Simplicity, Conventionalism and the Poincaré-Russell Debate

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I. Introduction

Poincaré's appeal to the notion of simplicity in his articulation of conventionalism has been a major subject of debate and discussion, particularly in light of his oft-maligned claim that we should always prefer Euclidean geometry over its alternatives on such a ground. Poincaré himself never offers an account of simplicity (or indeed what generally constitutes a proposal being the more 'advantageous') save in the broadest possible terms. In what follows I will first sketch out Poincaré's 'conventionalist' views, particularly in contrast to the logicist program—exemplified here by Russell—and then proceed to examine an account of 'simplicity' derived from algorithmic information theory. It is the task of this paper to argue that the notion can be made into a somewhat more rigorously defined aspect of the inferential process, which in Poincaré includes sundry non-logical devices. Unfortunately, Poincaré lacks clarity in his discussion of these, a weak point that ought to be addressed if his critiques of Russell are to be convincing. An adequate formalization of simplicity directly bolsters Poincaré's anti-logicism by providing adequate grounds for its use as a methodological constraint without contravening the central theses of his conventionalism.

II. Conventionalism

(a) Russell and Poincaré

In 1897, Bertrand Russell published the *Essay on the Foundations of Geometry,* a classic statement of the logicist approach to geometry, receiving overwhelmingly positive reviews—save perhaps for the lone dissenting voice of the mathematician Henri Poincaré, whose pugnacious dissection in an issue of *Revue de Métaphysique et de Morale* prompted Russell to respond rather pointedly to the Frenchman's criticisms.[1] Among Poincaré's more vigorous thrusts was the demand for the definition of a number of 'geometric primitives', a request which for Russell indicated that his interlocutor had somehow managed to miss the entire point of the exercise:

> Mr. Poincaré requests "a definition of distance and of the straight line, independently of (Euclid's) postulate and free from ambiguity or vicious circle." Perhaps he will be shocked if I tell him that one is not entitled to make such a request since everything that is fundamental is necessarily indefinable.[2]

But of course Poincaré understood very well that for Russell geometric primitives must acquire meaning before they can be used to formulate propositions, such as in a axiom system. What Poincaré found unsatisfactory was Russell's account of how the primitives become meaningful in the first place, through 'acquaintance' or 'intuition', not definition. As he laments in his review, Poincaré himself is "thoroughly deprived" of such an intuition as of the "equality of two distances," let alone one robust enough to found upon it all of geometry. [3]

Such an intuition is presumably required in order to make sense of Russell's claim that the metrical structure of the universe can be determined by experiment.[4] This empirical determination of physical geometry is precisely the Russellian claim Poincaré will seek to undermine in the interconnected arguments and thought-experiments of *Science and Hypothesis*. Russell's notion is that

> the Euclidean and non-Euclidean spaces give the various results which are *a priori* possible; the axioms peculiar to Euclid which are properly not axioms, but empirical results of measurement determine, within the errors of observation, which of these *a priori* possibilities is realized in our actual space. Thus measurement deals throughout with an empirically given matter, not with a creature of

[1] An episode discussed in Coffa [1991], whose presentation I am indebted to in what follows.
[2] Quoted ibid. 130.
[3] Ibid., 132.
[4] Indeed, right in the opening remarks of the *Essay on the Foundations of Geometry* he declares that the "axioms which distinguish Euclidean from non-Euclidean spaces … will be regarded as wholly empirical." Russell [1897] 6.

> the intellect, and its *a priori* elements are only the conditions presupposed in the possibility of measurement.[5]

Poincaré by contrast argues instead that no experiment could possibly count as determining the physical geometry of space and, as we have seen, a major part of his argument was the disparagement of Russell's rather mysterious epistemology—an easy enough target.

