KANT, GEOMETRY OF INTUITION, AND INTUITION OF GEOMETRY

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1. Introduction

Early in the last century, the logical positivsts and logicists attacked Kant's philosophy of space and time through his arguments in the Transcendental Aesthetic of the *Critique*. As interpreted by Rolf Horstmann, there are two general lines of attack, one made by Reichenbach and the other by Russell. Both are united in attacking Kant's philosophy of geometry, the thesis that the truths of geometry are truths of space and, simultaneously, synthetic *a priori*. Specifically, the readings of the Aesthetic favored by the analytic tradition see the four proofs that space is a pure *a priori* intuition as either failing completely or as requiring the 'argument from geometry' of the Transcendental Exposition of the Concept of Space (henceforth, the Transcendental Exposition), to go through. Putting all the burden on the Transcendental Exposition, it in turn is read so that it is either false or uninteresting if geometric truths *qua* spatial truths are not synthetic *a priori*. The difference in the two attacks comes in at the next and final step. For Reichenbach, spatial truths are strictly empirical, and hence geometric truths are at best empirical, and not a priori. For Russell, on the other hand, the proofs of theorems of geometry come through logical deduction of geometric concepts, defined using only basic logical concepts (alternatively, in a more formalist line, the theorems of geometry are provable statements in a formal system), and are synthetic only in the sense that logic is synthetic, ie, geometry does not require intuition[1].

There are three tactics one partisan of a (at least vaguely) Kantian view might use to respond, two of which are employed by Horstmann. These are to argue that, first, the conventional analytic reading of the Aesthetic is flawed, so that the four arguments Kant gives in the Metaphysical Exposition do not need the synthetic apriority of geometry to

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[1]Citations will be given below, when these arguments are reconstructed in more detail.

work; and, second, concluding that geometry is not synthetic *a priori* does not mean we need to abandon the conclusions of the Metaphysical Exposition. I believe that at least solid first steps are made in this direction by Horstmann, as well as (Shabel, 2003), (Warren, 1998), and (Carson, 1997). In this paper, I wish to consider the third response one might make: denying the final step of the positivist argument, and affirming that geometry is synthetic *a priori*. There are a number of ways such a response can be made, of course, but some will involve more damage to Kant's overall program than others, and hence an evaluation of such damage is appropriate to any response. This, then, is what I will undertake here. I will begin by considering the Reichenbachian argument, and then turn my attention to the Russellian.

2. The Reichenbachian argument

Following Horstmann, the critical point in the Reichenbachian argument is 'the claim that the assumption of the *a priori* character of space and time must be abandoned owing to the problem arising from the application of Euclidean geometry to physical reality' (Horstmann, 1976, 18) since, *prima facie*, relativity theory tells us the geometry of physical space is not Euclidean. But since Kant considers geometry to be the science of space, and in particular the science of physical space, geometry cannot be considered *a priori*.

The most obvious first response is to sheer off geometry, the mathematical theory, from relativity theory, the science of physical space. We simply recognize that mathematical truths enjoy a status as truths, independent of any physical referents. Then geometry retains its *a priori* status to the extent that all other branches of mathematics do. The question is how this relates to space. On the standard reading of the Transcendental Exposition, Kant uses the synthetic *a priori* status of geometry to argue that space, even physical space, is a pure *a priori* intuition; since we consider space to be at least *a posteriori* here, this reading will not be acceptable. Two subtly different (from each other) readings of the Transcendental Exposition can be used to avoid this difficulty, by essentially reversing the direction of the implication: rather than geometry leading to space, these readings maintain that 'the "argument from geometry" establishes that geometric cognition itself develops out of a pure intuition of space' (Shabel, 2003, 3). In particular, the *a priori* status of geometry is epistemically grounded in the fact that we must have *a priori* cognition of (at least some of) the properties of space, as established in the Metaphysical Exposition (Shabel, 2003, 13-4). Shabel offers some textual support for this reading, but does not seem to explain what this epistemic ground is constituted in, instead offering two contrasting examples to illustrate how space does this. Emily Carson, on the other hand, makes this connection more explicit: 'the generation of geometrical spaces presupposes a metaphysical space [ie, intuition] in which they are generated' (Carson, 1997, 497), so that the pure *a priori* intuition of space becomes the domain in which geometric constructions are carried out; in this sense space is necessary (hence *a priori*) to provide the objective reality of the mathematical (geometrical) concepts by constructing them as possible objects of intuition. Note that the pure/applied distinction we began with is preserved here by no longer talking about physical space but instead about space as a pure *a priori* intuition.

We can develop such an understanding of space into a self-contained argument, which does not depend on one particular reading of the Transcendental Exposition. Strawson does this in the appendix to *Bounds of Sense*; by contrast with Carson and Shabel, he maintains the conventional reading of the Transcendental Exposition, and actually argues that the arguments of the Aesthetic all require the conventional reading (Strawson, 1966, 62). Strawson's visual geometry is an attempt to understand Kant by providing a potential middle ground between two contemporary interpretations of geometrical propositions, which parallel the Reichenbachian and Russellian we are concerned with in this paper[2]. On one hand, we may interpret these by not interpreting them, taking them as meaningless strings within a formal system; on the other, giving the terms physical referents, we may use geometry to describe physical space. While the first interpretation gives us *a priori* geometry, and the second gives us synthetic geometry, neither does both: modulo the contemporary vs. Kantian understanding of analyticity, the first interpretation is analytic, and the Reichenbachian argument is that the second is *a posteriori* (Strawson, 1966, 278-9). The middle ground is to interpret the terms of the propositions of geometry with reference to 'the spatial, but not physical (nor physically determinable) objects of pure intuition', which he identifies as

> the spatial *appearances* of things .... The straight lines which are the objects of pure intuition are not physical straight lines .... They are not physical objects, or physical edges, which, when we see them, look straight. They are rather just the looks themselves which physical things

[2]Strawson ultimately doesn't feel this interpretation to be entirely plausible, but for our purposes we will consider it his position.

