Poincaré and the Invention of Convention

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Abstract Jules Henri Poincaré is famous for his "conventionalist" philosophy of science. But what exactly does this mean? Poincaré invented the category of convention because he thought that there are some central principles in science that are neither based on intuition, empirical data, nor that are arbitrary stipulations. His views here resemble those of Wittgenstein, in particular, as presented in *On Certainty*. The invention of convention is lauded (for example, by Robert DiSalle) as a genuine philosophical discovery. But it is also critiqued (for example by Michael Friedman) as yielding a vision of science that is too rigid – one that is refuted by general relativity. This paper aims to defend Poincaré's views about conventions by focusing on his central idea that conventional choices, though "free", are "guided" by experience. I will argue that conventionalism is not a commitment to *fixed* a priori stipulations, as DiSalle and Friedman propose. Rather, it mandates empirically motivated shifts in (even geometric) conventions – a view surprisingly in accord with Friedman's "relativized a priori", and thus more consistent with general relativity than is generally thought.

Poincaré's views about mathematics and science are fascinating and remain largely plausible. Highlights include the following. Logic is empty, so it is not a source of significant information. Mathematics is not empty; so logicism – the view that at least arithmetic (and possibly more of mathematics) is logic – must be false. In fact, Kant was right that a core of pure mathematical knowledge is based on a priori intuition and thus it has a synthetic a priori status. In this way, mathematics provides an a priori foundation for natural science, which should itself be viewed (at least in part) from a structural realist perspective.

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How does conventionalism fit into this semi-Kantian picture? Poincaré presents conventions as intermediary principles found in scientific disciplines that lie on the border between pure mathematics (the synthetic a priori) and the natural sciences (the synthetic a posteriori). The disciplines in question, for Poincaré, are geometry and parts of physics. What is the role of conventions? Why does Poincaré introduce this new category into the taxonomy of science? Why do references to conventions virtually disappear in his later philosophical writings? What aspects of his conventionalist philosophy, if any, can be salvaged from its apparent collision with General Relativity?

This paper argues that to better understand Poincaré's invention of convention we must focus on the role of experience in both choosing and evaluating conventions. Poincaré was intrigued by the fact that the principles he came to regard as conventions played an essential role in science, yet failed to fit any traditional semantic or epistemic categories. He argued that they are neither analytic, nor empirical, nor synthetic a priori. (In this way, his vision resembles that of the later Wittgenstein, in particular as presented in *On Certainty*.) This invention constitutes a genuine philosophical discovery, as Robert DiSalle has argued (DiSalle 2006, Chapter 3). I will review his reasons for inventing this scientific category, with the goal of showing that for Poincaré experience plays a crucial role in *determining* conventions, even those in geometry.

Experience also plays an important role in *evaluating* conventions; I appeal to this role, in particular, to reconstruct Poincaré's views as rationally as possible. Of course some of his views may simply be outmoded, such as his unwavering support of Euclidean geometry. He acknowledged that scientific conventions can, and do, shift; and he argued for the coherence and utility of non-Euclidean geometry. Yet he repeatedly asserted that Euclidean geometry need never be given up. This implies a vision of geometric conventions as having a special place in the scientific hierarchy. Indeed Poincaré compared the different geometries to languages, which are neither true nor false, and which cannot in principle be either confirmed or undermined by experience.

In light of this protectionism towards Euclidean geometry, Michael Friedman has argued that Poincaré's geometric conventionalism was refuted by general relativity, which treats physical geometry as empirical (Friedman 1999, especially Chapter 4). The shift to general relativity (GR) showed, in other words, that geometry need *not* be treated as a mere language; instead, it can be regarded as providing part of the (broadened) empirical content of physics. In this way, Friedman also corrects the logical positivists' appeal to Poincaré's conventionalism, showing how – far from supporting GR – it is inconsistent with it.

In the latter part of this paper I attempt to mitigate the inconsistency between GR and Poincaré's conventionalism. Certainly there are some very general conventionalist views that remain true – for example, that there are empirical-sounding sentences that don't play an empirical role in science; that we have to postulate, or presuppose, some (empirical or quasi-empirical) truths in order to test other hypotheses; that science has a framework, or structure, that includes both un-testable and indirectly testable components. Does anything more specific to Poincaré survive?

Now Friedman has shown that GR is inconsistent with Poincaré's central views about geometry; this includes the special, protected status of geometry, owing to its intermediary position in the scientific hierarchy – between the synthetic a priori truths of mathematics and the empirical truths of physics. Also, the way Poincaré appears to construe the function of the scientific hierarchy is no longer acceptable – in particular, the idea that mathematics offers us exactly three geometric alternatives (the three dimensional geometries of constant curvature) from which we must choose one as the basis for physical measurement. So Poincaré's philosophy of geometry cannot be reconstructed as consistent with relativity, no matter how charitable we may try to be. But I will argue that more of conventionalism survives than one might think; that is, conventionalism and relativity are not as inconsistent as they may at first seem.

What can easily be forgotten in hindsight is Poincaré's emphasis on the central role of experience, not only in *making* but also in *evaluating* conventional choices. That is, despite references to liberty in this context, Poincaré did not regard the choice of a geometric system, or any convention, as arbitrary or completely free. In using the term "convention" he is making a claim about convenience, rather than arbitrariness (as in a "mere" convention). Geometric choices are free but *guided by experience*; moreover, geometric choices, like all conventional choices, can be revised in the light of further experience. Why does this matter?

In their critiques of geometric conventionalism, DiSalle and Friedman both cite as a central error Poincaré's rigid classification of geometry as a priori. Rather than conventionalism in general, DiSalle argues that Poincaré's error lies in

> … his particular view of the privileged status of space. The theory of space will not be overturned by principles of physics, because space is exhaustively defined for us as a pre-physical notion, and because, therefore, the transition from geometry to physics must always introduce extraneous elements into the concept of space. That geometry has always involved such elements, … was an empiricist conviction that Poincaré never took to heart (DiSalle 2006, 94).

