to consist in assigning units of measurement and giving physical interpretations to abstractly defined quantities.

Poincaré's point was that the principles of mechanics cannot be genuinely tested (no empirical body is free from forces, for instance) and cannot be refuted (we can and do deflect experimental discrepancies by reference to disturbing factors). Does Duhem's argument really engage with his position, as Duhem claims ? [Poincaré 1906/1962, 149-51] Poincaré, Duhem felt, paid insufficient attention to the all-pervasiveness of what we might call the "irreducible theoreticity" of scientific reasoning. Against Poincaré, Duhem stressed the radical difference between the abstract language of "theoretical fact" and the concrete language of "practical fact" [*ibid.*, 151]. To see the force of this distinction in Duhem, we must place it in his conception of the structure of mathematical physics.

Within the formulation of physical theories, Duhem distinguished two levels of abstraction from everyday observation and two types of cases in which the determination of scientific theory formulation by "the evidence" fails. Physical theory correlates with everyday observation ("practical facts"), first, a level of mathematical formulations in terms of measurable quantities, "theoretical facts", systematised by means of equally mathematically formulated laws ("experimental laws", often called "phenomenal laws"), and, second, a level on which these experimental laws themselves are systematised and unified in a theory encompassing various sub-fields of inquiry (what he and Poincaré called "hypotheses"). The two cases of the failure of determination are "the indetermination of theoretical facts and practical facts" (also called "symbolic indetermination") and the underdetermination of a theory's hypotheses by experimental laws. The first concerns the logic of scientific language, the second the logic of theory testing.

This view of the language of physical theory spelt a holism which did not allow for epistemological distinctions amongst its denizens and simultaneously rendered problematic the continuity between the languages of "crude" and "scientific facts", which Poincaré had still assumed. Duhem quoted Poincaré : "The scientific fact is only the crude fact translated into a convenient language." [Poincaré 1905/1946,. 330] ; [Duhem 1906/1962, 149] By contrast, Duhem held that the theoretical fact "the current is on" does not reduce to practical facts. "The role of the scientist is not limited to creating a clear and precise language in which to express concrete facts ; rather, it is the case that the creation of this language presupposes the creation of a physical theory." [*Ibid.*, 151] In Duhem's opinion, Poincaré overlooked that the languages of theoretical and practical fact were rendered commensurate only by complex interpolations.

> A single theoretical fact may then be translated into an infinity of disparate practical facts ; a single practical fact corresponds to an infinity of incompatible theoretical facts. [...] Between the phenomena really observed in the course of an experiment and the result formulated by the physicist, there is interpolated a complex intellectual elaboration which substitutes for the recital of concrete facts an abstract and symbolic judgement. [*ibid.*, 152-3]

The phenomenon of "indetermination", of mutual failure of determination, may be understood as due to a difference in the linguistic frameworks within which practical and theoretical facts are comprehended. The indetermination of practical fact and theoretical fact results from the fact that intuitive and mathematical conceptualisations are each embedded in different types of representational frameworks : one defines terms individually, the second logically by axioms. Nor can the meanings of the terms of the scientific language proper be established by postulating a determinate correspondence between them and an isolatable feature of experience, because scientific terms are far more precise than the vague terms of practical observation, "everyday testimony". "There can be no adequation between the precise and rigorous theoretical fact and the practical fact with vague and uncertain contours such as our perceptions reveal in everything" [*ibid.*, 152] ; (typographical error corrected).

The phenomenon of indetermination, of mutual underdetermination of theoretical and practical facts — which represents not the failure of match between the precise quantitative language of theory and the intuitive one of everyday but rather an embarrassment of riches of matches between them — was, so Duhem, not noted by Poincaré. Duhem himself indicated that these problems of "approximations" might be overcome by the future development of measuring instruments. But given that all physical mathematical theories were underdetermined by observational evidence, there was a sense in which for him all theoretical statements bore a certain conventional flair : in principle, another set of theoretical concepts and axioms might equally "save the phenomena" at issue. They are best viewed as instruments of economical description. Duhem did not accept the division between conventions and hypotheses within (mathematical) physics which Poincaré had introduced. At the same time, however, he agreed with Poincaré that definitional conventions were voluntarily adopted. Indeed, for Duhem these defintional conventions assumed a much greater role. To them ultimately the phenomenon of indetermination is owed.