> have when, and in so far as, they look straight. An arrangement of physical lines or edges may look triangular. But it is not the physical lines, so arranged, which constitute the triangle which is the object of pure intuition; it is, rather, the triangular look which they have, the phenomenal [in the contemporary, not Kantian, sense] triangle which they present, which is the object of pure intuition. (Strawson, 1966, 281-2; his emphasis)

Here Strawson explicitly lays out the connection vaguely described in Shabel: like Carson, he seems to be saying that geometric constructions are carried out by working with the space of pure intuition itself. At first, this pure intuition is 'concretized' and connected more closely to outer appearances: a visual straight line is that which we recognize as common in the experience of perceiving a straight line, a visual triangle is that which we recognize as common in the experience of perceiving three straight lines which intersect in three points, and so on.

This close empirical tie, however, leads to some obvious problems. First, it is not clear why visual geometry is significantly more *a priori* than physical geometry: the empirical tie is so tight that we might as well ground geometry in physical space; that is, since our visual ability is limited (we cannot see the arbitrarily small or arbitrarily large), it seems not unreasonable to wonder why visual geometry can extend beyond the potentially visible:

> it seems we are justified in assuming that what persons can imagine accurately is limited to what they can see accurate pictures of, which in turn is determined by their powers of sight .... the geometry of imperfect sight in an unobvious world will be indeterminate. (Hopkins, 1973, 27)

Hence it does not stand up to the Reichenbachian challenge. Hopkins is especially critical in this way when visual geometry is construed as Euclidean: while space *locally* appears to be Euclidean, on a significantly large scale, our perception breaks down. But then this means that there is not a clear distinction between 'locally appearing Euclidean' and 'locally appearing non-Euclidean':

> The phenomenal triangle is Euclidean if the look of the physical one is; and this, presumably, is true if the physical triangle looks Euclidean.

> It follows at once that phenomenal geometry is *not* Euclidean .... No physical triangle looks Euclidean as opposed to non-Euclidean. The difference made by the assumption that a physical triangle is Euclidean as opposed to non-Euclidean is visually undetectable. It therefore looks just as much non-Euclidean as Euclidean. Local observation and measurement fit equally with Euclidean and non-Euclidean assumptions .... (Hopkins, 1973, 22-3)

Setting aside the worries Strawson has over transcendental psychology and transcendental idealism, we might reconstruct his visual geometry as ‘ideal geometry’, in which geometric constructions are idealizations of possible physical constructions. For example, the representation in pure intuition of a triangle would be understood as a representation of a construction that might possibly exist in reality, instead of the phenomenological elements common to actual experiences of physical triangles. This seems to address the first problem levelled against Strawson’s visual geometry by severing the strict tie to actual experience and allowing geometry to make claims that go beyond perceptible limits. Now we can counter the Reichenbachian argument since this idealized space is sufficiently independent from experience for its science, or the constructions carried out in it, to be *a priori*. However, we still cannot maintain that it is necessarily Euclidean or non-Euclidean: while the difference between the two geometries can be intuited without any difficulty (we can represent the idealized difference between a triangle of 180° and one of 179°), it is not clear that one or the other must apply to this unique space.

Consider the history of the parallel postulate. Much like the axiom of choice at the beginning of the last century, the parallel postulate was regarded with suspicion not because it was incomprehensible or independent or inconsistent, but because its truth was not self-evident. Cast into the terms we have used here, the parallel postulate can be understood via our idealized geometry, but idealized geometry does not force us to accept it as an axiom of geometry. It seems to be similar with other statements equivalent to the parallel postulate, eg, we can make sense of both a triangle of 180° and one with less, but nothing seems to force us to accept either as being necessarily true. We might stipulate that one should be true for other reasons – it is convenient for solving physical problems to think that all triangles have the same number of degrees, for instance – but this seems too conventionalist.

Hence it seems desirable to generalize in two apparently contradictory directions: a full account of the *a priori* status of geometry should work for *all* geometries, while simultaneously explaining how we can prioritize Euclidean geometry in the way we do. To this end, let us examine the idea of 'geometry' more closely.

Contemporary mathematicians understand 'geometry' in three principle ways. One of these, algebraic geometry, is principally concerned with solutions to systems of equations in arbitrary vector spaces; we will not discuss it here.

Another is more interesting: the work of Sophus Lie and Felix Klein in the nineteenth century led to an understanding of geometry as the study of invariants of symmetries, formally understood as groups of (and derived from) matrices over a field (typically $\mathbb{R}$ or $\mathbb{C}$). Since these are usually represented as acting on vector spaces over $\mathbb{R}$ (or *real affine spaces*), we may echo the projective geometers and build our representations of non-Euclidean geometries in the established representations of vector spaces over $\mathbb{R}$, according to the actions of the various groups which characterize the geometries. For example, lines in two-dimensional hyperbolic geometry can be represented as curves in $\mathbb{R}^2$ invariant under members of the group of transformations identified with hyperbolic reflections. Given that the representation in intuition of $\mathbb{R}^2$ is *a priori*, it follows that hyperbolic geometry in this understanding is actually a geometry, ie, the study of curves invariant under members of a group of transformations; furthermore, Euclidean geometry is given a representational precedence over other geometries: the spaces of those geometries are represented within Euclidean geometry.

