Conventions and Relations in Poincaré's Philosophy of Science*

Stathis Psillos
Dept of Philosophy and History of Science
University of Athens,
Greece
&
Rotman Institute of Philosophy & Dept of Philosophy,
University of Western Ontario, Canada
e-mail: psillos@phs.uoa.gr

1. Introduction

Henri Poincare's *La Science et l' Hypothése* was translated into English in 1905. One of the first reviews—published already in 1905—was by Bertrand Russell. After praising Poincaré for his "power of co-ordinating the whole domain of mathematics and physics in a single system of ideas" (1905, 412), Russell—in this short by pointed review—put forward the two main interpretations of Poincaré's thought that subsequently became standard. Poincaré was a conventionalist *and* a structuralist.

According to Russell, Poincaré argued that geometry is *wholly* conventional and that the principles of mechanics are definitions. He rather quickly dismissed this view by taking the line that conventions are merely hypotheses which have been willingly withdrawn from empirical testing and claimed that they were not really necessary qua a different epistemic category.[1] Interestingly, he spent more time explaining that for Poincaré "science teaches us, not about things in themselves, but about their relations" (1905, 412). As Russell understood Poincaré's main thesis, "if *a* really exists, a statement about *a* has no meaning unless it asserts a relation to a *b* which also really exists" (1905, 417). His prime disagreement with Poincaré was that he took that statements about qualities of real things are not devoid of meaning but simply unknowable. But apart from that, Russell endorsed this relationist reading of Poincaré and made two important points. The *first* is that it is by holding fast to relations that science "manages, on an empirical basis, to construct a world so different from that of perception" (1905, 417). For him, as well as for Poincaré, a key problem of philosophy of science was to explain how the abstract structure of a theory in mathematical physics gets connected with the world of experience and as Russell briefly noted in this review, the contact is by sharing structure and not by sharing any qualitative character. Yet the *second* point Russell made was pushing relationism to a certain direction, which we may call *pure structuralism*. Russell, as is well known, were to develop this view later on (cf. 1927).[2] In the review, he put the suggestion thus:

---

* I want to dedicate this piece to Bill Demopoulos and Robert DiSalle, two friends of mine who have taught me a lot about philosophy and Poincaré in particular. Research for this paper was partly co-financed by the European Union (European Social Fund—ESF) and Greek national funds through the Operational Program ''Education and Lifelong Learning'' of the National Strategic Reference Framework (NSRF)—Research Funding Program: THALIS–UOA-APRePoSMa.

1 Russell had engaged in a debate with Poincaré on the foundations of geometry and had aleady circiticised geometrical conventionalism. See Poincaré (1899; 1900a); Russell (1899); and Gray (2013, 76-82).

2 For more on Russell's structuralism and the famous Newman problem that brought it down, see Demopoulos and Friedman (1985) and Psillos (2001).

> We may push the theory further and say that in general even the relations are for the most part unknown, and what is known are properties of the relations, such as are dealt with by mathematics (1905, 417).

Pure structuralism is the view that only the formal properties of the relations among things are known; that science can reveal only them, and not what these relations are. Hence, the contact between the abstract theories of mathematical physics and the world of experience is not just structural, but *purely* structural, meaning that they share formal structure and nothing else. According to Russell, this was Poincaré's view too. But as we shall see, it was not.

Russell's review was forgotten, I guess, because subsequently the two strands of Poincare's philosophy followed distinct interpretative paths. The conventionalist interpretation became the dominant.[3] Actually, it seems that it is only fairly recently, thanks mostly to the work of Jersy Giedymin (1982), Elie Zahar (1994; 2001) and John Worrall (1989) that the structuralist interpretation acquired currency. Many philosophers, including myself, have adopted the line that Poincaré was a structural realist—or at least a precursor of structural realism. But was he, really? More importantly: how was his conventionalism related to his relationism? How, in other words, is it the case that the basic principles of geometry and mechanics are, ultimately, freely chosen conventions and that, at the same time, science reveals to us the structure of the world?[4]

This lengthy study aims to address these questions by examining in detail Poincaré's developing views about the status and role of *conventions* in science and the status and role of *relations* in science. Here is the road map. Section 2 presents the case for geometrical conventionalism. It is argued that Poincaré developed his conception of convention in an attempt to delineate a new epistemic category, which is occupied by some principles (of geometry and of mechanics) which cannot be taken to be either hypotheses or fitting in one of the three Kantian categories (synthetic a priori; analytic a priori; synthetic a posteriori). It is also argued that because of the way *geometrical* space is constructed, it is related to sensible space only 'up to isomorphism' and acquires empirical content only because of the fortunate event that there are empirical (solid) bodies which are similar to the geometrical ones. Section 3 presents the reasons for extending his conventionalism to the principles of mechanics and the reasons why he also *limits* his conventionalism to geometry and mechanics. It is noted that Poincaré devises a method to split the content of empirical claims into two propositions, one expressing a convention and another capturing its proper empirical content; he uses this method to show how conventions are not true by sheer stipulation (since that make 'some kind of contact' with experience) and yet they are freely chosen by the scientists. Section 4 compares geometrical conventionalism and physical conventionalism and argues that though the processes that lead to them are different (top-down vs. bottom-up) the conventions have the same epistemic status. Section 5 presents two important reactions to his conventionalism by Federico Enriques (who argued that conventions are not necessary) and Édouard Le Roy (who argued that all science is conventional). This section sets the stage for the discussion of Poincaré's structuralism in section 6. Poincaré structuralism is better understood as a form of *relationism*. It is argued that there were three motivations for Poincaré relationism. The *first* stems from the very nature of mathematical representation in science; the *second* comes from the need to

---

[3] See Stump (1989) and Ben-Menahem (2006). A recent important collection of essays on Poincaré's philosophy is de Paz & DiSalle (2014).

[4] In his magnificent (2013), Jeremy Gray does discuss the conventionalist interpretation (pp. 526-529) and the structural realist interpretation (pp. 534-542) but unfortuntely he does not discuss their connection. Instead, he prefers to offer another interpretation of Poincaré along the lines of Kripke's interpretation of Wittgenstein on rule-following.

ground objectivity in science; and the *third* is related to lessons drawn from the history of science. It is also argued that relationism mitigates Poincaré's conventionalism and helps him block the charge that scientific theories are simply networks of conventions resembling Penelope's web.

2. The case for geometrical conventionalism

Poincaré's first article on non-Euclidean geometries was published in 1887 and was entitled "'Sur les hypothèses fondamentales de la géométrie'". This was an allusion to Riemann's seminal "On the Hypotheses which lies at the Bases of Geometry". In his own article Poincaré examined the three plane geometries of constant curvature and, based on Lie's group-theoretic approach, showed their common assumptions and their different groups of transformations. But his main objective was not technical—though the paper was technical. He intended to take issue with the claim that at the basis of Geometry lie *hypotheses*. Euclid's fifth axiom was precisely a *postulate*, which however could be consistently denied by the non-Euclidean geometries of Lobachevski and Riemann. Given the Kantian trichotomy between analytic judgements, synthetic a priori judgements and experimental facts (empirical propositions), the 'hypotheses' that lie at the basis of geometry fit in none of them. They are not analytic truths like the principle 'two quantities which are equal to a third one, they are equal to each other'. They are not synthetic a priori judgements since we can conceive their negation; that is, they are not necessarily true. Finally, they are not experimental facts, because if they were considered such, geometry would no longer be an exact science; it would be subject to constant empirical revision.

2.1 From hypotheses to conventions

It is noteworthy that in this article (in 1887) Poincaré does not yet call *conventions* the indemonstrable propositions of geometry. He only points out that they do not fit within the three Kantian categories. Yet he does go on to make two important observations. The *first* is that the geometry of Euclid and the geometry of Lobachevski study different groups; hence, as such, they are compatible with each other; or as Poincaré put it "the truth of the geometry of Euclid is not incompatible with the truth of the geometry of Lobachevski, since the existence of the one group is not incompatible with the existence of the other group" (1887, 215). What is each geometry true of, then? Poincaré does not say, but it is quite clear from the context what he means: it is (in a sense trivially) true of the group of transformations that characterises it. But the study of the natural phenomena presupposes that they are in *space*; hence some geometry should be chosen anyway for the study of natural phenomena to be possible. This is clearly Poincaré's debt to Kant. But unlike Kant, he took it that the conceptual task at hand is not the co-ordination of *Euclidean* geometry with physics, but the choice of the suitable geometrical group; that is, the choice of the suitable *geometrical space*.

And here comes Poincaré's *second* important observation. The chosen group is, indeed, Euclid's. But not for reasons that Kant insisted, viz., that it captures the form of pure spatial intuition; but because it is simpler than non-Euclidean groups and—most importantly—because *we* encounter in experience (and this is a *contingent* fact) a "remarkable" kind of bodies, which we call solid and whose various possible movements are related very closely to the various operations of the chosen group. Hence Poincaré makes the point that though the "fundamental hypotheses of geometry are not experimental facts, (...) it is the observation of certain physical phenomena that makes their choice from all possible hypotheses" (1887, 215). In other words, the choice is based only on how commodious the group is in characterising 'spatial' relations between physical phenomena, which include the motion of *solid* bodies.

Four years later, in 1891, in his second piece on non-Euclidean geometry titled "Les géométries non Euclidiennes"—which appeared in translation in *Nature* in 1892—Poincaré announced to the world that he had invented a new epistemic category to place geometrical axioms, viz., *conventions* or *definitions in disguise*.[5] Here he distinguished geometrical indemonstrable propositions from arithmetical ones, like the principle of least number (or equivalently, the principle of mathematical induction), which he took to be synthetic a priori principles, because we cannot "free ourselves" from them. But we can free ourselves from the axioms of Euclidean geometry and besides we can actually base physics on non-Euclidean spaces. What is rather interesting is that Poincaré adds a further reason why geometrical propositions are not empirical truths: "we do not do experiments on straight lines or ideal circles; only material objects can be dealt with. On what would depend, then, the experiments that serve to found a geometry?" (1891, 773; 1892, 407). The point here is that geometry does not have the natural world as its *proper* object; rather, being an abstract science, it deals with abstract (or ideal) entities, which as such, are not encountered in the natural world. So geometry cannot be verified on the basis of experience, simply because, strictly speaking, the empirical objects fall outside its proper scope.

But could it be that some empirical situation can falsify geometry? The thought that this may happen is tempting because Poincaré wants to insist that the new epistemic category he has introduced—the convention or definition in disguise—is not *fully* devoid of empirical content. His point is rather subtle. Though geometrical conventions do not have the empirical world as their *proper* content, and though their choice is "free and (...) only limited by the necessity of avoiding all contradiction", they are, in a certain sense, applicable to the world we live in precisely because there are objects in the world that can be *suitably related* to the proper objects of geometry. Poincaré is never tired of repeating that the existence of solid bodies makes geometry applicable to the world: "one argues constantly as if geometrical figures behaved like solids" (1891, 773; 1892, 407). It might then appear tempting to think that there might be a mismatch between geometry and the behaviour of physical phenomena, such that the chosen geometry is falsified.

But this is not possible, according to Poincaré. The reason for this brings out the full strength of his new epistemic category. Any attempt to subject a geometry—that is a certain group—to empirical test would require making experiments (or observations) *on physical bodies* and this implies that we would have to assume some physical theory or other. Hence, we could never test geometry in isolation from physical theory but only in conjunction with it. By conjoining Lobachevskian geometry with astronomical theories, we would get the prediction of a finite parallax of a distant star. But if we conjoined them with Riemannian geometry, the prediction would be of negative parallax. Do we have a crucial experiment here? Far from it. When we assume that light travels in straight lines, we have thereby assumed that *a light ray is a straight line*. Hence, were we to discover a negative parallax or a parallax greater than a certain finite value, we could always respond to this situation by modifying the laws of optics, e.g., by modifying the law that light travels in straight lines. To make the point more precise, Poincaré argues that when we envisage an empirical test, we envisage a situation which tests the conjunction G&P (where G is some geometry and P is some physical theory); hence, when we find recalcitrant evidence, we are always free to stick to the preferred geometry and change the physical theory. That is why Poincaré confidently says that "Euclidean geometry (...) has nothing to fear from new experiments" (1891, 773; 1892, 407).