Friedman also argues that Poincaré's vision of physical geometry – though correct for classical physics – is incompatible with relativity theory, because of his presupposition that physical geometry belongs to "the a priori part of our theoretical framework" (Friedman 1999, 85). Similarly, according to DiSalle, Poincaré's understanding of the way the concepts of physical geometry function in science meant that "they had to be considered a priori rather than empirical" (DiSalle 2006, 95).

I want to call into question this way of framing Poincaré's error. It explains the inconsistency between Poincaré's views and GR by supposing that Poincaré was committed to a fairly sharp distinction between the a priori and the empirical elements of a theory; and that the function of geometric and other conventional principles makes them a priori and *not* empirical. Moreover it supposes that, once stipulated, geometric principles are isolated from empirical results owing to their a priori status. In contrast, I will argue that the point of inventing the category of conventions was to provide a *new* classification for certain principles that seemed to be neither a priori nor empirical. Rather, in my view, he regarded them as having an intermediary epistemic status, which he struggled to articulate.

Unlike Quine, Poincaré is not out to dispute the validity of our general distinctions. Poincaré is committed to principles that are clearly a priori – those of logic, mathematics and language (the synthetic and the analytic a priori). He is also committed to the fact that certain scientific assertions are clearly empirical – for example, the more experimental areas of science (often giving the example of optics). What he disputes is the exhaustive nature of these distinctions, introducing the idea that there are principles that cannot be categorized along ordinary lines, or by ordinary criteria. These include, in particular, the conventions of geometry and mechanics, which he argues are neither empirical nor a priori in the ordinary senses; perhaps they seemed to him to be a bit of both.

Conventions *act* a priori, in that they contribute some of the framework principles necessary for the methodology of science. But they are also both *suggested by* and *acted upon* by experience, in that conventions are rooted in experience and prompt choices that must respond to, and sometimes change in the light of, empirical data. In this latter respect, they resemble empirical assertions.

To reiterate, *given*, or *within*, a framework, conventions act as a priori principles. But Poincaré recognized that frameworks change, and these changes come from the influence of experience. (In this sense, he can also be considered a naturalist. See Stump 1989.) Thus Poincaré's view is not that conventions are absolutely a priori, but that they are only relatively a priori.[1] Both Friedman and DiSalle cite the rigidity of Poincaré's conventions as an obstacle to a more flexible view about the presuppositions of science; and that we have to wait for Reichenbach and Carnap for a more modern, empiricist, approach to framework principles (See, for example, Friedman 1999, Chapter 3). In contrast, I see Poincaré's conventionalism as itself providing the basis of this more flexible attitude. For certain periods of time conventions function like a priori truths, but unlike ordinary a priori truths they are susceptible to revision owing to changes in data and/or theory.[2]

To appreciate what might survive of Poincaré's conventionalism, and his geometric conventionalism in particular, we thus need to revisit the crucial roles of experience in articulating and evaluating conventions. Though Poincaré viewed geometry as holding a privileged, *more* protected, position in the scientific hierarchy, he did not regard it as *absolutely* protected by its position and function in the hierarchy. Despite his own comparisons, Poincaré's conventions should not be understood as arbitrary, or analytic like linguistic conventions; nor should they be understood as a priori in any ordinary sense. Scientific conventions have a special character. They must be free in that neither logic, nor mathematics, nor experience

[1]The idea of the relative, or relativized, a priori has recently taken hold, owing largely to the influence of Friedman 1999 and 2001. There is a growing body of literature on the topic, including several new essays by Friedman extending and modifying his views (for example, Friedman 2011, and 2012). See Stump 2011 for an account of Arthur Pap's similar "functional a priori", and Poincaré's influence on Pap. Like Stump, I support an interpretation of Poincaré's conventionalism as rather close to the relativized a priori.

[2]For just one example, he comes to accept the existence of atoms after first denying them, thus giving up a principle of the continuity of nature.

force particular principles on us. That is, there must be more than one viable alternative – a choice must be made. However conventions are also answerable to our empirical situation and data – the facts that guide our initial conventional choices as well as any revision of those choices.

Focusing too much on the freedom of conventions can encourage a misunderstanding of scientific conventions as arbitrary – something Poincaré was, in fact, determined to refute (See Poincaré 1905a/1958, Chapter X). It can also make the refutation of conventionalism seem inevitable, owing to changes in scientific frameworks. In contrast, on this interpretation, conventions are features of a scientific framework that respond to empirical information. Thus, conventionalism is not a commitment to fixed a priori stipulations. Rather, it mandates empirically motivated shifts in (even geometric) conventions – a view surprisingly in accord with the relativized a priori, and thus less in conflict with relativity.[3]

Background and Context

Conventionalism gets the spotlight in Poincaré's first book, *Science and Hypothesis*, where it is presented as a middle position between naïve realism and simple skepticism, one that recognizes both the *complexity* and *structure* of science. As he put it, "To doubt everything or to believe everything are two equally convenient solutions; both dispense with the necessity of reflection" (Poincaré 1902a/1952, xxii).

Complexity

On the one hand, conventionalism should be distinguished from a general skeptical attitude. Overemphasizing the role of choice and construction in science, and underemphasizing the role of experiment, can lead one to skepticism. But doubting everything is a superficial epistemic stance, which is neither justified nor fruitful. Poincaré aims to distinguish his view that conventions, and choices, are *necessary*

[3]In writing this paper I realize I have entered a thick territory. The literature on, and related to, Poincaré's conventionalism is enormous and I cannot pretend to have mastered it. I have approached the topic by first re-reading Poincaré's central texts and then by addressing just a few secondary works that have particularly influenced me. I hope to make a small contribution to this literature by supporting a slightly more empirical interpretation of conventions. I thank Maria de Paz and Robert DiSalle for inviting me to contribute to this volume; Michael Friedman, Robert Disalle and David Stump for their excellent work on this topic; and David Stump for comments on an earlier, written draft. Several audiences should also be thanked for putting up with early, half-baked talks on some of this material, including those at our Poincaré session at HOPOS, June 2012, and especially those attending the *Foundations of Physics and Mathematics Workshop* at the University of Western Ontario, May 2012.

from a view that renders them as closer to *sufficient*. Scientists do not create facts, as he argues (Poincaré 1905a/1958, Chapter X). The scientist may create a convenient language in which facts can be expressed, but the success of science shows that experience (of facts) is central to scientific methodology. Thus, Poincaré regards conventions as part of an account of science that acknowledges choice but also emphasizes the necessity of experience for objective knowledge. It is part of a picture of science aimed to repudiate both global skepticism and simple relativism.