Certainly the classical logicist programme insisted that purely foundational questions had everything to do with metaphysics (of, say, objects and propositions in Russell's case) and nothing at all with psychology. The axioms must contain all necessary information for the carrying out of inferences, which themselves are 'topic-neutral' and of a purely logical character.[6] This is to a certain extent at odds, at least for Russell, with the belief that the primitives are given in acquaintance, a weakness Poincaré was all too happy to exploit, characterizing Russell's views thus:

> …it is undoubtedly by analysis of perceived objects that we obtain acquaintance with what is *meant* by a straight line in actual space.[7]

In order to maintain philosophical consistency, Russell would have to explicitly endorse (as Frege did in talk of a 'third realm') both the independent existence of 'primitives' and some way we get to know them—the former taken for granted, and the latter a mere epistemological question he often dismissed as irrelevant, despite Poincaré's prodding on the subject.[8] Indeed, while he talks in the *Essay* about the determinable and "irreducible metrical properties of space"[9] nothing is said concerning how we come to know these fundamentals, only that they *must* be—and that we can come to see them clearly in intuition, such that "the mind may have that kind of acquaintance with them [the indefinables] which it has with redness

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[5] ibid., 147 (§140).
[6] I adopt this terminology from Detlefsen [1992].
[7] Poincaré quoted in Coffa [1991], 390 (fn. 18).
[8] "How we discover two actual spaces to be equal is no concern to the geometer; all that concerns him is the existence of equal spaces." Russell quoted ibid., 132.
[9] Russell [1897], §37.

or the taste of of a pineapple."[10] Needless to say such an answer was not seen as particularly satisfactory by Poincaré, who offered an alternative interpretation of the issue.

(b) Conventionalism in Science and Hypothesis

It is at this point that conventionalism emerges: for Poincaré insists that geometric primitives are *not* 'somehow' known in acquaintance prior to the elaboration of geometric axioms, but are instead implicitly *defined* by the axioms themselves. This was what Poincaré drew from the elaboration of non-Euclidean geometries: the 'primitives', be they 'point', 'distance', 'straight line' and so on, have no meaning prior to actual geometrical construction, neither in reference to actual physical objects (e.g. light rays) or to Platonic ones.[11] It is sufficient to have them determine meaning, not be meaningful (or informational) themselves. More radically still, he also seems to hold that the inferential process itself involves holistic extra-logical 'epistemic condensers' that "abridge our reasoning and our calculations," a necessary component of the extension of mathematical knowledge in a proof, for

> when a logician shall have broken up each demonstration into a multitude of elementary operations, all correct, he still will not possess the whole reality; this I know not what which makes the unity of the demonstration will completely escape him … now pure logic cannot give us this appreciation of the total effect.[12]

This is in quite stark contrast to the logicist view, that logic is homogeneous and globally valid. Instead, there is a choice of 'architecture' to be made—conventionalism is a thesis not only about axioms, but about inferences, the way in which premises are brought together to form something more than an empty sequence of

[10] Russell, quoted in Coffa [1991]: 126.
[11] Coffa puts it best: "Geometry does not depend on geometric *objects*, whether they be Platonic straight lines or Millian light rays; all it needs in order to have a life of its own is geometric concepts or meanings." Ibid., 134.
[12] Poincaré quoted in Detlesfen [1992], 360; 361. The term 'epistemic condenser' is from Detlesfen as well. Elsewhere Poincaré says "an accumulation of facts is no more science than a heap of stones is a house." Poincaré [1952]: 141.

propositions.[13] Poincaré is unfortunately vague on this topic in *Science and Hypothesis*, despite the clarity of his exposition concerning logical and mathematical matters. This point will become important when we look more closely at Poincaré's methodological theses in section III, after we attempt to articulate a clearer concept of simplicity.