Alternatively, we may prioritize it epistemologically using the third understanding of geometry, that derived from the work of Riemann and the modern concept of a *differentiable manifold*[3] (in the mathematical, not Kantian, sense, of course) of n dimensions, which is a topological space locally diffeomorphic to $\mathbb{R}^n$; that is, around every point x there is an open set U which is homeomorphic to an open ball in $\mathbb{R}^n$ so that the co-ordinate system of the ball under the homeomorphism has partial derivatives of all orders. Here we do not represent spaces *in* Euclidean geometry; the projective plane, for example, cannot be represented in $\mathbb{R}^3$ without self-intersection. Instead, the spatial properties common to manifolds in general come from their relationship *to* Euclidean space,

[3]There is a more general concept of a manifold, a topological space which is locally homeomorphic to $\mathbb{R}^n$, but we eliminate the most 'bizarre' topological spaces by considering only differentiable manifolds.

and it is this relationship which both makes the former geometries, rather than formal systems, and prioritizes the latter.

There are two convenient examples of this approach. Consider, first, the proposition that any differentiable manifold is Hausdorff, that is, given a manifold M and $x, y \in M$, there are opens sets U, V such that $x \in U, y \in V, U \cap V = \emptyset$.

Proof. Suppose not; set x, y so that, for all open sets U, V with $x \in U, y \in V$, there exists at least one $z \in U \cap V$. Choose U to witness the manifoldness of M relative to x and y, that is, U is diffeomorphic to $\mathbb{R}^n$, with x' the image of x; let z_0 be in the intersection of U and V. Let z_0' be the image of z_0 under the diffeomorphism which relates x and x'. Finally, let d_0 be the distance between x' and z_0' in $\mathbb{R}^n$; then the open ball of radius $d_0/2$ centred at x' corresponds under the diffeomorphism to an open subset U_1 of U containing x which does not contain z_0. By assumption there is z_1 in $U_1 \cap V$, which corresponds to z_1' in the open ball of radius $d_0/2$ centred at x' under the diffeomorphism. Repeating this procedure we obtain a sequence $\langle z_n' \rangle$ which converges in $\mathbb{R}^n$ to x', and hence a sequence $\langle z_n \rangle$ which converges in V to x. But V was arbitrary, so x is in any (non-trivial) compact set containing y; but we can also take a sequence of compact sets whose intersection is just y (since this is always possible in $\mathbb{R}^n$), so $x = y$. □

Reflecting on the proof, it is significant for our present purposes that $\mathbb{R}^n$ had certain topological properties – namely, that it was a Hausdorff metric space in its own right – and that the manifold M 'inherits' these properties by its reference to $\mathbb{R}^n$. A simpler example is given in the fact that, given any curve in M and any two points x, y on the curve, there is a point z on the curve between x and y, since a curve is diffeomorphic to the unit interval in $\mathbb{R}$, which has this same property. In Kantian terms, we might say that the continuity of a manifold is epistemically dependent upon the fact that (real affine) space has this property.

Note that in both of these approaches, that of Klein and that of Riemann, we have replaced the intuition of space with a concept ⌜x has a group of symmetries G⌝ or ⌜x is a differentiable manifold⌝. Since Kant motivates large portions of his work by appealing to the fact that space is an intuition, such an approach, while answering Reichenbach (these mathematical theories are *a priori* to the extent that any mathematical theory is), is not entirely Kantian as it stands. But there does not seem to be anything which precludes us from maintaining that the real affine spaces are intuitions, and since they are nested in the sense that $\mathbb{R}^n$ is a subspace of $\mathbb{R}^m$ if and only if $n \leq m$, one might hold that there is a unique intuition of each real affine space, and not a concept of 'real affine spaces in general' once the dimension of the space is fixed.

Let us phrase this another way. In 'pluralizing' our notion of space to accommodate our plural notions of geometry (hyperbolic, projective, Euclidean, and so on, not the three mathematical disciplines), we run the risk of turning space into a concept and not an intuition. But this can be recovered if we take a more abstract understanding of space, so that the properties which make something a space are given by relationship to a pure intuition. This may be the nested real affine spaces, as outlined in the last paragraph, or an even more abstract intuition of spatiality rather than space, which (in Kantian terminology) provide objective reality to such concepts as infinite magnitude, infinite divisibility, and so on, which cannot be understood empirically, but can be represented as possibilities through pure intuition; I have in mind here the *a priori* representation of objects in spatial relations of (Warren, 1998).

This still seems somewhat lacking relative to Kant's actual arguments for the intuitive status of space in the Metaphysical Exposition, especially if the third argument presupposes that space is a unity, ie, given as one, and if the fourth is read as a conditional implication off the antecedent that space is infinite. Other readings may work, however, by understanding the unity of space to refer to the fact that subspaces and points are constructed by limitations of the complete given space, so that 'the intuition of the singular individual space must be prior to the general concept' (Carson, 1997, 495). But let me leave this, optimistically, and move on; we seem to have reached an upper limit on abstractions of 'space' and 'geometry', at least as these relate to contemporary mathematics, so I now wish to consider other responses to Reichenbach.