On the other hand, Poincaré also distinguished conventionalism from the view that science is certain. Overemphasizing the basis of science in the certainty of logic and mathematics can lead one to naïve realism, which also overestimates the certainty of the experimental method (while oversimplifying the structure of scientific judgment). Poincaré describes this view as follows:

> To the superficial observer scientific truth is unassailable, ... Mathematical truths are derived from a few self-evident propositions, by a chain of flawless reasonings . . . . By them the Creator is fettered, as it were, and His choice is limited to a relatively small number of solutions. A few experiments, therefore, will be sufficient to enable us to determine what choice He has made . . . . This, to the minds of most people, ... is the origin of certainty in science (Poincaré 1902a/1952, xxi).

Poincaré here points out two things. First, mathematics is not simply making deductions from a small number of "self-evident" axioms and leading to a few mathematical options. Second, science is not simply conducting a few crucial experiments in order to decide between the narrow set of options provided by mathematics. Just as we shouldn't overemphasize the importance of convention, so we shouldn't oversimplify the roles of either mathematics or testing in science. Clarifying the nature and functions of conventions in science is meant to help correct both of these mistakes – that of the skeptic and that of the naïve, or over-zealous, realist. As a philosophical response to both naïve realism and skepticism, conventionalism thus supports the *complexity* of science.

Structure

Conventionalism is also a view about the *structure* of science. As a philosophical position, it opposes thoroughgoing empiricism and homogeneous holism. Geometry cannot be directly tested since tests are done on bodies, not space; because geometry cannot be directly tested, Poincaré argues that geometric empiricism has no rational meaning. And though the different parts of a scientific framework are connected, Poincaré argues, against holism, that a scientific theory is not a simple *set* of homogeneous propositions. The parts of science form a structure, which can be understood. Conventions constitute some of the implicit structure of science, structure that binds mathematics to the empirical world and enables empirical testing. Furthermore, *Science and Hypothesis* argues that the way to understand the parts of the structure of science is in terms of the different degrees to which those parts are testable; these different degrees will correspond to position in the scientific hierarchy.

Conventional sciences comprise the middle two sections of this hierarchy and of the book, between a first section on pure mathematics and a last section on "nature", or experimental science. Conventional principles thus lie between the truths of pure mathematics – the a priori domain provided by logic and intuition – and the truths of the experimental sciences – the empirical domain governed more directly by experience. The category of convention is in this way central to the sciences of geometry and mechanics, which occupy the two middle sections of *Science and Hypothesis*, and which correspond to two main types of conventions for Poincaré.

Conventions are thus situated in Poincaré's hierarchy – between pure mathematics and the more experimental areas of natural science – for they are neither a priori in any ordinary sense, nor straightforwardly empirical, or testable. As Michael Friedman points out, they provide Poincaré with a way to accommodate the quasi-empirical aspects of science that Kant mistakenly took to be a priori (metric and general, physical principles) (Friedman 1999, 81). Though geometric conventions may be closer to pure mathematics, and therefore further from experiment than mechanical conventions, I will argue that like any other conventional science, geometry relies on choice *and* experience.

The Nature of Conventions: Free but Guided

A coherent account of Poincaré's geometric conventionalism begins with the distinction between pure and applied geometry, and it emphasizes the freedom of conventions. That is, we are *free* to develop various pure geometric systems – within some minimal confines such as consistency. And the stipulation, or choice, of which geometric system to apply in physics is also free. It is only after these two steps of pure and applied mathematics that the account emphasizes the role of experience: when the combination of physics plus geometric system is tested. The nature of the scientific hierarchy, on this view, means that geometry is chosen strictly *prior to* testing; and the choice of a geometric system, like the choice of a linguistic convention, is free.

A virtue of this account is its clarity. There is both a logical distinction and a temporal separation between pure mathematics and its application, and between its application and any empirical testing. Furthermore, there is much to recommend it in Poincaré's own rhetoric. Indeed some of his remarks about pure geometry make conventionalism seem like formalism, such as the idea that axioms are *disguised definitions* of the basic geometrical concepts. Here he is arguing against a traditional conception of geometric axioms as meaning-*reflections* that result in *truths* about space. Instead he is endorsing a view closer to formalism about axioms – that they are meaning-*determinations* that *stipulate* how we *use* certain spatial concepts. The traditional view of axioms yields a traditional view of mathematical truth. The alternative view he seems to be endorsing yields a more relativized, or "indexed", concept of truth – that of "true in" one system or another.

In calling geometric axioms "definitions in disguise" (1902a/1952, 50) Poincaré is furthering a picture of axioms as truth-*makers*. Geometry does not lead us to truth; rather, in some sense it creates truth, in that it articulates a framework in which mathematical truths can be discovered. The results are "true in" Euclidean or Lobachevskian geometry, though not true *simpliciter*. Geometric axioms are meaning determinations for Poincaré, because he thinks we don't have a definite pre-theoretic concept of point, line, plane, etc. (See for example, 1905a/1958, 45–46). As in algebra, there is more than one legitimate mathematical structure that instantiates the basic geometric concepts. Thus, the tidy view of Poincaré's geometric conventionalism emphasizes its relation to both algebraic formalism, which preceded it, and Hilbertian axiomatics, the development of which was mostly subsequent.

Despite its clarity, I find this interpretation incomplete. Granted, Poincaré's philosophy of geometry is very close to formalism. Poincaré distinguished pure from applied geometry, and he maintained that results derived within a pure geometrical system remain "rigorously true" even if the empirical world fails to *precisely* satisfy them. But he also believed that if the empirical world failed to approximately satisfy our geometric results, then we would never have developed such a geometry.