---

[5] That convention is a new epistemic category is defended in my (1995) and in DiSalle (2006, chapter 3).

This move might be taken to be a response to a typical case of underdetermination of theories by evidence. It might be taken to imply that what Poincaré says is that we are always confronted with two empirically equivalent systems of the world, one having non-Euclidean geometry and ordinary physics, and another having Euclidean geometry and some new physics. Though Poincaré does not deny that this *is* a away to describe the epistemic situation, his point is deeper. I think his key contribution in the delineation of the new epistemic category of convention was his offering of a systematic way to disentangle the content of a convention from various empirical assumptions associated with it, which might create the impression that the convention is merely an empirical truth. We will see this strategy being fully developed when he talks about conventions in mechanics, but let us now see how he illustrates it in the case of geometry.

2.2 Conventional content vs empirical content

In "L'espace et la géométrie", in 1895, he told vividly the story of a possible world in which the underlying geometry is Euclidean but, due to the existence of a strange physics, all attempts to find out empirically the geometry of this world would lead its inhabitants to assume that the geometry is non-Euclidean. This world is the interior of a sphere *S*. Viewing this world from "the outside", we can easily infer that its geometry is Euclidean. Things are not so simple for the locals, however. For, unbeknownst to them, there is a medium permeating *S* such that the temperature at each point is variable, being a function of the distance of each point from the centre of the sphere. In particular, the temperature at each point is R^2-r^2, where *R* is the radius of the sphere and *r* is the distance of the point from the centre. The freely-moving inhabitants of this world cannot notice any difference because the laws of physics ensure that, wherever they move, thermal equilibrium is immediately restored. But the laws of physics also ensure that all bodies, including measuring rods, contract uniformly as they move away from the centre of *S* and towards the periphery.

Imagine that the inhabitants of *S* try to determine the geometry of their world. They will soon find out that they live in a Lobachevskian world of infinite extent. One of their relevant empirical findings will be that they can draw an infinite number of 'parallel' lines from a point outside any given line. The only operational procedure at their disposal will be enough to persuade them of this: they extend the lines indefinitely and they never meet. So, they will find irresistible the conclusion that their world is Lobachevskian. Yet, an eccentric mathematician of *S* suggests to his fellow scientists that the geometry of *S* is really Euclidean but that, due to a universal force (the temperature field) which makes everything contract as they move, the geometry appears to be non-Euclidean. Now, the inhabitants of *S* are faced with two empirically equivalent alternatives. How are they to choose between them? Poincaré says that whatever their choice be, it is not dictated by their empirical findings. The latter can be 'written into' any of the two geometrical languages, with suitable adjustments in the relevant physics.

But, as noted already, his point is not simply that evidence underdetermines theory. Take (A) to be the proposition that in S more than one parallels can be drawn from a point outside a line. Now (A) can be partitioned into two other propositions. The first is a *geometrical* proposition such as Euclid's fifth postulate, viz. (B) that from a point outside a line *only one* parallel to this is drawn. The second is a *physical* proposition, viz., (C) that because of the temperature field, all bodies in S, including measuring rods, contract uniformly as they move away from the centre of *S* and towards the periphery. Now, (A) is an empirical proposition, but its empirical content is (C) and only (C); (B) remains a convention, which is not amenable to empirical test.

In the same article (1895), Poincaré elaborated on the explanation of why geometry is only *indirectly* related to experience. This is that the space studied by geometry—the *geometrical* space—is not the sensible space; it has properties radically different from the sensible (what he called the "representative") space (e.g., it is homogeneous and isotropic). Better put, geometrical space is constituted (or constructed) as such by applying the concept of group to certain ideal (mathematical/abstract) entities—the displacements (without deformation). These are not empirical entities, but they can be seen, to some extent, as abstractions of empirical entities, namely rigid (solid) bodies. Geometrical space comprises the laws of displacements—that is, the group they form. That the set of displacements forms a group (e.g., that if two changes A and B are displacements, then the change A + B is a displacement) is not a priori true. Yet, if it were not taken to be the case, there would be no geometry. Geometrical space, in its turn, is a presupposition for doing science: "we *reason* about [external] objects as if they were situated in geometrical space" (1895, 636), though of course they are not.

It is well known that Poincaré took his inspiration from Helmholtz's view that the propositions of Euclidean geometry "were no other than the laws on motion of rigid bodies" and that the propositions of non-Euclidean geometries were alternative laws governing the motion of bodies analogous to rigid bodies—laws which do not apply to the actual world, but which *could* be conceived as applying without any contradiction (Poincaré 1903, 21). But he famously disagreed with Helmholtz's account of the status of these propositions.[6] Helmholtz was right in thinking that geometry requires free mobility—that is movement without deformation—but wrong in thinking that the propositions of geometry were "experimental", since "the solids of nature follow them only roughly, and since, besides, the fictitious bodies of non-Euclidean geometry do not exist, and cannot be accessible to experiment" (Poincaré 1903, 21). The turning point in Poincaré's appraising of the situation was the study of Lie's theory of groups.[7] Lie took each geometry to correspond to a group of possible transformations of a figure and proved that if we assume free mobility (that the movement of an invariable figure is possible), there are three and only three geometries possible: Euclidean (space has zero curvature); Lobachevskian (space has negative curvature) and Riemannian (space has negative curvature). All three are, notably, geometries of constant curvature.

Recall that Poincaré's debt to Kant requires that the spatial framework should be set *in advance* of any scientific investigation of natural phenomena. But the choice of the spatial framework (the choice of geometrical space) is not forced. It could be any of the three spaces of constant curvature, but Poincaré suggests that the choice of Euclidean space is the most commodious.

When Poincaré says that experience does not tell us which geometry is the truest, but which is the most commodious, he merely recapitulates his points a) that the content of geometrical propositions is not the physical space but the *geometrical* space; b) that there are alternative accounts of the structure of geometrical space; c) that none of these accounts is truer than the others, in and of itself; d) that the very possibility of science requires that *some* account of geometrical space is chosen so that we study physical phenomena as if they were located in this space; e) that the choice of this account is not dictated by experience; and that f) nonetheless, experience (in the form of the properties of actual solid bodies) guides this choice. This is, in a nutshell, Poincaré's geometrical conventionalism.

2.3 Form vs matter

What needs to be added is that for Poincaré the very concept of a group (*qua* a freely created mathematical concept) is something that pre-exists in the mind. As he says "it is imposed on us not as a form of our sensibility, but as a form of our understanding" (1895,

[6] For the relation between Helmholtz and Poincaré, see DiSalle (2006).

[7] For Poincaré's study of Lie groups see Schmid (1982).

645). The concept of group belongs to pure mathematics and as such it is a priori. But Poincaré says something stronger, viz., that it is a *form* of understanding. Speaking loosely, the group characterizes the laws that displacements obey. Displacements succeed one another (or, better put, our representations of them succeed one another). Now, if, as a strong empiricist view implies, all events are distinct and separate, then any displacement *could* be accompanied by any other. But this is not what Poincaré takes it to be the case. The very possibility of understanding the world requires that there is, so to speak, some *order* to the succession of displacements, some fixed laws of succession—and these are imposed by the group. It is in this sense that the group is a form of our understanding. It goes without saying that we could conceive of different laws of succession of displacements—and hence of different particular groups—that constitute the geometrical space. That is exactly what non-Euclidean geometries amount to. But this does not imply that the very concept of a group is conventional. Rather, being a form of understanding, the group can capture only the "structure" "of the movements of invariable solids" (1895, 644). Poincaré sticks to this view consistently. In his (1897a), where he offers response to criticisms of his views, he notes that experience is only the "occasion" for us to exercise our capacity to form the notion of a group.

Before we leave this section, it is important for what follows to stress that for Poincaré, geometry is about the *form* of geometrical space and not its matter. In this, as he stresses in his (1898), he differs from Helmholtz and Lie, who took it that geometry is directly applicable to the physical space—a continuum of three dimensions. "For me", Poincaré says, "the form exists before the matter" (1898, 40). And the matter is indifferent for the study of geometrical space in the same way in which the matter of a cube (whether it is made of iron or wood) is indifferent for the study of the geometrical properties of the cube. Matter matters, of course, but only when a group is chosen to characterise physical space. And when, in this process, there seem to be apparent deviations from the properties of the group, we do not thereby conclude that the chosen geometry is falsified. Rather, Poincaré retorts to his favourite move of *splitting* the content of an empirical proposition into two components, one purely geometrical and hence conventional and another physical and hence empirical. When, for instance, we realise that the motions of actual solid bodies satisfy a certain group—e.g. Euclid's—only approximately, we account for this by splitting the relevant motion into two components—one being a displacement which rigorously satisfies the operations of the relevant group and another which corresponds to a qualitative change (such as thermal dilations) of the body in motion (cf. 1898, 11).

3. The case for physical conventionalism

Poincaré often said that the principles of mechanics were conventions, or definitions in disguise. How exactly are we to understand this claim? There are two sources in Poincaré's physical conventionalism, as I will show in detail below. The *first* source is related to the very status of the principles of mechanics *qua* general and objective principles. Briefly put, the idea is that the principles of mechanics are neither inductive generalisations nor synthetic a priori principles. However, they are general, exact and universally applicable. The problem, as Poincaré sees it, is that the principles of mechanics do not directly apply to the world: the *proper* object of the principles of mechanics is not the world at large, at least in the first instance, but various idealised entities. Hence, the principles of mechanics can neither be verified nor falsified by actual objects in the world simply because they do not apply *directly* to them. Seen in this light, the principles of mechanics are conventions in that whatever epistemic status they have within a certain theory or theoretical scheme is not a function of empirical evidence or (either analytic or synthetic) a priori reasons. The *second* source of physical conventionalism is related to an important theoretical fact, viz., that there are conceptually distinct theories of mechanics, that is at least three different ways to formulate the key principles of a mechanical framework. This is something Poincaré fully realised after reviewing Hertz's *The Principles of Mechanics* (1894) in 1897. Put together, the two sources imply that not only are the principles of mechanics *per se* neither verifiable nor falsifiable, but that there is ineliminable room for choice among distinct sets of such principles. Let us take these two sources in turn and examine their consequences in more detail.

3.1 The *principles* of mechanics

Thinking about the principles of mechanics, Poincaré argued that they are neither a priori demonstrable principles nor experientially determined generalisations. As he explained in detail in his address to the 1900 Paris International Congress of Philosophy with a paper titled "Sur les principes de la mécanique" the principles of mechanics are neither a priori truths nor empirical ones. Take for instance Newton's first law, i.e., the law of inertia. Its truth cannot possibly be demonstrated a priori. This law states that if a body is not acted upon by any external forces, its velocity remains unchanged. Yet, one can conceive of worlds in which if a body is not acted upon by any external forces, either its position or its acceleration — and not its velocity — remain unchanged. In such worlds, Newton's first law would not hold. Different laws, expressed in a different mathematical form, would have to be formulated. Hence, the truth of Newton's first law cannot be demonstrated by a priori reasoning (cf. 1901, 460-463). Are, then, the principles of mechanics empirical generalisations established (and hence accepted) on the basis of experience? The answer is negative since the systems to which the laws of mechanics apply, such as perfectly isolated systems or systems that pertain to absolute motions, were not to be found in nature. No-one can really verify these principles by recourse to experience. No experiential situation can afford us with perfectly isolated systems and the like (cf. 1901, 474). Besides, no experience can ever falsify a principle of mechanics, for two reasons. *First*, since the principles of mechanics apply to systems not encountered in experience, they can never be submitted to a rigorous and decisive test. *Second*, even if a mechanical principle could be submitted to a rigorous test, it could always be saved from refutation by some sort of corrective move. Suppose, for instance, that we found that natural systems did not obey the law of inertia. We could always attribute this deviation to the motion of hidden masses or molecules.

As in the case of geometry, this latter point was elevated by Poincaré to a principled way to divide the content of a theoretical claim into two components, one expressing a convention and another having some empirical import. To see this, let us take a look at the following example.

(1) 'The stars obey Newton's law of gravitation'.

One would expect that (1) is a testable hypothesis. But (1) can be decomposed into two others:

(2) 'Gravitation obeys Newton's law'; and

(3) 'The only force exerted on the stars is gravitation' (cf. 1902, 276).