The first is a conventionalist understanding of the geometry of physical space. The simplest form of such an argument starts with the assumption that the laws of physics are smooth, in the sense that they have partial derivatives of all orders, and hence should be re-writable in ways which respect diffeomorphisms between manifolds, ie, if λ is a law of physics for manifold M, and M' is diffeomorphic to M via diffeomorphism f, then there is a law of physics λ' for M' such that $M \models \lambda(\bar{x}) \leftrightarrow M' \models \lambda'(f(\bar{x}))$. Hence properties of space can be reinterpreted consistently between diffeomorphic manifolds, and choosing which particular manifold represents physical space is merely a convention. Generalizing a bit more, the conventionalist says that it seems reasonable to say this re-interpretation can also be undertaken between non-differentiable manifolds, though it will involve more radical measures. For example, one may re-interpret the non-Euclidean geometry of physical space as a Euclidean theory, but this will involve denying that light moves along geodesics in this geometry. Since, according to this line, we may always interpret space as being Euclidean, the latter geometry is unfalsifiable and therefore *a priori*. But since this involves a more-or-less complete disconnect between experience, the objects of physics, and geometry, it is not clear how this could be appealing from any vaguely Kantian perspective, and we will not consider it in any further depth here.

By contrast, we may take a point of view that runs directly opposite conventionalism, by asserting that any geometry we may represent is ultimately based upon Euclidean geometry. This is similar to the mathematically-inspired interpretations considered above, but rather than assuming an intuition of Euclidean geometry upon which other geometries are founded, the approach here is to show that Euclidean geometry must be the foundation for any intuition of geometry which we may have. In a very early paper, Russell attempts an analysis of the logical foundations of any potential geometry of space, where geometry is here understood as a differentiable manifold equipped with a metric which allows us to integrate along paths in the manifold to determine their length; these are essentially three-dimensional analogues to curved surfaces in $\mathbb{R}^2$. Russell claims that

> The Geometry of non-congruent surfaces [surfaces of non-constant curvature] is *only* possible by the use of infinitesimals, and in the infinitesimal all surfaces become plane. The fundamental formula, that for the length of an infinitesimal arc, is only obtained on the assumption that such an arc may be treated as a straight line, and that Euclidean Plane Geometry may be applied in the immediate neighbourhood of any point. If we had not our Euclidean measure, which could be moved without distortion, we should have no method of comparing small arcs in different places, and the Geometry of non-congruent surfaces would break down. (Russell, 1896, 10; his emphasis)

While this account of the epistemology of Riemannian geometry is at best contrary to the Riemannian definition of path-integrals and at worst simply incoherent, the underlying point seems to be that we must represent a non-Euclidean space as actually being Euclidean, or at least Euclidean to such an extent that we may as well work within a Euclidean framework and have an underlying Euclidean co-ordinate system (Russell, 1896, 4). Discarding the details, I believe we have a reasonably defensible anti-Reichenbachian position: our representation of space is actually Euclidean, and all representation of non-Euclidean geometry, including that of the space of general relativity, takes place with reference to the Euclidean geometry of $\mathbb{R}^3$. This last clause may seem to make this line reduce back to the earlier situation, but, as mentioned before, the direction of our reasoning runs in the opposite direction here: we are presently starting with several potential representations, and arguing 'analytically' that Euclidean geometry lies under them, while before we argued 'synthetically' that non-Euclidean geometries could be represented within the context of real affine space.

As an illustration, consider the black hole diagrams encountered in discussions of the unusual possibilities of general relativity. The sharp curvature of space is represented in such diagrams by the deviation of the image of a Euclidean co-ordinate system embedded in the space from an implied Euclidean co-ordinate system. I do not wish to go into further detail, but it seems reasonable to believe that a case could be made that the necessity of including such reference to real affine space in such representations of spaces of non-zero curvature has more than psychological and pedagogical significance, and that hence we may regard the representation of real affine spaces as epistemically or representationally *a priori*.

To finish this section, let me point out that a major concession to the Reichenbachian argument has been made throughout, except in one argument. This concession is that we must have a sharp divide between physical space and the space of interpretation of geometric terms, or equivalently, between pure and applied geometry. The only exception was the conventionalist argument, which I feel is too abstract to be of any help to the Kantian here.

This sharp divide means that a reconstruction of Kant's argument in the *Critique* will, if even possible, require much more work to justify transcendental idealism, especially on a phenomenological understanding of space: to the extent that the geometry of the universe at large does not correspond to the geometry of our phenomenological equipment, we cannot claim that the former has a significant ground in the latter. But I believe this problem arises for the Kantian as soon as a plurality of geometries is admitted, in the same way it arose when we considered how phenomenological understandings of space can prioritize Euclidean geometry: either the geometry of our phenomenological equipment is Euclidean, in which case non-Euclidean situations must necessarily be incoherent (which I would contend is not the case), or it, and hence the space of the transcendental idealist, is indeterminate, and cannot justify the mathematical priority of Euclidean geometry.