Why then is Copernicus' theory better than Ptolemy's when both of them save the phenomena ? Here the methodological maxims of science come into play. Duhem stressed the role of "bon sens" in dealing with all these indeterminacies. He also agreed with Poincaré that the methodological maxim of simplicity be viewed as a convention, yet again he went beyond Poincaré. For Duhem, unlike for Poincaré, not only simplicity but also the unity of physics were conventions. Thus he extended the type of account of the adoption of the maxim of simplicity which Poincaré seems to have favoured — evolutionarily determined proclivities — to the maxim of unification of theory. Our preference for a unitary account of nature could not be justified logically but was itself fixed by the natural inclination of our "types of mind". The same held for simplicity. Judgments of economy were held to be subject to interpersonal differences. The desire for simplicity "results from an innate feeling of ours and cannot be justified by purely logical considerations" [*ibid.*, 104]. Since different "types of mind" thus arrived at different judgments concerning simplicity, Duhem, like Poincaré, viewed this maxim as somehow imposed upon us by our biological constitution.

So far, the differences between Poincaré and Duhem would seem to center mainly on whether we hold the constitutive definitions of mechanics to be irrefutable singular principles or wholistically refutable hypotheses. If we can allow to Poincaré, however, that the status of being a principle is a revocable one, then this difference is somewhat minimised. Did Poincaré think of these principles as irrevocable ? All along, Poincaré recognised that they might conceivably cease to be useful in extending the application of theories and aiding their unification. In such a case experience, "without directly contradicting a new extension of the principle, will yet have condemned it" [1902/1946, 144]. What Poincaré envisioned as an abstract possibility in 1902 — that the principles of Newtonian mechanics be condemned — had come to pass in 1904, as he recalled in the introduction to his *The Value of Science* [1905/1946, 207-8]. After his independent discovery of the principle of relativity Poincaré began mathematical work on "an entirely new mechanics, which would be, above all, characterized by this fact, that no velocity could surpass that of light, any more than any temperature can fall below absolute zero" [1905/1946, 312] ; cf. [Zahar 1989] ; [Gillies 1992, 92-4]. With this move Poincaré came to embrace a Duhemian position and the initial difference over the irrefutability of principles became rather minimal. Poincaré's view that "persistent failure to account for new facts" can prompt condemnation without contradiction [Psillos 1996] may well be regarded as a partial approximation of Duhem's view that physical theory faces experimental test as a whole and that all of its parts can be implicated in cases of predictive failure. (Poincaré would still deny that principles are hypotheses like any other.) On the other issue mentioned earlier, however, no such possible convergence was forthcoming. That is the question of the epistemological and ontological consequences of the adoption of conventions in scientific theories.

3. Poincaréan Realism and Duhemian Anti-Realism

So scientific theories contain several conventional elements — does this mean that, as Poincaré rephrased the radical conventionalist Le Roy, "[s]cience consists only of convention" and that it is due "to this circumstance solely" that it "owe[s] its apparent certitude", that "the facts of science and, a fortiori, its laws are the artificial work of the scientist", that "science therefore can teach us nothing of the truth" and that "it can only serve us as a rule of action" ? [1905/1946, 321] Poincaré did not think so.