For one thing, Poincaré did not approve of formalist approaches to mathematics in general. He famously argued against Hilbert that formal systems do not stand mathematically on their own; rather, they need to presuppose some basic truths of mathematics (such as induction) (See his circularity arguments in Poincaré 1905b–1906b/1996; see also Folina 2006). These basic truths of pure mathematics are synthetic a priori and are forced on us by the nature of our minds.

Secondly, though the basic truths of geometry are *not* synthetic a priori, since geometric axioms are *not* forced on us by our minds, neither are they *merely* formal, arbitrary rules added to the synthetic a priori, pure mathematical basis. This is because geometric principles are not chosen *only* by considerations of consistency. For example, he praises Hilbert for making great progress in geometry, but he also considers his approach to be "incomplete" because it focuses on "the logical point of view alone" (Poincaré 1902c/1994, 167). Lacking in Hilbert's account is a connection between the geometric terms and experience that could help us to understand and choose between different systems of postulates.

Any convention provides a "rule of action"; but unlike games, scientific rules of action are shaped by empirical conditions (Poincaré 1905a/1958, 114). Poincaré sees geometry as intimately connected to experience, and the empirical world, in ways that a *merely* formal set of rules is not. Though he considers geometric axioms to be disguised definitions of the basic terms, by this he means that the terms have no *precise* prior meaning, from informal or experiential contexts – not that they have no prior meaning at all! Experience provides rough, or crude, meanings for the basic geometric terms; and geometric postulates are not empty rules for this reason.[4]

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[4]Thanks to David Stump for making me clarify this point.

Geometry requires significant empirical conditions for its very existence, including conditions on the outer world as well as conditions on ourselves, our bodies. In terms of *truth*, work within an area of geometry has a formal-mathematical character: there are well-defined concepts, axioms, deductive proofs, etc. But in terms of *subject matter*, Poincaré saw geometry as less pure and more empirical than other areas of mathematics, owing to geometry's dependence on a number of empirical facts, which he tried to articulate. For him, geometry was, in a sense, too empirical to be like other areas of pure mathematics, yet not empirical enough to be a natural science. Some of the richness of conventionalism involves the ways in which even the pure geometric work is guided and supported by experience (to which we shall turn shortly).

Third, empirical information also plays a crucial role in choosing an applied geometry. The idea that there is a clean separation between the stipulation of an applied geometry and subsequent scientific activity – including physical tests and assessments of the results – just seems too simple for what Poincaré struggled to articulate. It implies that there is a one-way path from pure mathematics, through geometric and mechanical conventions, to physical experiments, with everything in the process, or hierarchy, fixed before the next step is taken. In contrast, in the latter part of this paper I will explore a more complex interpretation, where the "arrows", the influences, between conventions and experimental science go both ways.

Admittedly, Poincaré did seem to think that a geometric system is *ordinarily* fixed, or chosen, prior to physical testing; this would especially have seemed to be the case to him: an important figure at a transitional time.[5] But central to Poincaré's view of conventions is the provision that empirical results can reverberate back through the chain of sciences after the fact, so to speak, of the stipulation. This reverberation can then lead to a revision of those stipulations, those conventions, as well as the more straightforwardly physical parts of a theory. That is, it is *not* the case that once stipulated, conventions, including those of geometry, are absolutely fixed and not revisable. To put it yet another way, there is a little bit of holism and a little bit of empiricism in Poincaré's conventionalism.[6]

For these reasons I will emphasize in what follows the sense in which conventions are *guided* rather than in which they are *free*. Freedom, the idea of a multiplicity of structures, was well known by the early 1800s, from algebra. The inventiveness of the category of convention is in the sense in which they are *guided* by experience.

> Experience leaves us our freedom of choice, but it guides us by helping us to discern the most convenient path to follow .... Some ... have forgotten that there is a difference between liberty and the purely arbitrary (1902a/1952, xxiii).

[5]Thanks to Bill Demopoulos for this point; Friedman 2001 and DiSalle 2006 also emphasize this.

[6]Though note: Stump 1989 cautions that any holism Poincaré endorses is specific; that is, Poincaré does not advance a *general* holism or appeal to *general* under-determination considerations to advance the flexibility of conventions.

First we will review the empirical conditions that surround the *subject matter* of geometry; these are the empirical preconditions for the possibility of pure geometry. Second, we will review the ways in which experience plays a role in *assessing* our choice of applied geometry. Such choices are necessarily responsive to empirical data, owing to the fact that once chosen, a geometric system becomes part of a larger system, of geometry plus physics, which can then be more explicitly tested. I first turn to the role the empirical world plays in fulfilling preconditions for pure geometry.

Empirical Preconditions: The Subject Matter of Geometry

Poincaré argues that geometry begins more in our bodies and less in cognition. For example, it may be motivated by a desire to solve a puzzle: did that object move or did it change? Poincaré's point is that the very distinction between change of place and change of state presupposes something about the world; and we solve the puzzle with our *bodies* and by *observation*, not merely with our *minds* and by *thinking*. Poincaré emphasizes the following empirical preconditions for geometry in Part II of *Science and Hypothesis*. (References will be to page numbers from 1902a/1952, unless otherwise noted, for the rest of this section.)

1. *Solid bodies (or approximate solids) (45)*

If the world were entirely fluid, he argues, there would be no system of distance measures. In such a world we might develop topology, provided there were some jello-type substances or provided some detectable differences between the various fluids. But ideas central to metric geometries, such as identity of line segments, areas, angles, etc., require some roughly solid bodies.

2. *Motion – of solid bodies*

Poincaré points out that the mere existence of solids is not sufficient; if there were solids but they could not move, or did not move, we would not form an idea of displacements (60). So the solids have to be able to move – while remaining approximately in the same state – for the question of a change of position versus a change of state not only to be raised, but also to be intelligible. That is, the motion of (relatively) solid bodies outside of us is what prompts us both to distinguish, and to raise questions, concerning change of place versus change of state.