3. The Russellian argument

The logicist case against Kant is summed up ably by Russell himself:

> the abstract logical method, based upon the logic of relations, which had served hitherto, [is] still adequate, and enable[s] us to define all the classes of entities which mathematicians call spaces, and to deduce from the definitions all the propositions of the corresponding Geometries. (Russell, 1903/1938, 461)

This is a direct and negative response to what Russell identifies just prior to this as 'the [question] of chief importance to us, as regards the Kantian theory ... are the reasonings in mathematics in any way different from those of Formal Logic?' (Russell, 1903/1938, 457). Kant appears to identify two places where the judgements of mathematics are synthetic *a priori*: in establishing the truth of the axioms and in the inferences from the axioms to theorems. Logicists such as Frege, Peano, and of course Russell, worked to demonstrate that, in fact, both the axioms and mathematical inference (at least in domains such as number theory) can be replaced with more 'primitive' notions and hence mathematical reasoning does not require the Kantian intuitions of space and time for its justification.

This reduction takes two different forms, both of which I will attack in this section. The first is that used by Russell in the work cited above as well as *Principia Mathematica*: the logicist introduces a collection of 'primitive' notions such as 'class' and relational terms and a small calculus of formal deduction; then shows how the domain of mathematics under consideration, such as geometry at (Russell, 1903/1938, 382-3), can be represented as some complicated structure using these notions. For example, Russell asserts 'that Geometry is the study of series of more than one dimension' (Russell, 1903/1938, 372), that is, of linearly ordered classes whose elements are themselves linearly ordered classes. The other reduction comes from the formalist approach to mathematics which developed from Hilbert's axiomatization of Euclidean geometry. Here the primitive objects – such as points, lines, and planes and relations such as incidence, intersection, and so on – are left undefined, and the axioms are presented using the undefined predicates and variables in a formal language equipped with rules of deduction. In the first reduction, assuming the primitive notions are properly logical and the logical rules of inference suffice for logical proof of all the theorems of the mathematical domain in question, the conclusion that the domain requires nothing extra-logical follows immediately. Similarly, in the second, since reasoning is to proceed strictly by formal manipulation of strings of uninterpreted symbols, no intuition seems to be required. Hence we will understand 'synthetic' as meaning 'requiring no extra-logical/formal element'

While most critiques of logicism proceed by attacking the logical status of the axioms, I will not take this route (for precisely that reason), and will only touch on this argument briefly. Instead, I will consider a central problem of any reductionist program; then inadequacies of the logicist's characterization of logic, and its relation to mathematics; and finally whether contemporary logic can be accepted as genuinely logical.

Consider the following mathematical system: a group of permutations G of a set X of 54 elements, such that G is generated by three sets of three permutations each. Each of the nine generators has order 4; in each set, there are two permutations which act on 20 elements of X, and one which acts on 12, so that the elements one permutation acts on are fixed by the other two in the same set, and two elements are fixed by all three in that set; using group theoretic notation, we can say that the subgroup of G generated by each set is isomorphic to

$$\left\langle x, y, z \mid xy = yx, xz = zx, yz = zy, x^4 = y^4 = z^4 = 1 \right\rangle .$$

Finally, there is a relation between the nine generators which can be expressed in the language of group theory which I will call 'orthogonality' but not state explicitly here; it would be so long and convoluted as to be basically incomprehensible.

Now, given such a G and X, one can be asked a variety of questions. We will restrict our attention to one in particular, whether some permutation on X is in G or not. We can think of this as a game: I give you a permutation, and you have to tell me whether or not it is in G. We can also add an additional task, to write the permutation explicitly in terms of the generators.

Of course, any sane person presented with this problem in exactly the form above would immediately wonder why they should bother with what will obviously be a tedious and frustrating exercise. The system does not become interesting or significant until it is given a specific referent, which in this case is a Rubik's cube: the 54 elements represent the small squares, and the permutations in G all possible configurations of the squares from the nine basic rotations of the parts of the cube, three in each plane. The independence and orthogonality relations abstract the way the basic rotations interact. The problem in the previous paragraph is simply to solve the Rubik's cube, ie, perform (list) the basic rotations needed to cancel the action of the given permutation.

I will call this the Rubik's cube analogy, and will return to it throughout this section to illustrate analogically various faults with the logicist program. We can identify one right away: a representation of an object of study is not a substitute for the object itself. We may be able to use the representation as an aid in reasoning about the object – the specific instance of G pertaining to the Rubik's cube can be used to understand the interactions of the basic rotations and develop 'patterns' used to solve the cube, for example – but without the object the representation is completely facile. Furthermore, the representation may not even be useful without an accompanying interpretation; I did not include a formal statement of orthogonality because it would be so long as to be nearly incomprehensible without reference to the Rubik's cube from which it is derived.

This objection applis to both Russell and Hilbert's reductions. Hilbert's is the most obvious: an uninterpreted axiomatic system can provide us no reason for being interested in working out its theorems. I can write down any list of well-formed sentences in a given formal language, but without motivation, proving theorems in the resulting system will be an empty exercise in pushing around symbols. With Russell, there may be some intrinsic logical interest in certain structures defined using his approach, but as they become increasingly baroque and only tied to conventional mathematics by a choice of labels on the part of the logicist, they become just as empty and futile as Hilbertian formal systems: nowhere does Russell explain why anyone would care to investigate the properties of series of series.

This same argument seems to apply to any reductionist program: if chemistry is 'nothing more than' one flavor of particle physics, then the reductionist is obliged to explain why that particular flavor has all the attention others don't. But we will leave this extension of the argument undeveloped; it suffices for our purposes that it is a challenge to both forms of mathematical reductionism.