It is commonplace to note the distinctly neo-Kantian flavour of Poincaré's conventionalism—that the implicit definitions are constitutive in a fashion similar enough to the 'forms of sensible intuition,' but the important difference comes with the replacement of the synthetic *a priori* with the notion of a 'convention', an experiment-guided implicit definition. Geometrical axioms cannot be synthetic *a priori*, Poincaré argues, because

> they would then be imposed upon us with such force that we could not conceive of the contrary proposition, nor could we build upon it a theoretical edifice. There would be no non-Euclidean geometry.[14]

Poincaré does not generally deny the existence of the synthetic *a priori* (of which arithmetic would be an example), but only maintains that the existence of several incompatible geometries demonstrates that none of them can possibly be so. Neither can they be empirical or synthetic *a posteriori*, however, as we have already seen. Hence Poincaré also states in the same passage quoted above that geometry cannot be subject to experimentation, for it would no longer be an 'exact science'—there is no such thing as an "rigorously invariable solid," i.e., the kind of ideal object which geometry purports to be about.[15] Certainly this is not Poincaré's strongest argument in *Science and Hypothesis* for the non-empirical nature of the axioms of geometry—we will turn to another in a moment—but the passage

[13] For more on this, see Detlefsen [1992] 364-367. I've eliminated important details on the difference between mathematical and logic proof from Detlefsen's discussion in favour of a more general overview. Poincaré certainly holds that truths of arithmetic are *a priori*; but he objects strenuously to the logicization of mathematical proof as a purely syntactic procedure.
[14] Poincaré [1952], 48.
[15] ibid., 49.

does offer a rather succinct formulation of his understanding of convention, from which we can glean several theses:

1. That while choice of convention is *guided* by experiment, it is still essentially free;
2. Famously, that the "axioms of geometry … are only definitions in disguise;"
3. There is no sense in asking whether Euclidean geometry is true; it is a meaningless question.
4. Geometries are chosen not on the basis of their 'truth', but of their convenience.
5. The main criterion of convenience is *simplicity*, which is not merely a function of our mental habits, but a property of the geometry itself.[16]

Several comments can be made here. Most important for our purposes now is the remark made by Poincaré on simplicity in this passage, writing that Euclidean geometry is "simplest in itself, just as a polynomial of the first degree is simpler than a polynomial of the second degree."[17] While Poincaré is once again unclear on what this exactly means, it certainly suggests that a more rigorous notion of simplicity is possible. Indeed, a formalization of simplicity would not obviously contravene any of Poincaré's theses above, particularly considering none of it is meant to extend to mathematics—considered safely *a priori*. Whatever kind of thing simplicity *is*, it seems safe to think it could be a property of a concept or theory (or 'reality'), and not an arbitrary fabulation.

Another crucial point we should underline is the extent to which experiment *can*, in fact, guide us in our choice of theory. For while expediency may always counsel according to Poincaré a Euclidean geometry, we are also told immediately that beings with minds like ours could conceivably posit a non-Euclidean in response to "suitably chosen external-world impressions."[18] Experiment cannot *determine*

[16] Compiled from ibid., 50.
[17] ibid., 50.
[18] ibid., 51.

(as Russell would have it) the ultimate 'true' geometry, but it certainly can suggest the most convenient one—convenience dictated by several ill-defined factors, including simplicity but also the inertia of acquired hypotheses and habits, should we be presented the same 'non-Euclidean' impressions. [19] Poincaré's famous thought-experiment here is worth recounting in some detail before we turn to a closer look at simplicity itself, as it provides more compelling reasons to reject the synthetic *a posteriori* view.

(c) The Thought-Experiment and Problems of Theory-Choice

Poincaré's asks us to imagine a finite Euclidean world "enclosed in a large sphere" whose temperature is greatest at the center and decreases as we move outwards following the law R^2-r^2, where R is the radius of the sphere and r the distance from the centre. In this imaginary world, all objects contract and dilate in accordance with the temperature in a uniform and instantaneous fashion. Should the inhabitants of our imaginary world attempt to determine the physical geometry of their surroundings using measuring rods, they would conclude that they lived in an infinite Lobachevskian plane. Not only can they never reach the limits of their universe (as they shrink from cold the closer they get, hence concluding it is unbounded and infinite), but "geodesics are not straight lines but the geodesics of Lobachevski, and the ratio of circumference to radius is always greater than 2π".[20]

Poincaré's point, of course, was that we are in a precisely analogous situation to the inhabitants of his imagined world. The spatial experiences of these hapless folk would be entirely non-Euclidean, yet, as we know, their world can also in fact be interpreted as properly Euclidean—only with special physical laws in force. And