3. *Motion of our bodies*

In addition, *our* bodies have to move, because this is how we distinguish a change of state from a change of position (57–59). That is, though the motion of other objects may be what *prompts* such questions, determining an *answer* involves the motion of our bodies, according to Poincaré. And since the meaning of any question depends in part on how it is answered, the motion of our own bodies is also central for understanding the distinction between change of state and change of position. Consider an ice cream cone that is melting. I can't "correct" or un-melt the ice cream by moving my body around the cone, nor by moving the cone relative to my body. This is how I know that the ice cream is undergoing a change of state. In contrast, consider a horse that has run past me. I first see it from the side, and then it turns to face me. By moving my body (provided the horse stands still) I can "correct" the change in how the horse looked and return our relative positions back to an earlier one. Since I can "correct" our relative positions I infer that the horse changed place and not state. In this way, Poincaré thinks that the distinction between changes of position and changes of state requires mobility – that of both other objects and our own bodies (relative to the motion of outer objects).

4. *Consciousness – of our attempts to make corrective motions described above*
Our corrective motions must be voluntary/intentional to prompt us to make the central distinctions, and they must be accompanied by conscious sensations (59). I think the reasoning here is that without consciousness of our attempts to correct relative positions, there would be no consciousness of the group theoretic structure emerging in our encounters with moveable solid bodies.

5. *Homogeneity of space, or free mobility (approximate/empirical)*
Homogeneity and iteration are implicitly assumed in the ordinary geometric construction postulates; and these spatial properties are generated by the perception of (approximate) free rigid body motion. Poincaré acknowledges that there is an empirical catalyst here: the at least approximate homogeneity of space is shown by the at least approximate existence of rigid body motion – which we idealize and assume is indefinitely iterable (45). If physical space were not approximately homogeneous we would not be able to reliably distinguish between change of state and change of position; and the group structure would not emerge in experience.

These central empirical preconditions for geometry must then be joined with some a priori preconditions. According to Poincaré, we're also guided by our minds in certain crucial ways that shape the development of geometry.

1. *Group concept*
The idea of a group guides us to *be* geometrical because we apply it to displacements of rigid bodies. With it we can pursue the above distinction between state and position; and it perhaps motivates us to organize data into certain classes. The group concept, Poincaré asserts, pre-exists in our minds as an a priori concept (rather than an a priori intuition). (For example, 1902a/1952, 70; 1905a/1958, 126.)

2. *Time*
For the idea of groups of rigid motions to emerge we need to be able to comprehend our corrective motions in terms of sequences of sensations (58). And similarly to Kant, Poincaré argues that time is an a priori form of experience. Temporal ordering is imposed on us rather than chosen: "The order in which we arrange conscious phenomena does not admit of any arbitrariness. It is imposed upon us and of it we can change nothing" (1905a/1958, 26). But time is not empirical because if

it were, time would be perceived neither as infinite in extent nor continuous (or even everywhere dense). Suppose that time were the result of labeling and storing actual memories. To this Poincaré objects, "[b]ut these labels can only be finite in number. On that score, psychological time should be discontinuous. Whence comes the feeling that between any two instants there are others . . . . How could that be, if time were not a form preexistent in our mind?" (1905a/1958, 26, translation slightly modified). Though the measure of time requires conventions about simultaneity and duration, the nature of qualitative time is a priori imposed, and includes the awareness of the successive linear structure of time. Succession of processes is central to mathematical geometry, for geometric constructions are must typically be carried out in a definite order.

3. *Repetition*
Finally, the possibility of *repeating*, or iterating, spatial motions is also presupposed in the ordinary geometric construction postulates (64). This idea, and in particular, general or indefinite repetition, is – Poincaré argues – given a priori as the central a priori intuition underlying all of mathematics. (See, e.g., Poincaré 1902a/1952, chapter I, especially section V.)

Accepting that all of the preconditions are met, we are limited to three options for three-dimensional space: Euclidean (no curvature), Riemannian (constant positive curvature) and Lobachevskian (constant negative curvature). So logic plus intuition (indefinite iteration) plus the other a priori preconditions (psychological time and the group concept) plus the empirical preconditions (solids, motion, consciousness, etc.) yields three possible geometries. We choose Euclidean because it is the simplest model that accords well with experience (1905a/1958, 38–39).

One way experience might yield a different set of options is if no group-theoretic structure emerged from experience. That is, even if the group concept were a priori, we would not think it applied to motion if there were no approximate solid body motions. The point is that unlike arithmetic, Poincaré views geometry as depending crucially on both humans and the world having specific physical properties: we and other objects can move about while (roughly) retaining the rest of our properties (especially shape properties). For Poincaré this is an empirical precondition for the possibility of a mathematically codifiable system of length-measure.

Given the empirical preconditions on geometry, a natural thought is that geometry is empirical. However, Poincaré explicitly refutes this natural thought, arguing as follows. The subject matter of geometry is not actual physical bodies. If it were, it would be "experimental geometry", which would be refuted since physical bodies never precisely satisfy Euclidean definitions. They do not move exactly rigidly, and we cannot physically instantiate perfect circles, straight lines or right angles, for example. Yet mathematical geometry remains "rigorously true" (1902a/1952, 50), so its subject matter must be ideal objects (ideal straight lines, ideal rigid bodies, etc.). Though our experience of approximate rigid bodies provides an empirical precondition for our having the geometrical concepts (70), the subject matter of (pure) geometry is ideal.[7]

Poincaré's preconditions thus contribute to an explanation of geometry as a mathematical subject matter; they make sense of the fact that beings like us in a world like this would do geometry. And they largely define the subject matter of (traditional) geometry. As the simplest option to roughly model our sense experiences, Euclidean geometry is natural but not forced on us. (All of the options are mathematically legitimate.) Thus, he argues that experience also guides us in a second, posterior, sense – in providing criteria for choosing between the mathematical options.

Empirical Information: The Assessment of Geometry

Even though the options are severely limited by Poincaré's preconditions, there are options. So the scientist can raise the question, which geometry is the true model of space? Of course Poincaré famously ridicules this question, comparing it to that of whether the use of meters or yards is the "true" way to measure length. But he does argue that physics *guides* the more explicit choice between geometric systems via criteria such as simplicity and convenience. Whereas the empirical pre-conditions influence us in a pre-scientific sense (explaining the possibility, or maybe even likelihood, of the pure mathematical work), posterior empirical conditions include scientific evidence. Geometry begins with our bodies in the empirical world; and it returns to the empirical world after its mathematical development. At this point, Poincaré argues, convenience, or simplicity, guides the evaluation of a larger system that includes physics *and* geometry.[8]

In this context Poincaré emphasizes the intermediary status of geometric conventions. Though there cannot be a crucial experiment that determines the "true" geometry, testing guides us in our choice.