Suppose, Poincaré says (1902, 276), that astronomers discovered that the stars do not exactly obey Newton's law of gravity. What could they do? There are two options available. First, they might say that the gravitational force does *not* vary exactly as the inverse square of the distance between the two stars. If one thought of Newton's law of gravity as an *empirical* claim (an inductively established generalisation, say), such an observation would falsify it. The second option, however, is for them to say that the force of gravity *does* vary exactly as the inverse square of the distance, but in this particular case—and other cases of similar deviations—gravitation is not the only force acting on the stars.

Accordingly, one is free to treat (2) as a *definition* that "escapes the verification of experience". (2) is no longer an empirical statement. Nor, however, is it a priori true. The peculiarity of (2) is that it is independent of experience in that experience can neither verify it not falsify it, and yet, it is not a principle of reason or a synthetic a priori truth, since it can be consistently denied without contradiction. Hence (2) has a peculiar epistemological status—and this is precisely what Poincaré captures by calling it convention or definition in disguise. Unlike (2), (3) is a substantive claim that can and should be tested ("it will be on the proposition (3) that this verification [of (1)] will be exercised"). So, it is because of the testability of (3) that (1) is rendered testable, as it ought.

Conventions, then, are principles which are detached from experience, even though, as Poincaré repeatedly stressed, they bear some relation to experience: it is experience that "suggests", or "serves the basis for", or "gives birth to" the principles of mechanics (1897, 237; 1900, 557; 1901, 351). This relation to experience, weak though it may be, explains why conventions are applicable to experience.[8] The applicability of the principles of mechanics to experience is based on the fact—and this is a *contingent* fact about the world—that the worldly objects are similar to the proper objects of the principles of mechanics. Though, for instance, there is not, in nature "any system perfectly isolated, perfectly removed from all external action" for the principle of inertia to hold of it, "there are systems almost isolated" for which the principle of inertia is a good approximation.

Seen in slightly different light, conventions are empirical principles which have been *elevated* to general, exact and unconditionally true principles; principles which are used to interpret experience and define the proper object of a scientific theory. Here is how he put it: "The principles are conventions and disguised definitions. Yet they are drawn from experimental laws; these laws have, so to speak, been exalted into principles to which our mind attributes an absolute value" (1902b, 153). This *exaltation* is based on a free, but not arbitrary, choice.

Note that so far there is no tension between calling a principle convention and taking it to be true. On the contrary, a principle of mechanics is both a convention (in that it is neither an empirical generalisation nor a priori justifiable) *and* true (in that it defines the proper object of the scientific theory). More specifically, though the truth of a principle is not factual, since the principle is not empirical, the principle is nonetheless true in that it is *constitutive* of a theoretical framework. It emerges that Poincaré's physical conventionalism rests on a very rich and complex sense of convention. Conventions are not true by sheer stipulation (since they make 'some kind of contact' with experience) and yet they are freely chosen by the scientists.[9]

Being conventions, principles are not subjected to experimental test. Then, are they immune to revision? By no means. Principles that are no longer convenient should be abandoned. Poincaré was quite firm in that no experiments can ever contradict a principle of mechanics. For no experiment can conclusively refute such a principle. Yet, experiments can *condemn* a principle of mechanics, or even a whole mechanical framework, in that persistent failure to account for new facts renders a particular principle or a whole framework no longer convenient (cf. 1900, 19).

3.2 *Which* principles of mechanics?

When Poincaré reviewed Hertz's book in 1897, it became clear to him that there are competing mechanical frameworks; that is networks of principles of mechanics that are

---

8 This line of thought is developed in Folina (2014).

9 In Psillos and Christopoulou (2009), we have defended the view that conventions can be seen as capturing some sense of relative a priori. We have also explored some difficulties with the way Poincaré splits the content of a theoretical propositions into two components, one conventional and another theoretical and suggseted that, by using Ramsey-sentences, Carnap found a way to improve on Poincaré's method of splitting. See also Stump (2003).

conceptually distinct, though empirically equivalent. Hertz distinguished between the classical system, based as it was on Newton's law, the then popular energetic system, which was based on the principle of conservation of energy, and his own new system of mechanics, which dispensed with forces altogether.

Poincaré agreed with Hertz that the "classical system" ought to be abandoned as a foundation for mechanics (cf. 1897, 239). The chief reason for this was that a good definition of force was impossible. An attempt to define *force* as the product of mass times the acceleration of a body would not do. For, even if a definition for acceleration were considered available, the definition of force would require defining mass, and this, Poincaré thought, was impossible. For one, a definition of mass would rely on Newton's third law of the equality of action and reaction, i.e., $\mathrm{m_A}\vec{\mathrm{a}}_\mathrm{A} = -\mathrm{m_B}\vec{\mathrm{a}}_\mathrm{B}$, where $\mathrm{m_A}$, $\mathrm{m_B}$ and $\vec{\mathrm{a}}_\mathrm{A}$, $\vec{\mathrm{a}}_\mathrm{B}$ respectively stand for the masses and the accelerations of two bodies A and B. This would imply that Newton's third law of motion would acquire the status of a *definition*—no longer amenable to empirical confirmation. To apply this definition to the situation at hand, we would require that bodies A and B are the only bodies present. But this is not right. For we know that bodies A and B are not the only two bodies in the universe and that they cannot be completely isolated from other bodies in the universe; hence, we have to take into account all other forces acting upon bodies A and B. If we were to assume that the force exerted by a body C on B does not affect the action of B on A, but gets linearly added to this action, then we could find a way to disentangle the action of body B on A from the actions of the rest of the bodies in the universe. But Poincaré thought that this assumption, which amounted to the hypothesis of central forces, was unwarranted and, actually, false—since, in the case of the masses of the planetary bodies what is actually measured was gravitational mass and *not* inertial mass. In light of this failure of the hypothesis of central forces, the definition of mass becomes all the more difficult. For now, the only meaning that could be given to Newton's first law is this: the movement of the centre of gravity of a system that is not acted upon by external forces will be rectilinear and uniform. Since, obviously, the position of the centre of gravity of a system depends on the values given to masses, it may seem easy to calculate the masses so that the movement of the centre of gravity of the system under consideration is rectilinear and uniform. However, since the only system that it is not acted upon by external forces is the universe as a whole, the previous procedure for defining mass can only be applied to the universe as a whole. For only *this* system is free from all external forces and therefore its centre of gravity moves in a uniform and rectilinear way. Yet, Poincaré concluded, it is evidently absurd to think that the motion of the centre of gravity of the universe can or will be ever known (cf. 1897, 234-236; also 1902b, 120-123). Consequently for Poincaré "it is impossible to form a satisfactory idea of force and mass" (1897, 236).

Like Hertz, Poincaré was more sympathetic to the "energetic system", which was based on the principle of conservation of energy and Hamilton's principle that regulates the temporal evolution of a system (cf. 1897, 239-240). The basic advantage of the energetic system was that in a number of well-defined cases, the principle of conservation of energy and the subsequent Lagrangian equations of motion could give a full description of the laws of motion of a system. These cases concern systems of conservative forces, that is systems where forces depend only on the relative positions and mutual distances of a certain number of material points, and are independent of their velocities. Then, one can define a potential energy-function U which depends only on the position of material points and is independent of their velocity. In these cases, the principle of conservation of energy takes the following definite form: there is a conserved quantity which is accessible to experience and is the sum of two terms, one being dependent only on the positions of material points (potential energy U), the other being proportional to the square of their velocities (kinetic energy T).

Poincaré suggested that the foregoing procedure gives an unambiguous definition of energy. *Energy*, is then, a constant quantity which can be decomposed into two terms T and U such that T is a homogeneous quadratic function of the velocities and U is a function of the positions only (cf. 1897, 240). If one also takes into account other forms of energy such as thermal, chemical or electric, what Poincaré defined as energy in general is nothing but a quantity that remains constant and is the sum of three terms potential energy U, kinetic energy T and internal energy Q such that U is independent of velocities, T is a homogeneous quadratic function of the velocities, and Q is only dependent on the internal state of the system (cf. 1897, 241; also 1902b, 142). One cannot therefore fail to notice that what determines a certain quantity as energy is, apart from its constancy, a particular structural feature that this quantity possesses, viz., that it can be decomposed into three terms, each of them being defined as above. The distinctive feature of what Poincaré called *energy* is precisely this decomposition. For this only distinguishes energy from other conservative quantities, e.g., any arbitrary function of T+U, or of T+U+Q.

However, this definition of energy shows the limitations of the energetic system. When among the several conservative quantities of a system, someone cannot single out one which can be decomposed into three separate functions U, T and Q defined as above, he has no clue at all as to which of these conservative quantities they may call energy. For Poincaré this eventuality showed that the energetic system ends up proclaiming a very general, and rather *empty*, principle of the form "There is something that remains constant" (1897, 242). As it stands, this principle is not informative. The very fact that the world is governed by laws entails that there are quantities that remain constant. Which of them is to be taken as energy?

So, the energetic system presented a definite advantage over the classical system. In moving from the latter to the former "one has realised progress", yet at the same time, Poincaré noted, "this progress is insufficient" (cf. ibid.).

Finally, there is Hertz's own system. Based on the claim that forces acting at-a-distance were inconsistent with Maxwell's electromagnetic theory, Hertz developed a system of mechanics founded solely on the concepts of space, time and mass. The fundamental law of this system was a kind of a generalised law of inertia combined with Gauss's Principle of the Least Constraint. In order, however, to reduce every natural motion to an instance of this fundamental law, as well as to account for possible deviations from this law, Hertz had to posit a multitude of hidden, or concealed, masses that were connected with the visible ones, thereby forming an articulated (or, connected) system. In Hertz's system, force and energy give way to the action of masses and motion, but not necessarily to observable masses and motion. Hertz thought that this is just as well, since both of the rival systems posit invisible entities, viz., *forces* and *energy*. He took it that his own system is preferable to its rivals since the invisible entities it posits—bodies with masses—are of the same nature as the visible ones. Actually, as Poincaré (1897, 249) noted, due to a theorem proved by the French mathematician Gabriel Königs, any material system subject to forces can be re-interpreted as an articulated system of (visible and invisible) masses, subject to rigid connections but not forces. This new Hertzian system was more complicated than the classical one, but still possible and equivalent to it.

3.3 From conventions to hypotheses

In the course of his study in the foundations of mechanics Poincaré made two important observations. The *first* is that the principles of mechanics, far from being experimental or inductively established truths, are definitions. They are being used as definitions within the theory and constitute the proper object of the theory: they enforce the assumptions that are necessary for the theory to be rigorous and exact. But they are not devoid of all empirical content since the worldly objects are *similar*, to a good approximation, to the objects of mechanics. The *second* observation is that the proper analysis of the structure of the theories of mechanics should distinguish clearly between the definitions, the mathematical theorems and the experimental truths. In other words, the proper philosophical analysis of theories should delineate the proper object of theories and how this object is related to the empirical world. For Poincaré, Hertz's own system of mechanics complicated matters further by adding an extra element to the foundations of mechanics, viz., the *hypothesis*; and in particular the hypothesis of invisible masses and their rigid connections.

The very possibility of alternative systems of mechanics suggests that the principles of mechanics are not a priori true. Not only are there alternative systems, but they can be used as a basis for building the rest of science onto them. Hence, the principles of mechanics are not synthetic a priori principles. They are not conditions for the possibility of experience. And yet, since they are not experimental or inductive truths either, what is their status? Poincaré emphatically wants to block the view that the principles of mechanics are *hypotheses*. He too Hertz's own view to lean towards this claim, but he thought that treating the principles of mechanics as *hypotheses* is wrong because it fails to explain the quasi-foundational status of the principles and in particular that they are "postulates applicable to the totality of the universe and regarded as rigorously true".

There is no doubt that Poincaré took very seriously the fact that hypotheses are indispensable in science. Actually, he devoted much of his 1900 address to the Paris International Congress of Physics, titled "Relations entre la physique expérimentale et de la physique mathématique" to explaining the role of hypotheses in physics. "Every generalisation", he says, "is a hypothesis; the hypothesis has then a necessary role that no one has ever contested" (1900, 8). He did claim that hypotheses should be submitted to verification and that if they do not stand the test, they should be "thrown aside without regret". This is clearly *not* the fate of the principles of mechanics—they are not verifiable or falsifiable, though there are circumstances under which they are 'condemned' and abandoned.