> The rules of [a] game are arbitrary conventions and the contrary convention might have been adopted, which would have been none the less good. On the contrary, science is a rule of action which is successful, generally at least, and I add, while the contrary rule would not have succeeded. [*ibid.*, 323-4]

Poincaré's conventionalism — like Duhem's — was not as unconstrained as LeRoy claimed conventionalism to be. "[A]*ll the scientist creates in a fact is the language in which he enunciates it.* [...] facts are facts, *and if it happens that they satisfy a prediction, this is not an effect of our free activity*" [*Ibid.*, 332-3]. Conventional classifications helped to order data and and their generalisations in ways not antecedently determined. Once accepted and laid down, however, it remained an empirical matter whether the predictions so arrived at were true or false — and whether the conventions themselves were convenient. Concerning the principles Poincaré noted : "if *all* laws had been transformed into principles, nothing would be left of science" [*ibid.*, 324]. Still we can ask what alternative to Le Roy Poincaré and Duhem held in store. What kind of realism, if any, might Poincaré's and Duhem's conventionalisms allow ?

The question whether Poincaré was an instrumentalist with regard to abstract theory is raised by passages like this : "The object of mathematical theories is not to reveal to us the true nature of things ; this would be an unreasonable pretension. Their sole aim is to coordinate the physical laws which experiment reveals to us, but which, without the help of mathematics, we should not be able even to state" [Poincaré 1902/1946, 174]. This of course sounds very similar to Duhem's characterisation of a physical theory as "a system of mathematical propositions, deduced from a small number of principles, which aim to represent as simply, as completely, and as exactly as possible a set of experimental laws" [Poincaré 1906/1962, 19]. Duhem is generally known as an instrumentalist. How and why do his views differ from Poincaré's (if they do) ?

Duhem explicitly denies that "physical theory [ever] gives us the explanation of experimental laws ; it never reveals realities hiding under the sensible apprearances" [*ibid.*, 26] Duhem sharply distinguishes the "representative" from the "explanatory" role of abstract theory. Still, it may be objected that Duhem goes on to note that "the more complete it becomes, the more we apprehend that the logical order in which theory orders experimental laws is the reflection of an ontological order, the more we suspect that the relations it establishes among the data of observation correspond to real relations among things, and the more we feel that theory tends to be a natural classification." [*ibid.*, 26-7] Does this compromise Duhem's instrumentalism ? I do not think so. Duhem is explicit that this latter conviction — to have latched on to the "real relations among things" — is an act of "faith" : "The method at his [the physicist's] disposal [...] cannot prove that that the order established among experimental laws reflects an order transcending experience ; which is all the more reason why his method cannot suspect the nature of the real relations corresponding to the relations established by theory" [*ibid.*, 27]. Duhem recognised, of course, that if theory "anticipates experimental laws not yet observed and promotes their discovery" [*ibid.*, 30], then our inclination to regard it as a natural classification is increased. Indeed, Duhem was not ill disposed to natural classification claims. Yet he nevetheless stuck to this position : any claim to realism cannot be made from within science itself. Arguing throughout his book from within science, Duhem refused to make a realist claim concerning the objects and relations postulated by physical theory.

It seems that what Duhem here opposed is the view of Poincaré. Certainly Duhem's instrumentalism is prima facie in conflict with Poincaré's position. As Duhem put it in his review of A. Rey's *La Théorie de la Physique chez les physiciens contemporains* [1906] :

> We can say of propositions which claim to assert empirical facts, and only of these, that they are true or false. Of these and only these we can affirm that they cannot accomodate an illogicality and that of two contradictory propositions at least one of them must be rejected. As to propositions produced by a theory, they are neither true nor false ; they are only convenient or inconvenient. [...] Therefore to oblige physical theory to preserve a rigorous logical unity in its development would be to impose on the physicist's mind an injust and intolerable tyranny. [1908/1962, 333-34]

Note two things here. Duhem withheld truth claims from theory. But he also accepted the possibility that the goal of unity of physics — which he shared with Poincaré — may have to be given up. Unlike for Poincaré, as we saw, unity was a methodological maxim for Duhem. Like the "surmise" that natural classification "corresponds to a certain supremely eminent order", the belief in the unity of nature is a metaphysical belief. The "sole justification of physical theory" — that is, "the reason leading [the physicist] to construct a physical theory" — lies outside of science itself, indeed necessarily so for Duhem, for it is a "belief in an order transcending physics" [*ibid.*, 334-5]. Metaphysics remained based on faith ; on scientific grounds alone nothing but instrumental value could be accorded to physical theory.