> In fine, it is our mind that furnishes a category for nature. But this category is not a bed of Procrustes into which we violently force nature, mutilating her as our needs require. We

[7]It might be objected here that since mathematical geometry is about ideal objects, Poincaré's emphasis on the empirical preconditions for geometry – for its "genesis" – may simply result from confusing the context of discovery with the context of justification. That is, just because certain empirical conditions need to be met in order to account for the existence of geometry does not mean that, once established, the empirical preconditions are relevant to its subject matter. After all, we also need blood in our brains to do arithmetic; but arithmetic is still a bona fide a priori area of mathematics despite this precondition. The difference is that there being blood in the brain is a mere precondition; and has no bearing on what arithmetic is about. In contrast, the empirical preconditions in part *define* the subject matter of geometry: systems of rigid body motions. This context of discovery, in other words, is relevant to the subject matter of geometry in a way unlike the arithmetic case.

[8]Of course, a physical theory that is not empirically adequate would be very inconvenient!

> offer to nature a choice of beds among which we choose the couch best suited to her stature (Poincaré 1898/1996, 1011).

Nature has a stature, which testing and empirical evidence help to reveal. Empirical results guide our choice of a best "couch" for nature's "stature". Remarks like these, where Poincaré emphasizes that conventions are choices *guided by empirical evidence*, are central to my interpretation of conventions as responsive to experience, rather than merely fixed by stipulations in advance of physical testing.

Of course Poincaré also, repeatedly, emphasizes that experience does not make the choice *for* us. The reason is that the various geometric options are all consistent with experience – *provided* one is willing to modify, or add, other physical hypotheses. Chapter V of *Science and Hypothesis* is largely an argument that a geometric system cannot be *directly contradicted* by experience (1902a/1952, 75). The "Euclidean hypothesis" and the "non-Euclidean hypotheses" can both always be used to *interpret* a series of experiments (76); this is why geometric empiricism is meaningless (79).

But any part of a scientific theory can be protected if we are willing to adjust other parts. This is essentially Quine's holism. To be distinctive, conventionalism must connect some special feature of the convention in question, such as its position in the hierarchy, with the *propriety* or *impropriety* of such protection schemes. That is, to distinguish conventionalism from holism, it must be committed to the view that conventions *are*, and *should* be, more protected from refutation than the more empirical parts of science.[9]

Indeed this is Poincaré's view. His remarks about untestability, inter-translatability, and geometry's privileged place in the hierarchy all support a view of geometry as rather like a linguistic or conceptual framework, which Michael Friedman explains rather well:

> [G]eometry cannot depend on the behavior of actual bodies. For, according to the above-described hierarchy of sciences, the determination of particular physical forces presupposes the laws of motion, and the laws of motion in turn presuppose geometry itself: one must first set up a geometry before one can establish a particular theory of physical forces. We have no other choice, therefore, but to select one or another geometry on conventional grounds, which we then can use, so to speak, as a standard measure or scale for the testing and verification of properly empirical or physical theories of force (Friedman 1999, 78).

To determine what forces there are on objects, involves determining which motions are non-inertial motions; and this presupposes some geometric principles (by which

[9]As mentioned above, along these lines, Stump argues that in contrast with Quine, Poincaré's holism is not general, but limited to special cases. Poincaré rejects Newtonian absolute space, and any substantival understanding of space. So adjustments in geometry are legitimate because geometry is not describing a thing with its own properties (space); rather it is just a tool for describing the relationships of bodies. (And Poincaré comes into conflict with GR precisely here since GR is essentially a substantival theory of space.) Whereas I am connecting the degree of protection a principle gets to its position in the hierarchy, Stump is furthering an account of a deeper reason *why* something is a convention, and thus why it occupies a protected position in the hierarchy. See Stump 1989, section V.

we can decide shortest distances and the like). But if such measurements presuppose a system of geometry, then we cannot *decide* geometry *from* them.

This gives geometry a special status (which GR of course rejects). If geometry is prior to testing, and more like a linguistic framework than an empirical hypothesis, then good scientific practice means it *should* be more protected than the empirical aspects of the theory. Along these lines Poincaré argues elsewhere that science would not be fruitful if, in response to negative data, scientists routinely changed the meanings of some terms rather than the empirical content of a theory (1905a/1958, 123). This shows that Poincaré thought that (even if there is no sharp boundary) there is a workable distinction between linguistic and empirical principles. Good scientific practice, then, would seem to recommend the protection of convention along with the explicit linguistic, analytic, principles presupposed.

The protection of geometric conventions, in particular, is furthermore supported by the fact that Poincaré regarded them as more fundamental than those of mechanics – the other "home" of conventions.

> The experiments which have led us to adopt as more convenient the fundamental conventions of geometry refer to bodies which have nothing in common with those that are studied by geometry. They refer to the properties of solid bodies and to the propagation of light in a straight line. These are mechanical, optical experiments. In no way can they be regarded as geometrical experiments (1902a/1952, 136–137).

Geometry is about *ideal* objects, whereas mechanics is about *real* objects.

> On the other hand, the fundamental conventions of mechanics and the experiments which prove to us that they are convenient, certainly refer to the same objects or to analogous objects. Conventional and general principles are the natural and direct generalisations of experimental and particular principles (137).

Principles are empirical laws that have been elevated, but geometric conventions were never empirical.

> Principles are conventions and definitions in disguise. They are, however, deduced from experimental laws, and these laws have, so to speak, been erected into principles to which our mind attributes an absolute value (138).

For Poincaré, then, geometric and mechanical principles are both conventional, in that they both contribute to a testing *framework*. But geometric principles are about ideal objects, and thus they are more abstract than mechanical principles.