Poincaré famously went on to distinguish between three types of hypotheses. The first type included all those assumptions that make mathematical physics possible, e.g., certain idealisations and simplifications, principles of symmetry and the like. The second type—which Poincaré called 'indifferent'—has a similarity with the first in that it includes assumptions that ease calculations. But they are significantly different from the first type in that the same results—or predictions—can also be achieved, perhaps in a more cumbersome fashion, by their negations. Poincaré used the atomic conception of matter as an example of an indifferent hypothesis. The core of the atomic conception of matter was that matter was discontinuous, that is that it had a granular structure: it consisted of a huge—but not infinite—number of particles in constant motion subject to mechanical laws. So the rival hypothesis—indifferent too—was that matter was continuous. The third kind of hypothesis is what he called "true generalisations" (1900, 10), which are subject to experimental confirmation or disconfirmation. This latter category includes the bulk of hypotheses in physics, including Maxwell's laws.

There is an obvious reason why Poincaré intended to *limit* physical conventionalism to some principles and not all of them—that is, to some but not to all putative laws that could be elevated to principles. Science is by and large an empirical enterprise and this character would be totally lost is conventions ruled everywhere in science. As we shall see later, philosophers such as Le Roy interpreted Poincaré as being an extreme conventionalist and Poincaré made a special effort to warn off such interpretations of his views.

3.4 Why the principles of *mechanics*?

This has to do with elements of Kantianism in Poincaré's conception of science.[10] Poincaré substantially transformed the Kantian idea that there is a hierarchical order among the various sciences, according to which the very possibility of empirical and testable physical science requires that there are in place the axioms of Euclidean geometry and the principles of Newtonian mechanics. But he did not deny a kernel of truth in this idea, viz., that the very possibility of empirical and testable physical science requires that there is in place some spatio-temporal framework and some general principles of motion. The chief difference with Kant is that the required spatio-temporal framework and the general principles of motion was not uniquely given as principles of the understanding and forms of pure intuition, but freely chosen among competing frameworks. Mechanics does *not* require Euclidean geometry for its formulation; as he put it: "mechanical facts might be enunciated with reference to a non-Euclidean space which would be a guide less convenient than, but just as legitimate as, our ordinary space; the enunciation would thus become much more complicated, but it would remain possible" (1901, 498). Electromagnetic facts do *not* require Newtonian mechanics for their enunciation either. In fact, it might be more natural to try to capture electromagnetic facts with in an energetic framework, based on the principle of conservation of energy.[11]

So there is a considerable degree of *flexibility* in choosing spatio(-temporal) and mechanical frameworks for doing science—but a choice has to be made! The empirical models of the world—those that are licensed by ordinary inductive methods, hypotheses, and experimental tests—should be constrained from above by (freely chosen and hence conventional) frameworks of principles of geometry and mechanics. These are universally applicable, certain, and exact principles who define the *proper* object of science. But the object of knowledge of science is, ultimately, the world and hence a story must be told as to how the object of science, as defined by the spatio-temporal and mechanical framework, is related to the world. And this is where relationism becomes pertinent, as we shall see in section 6.

There is, however, an inelastic constraint on scientific theorising—and hence on all empirical models of the world as well as on the spatio-temporal-mechanical frameworks. This is *pure mathematics*. Poincaré was adamant that (only) the principles of arithmetic are synthetic a priori, mainly because he thought that the principle of mathematical induction requires synthetic a priori intuition. Synthetic a priori principles are "imposed on us with such a force that we could not conceive of the contrary proposition, nor could we built upon it a theoretical edifice" (1892, 406). The last requirement is very interesting, since it suggests that synthetic a priori principles are constitutive of our form of *understanding*. Conventions, on the other hand, are such that they can be meaningfully negated and alternative theoretical structures can be built on their negations.

4. Geometrical conventionalism vs physical conventionalism?

---

[10] For some detailed analysis of this, see Friedman (1999, chapter 4).

[11] Moreover, as Poincaré himself proved, a necessary and sufficient condition for a complete mechanical explanation of a set of phenomena is that there are suitable experimental quantities that can be identified as the kinetic and the potential energy such that they satisfy the principle of conservation of energy. Given that such energy functions can be specified, there will be *some* configuration of matter in motion (that is, a configuration of particles with certain positions and momenta) that can underpin (or model) a set of phenomena. As he put it:

> In order to demonstrate the possibility of a mechanical explanation of electricity, we do not have to preoccupy ourselves with finding this explanation itself; it is sufficient to know the expressions of the two functions T and U which are the two parts of energy, to form with these two functions the equations of Lagrange and, afterwards, to compare these equations with the experimental laws (1890/1901, viii).

For more on this see my (1995).

When Poincaré describes the relation between conventions in geometry and conventions in mechanics, he seems to waver a bit. He offers his account in *La Science et l' Hypothése* in 1902, in a section of general conclusions which follows his chapters on geometry and mechanics. And though these chapters were simply the papers he published elsewhere in the 1890s, the section with the general conclusions is specially written for his book. The wavering has to do with Poincaré's conviction that mechanics, unlike geometry, should *still* be treated (and taught) as an experimental science. Geometry is formal and abstract—essentially a mathematical science. Mechanics is not. But this seems to be in tension with his systematic unravelling of the role of conventions in mechanics as well as in geometry. Are they different types of convention after all? Here is how Poincaré (1902b, 151) describes the difference:

> This convention [the principle of mechanics], however, is not absolutely arbitrary; it does not spring from our caprice; we adopt it because certain experiments have shown us that it would be convenient. Thus is explained how experiment could make the principles of mechanics, and yet why it cannot overturn them. Compare with geometry: The fundamental propositions of geometry, as for instance Euclid's postulate, are nothing more than conventions, and it is just as unreasonable to inquire whether they are true or false as to ask whether the metric system is true or false.

At first glance, it seems that geometrical conventions are *nothing but* conventions, while mechanical conventions are *not* nothing but conventions. And to some extent this is true. For as Poincaré goes on to explain there is a difference in the proper objects of geometry and of mechanics. Geometry has nothing to do with ordinary physical objects—its objects are ideal and abstract entities. And yet, when we choose one geometry as the most convenient for reasoning about spatial relations among physical objects (and for doing mechanics and science) our choice is guided (but not dictated) by facts about ordinary physical objects. In mechanics, on the other hand, there is no such a huge gulf. The objects of mechanics are, *ultimately*, ordinary physical objects and the experiments that guide the choice of the most convenient mechanical framework are experiments on ordinary physical objects. Here is how Poincaré (1902b, 152) puts it:

> The experiments which have led us to adopt as more convenient the fundamental conventions of geometry bear on objects which have nothing in common with those geometry studies; they bear on the properties of solid bodies, on the rectilinear propagation of light. They are experiments of mechanics, experiments of optics; they can not in any way be regarded as experiments of geometry. (...) On the contrary, the fundamental conventions of mechanics, and the experiments which prove to us that they are convenient, bear on exactly the same objects or on analogous objects. The conventional and general principles are the natural and direct generalization of the experimental and particular principles.

But in the end of the day, the difference is not between two types of convention but, I think, with two different ways to arrive at them. In mechanics, the process is, by and large, bottom up, while in geometry the process is top down. In mechanics, we start with experiments and experience which guide but not dictate the formation of the principles of mechanics—the experimental laws that are at the basis of the principles of mechanics are "elevated", as we have seen, into conventions or definitions in disguise. In geometry, we start from the axiom of free mobility and a notion of a group of transformations which apply to *displacements* (that is to geometrical objects) and then 'connect' them with ordinary objects (solid bodies) which satisfy the operations of the chosen group approximately. To put it bluntly, in mechanics, matter is prior to form, whereas in geometry form is prior to matter.

But the product of these two distinct processes, the conventions, is the same—it is a new epistemic category which is *distinct* from the three Kantian epistemic categories.

Conventions, both in mechanics and geometry, constitute the objects of science and constrain from above proper empirical scientific inquiry. Conventions are neither verifiable; nor falsifiable. They are freely chosen but the choice can be more or less commodious. So, one category—conventions—but two ways to get to it.

5. All conventions or no conventions?

5.1 Federico Enriques

One important criticism of Poincaré's conventionalism came from the Italian mathematician Federico Enriques, who in 1906 published an important book, titled *Problemi della scienza*, and was soon translated into French and English. In effect, Enriques challenged the need for a new epistemic category alongside the three Kantian ones and noted that, in a certain sense, conventions are the Kantian a priori principles 'born again' in a new form (1914, 165): they are "the canons whereby experience is interpreted". His diagnosis why this is so bears some examination.

The *first* reason Enriques cites is related to what he calls 'the vicious circle of science'. He rightly points out that the scientific representation of reality relies on two kinds of hypotheses or assumptions. There are the explicit hypotheses (the postulates of a theory) which state the basic principles of the theory. But there are also the *implicit* hypotheses, which are typically not stated within the theory and are not an explicit part of it, but are presupposed for the formulation—mostly the mathematical formulation—of the principles of the theory. Implicit hypotheses involve various idealisations and simplifications which make possible the exact formulation of the principles of the theory as well as various assumptions about the object of knowledge of scientific theory, viz., the object to which the principles apply (cf. 1914, 149). The implicit hypotheses, as he put it, "give us some knowledge beforehand which limits the field of *experience* and enables us to interpret it". When a theory is subjected to empirical test, Enriques says, the implicit hypotheses are not under test for they make the testing of the theory possible. It is only the explicit hypotheses—the principles of the theory—that are being tested. But the implicit hypotheses themselves are not arbitrary; in fact, they are employed because it is thought that they have already been subjected to test and verified by "means of other theories and explicit hypotheses, and are corrected when needful by a wider comparison with the knowledge already acquired" (1914, 165). Hence, though it is not the case that implicit hypotheses are under test when the theory is put to the test, they are not tested *then*, but they are presupposed as already being tested and perhaps corrected. It follows that "the progress of science is dependent upon science itself" and this is what he calls the *vicious circle of science*. It is precisely in order to avoid this kind of vicious circle that Kant, according to Enriques, introduced the synthetic a priori principles, which are independent of experience and absolutely presupposed by it. And it is for the same reason that post-Kantian French conventionalists introduced *pure conventions*. But, he says metaphorically, instead of introducing gold as the one and only standard of value, they took science to be in a state of "multi-metalism", since there is now a choice of standards of value.

Enriques thinks that this is not the right way to address the problem of the alleged vicious circle and that the circle evaporates when we think of explicit *as well as* implicit hypotheses as "assumptions gradually becoming determinate rather than as conventions" (1914, 165). In fact, as he explains elsewhere, if conventions are introduced into science, it is not clear why all postulates might not be transformed to definitions, thereby losing all contact with reality (1914, 251). As we saw it is precisely this 'overspilling' of conventions that Poincaré wanted to avoid.

The *second* of Enriques critiques of conventionalism is that the need to introduce a priori principles or conventions rests on a "fundamental illusion which comes from regarding our knowledge solely in its *completed*, that is in its *actual* aspect, without taking account of its genesis" (1914, 180). He explains that both Kant and Poincaré, in different but related ways, share the thought that because geometry precedes mechanics and physics and because geometry is used in the construction of the principles mechanics and mechanics grounds the various principles of physics, there is a relation of necessary dependence among them; a relation which "finds expression in Kant's *a priori*" and in Poincaré's conventions.

We have already seen that Poincaré did adopt this hierarchical view of science and that he did think that there is a strict order of dependence in science: from geometry, via mechanics to physics. But knowing the existence of alternative geometries and mechanics, he thought—unlike Kant—that this order of dependence does not determine uniquely the required principles of geometry and of mechanics. Enriques's reaction to this claim is, we might say, naturalistic: scientific knowledge is a dynamic process of mutual adjustment, where experience is indeed interpreted by means of previous knowledge. And our knowledge of the world relies on "progressive correction" (1914, 28-29).