That Poincaré was not an instrumentalist in the common understanding of that term is suggested by his argument against what is nowadays called the "pessimistic meta-induction" [Laudan 1981]. This argument holds that since there has been no referential continuity even between mature scientific theories across history, we must not expect our present theories to hold up — realism must therefore be false. Poincaré responds :

> No theory seemed more solid than that of Fresnel which attributed to light motions of the ether. Yet now Maxwell's is preferred. [...] Now, Fresnel's theory permits of [forseeing optical phenomena], today as well as before Maxwell. The differential equations are always true ; they can always be integrated by the same procedures and the results of this integration always retain their value. And let no one say that thus we reduce physical theories to the role of mere practical recipes ; these equations express relations, and if the equations remain true it is because these relations preserve their reality. They teach us, now as then, that there is such and such a relation between some thing and some other thing ; only this something formerly we called motion ; we now call it electric current. But these appellations were only images substituted for

> the real objects which nature will eternally hide from us. The true relations between these real objects are the only reality we can attain to, and the only condition is that the same relations exist between these objects as between the images by which we are forced to replace them. If these relations are known to us, what matter if we deem it convenient to replace one image by another. [1902/1946, 140]

John Worrall has called this view "structural realism" [Worrall 1989, 1994] ; [Zahar 1996], [Psillos 1995]. It says that what is merely instrumental are our conceptions of the entities we theorise about. These conceptions undergo alterations, sometimes very radically, as in the case at hand : from Fresnel's notion of vibration in an elastic ether to Maxwell's notion of a displacement current in an electromagnetic field. In fact, for Poincaré, "[h]ypotheses of this sort have [...] only a metaphorical meaning" [1902/1946, 141]. They are instances of the "neutral" or indifferent hypotheses noted earlier. Assertions about the true nature of things, we can thus rephrase him, are but what I called "pseudo-conventions" — that is, apart from their perhaps heuristic value, negligible components of theories.

What remains constant in this theory change, however, is the structure of the phenomena investigated. As Worrall puts it, "disturbances in Maxwell's field do obey formally similar (in this case, and unusually, mathematically identical) laws to some of those obeyed by the 'materially' entirely different elastic disturbances in a mechanical medium" [Worrall 1994, 340]. Poincaré thus resisted wholesale instrumentalism.

> Since the enunciation of our laws may vary with the conventions that we adopt, since these conventions may modify even the natural relations of these laws, is there in the manifold of these laws something independent of these conventions and which may, so to speak, play the role of *universal invariant* ? [...] The invariant laws are the relations between the crude facts, while the relations between the 'scientific facts' remain always dependent on certain conventions. [1905/1946, 338-40]

You will notice that here Poincaré distinguished between the "crude facts" of everyday observation and the "scientific facts" of theory. Does this not support instrumentalism after all ? Note first that what is preserved are relations and that Maxwell's equations do not concern the phenomenological level but express theoretical relations. Second, recall that, unlike Duhem, Poincaré thought of "scientific facts" as more or less conventionally conditioned translations of "crude facts". The universal invariants were relations retained in different guises in succeeding theories. What changes is the terminological guise in which these relations are expressed. The universal invariants are not conventions but empirical claims. Consider the delightful reprise of his argument against the pessimistic meta-induction in his critique of Le Roy :