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[19] ibid., 71.
[20] As paraphrased in Sklar [1977] 92. Readers should note I am skipping over the 'refraction medium' force which Poincaré postulates to 'fool' the inhabitants who may be inclined to experiment with light rays.

for all we know the very same could be true of us: though our experiences are Euclidean, no observational evidence could 'prove' this was the correct physical geometry. As Poincaré notes, "experiment plays a considerable role in the genesis of geometry; but it would be a mistake to conclude from that, that geometry is, even in part, an experimental science." [21] From this Poincaré infamously concludes that Euclidean geometry need not fear fresh experiments, as its simplicity will always triumph.[22] We may simply hold on to our geometry by modifying physical laws appropriately.

A later general summation of the problem is given by C.G. Hempel:

> ...the test of a physical geometry G always presupposes a certain body P of non-geometrical physical hypotheses (including the physical theory of the instruments of measurement and observation used in the test), and that the so-called test of G actually bears on the combined theoretical systems G - P rather than on G alone. Now, if predictions derived from G - P are contradicted by experimental findings, then a change in the theoretical structure becomes necessary.[23]

So with a little reflection we see that by appropriate modification of P, the physical theory, we could preserve any given G, and vice-versa. The experimental evidence does not help settle the issue of the 'truth' of G, for several theories with different, mutually incompatible geometries could have the same observational consequences. In the end, the choice of G is one of convenience, not of experiment. However—as Hempel rightly points out—Poincaré's assertion that a Euclidean geometry will always be simpler can be belied by the simple thought that what is at issue is not simply the geometry, but

[21] Poincaré [1952] 65. Much can be said over whether the geometry of general relativity is 'conventional', a term whose meaning varies enormously from Poincaré to Carnap to modern 'conventionalists' such as Dummett or Sklar. Mormann [2005] has an argument suggesting that conventionalism in the 'Carnap' sense is belied by results in mathematical topology. Discussing these would go far beyond the bounds of the present essay. Several authors have vigorously attacked conventionalism in the post-Quine era. But we are concerned here with Poincaré in a more modest historical setting.

[22] The 'parallax' example is the most often cited of this. ibid., 73.

[23] Hempel, C.G. [1945] *Geometry and Empirical Science. §6*

the physics as well; and as such the total simplicity of a non-Euclidean geometry coupled with appropriate physical laws may be such that it is preferable to the Euclidean alternative.

Thus this ungainly appendage—of the unimpeachability of Euclid—can safely be removed; and it is hardly the most radical or interesting claim we can take away from Poincaré. It has been suggested that in fact Poincaré is not merely suggesting that it is always possible merely *in principle* of creating many empirically identical theories. Rather, "he makes the much stronger claim that there is a constructive method for actually producing such equivalent descriptions," which is guaranteed by the inter-translatability of geometries.[24] If this is so we have all the more reason for attempting to seek out principled methods of theory choice as a means of choosing *well* (given interests and goals), if not 'correctly'—a supposedly meaningless question. This is why we flagged earlier the particular, peculiar natures of both axioms *and* inferences. The real possibility of alternative constructions requires an understanding of the conceptual issues in play that far outstrips the dull automaton's syntacticism put forward by Russell, where choice boils down to some *experimentum crucis*. But that's not all there is surrounding the construction of theories and their subsequent adjudication. Thus in the next section we will examine the possibility of formalizing Poincaré's central methodological criterion—simplicity—using insights from algorithmic information theory and proceed to examine whether a more rigorous development can bolster the view that the physical geometry of space cannot be determined by experiment. Several authors have put forth arguments linking well-defined simplicity to truth, or at least increased probability, in inductive reasoning frameworks. At the very least a clearer grasp of simplicity may help save Poincaré from resembling Russell too much in his use of obscurities. For all his gainsaying of Russell's own strange

[24] Ben-Menahem [2001], 487.

commitments we are little better off left at the vagaries of intuitive concepts such as 'simplicity' or 'convenience'.