The idea that there is no need for a special epistemic category—convention—which acts as the 'born again' form of the Kantian a priori, in light of the multiplicity of geometrical and mechanical theories—get a real bite when Enriques criticises Poincaré's geometrical conventionalism. His key point is that Poincaré's geometrical conventionalism rests precisely on his hierarchical account of science and presupposes that geometry is distinct from physics. It is precisely because geometry is taken to vary independently of physics that Poincaré can treat geometry as resting on conventions which can he held upon come what may by making suitable adjustments in the physical theory. Making a point that presages Reichenbach and Einstein, Enriques notes that "geometry is part of physics" (1914, 174), a part "which has attained to a high degree of perfection by virtue of the simplicity, generality and relative independence of the relations included in it" (1914, 181). Discussing in detail Poincaré's story of the spherical possible world in which the temperature field makes all objects shrink uniformly when they move (see section 2.2 above), Enriques says that a temperature field acting on *all* bodies in a uniform way would have "a true geometrical character", since all bodies in the world would have the temperature that belongs to the place they are in; hence, in this world, it would no longer be possible to say that bodies expand with the increase of their temperature. The geometry in this world would be genuinely different from ours. Exactly the same is the case if we were to admit that light-rays did not travel in straight-lines. An attempt to make this a universal trait of the possible world Poincaré discusses would presupposes not just local refractions of light-rays by certain media, but a universal, all-pervading heterogeneous medium which affects all light-rays without affecting anything else. This model, Enriques says, amounts to "a geometrical world different from ours" (1914, 179).

The way Enriques developed the thought that Geometry is part of physics was to argue that given a theory of margins of errors in measurement, actual physical objects can be taken to satisfy the properties of geometrical objects within some relatively specified margin of error. But even if this was too quick, his main thought, viz., that Poincaré's conventionalism required that physical facts could vary totally independently of geometrical facts was to the point.

Enriques's overall reaction was that understanding science as a dynamic process of theoretical development and change did not require the elevation of some principles to *conventions*; it would suffice to take some principles as fixed when others are being tests in a process of mutual adjustment subject to empirical testing.

As noted already in the *Introduction*, the very same critique was briefly launched by Russell in his review of *Science and Hypothesis* in 1905. His point was that Poincaré's conventions could be simply seen as hypotheses "which are withdrawn from the region of experimental verification" (1905, 416). As such, they do not have a special epistemic status; rather, they are like any ordinary theoretical hypothesis, but simply regarded "beyond doubt" *relatively* to all other hypotheses of a theory, when the theory is put to the test. Russell does explore Poincaré's idea that some principles are "elevated" to conventions and notes that ascribing the status of a convention to a hypothesis is simply a measure of the *unwillingness* of the scientists to abandon it.

What is noteworthy is that both Enriques and Russell challenge Poincaré's hierarchical conception of science and transfer Poincaré's conventionalism in the context of theory-testing. It then becomes possible for them to argue that ascribing to a certain principle a *sui generis* epistemic status—convention—is not necessary for the explanation of the fact this principle is not put to the test when the theory of which it is part is being tested.

5.2 Édouard Le Roy

But if science rests, at least partly, on conventions, why not assume that *all* scientific assertions are conventions? This is a thought that found fertile ground in Paris around 1900 and, in a certain sense it received inspiration and some *prima facie* legitimacy from Poincaré's writings until the turn of the century. In this period in Paris, the so-called 'bankruptcy of science debate' was still raging. I am not going to discuss this extremely interesting debate in any detail here,[12] but the gist of the debate, if you like, was that science had failed to deliver on its supposed promise to render the world intelligible and to ground and explain the role of human beings in it. The debate erupted in the early 1880s and went through various phases but by 1900 a new philosophical critique of science started to become prominent, viz., that there are no standards of objectivity in science—all science, including all scientific 'facts' are free constructions of the human mind. This idea acquired currency because among other things it granted the mind the freedom to construct the world; this idea of freedom was supposed to leave room for religious belief. Though some of this criticism was developed—from a pragmatist point of view—by the philosopher Frédéric Rauh (1861-1909), who aimed to combat "the superstition of objectivity",[13] the main critic was Édouard Le Roy, a young mathematician and philosopher and protégé of Henri Bergson, who in 1899-1900 published a series of four controversial pieces titled '*Science et philosophie*'.

Le Roy embarked on a triple critique of science: facts, laws and theories. According to him, facts are not discovered, but they are constituted and created by the scientists in "the amorphous matter" (1899 II, 515). Nature forms a whole in which there are no isolated objects. There are no "intrinsically definable facts". The isolation of facts, the "cutting up" of the amorphous and continuous matter, is done by the mind and it is always perspectival—from a chosen in advance point of view (1899 II, 517). The scientific fact is constituted symbolically out of the facts of common sense by means of scientific concepts—without them the scientific fact does not exist. As he put it, "All facts are the result of a collaboration between nature and us; all facts are symbolic points of view adopted to look at the real" (1899 II, 518).

When it comes to laws, he claimed that they are mental constructs. They express relationships, in fact invariable relations, among scientific facts. "Constancy in the variation: that is the general formula of the laws" (1899 II, 518). But laws are not part of reality; they are "products of our ability to endlessly vary the angles at which we look at the constancy in the world". It is because scientists look for constancies in the phenomena that they devise laws. Their objectivity is "primarily the consequence of our discursive operations which they express". Laws do not reveal Nature: they are schematic summaries, convenient modes classification of scientific facts.

---

[12] There is separate paper in the pipeline for this. A talk on the 'bankruptchy of science' debate is at http://www.youtube.com/watch?v=zwEYZNKeCpQ

[13] For Rauh's crticism see Horner (1997).

In a similar fashion, theories do not aim to anticipate the unknown “except incidentally”; rather, they provide general representational schemes “that can be adapted to a class of laws”. A theory, Le Roy says, is to the law what a law is to the fact: a symbolic image (1899 II, 527).

Briefly put, Le Roy says that when we do science we “are immersed in an ocean of images that are by definition what we call reality”. Science is a scheme which imposes order to this “huge mass” (1899 I, 381). It is a gradual rationalization of reality. For him “the constant preoccupation with assuring the rigor and the objectivity of knowledge has too often hidden from view the immense role played in the experimental determination of the truth by the free activity of the mind, and, as a consequence, it has generally hindered a clear understanding of the contingence and the relativity of the sciences of Nature.”(1899 II, 513-514)

What is important for our purposes is that this extreme constructivist account of science found its inspiration in Poincaré’s writing. Le Roy was quite explicit that the view he was canvassing—the *critique* of science—was based on Poincaré’s views on geometry and physics; views which he tried to summarise (cf.1899 II, 535 ff). Motivating his own views in the first instalment of his article, Le Roy noted that Poincaré’s articles on *Space and Geometry* in 1895 inspired his views. He took Poincaré’s main message to be that understanding the world requires the choice of “a general scheme of representation” which imposes a “form” on the sensible world, where this scheme draws its rational value from the symbolic powers of our mind. Le Roy concluded that in the basis of scientific theories are “an enormous mass of conventions and decrees”, which are not necessary and whose “true nature” lies in a “clever clever compromise between the world and us”. Science has as its mission to fabricate truth, as he put it (1899 II, 559).

But, one may wonder, is there any contact between science and the world according to this view? In his piece ‘Un positivism nouveau’, which appeared in 1901, Le Roy suggested that scientific assertions can be considered “as practical recipes for getting some useful results” and that this is the strongest positive result concerning science: *science is a rule for action*. What is also worth stressing is that for Le Roy it is precisely this conventional and practical character of science that is underwritten by human freedom and makes science compatible with freedom.

As we are about to see, it was precisely this ‘extreme nominalism’ that made Poincaré mitigate his conventionalism by trying to strike a balance between the freedom of conventions and the constraints that nature puts on them. With an explicit reference to Le Roy, he noted in the Introduction to *La Science et l’ Hypothése*, which appeared in 1902:

> Some people have been struck by this character of free convention recognizable in certain fundamental principles of the sciences. They have wished to generalize beyond measure, and, at the same time, they have forgotten that liberty is not license. Thus they have reached what is called nominalism, and have asked themselves if the savant is not the dupe of his own definitions and if the world he thinks he discovers is not simply created by his own caprice. Under these conditions science would be certain, but deprived of significance (1902b, 25).

Poincaré’s reaction was to stress that science does *tell* us something about reality—but this something is the “relations among things” and not the things themselves. Incidentally, he did agree with Le Roy on the point of the unknowability of the things-in-themselves. For Le Roy too the “scientific noumenon” is scientifically impossible to represent”. But while Le Roy thought that the scientific phenomenon is constructed, Poincaré thought that it is not. As we shall see, his relationism was, to a good measure and attempt to block extreme conventionalism. In fact, most of the argument for relationism was built in Poincaré’s lengthy reply to Le Roy titled 'Sur la valuer objective de la science', published in *Revue de Metaphysique et de Morale* in 1902.[14]

6. The case for relationism

Poincaré rarely talks about structure, but he frequently talks about *form*. Mathematical physics makes essential use of mathematics and mathematics is about form. The key methodological problem that Poincaré faced was how the rather abstract and formal structure of mathematical physics is connected with experience; how it acquires empirical content. We have already explored his claim that *part* of the content of scientific theories is conventional, meaning that it defines the objects of knowledge of theories, which is not, in the first instance the empirical world, but rather the idealised entities that satisfy the principles of geometry and mechanics. But scientific theories should be, and are, about the empirical world; hence, their content, ultimately, should be the world. This creates a problem: how can it be that the object of knowledge of scientific theories is not, in the first instance, the empirical world, but it is, in the last instance, the empirical world? It is in order to address *this* problem that Poincaré takes a relationist view of scientific representation and knowledge. But it turns out that relationism can help him block the charge that scientific theories are like simply networks of conventions resembling Penelope's web.

In this section, we will explore three important (and related) motivations for his relationism. The *first* stems from the very nature of mathematical representation in science; the *second* comes from the need to ground objectivity in science; and the *third* is related to lessons drawn from the history of science.

6.1 Mathematical representation

For Poincaré, relationism is motivated by the way mathematics represents its objects. In fact, it does not care about its objects; it cares only about the relations they have to each other. Poincaré could not be clearer: "Mathematicians study not objects, but relations between objects; the replacement of these objects by others is therefore indifferent to them, provided the relations do not change. The matter is for them unimportant, the form alone interests them." And elsewhere: "It is the mathematical spirit, which disdains matter to cling only to pure form". Mathematics represents up to isomorphism. This makes mathematics, among other things, useful in representing diverse physical processes or phenomena "which have no physical relation either apparent or real" and yet equations of the same form characterise them (1897, 108). Such an example is Laplace's equation, which "is met in the theory of Newtonian attraction, in that of the motion of liquids, in that of electric potential, in that of magnetism, in that of the propagation of heat and in still many others" (ibid.).

We have already seen how this works in the case for geometry, *qua* mathematical science. Geometry represents up to isomorphism and precisely because of this it can be made to represent relations among non-geometrical objects too, such as the relations among actual solid bodies (subject to conditions and idealisations noted above). Here is how Poincaré put it: "We now know that in a group the matter is of little interest, the form alone counts, and that when we know a group we thus know all the isomorphic groups". But the same holds for analysis too. Mathematical physics represents physical processes and phenomena by equations and equations express *relations* among magnitudes.

Now, from this it does not follow that we cannot know the *relata* that are related by the relevant relations. But Poincaré had independent reasons to deny that this knowledge is possible; or that it is required for an adequate understanding of how scientific theories represent the world. For scientific theories do not aim to describe how things *really* are but

[14] Part of the aim of this paper was to rebut Le Roy's claim that scientific facts are 'fabricated'.

to co-ordinate various empirical laws which are discovered experimentally and this co-ordination is best achieved by specifying the *mathematical form* of these laws—that is, the mathematical equation which best expresses the relations that the experiment has revealed—and by uniting these mathematical forms into a single mathematical framework. Note that the mathematical equations that capture—in the most convenient way—the phenomenon under study capture, by the very nature of mathematical representation, the *form* of this phenomenon. Even this is, actually, an idealisation. For the mathematical equation describes, strictly speaking, not the form of the physical phenomenon itself directly, but the form of an idealised analogue of this phenomenon—one that is amenable to a formal mathematical description. In other words, the mathematical representation of physical phenomena relies indispensably on idealisations and abstractions. Here is an example discussed in some detail by Poincaré (1900, 11).