> At first blush it seems to us that the theories last only a day and that ruins upon ruins accumulate. Today the theories are born, tomorrow they are the fashion, the day after tomorrow they are classic, the fourth day they are superannuated, and the fifth they are forgotten. But if we look more closely, we see that what thus succumb are the theories properly so-called, those which pretend to teach us what things are. But there is something in them which usually survives. If one of them taught us a true relation, this relation is definitively acquired, and it will be found again under a new guise in the other theories which will successively come to reign in place of old. [*ibid.*, 351]

No restriction of true relations to observable ones is made here. Poincaré's summary is equally non-restrictive : "It will be said that science is only a classification and that a classification cannot be true, but convenient. But it is true that it is convenient, it is true that it is so not only for me, but for all men ; it is true that it will remain convenient for our descendants ; and it is true that this cannot be by chance. In sum, the sole objective reality consists in the relations of things..." [*ibid.*, 352] These two additionnal passages suggest that Poincaré's appeal to universal variants as relations between crude facts cited above does not support an instrumentalist reading. In addition note Poincaré's appeal to the so-called "no miracle" argument ("this cannot be by chance"). He invoked (a species of) realism as an explanatory hypothesis for the success of science. According to Poincaré, we are free to adopt a type of Correspondence Principle which says that successors to theories which have enjoyed genuine predictive success (as Fresnel's did) must incorporate "the mathematical equations of the old theory as limiting cases of the mathematical equations of the new" [Worrall 1989, 120]. Such then is Poincaré's structural realism : in mature and successful theories, not the entities talked about, but the relations specified to hold between these entities are real.

Yet how much does this view of Poincaré's really differ from Duhem's positive attitude towards the quasi-regulative idea of "natural classification" ? Duhem wrote, for instance, that in theory change "the purely representative part enters nearly whole into the new theory, bringing it to the inheritance of all the valuable possessions of the old theory, whereas the explanatory part falls out in order to give way to another explanation." [1906/1962, 32] All the same, Duhem abstained from affirming realism. His abstention was voiced from the scientific point of view. The question arises whether Poincaré understood his version of the miracle argument to proceed within science itself. Reduced to its strict scientific content and abstracting from historical speculation, the Correspondence Principle only gives conditions for the predicability of reality to structures : it does not assert that these conditions do obtain. Poincaré and Duhem may well have agreed that science itself cannot make a claim about the grounds of its success that would possess a higher epistemological status than its more common first-order claims.

Still, differences between them remain. First, Poincaré asserted realism whereas Duhem did not, seeking to dissociate science from metaphysics. From Duhem's point of view, Poincaré voiced a belief that supported the knowledge claim of science from without : that the conditions specified by the Correspondence Principle do and will continue to obtain was not something science itself could claim. Insofar as Poincaré's confidence transcended possible scientific evidence, his structural realism must count as a metaphysical thesis. (Here the contrast between Poincaré's version of Kantianism and Duhem's Catholicism becomes important [Zahar 1996].) Second, this structural difference between their conceptions of theories obtained : Poincaré assumed the continuity of ordinary and scientific language which Duhem questioned. Poincaré could distinguish between definitions and empirical claims in scientific theories because for him the scientific terms had not yet lost their moorings in ordinary language. Duhem,thinking of theory as a holistic abstract symbolic system disjoint from ordinary language, felt unable to draw this distinction. Thus for him the ultimately arbitrary delimitation of a class of theoretical definitions from that of theoretical empirical claims could not bear the epistemological weight which the claim that only the invariant is the real placed on it. With Duhem, the sharp distinction of convention and factual assertions within scientific theories themselves collapsed into theoretical holism. But this did not undermine the possibility of drawing a sharp distinction of theory and observation that recaptured some of the force of Poincaré's of convention and fact, namely, in terms of theoretical facts-and-hypotheses on the one hand and practical facts on the other.