III. Simplicity *non Simpliciter*

(a) Algorithmic Information Theory

A formal characterization of simplicity faces two obvious difficulties: first, an adequate definition must be given of what simplicity consists of; and second, the measure of simplicity must be preserved across logically equivalent transformations. Now simplicity (or parsimony—I will treat them as interchangeable[25]) is often tied to the application of Occam's razor ('*entia non sunt multiplicanda praeter necessitatem*') but expressed as such it is at best an *ad hoc* methodological heuristic, certainly not a piece of first philosophy declaring the simplicity of Nature itself. This remains more or less true today despite valiant efforts made since by several authors to add some rigour to the use of simplicity in formal induction processes, an approach which adds the additional problem of creating and justifying a balance between descriptive accuracy and simplicity in the calculation of probabilities.[26] We'll call these issues the *characterization*, *translation* and *description* problems respectively.

The characterization and translation problems have been largely solved, at least in mathematics, and much headway has been made in extending the domains of application. Algorithmic information theory (often termed *Kolmogorov-Chaitin* complexity after its inventors) can quantify the amount of information found in a given string via

[25] There may be a fine distinction here between over-all simplicity and parsimony of parameters. A hypothesis may be simpler because it has several parameters giving it informational content; whereas incorporating in the hypothesis functions giving the parameters results in something more complex. Can't comment on the issue here—I'm in over my head as it is.

[26] Kemeny's [1953] discussion is widely viewed as seminal, along with the lesser-known Wrinch and Jeffreys [1921] paper. Jeffreys returned to the topic in Jeffreys [1961]. These predate the mathematical work in complexity theory I will turn to momentarily; I am indebted to Keuzenkamp & McAleer [1995] for these references.

straight-forward computational methods: it is equivalent to the length of the smallest program capable of producing the string. For example, the first few billion digits of Pi actually have very little information content, for they can be produced by a rather short algorithm. This also results in a definition of randomness, which is equivalent to incompressibility: a large sequence of fair coin tosses represented as a string of binary digits should have no function describing it other than the enumeration of its own content.[27] Fortunately for us, the mathematical features of complexity also give us a partial answer to the translation problem. While certainly complexity is in part a function of the expressing language chosen to 'encode' the string, it remains an objective property, as it has been demonstrated that an actual minimum code size for a string *does* exist—it is, however, not computable (for reasons linked to Turing's halting problem). Despite this,

> it can be shown that all reasonable choices of programming language lead to the quantification of the amount of 'absolute' information in individual objects that is invariant up to an additive constant. We call this quantity the 'Kolmogorov complexity' of the object.[28]

The possibility of calculating to this constant offers the tantalizing possibility of a "formal justification of Occam's razor," despite the fact that "the implementations of the resulting principle [will be] arbitrary."[29] This arbitrariness is due to the approximate nature of the complexity measure of the hypothesis.

The main idea behind previous attempts to link a formal notion of simplicity to induction has been to utilize it to formulate prior probabilities in a Bayesian context, which get smaller with the complexity of hypotheses—an idea developed in the fields of both

[27] I am indebted to several sources for the discussion here, including Keuzenkamp & McAleer [1995]; Li & Vitanyi [1997], [1992] and Rissanen [1983].

[28] Li & Vitanyi [1997], v. Interestingly, in the introduction from which this quote in taken, the authors single out "using the compressibility of strings as a selection criterion" as one of the possible applications of the theory. Ibid., vi.

[29] Keuzenkamp & McAleer [1995], 6.

machine learning and econometrics.[30] Why should we even believe this? As it turns out, there's a fairly simple and intuitive answer:

> If a hypothesis, H, explains the same evidence as a hypothesis G, but does so by postulating more entities than G, then, other things being equal, the evidence has to bear greater weight in the case of H than in the case of G, and hence the amount of support it gives H is proportionately less than it gives G.[31]

While the above argument speaks of 'entities' rather than informational content, it is easy to see how it could be extended to apply in the present context: we assign a lower prior probability to more informationally complex hypotheses on the grounds that a content-rich hypothesis requires more evidence overall than a slender one, which we will look at in more detail shortly. Although such a proposal seems *prima facie* plausible, we are still left without a good method of actually calculating what the prior probability should be. How do we move from a very approximate complexity measure to a reasonable prior probability figure? How do we decide on a balance between descriptive accuracy and simplicity?