To summarize, there are at least three ways in which Poincaré regards geometric systems as similar to linguistic conventions: (i) a geometric system must be fixed prior to testing some physical hypotheses (geometry is relatively necessary); (ii) any of the main geometric "languages" can be substituted (inter-translated) with some systematic changes in the description of the results; and (iii) geometry is about ideal, rather than real, objects. Because geometric principles are more ideal, they closer to mathematics and more like a language than mechanical principles. They will thus naturally be more protected from refutation – on grounds consistent with good scientific practice. So, the vision is the following: all scientific conventions should be somewhat protected from empirical refutation; and geometric conventions should arguably be the most protected since they are the most ideal, or the closest to linguistic conventions. At the two extremes, the truths of pure mathematics and logic are absolutely protected and the experimental areas of science are much more open to revision.

The problem of relativity is that it shows that scientific practice violates this account, which seems to require the relatively strong protection of geometry in the light of new evidence. Can Poincaré explain what leads physics to adopt a new, empiricist approach to space? I will argue below that geometric conventionalism is not as inconsistent with GR as it may seem. Though its position in the hierarchy means geometry is *more* protected than mechanics and the experimental sciences, conventionalism dictates that applied geometry is not to be *absolutely* protected.

The Revisability of Geometry

Poincaré regarded geometric choices as revisable in the light of empirical evidence; this follows simply from the fact that all conventions are subject to empirical checks. Regarding mechanics, he remarks, "if a principle ceases to be fecund, experiment without contradicting it directly will nevertheless have condemned it" (1905a/1958, 110). A convention can thus be fruitful or not without being directly verified or contradicted. It can also "cease to be fecund", so it can be fruitful for a while, after which it is less fruitful. This speaks to a view of scientific theory as in flux, and a view of conventions as responsive to change. That is, changes in scientific evidence and its interpretation can lead to changes in conventions. Is there any reason to think that this attitude applies to mechanical conventions only?

Poincaré is unclear on this point. For example, he writes, "Which . . . . [of the Lie groups] shall we take to characterize a point in space? Experiment has guided us by showing us what choice adapts itself best to the properties of our bodies; but there its role ends" (1902a/1952, 88). This seems to imply that experience plays a pre-conditional role only, after which its "role ends". Yet he writes earlier in the same chapter about the possibility of revising geometry after an initial choice, in the light of experimental data:

> If, therefore, we were to discover negative parallaxes, or to prove that parallaxes are higher than a certain limit, we should have a choice between two conclusions: we could give up Euclidean geometry, or modify the laws of optics, and suppose that light is not rigorously propagated in a straight line (73).

Here a different picture is forwarded, whereby a geometric choice might need to be reconsidered after the fact, owing to later experimental results.[10] Thus Poincaré

[10]Of course Poincaré famously did not think we would ever make such a choice; continuing: "It is needless to add that every one would look upon this solution as the more advantageous. Euclidean geometry, therefore, has nothing to fear from fresh experiments" (1902a/1952, 73). This confidence was probably based on the assumption that the alternative would require too deep of a shift to be worth the scientific and conceptual upheaval. Indeed, relativity was a revolutionary shift, involving

makes apparently inconsistent remarks about the relationship between experience and geometry.

Admitting that there is a lack of consistency, or clarity, I nevertheless think that the more flexible interpretation is more charitable to Poincaré. As he says:

> If our experiences should be considerably different, the geometry of Euclid would no longer suffice to represent them conveniently, and we should choose a different geometry (Poincaré 1898/1996, 1011).

Geometric choices, like other conventions, can cease to be fruitful; and in this way they, like other conventions, are answerable to future scientific findings and choices.

How flexible might Poincaré have been had he lived long enough to know about GR? GR does not simply choose a different geometry from among the three geometries of constant curvature, which is what Poincaré admits as possible. Instead, it introduces a new relationship between space and physics. So we cannot merely invoke his views about choice on the basis of the two quotes above; we must also extend his views.

As we saw above, Poincaré essentially articulates the two basic interpretive options for understanding the crucial results about space regarded as supporting GR.[11] Space (empty) can be fixed as Euclidean, and we can interpret results such as parallax and planetary orbits as physical relationships involving mass, gravity, etc.; we can say, for example, that light bends owing to the gravitational force exerted on it by massive bodies. Alternatively we can adopt a new paradigm, stipulating that light travels in straight lines, or shortest paths, and accept that space is a non-Euclidean, variably curved manifold. On this second alternative what has shifted is which aspect of the theoretical framework is playing the conventional role. For example, it is not just that we have given up the stipulation of Euclidean geometry; in its place is a new stipulation about light (among many other changes of course).[12] The choice at this point depends on the criterion of overall simplicity in the light of the two scientific "packages".

The more modern, holist/empiricist understanding behind GR would I suspect have been difficult for Poincaré, for reasons I will sketch below. Still, in defense of conventionalism more generally, he could point out that GR still requires the stipulation of a convention – that light travels along shortest paths. In addition, in

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changes at many levels, including conceptual levels, and resulting in new relationships between mathematics and physics. Nevertheless Poincaré recognized the *possibility* of revising a geometric choice when faced with new data; it was not the *policy* of conventionalism to prohibit such changes. He asserted that we *would* not adopt a different geometry, not that we could not or should not.

[11] Friedman points out, interestingly, that Poincaré's conventionalism is the only option to GR that Einstein himself recognized (Friedman 2001, 111).

[12] Similarly, Friedman argues that the special theory of relativity proceeds "in perfect conformity with Poincaré's underlying philosophy in *Science and Hypothesis*, by 'elevating' an already established empirical fact into the radically new status of what Poincaré calls a 'definition in disguise'" (Friedman 2001, 111).

its favor is a gain in overall efficiency, which is what conventionalism generally endorses. Nevertheless, it must be conceded that GR does seem to clash pretty squarely with geometric conventionalism.