4. Varieties of Conventionalism

It is not easy then to say precisely what Poincaré and Duhem shared when they spoke of conventions in science. The thought that evidential underdetermination indicates the presence of conventions will not get us far. As we have seen, they differed on the extent to which they thought scientific knowledge was affected by it. It is only with regard to the methodological maxim of simplicity that both Poincaré and Duhem lived up to the popular reputation of conventionalism. No knowledge claim at all attached to it, even though it set the terms and direction of inquiry. (It was "constitutive" in the sense in which neutral hypotheses or pseudo-conventions, which were also denied the status of knowledge claims, were not). Beyond that matters get complicated.

Poincaré's different types of convention do not seem to share a feature that is also acceptable to Duhem. For Poincaré geometry was conventional because it lacked a unique determination ; it was not conventional because it was *a priori*. The principles, by contrast, derived from past experience and do not seem more underdetermined than inductive generalisations are ; they are simply held to be above the empirical fray. The methodological maxims, finally, found their roots partly in biologically determined mentalities and partly in the pragmatic dimension of inquiry ; some of them were constitutive for our practice but non-representational. Now all three types of conventions shared the feature that they stood immune from contradiction. Just this common characteristic of Poincaré's conventions was rejected by Duhem, however. For him, all of theoretical physics was conventional as geometry was for Poincaré (albeit for different reasons). The shared sense of conventionality in play in Poincaré, which betokened unassailability, was denied by Duhem : epistemologically, all assertions of theoretical physics were equally revisable. With Duhem the principles lost the distinguished epistemological status Poincaré had still ascribed to them. (Admittedly, Poinacré's concept of "condemnation" complicates the picture, but the outlines remain substantially the same.)

There remains, of course, one clear sense of conventionality that is common to the French conventionalists. They agreed that there are facts which are unimpugned by any of the conventionalities of theory however conceived, namely the "crude" or "practical facts" which we know for sure [Poincaré 1905/1946, 328] ; [Duhem 1906/1962, 163]. The significance of this becomes clear by comparison of Duhem with Quine. Did Duhem hold that "any statement can be held true come what may, if we make drastic enough adjustments elsewhere in the system" [Quine 1951/1982, 43] ? First, Duhem made his holistic claims only for theoretical physics (possibly a special case amongst the sciences), secondly, no doubt was cast on the well-foundedness of natural language, as shown by the certainty he ascribed to everyday testimony. Given "indetermination" relative to practical facts and the unability to draw a sharp distinction between conventions and factual assertions, Duhem also cast an air of artificiality over all the facts of theoretical physics that constrasted with the practical facts of everyday testimony. While the dividing line between fact and convention was delineated differently by Poincaré and Duhem, they agreed in its importance. To say that a statement was to some degree or other conventional in this sense was to say that this statement did not in its entirety faithfully represent reality — it did not stand wholly for a fact, but to some degree represented an artefact. For the conventionalists crude or practical facts were epistemologically foundational in a sense in which scientific or theoretical facts were not (we may call this their "artefactuality"). That Poincaré's and Duhem's agreement was pretty thin, however, becomes clear when we ask whether their conceptions could serve to model the notion of a merely constitutive but not apodictic a priori : not only does Duhem's holism forbid this epistemological distinction, but he also presents no criterion by which conventions and empirical claims could be distinguished within theoretical physics. Nevertheless, the artefactuality by which scientific knowledge is distinguished from unproblematic knowledge of practical fact remains the admittedly low common denominator of Poincaré's and Duhem's conventionalisms.