Next, we consider several inadequacies of logicism. The first is familiar and, as mentioned above, I will only cover it briefly. In a reductionist presentation of a mathematical domain, we are given a collection of axioms. To avoid the criticism discussed immediately above, these axioms must be the basic truths of a previously given mathematical system. This is not to say that they must be self-evident; the burden is somewhat less than this, being only that the axioms, put forth to represent the fundamental assumptions of some subject matter, actually do so. We can think of this as a requirement for a connection between the axiomatization and the mathematical system. But, assuming this connection has been established, what is its epistemological status? It does not seem that it can possibly consist of purely logical judgements, particularly in the case of geometry, since the form of the connection must be that certain symbolic states of affairs correspond to certain spatial states of affairs, which seems to be almost a paradigm example of Kant's synthetic judgements.

This claim is familiar because it has been levelled against the neo-Fregeans before: it says that their axioms are synthetic, not logical, truths. But while (I have no cites for this) it has been argued that various neo-Fregean reconstructions of Basic Law V are synthetic (or at least not logical truths), I believe it can be applied more uniformly to all axioms and rules of inference in any axiomatic system, with the possible exception of axioms governing symbolic representation, in the sense of 'axioms for writing down strings and using rules to write down other strings'. Indeed, the axioms of such formal systems as the propositional calculus should themselves be considered synthetic judgements, because logic, the science of universally valid judgements and inferences, is not the science of symbolic manipulation. The propositional calculus, for example, can only be taken to represent universally valid inferences once the synthetic connection has been made between the symbolic calculation (schema)

$$\begin{array}{l} A \to B \\ A \\ \hline B \end{array}$$

and *modus ponens*.

This result seems to be one aspect of what Janet Folina has in mind by the phrasing that 'logicist logic' requires 'intuitionist intuition' (Folina, 2003): the Kantian-inspired intuitionists of the nineteenth and early twentieth centuries argued that the formal manipulations of modern logic need such an extra-logical connection. I believe another may be found in the idea of infinity.

(Russell, 1903/1938, 121) presents infinity as a property of certain sets, which we presently call Dedekind-infinite: a set A is *Dedekind-infinite* if there exists a bijection between A and $B \subsetneq A$. Michael Friedman argues that infinity can be represented conceptually/logically using polyadic or nested quantifiers: in a language where $<$ is a linear order, for example, the infinite extension of the set it orders can be captured in the axiom

$$\forall x \exists y \exists z (z < x \land x < y). \tag{1}$$

According to (Friedman, 1985, 466), this means we no longer need an intuition of space to capture the idea of infinity necessary for geometry. This position is problematic in a number of ways, but the one I am concerned with at this particular point is why (1) accurately represents infinity: applying the same anti-reductionist argument as above, it seems clear that for this axiom to have any significance, we must already possess some understanding of the term 'infinity'. The positions of (Warren, 1998), (Shabel, 2003), and especially (Carson, 1997) (or at least the reading of Kant they favor) seem to be that such an understanding comes through the pure intuition of space. On the other hand, quasi-Kantians and the intuitionists of the late nineteenth and early twentieth centuries such as Poincaré argue that the understanding of infinity comes from the intuition of time or an intuition of indefinite iteration or potential infinity, and was exemplified by such non-geometric mathematical reasoning as mathematical induction[4]. Astoundingly, in the last year or so of his life, Frege himself believed that infinity had its origins not in logic (he considers it 'nothing mysterious or wonderful' in the *Grundlagen*, and defines the infinite number we would today designate ω as 'the Number which belongs to the concept "finite number"' (Frege, 1884/1950, 96)) but instead in 'the geometrical source of knowledge' (Frege, 1979, 273).

It seems appropriate to touch on Poincaré's treatment of induction in further detail here. At least in his earlier popular works, attacking the logicists, he argues that induction represents the deduction of 'an infinity of syllogisms': while each proof that some property ϕ is true of a given natural number n may be require nothing more than logic, proving that $\forall n \phi(n)$ requires a 'jump' from the finite to the infinite. This 'jump' is captured by 'reasoning by recurrence', and not by logic:

> And yet, however far we thus might go, we could never rise to the general theorem, applicable to all numbers, which alone can be the object of science. To reach this, an infinity of syllogisms would be necessary; it would be necessary to overleap an abyss that the patience of the analyst [logicist], restricted to the resources of formal logic alone, never could fill up. (Poincaré, 1946, 38; cf 440).

[4]For the remainder, by induction I mean mathematical induction.

However, Poincaré is not an orthodox Kantian: the origin of induction does not come from time, but is given in itself, seemingly as an '*a priori* intuition of recurrence':

> Why then does this judgment force itself upon us with an irresistible evidence? It is because it is only the affirmation of the power of the mind which knows itself capable of conceiving the indefinite repetition of the same act when once this act is possible. The mind has a direct intuition of this power, and experience can only give occasion for using it and thereby becoming conscious of it. (Poincaré, 1946, 39)

We might wonder how all this applies to geometry. I initially thought a similarity might be found with the axioms of 'potential infinity' and Friedman's logical analysis of them, as exemplified by (1). But the logical form of (one instance of) the simplest form of induction is

$$(\forall n \phi(n) \rightarrow \phi(n+1)) \rightarrow \forall n \phi(n). \tag{2}$$