> If we wished to study in all its complexity the distribution of temperature in a cooling solid, we could never succeed in that. Everything is simplified, if one reflects on that a point in the solid cannot directly impart heat to a distant point; it will immediately impart that heat only to the nearest points, and it is but gradually that the flow of heat will reach other portions of the solid. The elementary phenomenon is the exchange of heat between two contiguous points. It is strictly localised and relatively simple if we admit, as is natural, that it is not influenced by the temperature of the molecules whose distance is sensible.

Clearly, the 'elementary phenomenon' i.e., the exchange of heat between two contiguous points, is an idealisation, which, however, is such that "the knowledge of the elementary fact permits us to state the problem in an equation" (1900, 13).

In light of this, it is no surprise that Poincaré (1889, iii; 1902b, 215) says emphatically that

> [t]he 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. It matters little whether the ether really exists; that is the affair of metaphysicians. The essential thing for us is that everything happens as if it existed, and that this hypothesis is convenient for the explanation of phenomena. After all, have we any other reason to believe in the existence of material objects? That, too, is only a convenient hypothesis; only this will never cease to be so, whereas, no doubt, some day the ether will be thrown aside as useless. But even at that day, the laws of optics and the equations which translate them analytically will remain true, at least as a first approximation. It will always be useful, then, to study a doctrine that unites all these equations.

We can therefore know only the relations 'among things' because all we can represent mathematically in the sciences are these relations. Even then, we know, strictly speaking, 'the elementary phenomenon'—which is a simplified and idealised analogue of the complex natural phenomenon—because only *this* phenomenon is amenable to a mathematical description.

6.2 The things themselves

In the Introduction to *La Science et l'Hypothése*, where he presented in summary form the basic tenets of his research in the 1890s, Poincaré gave this point a more radical twist by claiming that "the things themselves are not what it [science] can reach, as the naive dogmatists think, but only the relations between things. Outside of these relations there is no knowable reality" (1902b, 25). Poincaré does not deny that there is reality outside relations; he denies that this reality is *knowable*.

Note that Poincaré does not use the expression 'things in themselves' (*choses en soi*) but the expression 'things themselves' (*chose elles-memes*). Elsewhere he talks about the "nature of things" or "real objects". It is plausible to think that Poincaré's motivation is Kantian nonetheless. He might not have taken on board the full Kantian distinction between the noumena and the phenomena, but it is quite clear that he wanted to draw a distinction between how things are—what their *nature* is—and how they are related to each other. A plausible way to draw this distinction is to differentiate between the intrinsic and perhaps fully qualitative properties of things—what he plausibly calls 'nature' of things—and their relations. The former are unknowable, whereas the latter are knowable. Though, to the best of my knowledge, there is no detailed discussion of these issues in Poincaré's writings, *if* he did mean to draw this kind of distinction, then he would be a pioneer of the view that came to be known as "Kantian Humility", viz., the view (attributed to Kant himself by Rae Langton 1998) that "we have no knowledge of the intrinsic properties of substances" (1998, 21). This kind of Humility would presuppose that all knowledge of a thing is via and through its relations (to other things), and of course, *this* kind of view is clearly Poincaré's, if only because, as we have seen, all knowledge in mathematical physics is mediated by mathematical representations. But, as Langton (1998, 12) has claimed for Kant, this kind of Humility would also presuppose that the relations of things are irreducible to the intrinsic properties of things.[15] And it is hard to say that this was Poincaré's view.[16] What he says quite clearly is that "not only science cannot teach us the nature of things but nothing is capable of teaching it to us" (1902, 291). And he adds meaningfully: "if any god knew it, he could not find words to express it". I take this to mean that the knowledge of the nature of things would require intellectual intuition, which we humans lack.

6.2.1 Boutroux's relationism

Now, it is prima facie plausible to argue that the origin of this Poincaréan view about relations is in Émile Boutroux, who was a famous neo-Kantian philosopher and Poincaré's

[15] Langton argues at lenght that Kant adopted the view that "the relations and relational properties of substances are not reducible to the intrinsic properties of substances" (1998, 109). Intrinsic properties are those that a thing can have on its own, that is, even if it were a lone object in the universe. Relational properties, on the other hand, imply "accompaniment", that is the existece of something other than the bearer of the relational property. James van Cleve (1999, 150ff) offers an excellent discussion of the claim that Kant took things-in-themselves to be things-as-they-are-intrinsically. But his considered judgement (1999, 155) is against this interpretation.

[16] I do not know, nor can I find in his writings, what Poincaré thought about relations. At about the same time as he was developing his relationism, Russell had been working on the nature of relations. There is ample evidence that Poincaré knew Russell's work on logic and the foundations of mathematics, as he engaged critically with Russell's logicism and defended a kind of neo-Kantian philosophy of arithmetic (cf. 1905b). But I cannot tell whether he paid any attention to Russell's theory of *relations*. The fact is that Russell did develop an account of relations that *could* underscore the irreducibility of relations and ground Poincaré's Humility. In (1899a) and then again in (1903), Russell famously argued that asymmetrical relations cannot consistently be taken to be reducible to properties of their relata. That is, no asymmetric relation *aRb* is equivalent to two relational properties *a(Rb)* and *b(Ra)* attributed to its relata *a* and *b* respectively. He also argued that no asymmetric relation *aRb* is equivalent to a property of the *whole (ab)*. (This last point was taken to decisively refute Bradley's view that all relations are internal.) As Russell noted, a view that takes an asymmetric relation, such as *a is greater than b* to be equivalent to a claim about *both a* and *b* taken together fails to explain why it is *a* that is greater than *b* and not *b* greater than *a*. Russell concluded that at least some relations (he actually thought that *all* relations) should be external, meaning irreducible to the properties of their relata. Now, asymmetric (and transitive) relations are vital in science and mathematics as they capture the content of key relations, such as of order, temporal, quantitative and causal. Given a Russellian account of relations, it is no wonder that the relata can be unknowable even if all relations are known. But whether or not this view was actually shared by Poincaré is only a matter of (plausible) speculation.

brother-in-law. Boutroux delivered a series of lectures at the Sorbonne in the early 1890s pursuing a relational approach to reality.[17] On Boutroux's view, science does not aim to offer "knowledge of the intrinsic nature of things", but instead to extract "from the given reality, sensibly constant rapports". It then "declares that such a rapport is explained, when it has been possible to reduce it to some other rapport already known and recognized as permanent and general" (1914, 4-5). Science, for Boutroux, is essentially mathematical and the scientific explanation of the universe would be "a certain formula, one and eternal, regarded as the equivalent of the entire diversity and movement of things". But for Boutroux, precisely because science can only reveal "abstract relations" whose relata "are themselves relations, science remains ultimately *disconnected* from the world of concrete relations, whose relata "are real subjects, true beings". Hence, there is a limit to the intelligibility of the world as offered by science, though the "scientific form of intelligibility" is the most objective form. The problem as Boutroux sees it is that, science becomes objective by "setting aside individuals, natural beings", whereas he thinks that there is also need to understand concrete relations among concrete entities, which cannot "be reduced to mathematical relations". Boutroux does not deny that things have a substantial nature. But this cannot be known: "We are foiled when we try to determine the substantial nature of things; all the same, we cannot abolish them". (1914, 67)

Boutroux had started to develop these views already from *De la contingence des lois de la nature*, which appeared and 1874 and made him quickly famous. The key theme of the book was that there should be room for human freedom in the world of science; and the means by which he chose to make such room was to claim that laws of nature are not necessary but, ultimately, products of human freedom. In the course of defending this view, Boutroux took it that science is unable to reveal "the inmost nature of being" and that it has "for its object a purely abstract and exterior form" (1920, 27). Besides he took it that science succeeds in offering exact definitions—meaning: principles and laws—only because it deals with abstractions. Hence, "we cannot apply to things themselves the determination inherent in the definitions of the deductive sciences" (1920, 153).

In his aforementioned Sorbonne lectures, Boutroux took the view he was opposing to be that "laws of nature may be compared to laws proclaimed by a legislator and imposed a priori upon reality". This is emphatically a view that Poincaré did *not* accept. Actually, Poincaré's own appeal to the conventional character of the fundamental spatio-temporal-mechanical framework for doing science might be taken to highlight precisely the role of human freedom; freedom in choosing the fundamental principles of this framework. But unlike Boutroux, Poincaré also stressed that this choice, though free, is not arbitrary, but guided by experience. Hence, though both Boutroux and Poincaré subscribed to a relationist understanding of representation in science, Poincaré thought that the scientific relations do, in some way, get in touch with the world of experience even if they (can) tell us nothing about things themselves.

Interestingly, Boutroux thought he was finding an ally in Poincaré's conventionalism since he took it that it emphasised the role of the mind in making scientific knowledge of the world possible in that it is ultimately the *mind* that chooses the theoretical framework through which experience is interpreted. In his Gifford lectures in 1903-05 at the University of St Andrews, Boutroux stressed that "A systematic intervention of the mind can alone explain the transmutation that science makes experience undergo" and having also stressed that the mind is free to choose frameworks which "are different and even contradictory in their fundamental hypotheses", he credits this point (in a footnote) to Poincaré (1911, 245).

[17] The lectures were published under the title 'De l'idée de loi naturelle dans la science et la philosophie contemporaines' in 1895. As mary Jo Nye (1979) has documented, Poincaré was a member of the Boutroux Circle and was heavily influenced by his philosophy. For a detailed account of Boutroux's philosophy of nature, see Capeilleres (2009).

Still, for Boutroux—and for Poincaré too—scientific theories cannot 'penetrate', as it were, through the veil of relations. Boutroux makes exactly this point when he says that "Science no longer expects to endow the mind with a close copy of external things, which apparently, just as we suppose them, do not exist. She discovers relations that experience verifies through the senses. It is enough that she may and must be called true, in the human meaning of the word" (1911, 246).

As noted already, conventionalism was widely taken to be an answer to the issue of how the world of science can be compatible with human freedom. But there were softer and harder versions of conventionalism. The key thought, as it were, of "the representatives of a philosophy widely circulated in recent years", as Boutroux put it, was that "the postulates, principles and definitions of science are mere agreements, the outcome of an arbitrary Choice" (1911, 282-3), which, though "occasioned, suggested perhaps by experience they neither are nor can be prescribed by it". Poincaré was lumped together with Pierre Duhem, Le Roy and Gaston Milhaud, by Boutroux, as part of this movement. But in other writings (cf. 1908, 701), Boutroux was more careful to distinguish between Poincaré and Le Roy. As Boutroux saw the difference, Poincaré was giving freedom to the human mind to consider "the most general propositions of any real science" as conventions and to make them an integral part of science, but Le Roy was moving even further by taking it to be the case that "not only theories and laws, but the scientific facts themselves are shaped and manufactured by the human mind": science "in an invention of the mind". Boutroux claimed that the opponents of this conventionalist movement are scientists such as Paul Painlevé and Jean Perrin who do not grant the human mind the power to find any truth of the world "apart from the established principles of science" (1908, 702).

6.3 The objectivity of relations

Though Poincaré based his theory of geometrical space on the sensible space, he took it that "sensations cannot give us the notion on space", since "by themselves they have no spatial character" (1898, 1). Sensations have an irreducible qualitative character and they differ from one another qualitatively. Yet, though none of our sensations, taken in isolation, would have led us to the idea of space, Poincaré argued (1895, 636) that we are led to the idea of space "solely by studying the laws, according to which these sensations succeed each other". In other words, it is the *relations among sensations* that are at the basis of our notion of representative space.

In his reply to Le Roy in 1902, Poincaré used this kind of thought to build an account of the objectivity of relations. Sensations, he noted, are private and inaccessible from a third-person point of view (1902, 288). They do not have precise criteria of identity. There is no way to say whether sensation A had by S is the same as sensation B had by S' even if they look at similar or exactly the same objects in exactly the same circumstances. Suppose that S sees a red poppy and a red cherry and has a sensation A and S' sees a red poppy and a red cherry and has a sensation B. It is possible that the sensation A in S is exactly the same as the sensation S' has when he sees a green leaf and conversely. Poincaré thinks there is no way to tell whether A is identical to B. However both S and S' see a difference between red cherries and red poppies on the one hand and green leaves on the other, even if the sensations that 'correspond' to these objects are ineffable. This leads Poincaré to argue that "all that is objective is devoid of all quality and is only pure relation (1902, 289).