The solution is due to Solomonoff, whose celebrated answer involved the creation of a unique 'universal' prior distribution which approximates rather well valid distributions in all but special cases. The idea is to use it as an alternative to the unknown prior probability in the Bayesian calculation, and it is created by considering the complexity of a given string or 'hypothesis' in a special manner. Since it can be shown that the complexity of a string/hypothesis is not contingent on the manner of encoding (up to a constant), we are able at the same time of defining a universal prior probability, given by considering the input of a recursive universal Turing machine U and the probability of it generating a program p:

> We think of the input to the reference prefix machine U as being provided by indefinite long sequences of fair coin flips. Thus, the

[30] ibid., 6; see also Li & Vitanyi [1992] 348-349. Conversely, the posterior probability *increases* if elaborateness correlates with the hypothesis 'fitting' the data more closely. The resolution of this antagonistic relationship constitutes the *description* problem.
[31] Katz, J. *Realistic Rationalism*. SOURCE. SOURCE. Source. Sauce.

probability of generating a program *p* for *U* is $P(p)=2^{-l(p)}$, where *P* is the standard 'coin-flip' uniform measure […] The Solomonoff-Levin distribution can be interpreted as a recursively invariant notion that is the formal representation of 'Occam's Razor': the statement that one object is simpler than another is equivalent to saying that the former object has higher probability than the latter.[32]

Despite the complex mathematical logic involved in the proof of this rather startling fact, the underlying idea is straightforward enough: simpler objects (i.e., with a low Kolmogorov complexity) are more likely for the simple reason that their generation from an appropriate, randomly-fed Turing machine has a higher probability. And this makes sense: an object's complexity needs to be *spoken for* in some sense, and the more needs to be explained, the more 'evidence' is required, providing some mathematical justification for appeals to simplicity. Solomonoff has created a rather sophisticated, and seemingly objective, inductive apparatus—which unfortunately has limited practical usefulness as a 'pure' objective measure given the non-computability of complexity. Approximations to it work well however, and it can be shown that many formal models of inductive reasoning can be derived from Solomonoff's theorem here, which form the basis of several machine learning models (such as e.g. character recognition).[33]

A crucial point to make is that no hypotheses are *discarded* during this process: a given string has multiple generating functions, and should it have a dense cluster of relatively short programs its probability is thereby increased: as Rissanen put it, "a string with many short programs in a universal computer gets assigned a high probability."[34] Due to computational limitations, of course, it is impossible to find *all* generating programs and thus the universal

[32] The full proof and discussion can be found in Li & Vitantyi [1992]: 352-. Needless to say I was scarcely able to follow the intricacies here! A 'prefix' Turing machine with an indefinitely long input tape; if the machine halts, the bits *p* it has read to that point is called its program.
[33] ibid., 362. This includes Gold's and Rissanen's pioneering work.
[34] Rissanen [1983]: 421.

probability assignment must be done in a rather modest fashion, using a restricted class of models "whose selection requires human ingenuity and intuition."[35] Because of this limited aspect of the formal inductive process, in the end we are not able to single out a specific hypothesis but rather we must

> …maintain all hypotheses consistent with the evidence and just transform the probability distribution on the hypotheses according to the evidence.[36]

This dovetails nicely into our concern with Poincaré's use of simplicity as a methodological criterion. Despite the formalization of simplicity in induction, we are not led down the garden path of ontological certainty thereby: indeed, we are still in no position to affirm a given hypothesis must be the 'correct' one in the Russellian sense. Instead it seems Poincaré's account of the inferential process is vindicated. We will discuss this in some more depth in what follows.