Here, the extra-logical element seems to come in at the point where we believe the axiom: we need, say, Poincaré's intuition of recurrence to recognize that the process of adding one is how all the natural numbers can be constructed. With the equipment of second-order logic (which I'll understand as quantification over sets of natural numbers) and the natural order on the naturals, (2) is equivalent to

$$\forall X \exists n (n \in X \wedge \forall m (m \in X \rightarrow n \leq m)), \tag{3}$$

which initially seems somewhat amenable to Friedman's analysis. But the crucial element there is that our universe have only one sort of object, so that the object shown to exist by one application of the axiom can be used in eliminating the universal quantifier (Friedman, 1985, 466). Here we have two sorts, natural numbers and sets of them, so we cannot apply Friedman's analysis. The only point where recurrence seems to come into play is at the level where we believe the axioms: we must understand that the operation $n \mapsto n+1$ generates all the natural numbers, and that the infinite extension of a line means we can always find another point further on. Consequently, an independent intuition of recurrence does not seem to add anything, except to the extent that it might replace an intuition of infinite space.

Next, I wish to consider two aspects of Poincaré's other objections to logicism. These are combined by (Detlefsen, 1992) into one notion of an 'architecture', but I wish to recognize them separately as ideas of 'epistemic pedigree' and 'directive impulse'. Both are criticisms not of logicism on the level of axioms but on the level of inference: Detlefsen identifies a contradiction recognized by Poincaré, between the statements

(I) All the inferences used in mathematical proof are of a purely logical character.

(II) Conclusions of mathematical proofs can, and often do, constitute extensions of the mathematical knowledge represented by the premises.

The two Poincarean ideas are directed at (I), maintaining, first, that mathematical inferences themselves, not just axioms and the propositions which follow from them, must be mathematical truths; and, second, that mathematical inferences are only justified when they work towards the overall development of a mathematical program (Detlefsen, 1992, 354ff.). Note that this could be phrased positively: any mathematical epistemology must be compatible with these ideas; since logicism is not compatible with them, it is not an adequate theory of mathematical knowledge.

Epistemic pedigree seems best understood as requiring that every transition from one statement to the next in a proof must be mathematically acceptable, and logical acceptability is neither necessary nor sufficient for mathematical acceptability. This means epistemic pedigree requires a mathematical and not logical sense of rigor, 'according to which it [rigor] consists in the lack of "gaps" in mathematical understanding. One need have no fear of missing elements in a mathematical inference provided that one grasps the mathematical reason behind it' (Detlefsen, 1992, 355). Since Detlefsen does not develop a concrete epistemology which includes this notion of rigor or make the requirements more explicit, I can offer only a tentative sketch of what one might look like.

Consider the formal system of Peano Arithmetic (PA), and the genuine mathematical system of the natural numbers. A theorem of the natural numbers can be proven, in the formal sense, in Peano Arithmetic. Where is the difference between the mathematical and formal proofs? Suppose we add to our epistemology of mathematics the requirement that for every mathematical inference there must be the possibility of accompanying it with a representation of the mathematical situation at hand, which justifies the inference in the sense that

(1) the antecedent is an accurate description of the situation at hand;
(2) mathematically acceptable (ie, generally considered legitimate among the community of mathematicians in discussion of such representations) alterations of the representation demonstrate that the consequent does indeed apply to the situation at hand;

(3) no mathematically acceptable but non-standard cases invalidate the alteration of the representation and the conclusion that the consequent does apply.

Less formally, this is supposed to mean that we can have an intuition of the mathematical situation which shows the last sentences applies to the situation, the next sentence follows from it, and there aren't any weird but legitimate cases to consider that might make the transition between sentences suspect.

Given such a requirement, it is immediate that the PA proof is likely to be completely inadequate, while the proof in regular number theory is entirely acceptable: the PA proof will be long and complicated, and only mathematically acceptable once a commentator (which may just be the mathematician reading the proof herself) describes the accompanying representation(s). The context of the mainstream number theory proof – within a text or lecture on number theory, where both formal proofs and informal discussion are given – guarantees the possibility of the appropriate representation.

We can also give an example using the Rubik's cube. Epistemic pedigree for the Rubik's cube means that, even if the statement of a proposition about the cube could be translated readily from the cube to the group, a proof in group theoretic language would not (necessarily) be acceptable: each of the inferences should be acceptable to someone who takes the group as simply being a representation of the cube. More concretely, a cubic theorist might assert that the mathematician's inferences are only admissible to the extent that they can be translated into cubic theoretic terms.

This may be interpreted as a psychological or pedagogical requirement, and to some extent it is: rather than having abstract proofs freely floating, this requirement ties them to human mathematicians. But it is also epistemological if we adopt the Kantian epistemic position that knowledge, to be legitimate, must have a referent in possible experience (Carson, 1997, 505), which is guaranteed in my interpretation by the possibility of representation. Also, note that this is presented as a sketch of one possible epistemology which maintains an epistemic pedigree, and I strongly encourage reading it as an example to illustrate what is meant by that term, rather than a fiat of a new requirement.