What makes qualities subjective is that they are "intransmissible"; that is, unshareable by different subjects. What is objective, Poincaré says "must be common to many minds and consequently transmissible from one to the other" (1902, 288); hence objectivity requires communication. His argument may be reconstructed as follows:

Whatever is objective must be common to many subjects (minds).

Hence, whatever is objective must be transmissible from one subject to another.
If something is transmissible, then it is communicable and intelligible.
Sensations and qualities (whatever is purely qualitative) are *not* 'communicable'.
Therefore, sensations and qualities (whatever is purely qualitative) are *not* objective.

Hence, objectivity lies in relations. Why? If everything is such that it has qualities and it is related to other things, and since qualities are not objective, only relations will be objective, if objectivity is possible at all.

Poincaré realized that this position could be pushed to its extremes if relations are taken to be purely quantitative and formal. But if this road were taken, "someone could have been carried away into saying that the world is only a differential equation" (1902, 289). So relations are objective but not all objective relations are purely formal and quantitative; non-formal (which I take to mean 'interpreted' and not *purely* mathematical) relations can be objective too. In other words, it is transmissibility that is required for objectivity; not formality.

Even if we accept that *only* relations are objective, it does not follow that *all* relations are objective. Actually, it does not even follow that all *transmissible* relations are objective. Le Roy could accept that some relations are transmissible, but he would still question whether they tell us anything at all, even in principle, about the world. Poincaré was well aware of this. He wanted to separate objectivity (or reality, as he put it) from dreams *as well as* from the fiction (1902, 290). The transmissiblity (or inter-subjectivity) condition separates objectivity from dreams, but how about fiction? Here is where he takes it that the relations that are transmitted should reflect (or correspond to) *real relations* among things (that is, among their unknown qualities). So objectivity lies in transmissible relations which correspond to real relations among things in the world (or among qualities in general).

Poincaré's relationism has led him to the thought science is "a system of relations". His new take on objectivity suggests to him that the objectivity of this system of relations requires that two conditions are met. First, the relations (as expressed in theories) are transmissible; that is, they survive theory-change. Let's call this *Invariance*. Second, the transmissible relations (the invariant elements of theory-change) should reflect real relations among things. Let's call this, perhaps not using the exactly right word, *Correspondence*.

Before we say a bit more about these conditions, let us note that Poincaré wants to block the view that objectivity requires more than relations (subject to the two conditions mentioned). For this he argues that asking for more is, in essence, asking for more relations. For as he put it "External objects, for instance, for which the word object was invented, are really objects and not fleeting and fugitive appearances, because they are not only groups of sensations, but groups cemented by a constant bond. It is this bond, and this bond alone, which is the object in itself, and this bond is a relation" (1902, 290-1). Hence, "it is relations alone which can be regarded as objective".

When the claim is made that science teaches us the true nature of things, Poincaré takes it to mean that it teaches "true relations of things". If something more is asked, if the true nature of things is supposed to be something over and above their relations, then Poincaré emphatically stresses, as noted already, that this nature is unknowable by us and inexpressible to us even by God!

6.4 The History of Science

Note that of the two conditions of objectivity above, the second (Correspondence) is independent of the history of science. Though I will not argue for this here in any detail, it captures an early version of the so-called 'no miracles argument', which Poincaré puts it in terms of how unlikely it is that a theory is radically false and yet it yields successful predictions. Science makes better than chance predictions ("the scientist is less mistaken than a prophet who should predict at random") and this suggests that science "is not without value as a means for knowledge" (1902, 265). I am not claiming that Poincaré was a scientific realist, though, as I have argued elsewhere, he progressively became more convinced of the reality of unobservable entities such as atoms (see my 2011; also Poincaré 1913, 90). But what I called *Correspondence* was meant to strengthen Poincaré's relationism by providing grounds for the claim that the relations science establishes are, in a certain sense, objective and real—not mere fictions.

The first condition of objectivity, however, does raise an *empirical* issue that only the history of science can answer. As Poincaré put it, the relations established by science, or at least some of them, should still be the same "for those who shall come after us" (1902, 291). But the only ground for this claim should be historical: viz., that the relations established by past theories have been the same for subsequent theories. Now, Poincaré thinks that "science has already lived long enough for us to be able to find out by asking its history whether the edifices it builds stand the test of time, or whether they are only ephemeral constructions" (1902, 292).

The context in which this historical issue is raised is instructive. With the 'bankruptcy of science' debate still raging in Paris, and with a claim of growing popular reputation that scientific theories are ephemeral, the need to defend a level of continuity in theory-change in science was not just a theoretical need licensed by Poincaré's second condition of objectivity, but a practical need too, urged by the growing public distrust of science as a means to deliver some knowledge of the world.

In the Introduction to *La Science et l'Hypothése*, Poincaré made clear what he took his task to be:

> Without doubt, at first blush, the theories seem to us fragile, and the history of science proves to us how ephemeral they are; yet they do not entirely perish, and of each of them something remains. It is this something we must seek to disentangle, since there and there alone is the veritable reality (1902b, 26).

If what survives theory-change is the true reality, then the issue is to find out what, if anything survives, and to show that it does survive. If that relations *should* survive is a conceptual issue, that relations *do* survive is an empirical-historical one. Settling this empirical issue is important for another reason. Poincaré was aware of the various raptures in the foundations of science that were then taking place. In his address to the St Lewis International Congress of Arts and Sciences, Poincaré noted that "there are indications of a serious crisis" in the principles of mathematical physics (1905, 1). A bit later, in the preface to the English edition of his three books (*Foundations of Science*, 1913a), he spoke of an eve of a revolution in physics. So it became imperative for Poincaré to show the constructions of science are not ephemeral, that they are not like "Penelope's web" (1905, 23).

Poincaré does not embark on a detailed account of how the history of science bears his relationism, but he does outline the strategy that should be followed, viz., look for invariant elements in theory-change. Here is where the various motivations for his relationism converge. For relations are expressed mathematically by *equations*; relations are what science should look for; relations that survive theory-change are the locus of objectivity; hence retained equations are the locus of objectivity in science. The history of science should bear this out, and Poincaré offers a famous case as a prime example: the Fresnel-to-Maxwell case. As is well-known, Fresnel's laws relating concerning the amplitudes of reflected rays vis-à-vis the amplitude of incident rays in the interface of two media were retained within Maxwell's theory of electromagnetism.

Here is his point:

> 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 (1900, 15).

In his reply to Le Roy (1902), Poincaré repeated essentially the same point, adding that this kind of invariance is the mark of the real. But we should not lose sight of two important points. The *first* is that invariance is one of the two conditions of objectivity; the other being that the invariant elements in theory change should 'latch onto' the world; and it is precisely because they latch onto the world that science's success is not a matter of chance. The *second* point is that for Poincaré science is, ultimately a creation of the human mind—a free creation, to be exact—which, however, is constrained by Invariance and Correspondence in that the latter show us that this creation, though free, is not arbitrary but tells us something about the world.

6.5 *Pure* Structuralism?

In the *Introduction*, we saw Russell taking Poincaré to endorse pure structuralism, viz., that "even the relations are for the most part unknown, and what is known are properties of the relations, such as are dealt with by mathematics". This claim is clearly exaggerated. For one Poincaré himself explicitly resisted the claim that "the world is only a differential equation" (1902, 289), meaning that the relations that science establishes, though expressed mathematically, are theoretical, and hence *interpreted*, relations among physical magnitudes. For another, Poincaré makes clear that the order of dependence between the worldly relations and mathematical equations is from the former to the latter. That is to say, it is because real relations are invariant that they are represented by invariant mathematical equations in different theories. Now, the retention of mathematical equations in theory-change is clearly a sign for an underlying invariant natural relation. But this should not obscure the fact that the invariance of the equation is explained by the invariance of the natural relation. As Poincaré puts it: "the equations express relations, and if the equations remain true it is because these relations preserve their reality" (1900, 15). As we have seen, the very nature of mathematical representation in mathematical physics makes it the case that only the relations 'among things' can be known because all that can be represented mathematically in mathematical physics are these relations. But it would be wrong to conclude from this that for Poincaré only the formal properties of these relations can be known. Discussing in passing the kinetic theory of gases, long before he came to accept the reality of molecules based on Perrin's work on the Brownian motion, Poincaré observed that the theory has revealed to us a "true relation" which had been "profoundly hidden", viz., the relation between gaseous pressure and osmotic pressure (cf. 1900, 16). This is far from being a formal relation. Actually, it was precisely *this* relation that Perrin explored when he devised a model of the Brownian particles suspended in an emulsion based on the kinetic theory of gases (see my 2011).

What Poincaré resisted, as we have noted, was the idea that there will be knowledge of how things themselves are, meaning: what their qualitative, non-relational nature is. In the discussion that followed his talk at the World Congress of Philosophy in 1900, Poincaré (1900b) was pressed by the mathematician Paul Painlevé to mitigate his conventionalism by adopting the "method of successive approximations". His thought was the truth of scientific theories could be justified by the successively more precise agreement between them and reality. In his reply, Poincaré did not deny that science has progressed by "successive approximations". But he emphatically denied that we can reach a stage at which we know how things are. The nature of things, he claimed, is an "unknown" for which we will never have enough equations to determine. We will always have "less equations than unknowns". It is this kind of thought that explains his epistemic Humility about the qualitative properties of the physical magnitudes. And it is the closest we can get to Poincaré's admitting that this is because what things are does not determine how things relate to each other.

To see the significance of Poincaré's claim, let us contrast it with a point made by Enriques a few years later. Enriques adopted a form of relationism too (cf. 1914, 67). But he thought that we can hone in real things by means of relations. That is, he thought (in a certain sense, like Carnap did in his 1928, 25) that as more and more relations are known, the objects that bear these relations are known too. [18] According to Enriques:

> A real thing always implies various associated relations between series of sensations that are produced under given conditions. And by virtue of this multiplicity of relations which is capable of endless extension, the hypothesis that there is a reality is enlarged, beyond the world which can be immediately perceived by our senses. Especially by means of psychological hypotheses, reality acquires a social significance (as Comte has taught). The real gets defined, in this way, as an invariant in the correspondence between volition and sensation (1914, 65).

The idea we need to focus on is that of "the multiplicity of relations which is capable of endless extension". For Enriques it is precisely this 'endless extension' of relations that takes science beyond the world of phenomena and offers some, perhaps limiting, knowledge of the qualitative natures of the entities that science describes. Circa 1905, Poincaré would resist this conclusion, though c. 1912 (towards the end of his life) he did come to believe in the existence of atoms, interestingly based on Perrin's account of Brownian motion and the almost 'endless extension' of the way in which Avogadro's number could be calculated.[19]

7. Conclusions

Though conventionalism and relationism are independent of each other, Poincaré endorsed both. Part of the reason was that he found in their *combination* a platform from which he could re-conceptualise and transform some key Kantian ideas in light of the developments in mathematical physics of his time. However, conventionalism was going beyond relationism in grounding how the object of science is constituted; and relationism was going beyond conventionalism in showing how the object of science is related to the natural world.

Conventions were supposed to capture the Kantian element of the spontaneity of understanding. *Convention* is a new epistemic category. Conventions are neither synthetic a priori principles nor empirical ones, though they retain *some* elements of both. They are constitutive of the object of knowledge of science; better put, they are constitutive of the fundamental spatial and mechanical framework for the development of scientific theories of the world. But they are not necessarily true. In fact, they are freely chosen among competing sets of principles, the choice being guided by considerations of convenience. At the same time, conventions are not arbitrary since they are suggested by various empirical considerations, without in any way dictated by, or made probable on the basis of, experience. And though they can never be contradicted by experience, they can be condemned by it and be abandoned as being no longer convenient.

---

18 For a discussion of Russell's and Carnap's structuralisms see my (2009, part II).

19 In (2011), I have argued that Poincaré and other scientists who were initially antihtetical to the existence of atoms were characterised by a spirit of epistemological openess which made possible their conversion to atomism.