Note then that already the French conventionalists would have disagreed concerning the issue whether the constitutive *a priori* could be distinguished from other, also underdetermined statements of abstract theory. This raises interesting questions about the development of logical empiricism, for it was precisely this problem that brought early Vienna Circle conventionalism to a fall. As Friedman has argued [1993], the inability to distinguish conventions from assertions within theory proper was the problem that debilitated Schlick's early theory of knowledge — to be precise, one might add, its revised version in the second edition of *Allgemeine Erkenntnislehre* [1925], the first still being characterised by a robustly realist holism [Howard 1993]. It also is not clear whether Neurath who had embraced theoretical holism already by 1913 [Haller 1982], [Uebel 1996] fully noticed this this problem, given his enthusiasm for the Circle's standard form of rejection of Kant's synthetic a priori, which often involved conventional determinations. This issue was not resolved until Carnap's *Logical Syntax* [1934] — if, indeed, it was resolved at all — for only there was the phenomenon of the artefactuality of logico-linguistic frameworks dealt with rigorously enough to yield what promised to be the required class of a non-apodictic, merely constitutive a priori. Reaching that "solution", however, seems to have required the Circle's anti-metaphysical turn that made it impossible to raise the questions concerning realism which Poincaré and Duhem disagreed about. With Carnap, the sharp epistemological distinction between practical and theoretical facts was abolished while analyticity was reduced to a purely explicatory, non-foundational tool of logico-linguistic analyses which in turn bore no ontological import. (If Carnap's proposed definition of analyticity is deemed to fail, of course, the problem remains unresolved).

In light of the gulf that thus opens up between the conventionalism of the later left Vienna Circle and of its precursors, it may be stressed in closing that it was not only for the notions of convention and under- and indetermination that the Circle was indebted to the French conventionalists. Of fundamental importance for the Circle's development of the "semiotic conception of scientific knowledge" [Ryckman 1991] was their appreciation of the conventionalists' relationism (or, perhaps better, structuralism). This relationism represented a view on which, in the eyes of their Circle readers, the conventionalists wholly converged with that of other leading theorist of science of their time. The members of the first Vienna Circle (Frank, Hahn, Neurath) may well have viewed it primarily as a development of Mach's views concerning the irreality of substance [eg. 1882/1986, 200-1] ; [1883/1960, 579] ; [1886/1897, 6] ; [1896/1986, 384-390]. For Schlick, instead, the influence of Poincaré's relationism was paralleled by Russell's repeated stress of the importance of relations and structure [Demopoulos and Friedman 1985], a stress that also wielded a similar influence on Carnap, together with relevant passages in Frege [Hart 1992]. All of them were concerned to determine, as definitive of scientific knowledge, the conditions on the structure of relations that can be warrantedly asserted. And though the members of the Vienna Circle seem to have missed Poincaré's structural realism (perhaps misled by his reference to "relations between crude facts") — and thus not only misread his argument for the conventionality of geometry [Friedman 1995] — they did not miss another aspect of the paramount importance which the conventionalists accorded to relations and the structural features of scientific theories.

Relations provided the key for the objectivity of science. Poincaré wrote, anticipating many later debates :

> Such ... is the first condition of objectivity ; what is objective must be common to many minds and consequently transmissable from one to the other... The sensations of others will be for us a world eternally closed. We have no means of verifying that the sensation I call red is the same as that which my neighbour calls red. [...] Sensations are therefore intransmissible, or rather all that is pure quality in them is intransmissible and forever impenetrable. But it is not the same with relations between these sensations. From this point of view, all that is objective is devoid of all quality and is only pure relation. [...] nothing is objective which is not transmissible, and consequently [...] the relations between the sensations can alone have an objective value. [1905/1946, 347-349]

There can be little doubt that Schlick's *Allgemeine Erkenntnislehre* [1918] took its inspiration from this passage, even though it was other aspects of Poincaré's conventionalism that were cited. Carnap's Aufbau [1928] expressly sought to further desubjectivise this conception and Neurath provoked the notorious debate about protocol statements by his radical rejection of the sensationalist base still tolerated there in turn [Uebel 1992]. With Carnap and Neurath in particular, objectivity came to be understood as the condition of intersubjective controllability of assertions. The spectator-view of knowledge and the view of objectivity as undistorted representation were abandoned — and with it also any attempt to answer the philosophical scepticism that still exercised Poincaré. The starting point of Carnap's and Neurath's endeavour, however, is to be found in Poincaré, whose structuralism founded not only an ontological position but also — and perhaps more importantly historically — an epistemologically defining characteristic of scientific objectivity.

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