Directive impulse asserts that mathematical thinking and proofs must take place within an overall mathematical project. This project may be very large – solve Diophantine equations – or very small – find the representations of some group G over $\mathbb{C}$. Obviously mathematicians use such projects as sources of inspiration, reasoning informally, then going back and making the arguments rigorous; but Poincaré posits such a project as an epistemic necessity, for both the application and justification of mathematical knowledge. Poincaré gives examples from biology, of humans and elephants, arguing that knowledge on the microscopic level does not consist complete and adequate knowledge, and that

> It is the same in mathematics. When the 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. (Poincaré, 1946, 436)

Detlefsen reads the section of *Science and Method* containing this quotation as indicating that mathematical inference should include 'a grasp of how the movement from premises to conclusion contributes to the 'development' of some architectural theme of the local subject-matter' (Detlefsen, 1992, 361). Hence by directive impulse I do not mean simply the informal reasoning in some topic, but along with it the understanding of the broad direction of a theorem and its proof and how these contribute to the project(s) they are embedded in. Thus a group-theoretic proof of a cube-theorem or a proof in PA of a statement of number theory cannot stand by themselves, but must be accompanied by the informal discussion which demonstrates their place in the general project of solving the Rubik's cube or investigating numbers.

These are ideas are defensible on both naturalistic and epistemic grounds: naturalistic because they reflect actual beliefs most working mathematicians have about what constitutes a good proof, as well as why the same reject strictly formal proofs; similarly, in an epistemic sense, these ideas allow one to establish (or even make coherent) the mathematically proper relationship between propositions, as opposed to the strictly logical orderings of Leibniz, Frege, and so on (Detlefsen, 1992, 358 and 375,n.19), and provide a first step in establishing a sense of mathematical rigor which can consider both pre- and post-Fregean mathematics. Thus I do not see them as being motivated (solely) by an effort to oppose logicism, but instead as legitimate desiderata for an epistemology of mathematics.

Finally, I wish to touch briefly on the question of whether contemporary logic is genuinely epistemically prior to mathematical reasoning. Such an assertion is absolutely necessary to any epistemology of mathematics which might be called reductionist. Consider, for example, the analysis of (Friedman, 1985, 468), that Kant's constructions correspond to contemporary Skolem functions, eg, proofs that given two points A, B there is a point C on the line AB such that the distance from A to C is the same as the distance from B to C are given by constructions/functions which take A, B as parameters and output C as desired. The conclusion of this analysis seems to be that, now that we can represent such a statement formally, eg,

$$\forall A, B \exists C (C \in \ell(A, B) \wedge d(A, C) = d(B, c)),$$

and can understand construction as being nothing more than giving an explicit Skolem function, we do not need the construction or any other extra-logical element (Friedman, 1985, 493ff.). But it seems open to disagreement whether the logical form $\forall\exists$ is an abstraction of 'reasoning by Skolem functions' or, as a reductionist would need to show, constructivist reasoning is simply making a narrower use of the $\forall\exists$ form; indeed, I believe a historical investigation is appropriate, starting from the sketch of (Friedman, 1985, 475ff.), and one might very well be able to make the case that the contemporary polyadic logical form is epistemically dependent upon constructivist reasoning.

Turning back to Kant and the Aesthetic now, it seems the biggest gap between his arguments and these attacks on the Russellian argument is that the extra-logical element need not be a pure intuition of space or time. Indeed, 'intuitionist intuition' may be far more abstract than either of these, in the form of an intuition of recurrence, two-oneness, or the relationship of representation between formalist symbolism and given objects of study. But this does not seem to be anywhere near as disruptive to the Kantian program as the concession to Reichenbach, since there seems to be nothing preventing us from working with a nearly Kantian notion of intuition. The one concession we must make to the logicists is that rigorous mathematical inference will most likely hew more closely to Hilbert than Euclid, and there will be times of mathematical crisis when mathematicians will be obliged to temporarily abandon intuition to maintain rigor (Detlefsen, 1992, 367), which seems to make logic slightly more important in times of crisis, while intuition takes precedence in the initial development of mathematical disciplines.

4. Conclusion

I have now considered several counterarguments to both of the attacks on Kant made in the style of the logical positivists, and believe that two major concessions must be made: we cannot freely apply the theorems of geometry to physical space, and intuition will have to be periodically set aside when mathematical methodologies are in flux. The first is a critical blow to the Kant's positive efforts in the *Critique*, providing synthetic *a priori* support for the principles of Newtonian physics; but there seems to be some scepticism about Kant's ability to do this even given the *a priori* intuitions of space and time, and it certainly isn't a goal contemporary neo-Kantians should have.

More generally, I think the Reichenbachian argument is based on nothing more than an overly simplified understanding of the *a priori/a posteriori* distinction, and, while it does raise issues of the representation and ontology of mathematics, as well as the applicability of mathematics to physical reality, I do not believe the argument itself is of significant interest to contemporary philosophers of mathematics. The responses to the Russellian, on the other hand, demonstrate severe inadequacies of the logicist and formalist epistemologies of mathematics. To the extent that these have been the dominant position within the philosophy of mathematics for the past century, I believe the arguments I have laid out here challenging this view are of some philosophical interest.

Hilbert recognized both 'axiomatic analysis' – working out general conclusions from axioms representing a variety of interesting systems – and 'genetic analysis' – working out conclusions specific to previously given mathematical systems. The philosophy of mathematics of the twentieth century may have given us an epistemology of the first sort of analysis, but I am lead to the conclusion that the second has been severely neglected, if not swept under the rug entirely; and that a logical notion of rigor, with origins in Leibniz' metaphysics, has been developed in place of a naturalist, mathematical notion of rigor which would be based upon the actual practices of working mathematicians.

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