(b) Conventionalism and Simplicity, Redux

Of course the point of this digression into the niceties of information theory was to evaluate Poincaré's own use of 'simplicity' as a methodological criterion. Remarkably little in the secondary literature has been said on the remarkable intersection of information theory and Poincaré; most author's exegetical accounts of Poincaré are content to repeat the assertion that we are to choose a physical geometry based on 'usefulness', 'convenience' or 'simplicity'. It is never perfectly clear whether these are the same one idea expressed different ways or multiple, genuinely different, methodological criteria.[37] In fairness, Poincaré himself is unclear on the topic. He

[35] ibid., 422. Note again the similar bald appeals to 'intuition' as in Poincaré.
[36] Li & Vitanyi [1992]: 362.
[37] For example: "In geometry, the choices we make are arbitrary … [but] they are influenced by experience, for we may find the consequences of one choice easier, more convenient, or simpler." Gower [1997]: 658. Gower does not elaborate here. Neither does Michael Friedman: convention for Poincaré, Friedman explains, is based "on the greater mathematical simplicity of the Euclidean system." Friedman 2000: 370. Examples of the sort could be multiplied endlessly. The guilt extends to this paper in the equivocations over simplicity and parsimony.

alludes constantly throughout *Science and Hypothesis* to the 'instincts' or non-formal 'reasons' of scientists, which are to be trusted—for the most part. For instance, discussing the refusal of the Académie des Sciences to consider alleged methods of squaring the circle, though it was only proved impossible in 1885, Poincaré says the Académie "knew quite well that its instinct did not deceive it."[38] Elsewhere, Poincaré writes that "most important of all, the man of science must exhibit foresight."[39] These are not rigorous notions, nor indeed notions even admitting of the possibility of rigour. We can conceive of them as *epistemic virtues*, perhaps much in the same fashion Aristotle perhaps understood the moral virtues. Set into the framework of science, they become the 'condensers' alluded to earlier. This can be (and in fact is) most of the time unremarkably justified in terms of expediency or pragmatics: the virtues *work* and require no more excuse from us. Poincaré certainly treats some of them rather lightly, as attitudes necessary to treat the collection of facts as a coherent whole.

For Poincaré, the move from a single fact to a prediction involves a certain *leap*, one which the methodology of science must oversee—generalisation from the particular instance of an experiment. Poincaré treats this as a type of *analogical* process:

> The circumstances under which one has operated will never again be reproduced simultaneously. All that can be affirmed is that under analogous circumstances an analogous fact will be produced. To predict it, we must therefore invoke the aid of analogy—that is to say, even at this stage, we must generalise.[40]

This process invokes 'preconceived ideas' of a non-scientific sort that simply cannot be put aside. Their existence *makes* science possible—and there is a parallel to Hume, here—but the salient point is that the inferential process in science does not proceed with the utmost logical rigour. It involves intuition, creativity, correction, assumption,

[38] Poincaré [1952]: 192.
[39] Ibid., 141.
[40] ibid., 142.

preconception, foresight and myriad other vague qualities that can hardly be defined, but are necessary for the transformation of inert 'fact' into an architecture of meaning.[41] These ideas, of course, appear in a vague and often contradictory fashion in Poincaré's writings. Their influence on subsequent philosophy of science—notably on Rudolf Carnap and other logical positivists—should not be underestimated, however. The break with classical logicism that is represented in the works of the logical positivists can find much of its roots not only in Poincaré's 'neo-Kantian' conventionalism, but also in his understanding of the inferential process. If we dallied in the opening pages so long on the differences between Russell and Poincaré, it was in part to underscore how a simple epistemic critique (of primitive indefinables 'known' in acquaintance) can lead to a profound methodological difference.