Conventions, for Poincaré, make scientific knowledge of the world possible. Scientific knowledge requires a notion of time, of *geometrical* space and principles of motion and dynamics. All these impose from above freely chosen constraints to proper empirical scientific inquiry. Though the objects of science as constituted by the conventions are *not* directly encountered in the world, it is a fortunate and contingent fact that the empirical objects are in many ways similar to the objects of science. This explains the applicability of conventions to experience. In fact, the contact between the objects of science and the empirical objects is structural: abstract scientific theories are related to the world of experience by sharing structure; that is, by capturing and expressing relations among empirical objects by means of their mathematical equations. This thought expresses Poincaré's relationism. For him, relationism is motivated by the way mathematical physics represents its objects. We can know only the relations among objects because all we can represent mathematically in the sciences are these relations.

Relations were supposed to capture the Kantian limits to knowing the things themselves. Poincaré's relationism was at the same time an expression of Humility and expression of optimism, viz., that some knowledge of the world is possible. But the sought after relations were not purely formal—as Russell was pushing for. They were clearly expressed by mathematical equations, but they were theoretical relations. Relations were also meant to mitigate conventionalism. For Poincaré, objectivity lies in invariant relations—both synchronically and diachronically. Precisely because relations remain invariant under theory-change (which is supposed to be brought out by the study of the history of science), we have reason to believe that science captures real relations among physical magnitudes and that science does not weave the web of Penelope.

Insofar as Poincaré was a conventionalist, he was not a rampant conventionalist. Insofar as he was a structuralist, he was not a pure structuralist. Insofar as he was both, he was delineating a position which allowed room for both freely, but not arbitrarily, chosen constitutive principles of science and the acquisition of objective, though, relational knowledge of the natural world.

In the Centenary of Poincaré's birth in 1954 André Lalande, a French philosopher and contemporary of Poincaré, summarised *one half* of Poincaré's position in the following very eloquent way:

> The world which science causes us to become acquainted with is but the most recent stage of the constructions raised in common by scientists and philosophers on the basis of our sense impressions and in accordance with the guidance of our reason (1954, 597-8).

The missing half is that the most recent stage of the scientific constructions is related to previous ones and hopefully future one by means of invariant elements, which express relations among real things in the world.

References

Ben-Menahem, Yemina. 2006. *Conventionalism: From Poincaré to Quine*. New York: Cambridge University Press.

Boutroux Émile. 1908. La philosophie en France depuis 1867. *Revue de Métaphysique et de Morale* 16: 683-716.

Boutroux Émile. 1911. *Science and Religion in Contemporary Philosophy*. (Translated into English by Jonathan Nield). New York: The MacMillan Company.

Boutroux Émile. 1914. *Natural Law in Science and Philosophy*. (First published in French in 1895 and Translated into English by Fred Rothwell). New York: The MacMillan Company.

Boutroux Émile. 1920. *The Contingency of the Laws of Nature*. (First published in

French in 1874 and Translated into English by Fred Rothwell). Chicago and London: The Open Court Publishing Company.
Capeilleres, Fabien. 2009. 'To reach for metaphysics: Émile Boutroux's philosophy of science'. In Rudolf A. Makkreel & Sebastian Luft (eds), *Neo-Kantianism in Contemporary Philosophy*. Indiana University Press, pp. 193-249.
Carnap, Rudolf. 1928. *The Logical Structure of the World*. (English translation by R A George, 1961). Berkeley: University of California Press.
De Paz, María & DiSalle, Robert eds. (2014). *Poincaré, Philosopher of Science: Problems and Perspectives*. *The Western Ontario Series in Philosophy of Science* 79, Dordrecht: Springer.
Demopoulos, William & Friedman, Michael. 1985. 'Critical notice: Bertrand Russell's *The Analysis of Matter*: its historical context and contemporary interest'. *Philosophy of Science* 52: 621-639.
DiSalle, Robert. 2006. *Understanding Space-time: the Philosophical Development of Physics from Newton to Einstein*. Cambridge: Cambridge University Press.
Enriques, Federico. 1914. *Problems of Science*. (Translated into English by Katharine Royce). Chicago and London: The Open Court Publishing Company.
Folina, Janet. 2014. 'Poincaré and the invention of convention'. In de Pas & DiSalle (1914), pp. 25-45.
Friedman. Michael. 1999. *Reconsidering Logical Positivism*. Cambridge: Cambridge University Press.
Giedymin, Jerzy. 1982. *Science and Convention: Essays on Henri Poincaré's Philosophy of Science and the Conventionalist Tradition*. Oxford: Pergamon.
Gray, Jeremy. 2013. *Henri Poincaré: A Scientific Biography*. Princeton: Princeton University Press.
Hertz, H. 1894. *The Principles of Mechanics Presented in a New Form*. (First English Trans. 1899, reprinted by Dover, 1955) New York: Dover Publications Inc.
Horner, Richard. 1997. 'A pragmatist in Paris: Frédéric Rauh's 'Task of Dissolution''. *Journal of History of Ideas* 58: 289-308.
Lalande, André. 1954. 'From *Science and Hypothesis* to *Last Thoughts* of H. Poincaré (1854-1912)'. *Journal of the History of Ideas* 15: 596-598.
Langton, Rae. 1998. *Kantian Humility: Our Ignorance of Things in Themselves*. Oxford: Oxford University Press.
Le Roy. Édouard. 1899 I & II. 'Science et philosophie'. *Revue de Métaphysique et de Morale* 7: 375-425; & 503-562.
Le Roy. Édouard. 1901. 'Un positivism nouveau'. *Revue de Métaphysique et de Morale* 9: 138-153.
Nye, Mary Jo. 1979. 'The Boutroux Circle and Poincaré's conventionalism'. *Journal of the History of Ideas* 40: 107-120.
Poincaré, Henri. 1887. 'Sur les hypothèses fondamentales de la géométrie'. *Bulletin de la Société Mathématique de France* 15: 203-216.
Poincaré, Henri. 1889. 'Leçons sur la théorie mathématique de la lumière'. Volume 1 *Cours de Physique Mathématique*, Paris: Georges Carré.
Poincaré, Henri. 1890/1901. *Électricité et Optique: La Lumière et les Théories Électromagnétiques*, 2nd edition, Paris: Gathier-Villairs.
Poincaré, Henri. 1891. 'Les géométries non euclidiennes'. *Revue Générale des Sciences Pures et Appliquées* 2: 769-774—reprinted as Chapter 3 of *Science and Hypothesis*.
Poincaré, Henri. 1892. 'Non-Euclidian geometry'. *Nature* 45: 404-407
Poincaré, Henri. 1893. 'Le mécanisme et l'expérience'. *Revue de Métaphysique et de Morale* 1: 534-537.

Poincaré Henri. 1895. 'L'espace et la géométrie'. *Revue de Métaphysique et de Morale* 3: 631-636—reprinted as Chapter 4 of *Science and Hypothesis*.
Poincaré, Henri. 1897. 'Les idées de Hertz sur la mécanique'. *Revue Génerale des Science* 8: 734-743; reproduced in *Ouevres de Henri Poincaré*, VII, 231-250, (1952) Paris: Gauthier-Villars.
Poincaré, Henri. 1897a. 'Réponse à quelques critiques'. *Revue de Métaphysique et de Morale* 5: 59-70.
Poincaré, Henri. 1898. 'On the foundations of geometry'. *Monist* 9: 1-43.
Poincaré, Henri. 1899. 'Des fondements de la géométrie: à propos d'un livre de M. Russell', *Revue de Métaphysique et de Morale* 7: 251–279.
Poincaré, H. 1900. 'Relations entre la physique expérimentale et de la physique mathématique'. *Rapports présentés au Congres International de physique de 1900*, pp. 1-29; also in *Revue Generale des Sciences* 11: 1163-1175 (1900)—reprinted in *Science and Hypothesis*, chapters 9 & 10
Poincaré, Henri. 1900a. 'Sur les principes de la géométrie: réponse à M. Russell. *Revue de Métaphysique et de Morale* 8: 73–86.
Poincare, Henri. 1900b. 'Séance Générale: Logique et Histoire des Sciences'. *Revue de Metaphysique et de Morale* 8: 555-561.
Poincaré, Henri. 1901. 'Sur les principes de la mécanique'. *Logique et Histoire des Sciences: Bibliotéque du Congres Internationale de Philosophie*, Volume 3, Paris: Librarie Armand Colin, pp.457-494—reprinted (with a few alterations) in *Science and Hypothesis*, chapters 6, 7 & 8.
Poincaré, Henri. 1902. 'Sur la valeur objective de la science'. *Revue de Metaphysique et de Morale* 10: 263-293—reprinted as chapters X and XI of *The Value of Science*.
Poincaré Henri. 1902a. 'Les fondements de la géométrie'. *Bulletin des Sciences Mathématiques* 26: 249-272—translated into English as 'Poincaré's review of Hilbert's 'Foundations of Geometry'', *Bulletin of the American Mathematical Society*, October 1902, pp. 1-23
Poincaré, Henri. 1902b. *La Science et L'Hypothése*. (1968 reprint), Paris: Flammarion.
Poincaré, Henri. 1905. 'The principles of mathematical physics'. *Monist* 15: 1-24.
Poincaré Henri. 1905b. 'Les mathématiques et la logique'. *Revue de Métaphysique et de Morale* 13: 815-835.
Poincaré, Henri. 1913. *Mathematics and Science: Last Essays*. New York: Dover.
Poincaré, Henri. 1913a. *The Foundations of Science*. New York: The Science Press.
Psillos, Stathis. 1995. 'Poincaré's conception of mechanical explanation'. In J-L. Greffe, G. Heinzmann & K. Lorenz (*eds*) *Henri Poincaré: Science and Philosophy*, Berlin: Academie Verlag & Paris: Albert Blanchard.
Psillos, Stathis. 2001. 'Is structural realism possible?'. *Philosophy of Science* 68: S13-24.
Psillos, Stathis. 2009. *Knowing the Structure of Nature*. London: MacMillan-Palgrave.
Psillos, Stathis. 2011. 'Moving molecules above the scientific horizon: On Perrin's case for realism'. *Journal for General Philosophy of Science* 42: 339-363.
Psillos, Stathis & Demetra Christopoulou. 2009. 'The a priori: Between conventions and implicit definitions. In Nikola Kompa, Christian Nimtz, Christian Suhm (eds) *The A Priori and its Role in Philosophy*, Mentis, pp.205-220.
Russell, Bertrand. 1899. 'Sur les axiomes de la géométrie', *Revue de Métaphysique et de Morale* 7: 684–707.
Russell, Bertrand. 1899a. 'The classification of relations'. *Philosophical Papers*

Volume 1896-1899. London and Boston: Unwin Hyman (1990)
Russell, Bertrand. 1903. *The Principles of Mathematics*. New York: W. W. Norton.
Russell, Bertrand. 1905. 'Review of *Science and Hypothesis* by H. Poincaré', *Mind* 14: 412–418.
Russell, Bertrand. 1927. *The Analysis of Matter*. London: George Allen & Unwin.
Schmid, Wilfried. (1982) Poincaré and Lie groups'. *Bulletin of the American Mathematical Society* 6: 175-186.
Stump, David. 1989. 'Henri Poincaré's philosophy of science'. *Studies in History and Philosophy of Science* 20: 335-363.
Stump, David. 2003. 'Defending conventions as functionally a priori knowledge'. *Philosophy of Science* 70: 1149-1060.
van Cleve, James. 1999. *Themes From Kant*. Oxford: Oxford University Press, 1999.
Worrall, J. (1989). 'Structural realism: The best of both worlds?'. *Dialectica* 43: 99-124.
Zahar, Elie. 1995. 'Poincaré's structural realism and his logic of discovery'. In J-L.
Greffe, G. Heinzmann & K. Lorenz (*eds*) *Henri Poincaré: Science and Philosophy*, Berlin: Academie Verlag & Paris: Albert Blanchard.
Zahar, Elie. 2001. *Poincaré's Philosophy*: *From Conventionalism to Phenomenology*. Chicago and La Salle IL: Open Court.