In addition, several more specific factors favor the view that Poincaré would have (at least initially) preferred the first option – that of stipulating that empty space is Euclidean and treating the relevant results as physical. Firstly, and most obvious, is Poincaré's consistent defense of Euclidean geometry. Secondly, Poincaré does distinguish between mathematics and physics; and he aligns (pure) geometry with the mathematical. For example, he writes that:

> experience brings us into contact only with representative space, which is a physical continuum, never with geometric space, which is a mathematical continuum. At the very most it would appear to tell us that it is convenient to give to geometric space three dimensions, so that it may have as many as representative space (1905a/1958, 69).

There are at least three "spaces" for Poincaré (possibly more if we differentiate between the "spaces" of our different senses): perceptual – which is involved in the pre-conditions for geometry; mathematical – the study of the idealized objects after the mathematical systems are developed; and physical – the "space" of our best physical theories, including the behavior of light and masses in relation to one another. (On this point see also Stump 1989.) The separation of mathematical/geometric space from the "space" of GR is consistent with his differentiation between mathematical space and physical space.

Third, it is not clear that Poincaré regarded Riemannian, variably curved, "geometry" as a *bona fide* geometry. On the one hand, his insistence on generality and the iterability of mathematical operations leads him to dismiss geometries of variable curvature as merely "analytic" (1902b/1982, 63). Distinctive of mathematics, he argues, is generality and the fact that induction applies to its processes (1902a/1952, Chapter I). For geometry to be genuinely mathematical, its constructions must be everywhere iterable, so everywhere possible. If geometry is in some sense about rigid motion, then a manifold of variable curvature, especially where the degree of curvature depends on something contingent like the distribution of matter, would not allow a thoroughly mathematical, idealized treatment. Yet Poincaré also writes favorably about Riemannian geometries, defending them as mathematically coherent (1902c/1994, 163–164). Furthermore, he admits that geometries of constant curvature rest on a hypothesis – that of rigid body motion – that "is not a self evident truth" (1902b/1982, 61). In short, he seems ambivalent.

Whether his conception of geometry includes or rules out variable curvature is unclear. I think we can surmise that he recognized Riemannian geometry as mathematical, and interesting, but as very different and more abstract than geometries of constant curvature, which are based on the further limitations discussed above (those motivated by a world satisfying certain empirical preconditions). These limitations enable key idealizations, which in turn allow constructions and synthetic proofs that we recognize as "geometric".

In any case, it remains plausible that Poincaré would have viewed GR's codification of space as more or less detaching physical "space" from (mathematical) geometry. He might, in other words, still think of mathematical, inert, space as relational, homogeneous, etc.; but physical "space", as GR conceives it, is substantival, for it has an effect on the behavior of objects in it, and it is not homogeneous. Since Poincaré rejects absolute space but accepts (at least the possibility of) the ether, his position on Euclidean geometry may thereby be salvageable along the following lines. Traditional (constant curvature) geometries are mathematical theories of space, while GR, in effect, forwards a physical theory about the ether (one that entails, in fact, that *none* of the ordinary mathematical-geometric options apply).[13] Though (mathematical) geometry may be conceptually prior to physics, and also more ideal than mechanics, we can change our decision about *which* geometric structure should be presupposed, if *any*. "If our experiences should be considerably different, the geometry of Euclid would no longer suffice to represent them conveniently, and we should choose a different geometry" (1898/1996, 1011). If experience could lead us to choose a different geometry, then neither geometry's position in the hierarchy, nor its similarity to a language, ensures it either absolute protection or scientific applicability.

Conclusion

Convention is an invention that plays a distinctive role in Poincaré's philosophy of science. In terms of how they contribute to the framework of science, conventions are not empirical. They are *presupposed* in certain empirical tests, so they are (relatively) isolated from doubt. Yet they are not pure stipulations, or analytic, since conventional choices are guided by, and modified in the light of, experience. Finally they have a different character from genuine mathematical intuitions, which provide a fixed, a priori synthetic foundation for mathematics. Conventions are thus distinct from the synthetic a posteriori (empirical), the synthetic a priori and the analytic a priori.

The importance of Poincaré's invention lies in the recognition of a new category of proposition and its centrality in scientific judgment.[14] This is more important than the special place Poincaré gives Euclidean geometry. Nevertheless, I think it's possible to accommodate some of what he says about the priority of Euclidean geometry with the use of non-Euclidean geometry in science, including the inapplicability of any geometry of constant curvature in physical theories of global space. Poincaré's insistence on Euclidean geometry is based on criteria of simplicity and convenience. But these criteria surely entail that if giving up Euclidean geometry somehow results in an overall gain in simplicity then that would be condoned by conventionalism.

[13]Interestingly, Einstein also saw GR as a kind of ether theory. Einstein 1920, referenced in Stump 1989, 362, note 104.

[14]Indeed, as Wittgenstein argues, in *any* domain of objective, empirical judgment (See Wittgenstein 1969).

The a priori conditions on geometry – in particular the group concept, and the hypothesis of rigid body motion it encourages – might seem a lingering obstacle to a more flexible attitude towards applied geometry, or an empirical approach to physical space. However, just as the apriority of the intuitive continuum does not restrict physical theories to the continuous; so the apriority of the group concept does not mean that all possible theories of space *must* allow free mobility.[15] This, too, can be "corrected", or *overruled*, by new theories and new data, just as, Poincaré comes to admit, the new quantum theory might overrule our intuitive assumption that nature is continuous. That is, he acknowledges that reality might actually be discontinuous – despite the apriority of the intuitive continuum (1913/1963; compare p. 44 and chapter VI).[16]

As with quantum mechanics, so with relativity: the practice of science depends both on our making certain conventional choices and on our treating these choices as provisional; they are revisable in the light of further experience and better, more efficient theories. For these reasons I have urged an interpretation of Poincaré's conventionalism that de-emphasizes the rigidity of conventions and the hierarchy in which they exist, and focuses instead on their flexibility and responsiveness to new data and new theories. If this is right, then conventionalism is closely continuous with, rather than distinct from, recent work on the more flexible "relativized", or provisional, a priori.

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[15] Recall, the group concept is part of an explanation of why we are geometrical beings, and why we are naturally limited to a small class of geometries.

[16] Apriority for Poincaré does not thereby guarantee applicability; this is just one way his vision seems quite different from Kant's.

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