In any event, unfortunately we cannot treat simplicity likewise as an inoffensive pragmatic decision. The generalization process requires the assumption of 'simplicity', as

> it is clear that any fact can be generalised in an infinite number of ways, and it is a question of choice. The choice can only be guided through considerations of simplicity.[42]

But Poincaré struggles to justify recourse to the principle, noting *inter alia* among the difficulties faced is the inherent complexity found in the interdependence of natural phenomena; all he can do is adduce examples from the history of science, concluding that simplicity is only apparent—that behind it lies hidden complexity. Nevertheless simplicity is the only satisfactory ground upon which to build the edifice of science upon.[43] We say this not because we believe nature

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[41] Another parallel that could be drawn here is with Wilson's *Wandering Significance* [2006], which critiques the Russell-Frege 'classical view' of concepts in terms Poincaré might've been sympathetic to. Indeed an anecdote of Wilson's concerning Poincaré's discovery of automorphic functions underscores the 'intuitive', unconcious inferential processes we sometimes use (482-483).

[42] Poincaré [1952], 146.

[43] ibid., 149.

itself is simple—this is a recourse no longer available. Indeed, Poincaré notes that

> if the simplicity were real and profound it would bear the test of the increasing precision of our methods of measurement … we must conclude that it is an approximate and not a rigorous simplicity. This is what was formerly done, but it is what we have no longer the right to do.[44]

So while simplicity remains an essential methodological facet of the scientific enterprises, it is not, Poincaré thinks, a reflection of a real simplicity found in nature; much like the other 'epistemic condensers' involved in the inferential process delineated above, its presence is justified on a pragmatic ground: it just *works*. It could be argued that an alternative would be to have central epistemic condensers, such as simplicity, be treated as implicitly defined within the 'axioms' of scientific methodology—but this only shuffles the issue one remove, at risk of circularity, as the choice of axiom system cannot justify itself based on methodological considerations incorporated in it. It seems Poincaré has placed himself in a fairly difficult position ironically akin to Russell's: namely, there is an appeal to a mysterious epistemology to justify construction of a given structure. It is hardly sufficient when giving a philosophical account of science to engage in hand-waving about usefulness in practice: *why* does it work?

The formalization of simplicity found in information theory, and the resulting rigourization of the notion in systems of inductive inference offer a 'post-Poincaréen' way out not available to the logicists, however. Mathematical proof for Poincaré *is* synthetic *a priori* and the work done suggests that simplicity and probability are broadly equivalent notions. Poincaré inferential schema involves moving from 'fact' to 'generalisation' and prediction—every generalisation, he tells us, *is* a hypothesis[45]. Our goal as scientists just is the creation of good hypotheses, the ones most likely to bear fruitful predictions; and now we have not only a formal definition of

[44] ibid., 150.
[45] ibid., 152.

simplicity, but a rigorous mathematical argument demonstrating the overlap of probability and simplicity. We dissolve the problems surrounding the alleged intrinsic 'simplicity' of nature as a meaningless question—much like questions surrounding the 'true' physical geometry—and replace it with a statistical analysis of the complexity of multiple competing hypotheses, provisionally electing to choose the simplest one of the basis of a higher probability, without discarding the others. Simplicity need not be conceived as a property 'out there' in the world to be justified; it is sufficient to link it with probability measures to remove any trace of epistemic impropriety. Poincaré's triumph over Russell is complete.

IV. Conclusion

While the modern debate over conventionalism continues unabated, one ought to keep in mind how different Poincaré's version of it is from Carnap and his successors. There certainly is a sense in which geometric conventionalism is the spiritual grandfather to the principle of tolerance. Nevertheless, contemporary debates are not the concern here. Rather, we sought to defend Poincaré's criticisms of Russell. For Poincaré it was inconceivable that Russell should appeal to 'primitives' known through the mysterious agency of 'acquaintance'. Unfortunately for Poincaré his own inferential process, laden with ill-defined 'epistemic condensers' left us in the uncomfortable position of acquiescing to a definite contingency in theory-selection without providing an account of *what* the selection criteria consist, *how* we come to know and use them, of or even *why* science is possible at all. The way out of the dilemma for Poincaré is to hold that, since mathematics remains steadfastly *a priori*, any formalization of simplicity that demonstrates its co-extensiveness with higher probability both defines and justifies its use in induction and science. The results of algorithmic information theory do just that; had they been known at the time, Poincaré no doubt would've helped himself to them.

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