The Absolute Motion Detector

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Abstract

Absolute motion — motion relative to space itself — is detectable in the classical non-Euclidean geometries. Although this claim can be conveyed with a simple thought experiment, I argue that it was out of sight to the people who were best placed to take note of it in the nineteenth century, when non-Euclidean geometry was a central object of study. This is a perplexing state of affairs, and in the first part of this paper I offer an explanation for it. A central component of this explanation involves the shift from spatial to spatio-temporal thinking that had to wait for Einstein's development of the theories of relativity in the early twentieth century. In brief, once geometry was used to describe spacetime, a *straight line* could be understood as an *inertial trajectory*. I suggest that the absence of this idea in the nineteenth century obscured the detectability of absolute motion in the classical non-Euclidean geometries. In the second part of this paper, I discuss how significant this lacuna was. If the possibility of detecting absolute motion *had* been recognized, I argue that it would have impacted all of the major positions in the philosophy of geometry. Indeed, some figures would have responded to it by claiming that space must be necessarily Euclidean after all.

1 Introduction

The central idea of this paper is very simple. The idea is that the impact of spatial curvature on bodily motion — a familiar phenomenon from general relativity — is apparent in any space which deviates from "flat" Euclidean geometry, including, in particular, in spaces of constant non-zero curvature. My argument will be that, despite the relative simplicity of this idea, it was completely out of sight to the figures who grappled with physical geometry in the nineteenth century (not to mention most of the rest of us more recently).

My more specific claim will be that all the major figures who engaged with the philosophy of geometry in the nineteenth century failed to recognize that *absolute motion is detectable in the classical non-Euclidean geometries*. I will examine the implications of this for the main positions in the philosophy of geometry that arose in this period, and suggest three responses that Helmholtz, Poincaré, and the early Russell might have had. The first (empiricist) response would have been to accept that absolute motion might indeed be detectable, and regard it as a way to measure the curvature of space. The second (conventionalist) response would have been to insist that the choice of a physical geometry was a convention, but similar to the "convention" of adopting the Copernican system over the Ptolemaic. The third (metaphysical) response would have been to deny on a priori grounds that absolute motion is possible, and conclude that space must be necessarily Euclidean after all.

Along the way, an important theme in this discussion will concern the distinction between *metric* structure and *affine* structure; the distinction between *shortness* and *straightness*. The classical definition of a straight line is the shortest distance between two points; hence the fundamental affine notion is explicitly defined in terms of the fundamental metric notion. Although the possibility of defining affine structure independently of metric structure was latent in Riemann's introduction of a variably curved manifold in 1854, its full recognition did not emerge for another seventy years, when mathematicians like Tullio Levi-Civita and Hermann Weyl started to analyze the significance of Einstein's use of Riemannian geometry in general relativity. It is this historical lacuna that helped create the blind spot around the detectability of absolute motion in the classical non-Euclidean geometries, or so I will argue.

2 The Problem of Space

The central problem of physical geometry in the nineteenth century was the "problem of space" (or the "Helmholtz/Lie problem of space").[1] What was beginning to come into view in this period was that the geometrical part of the science of physical space was a species of a more general mathematical genus. Thus Riemann wrote that "a multiply extended quantity is susceptible of various metric relations, so that space constitutes only a special case of a triply extended quantity" (Riemann, 1876, 304). Following Riemann, Helmholtz similarly set himself the task "to investigate which properties of space belong to any manifold which depends on several variables and goes over continuously into itself... and which properties, on the other hand, are not conditioned by this general character, but are peculiar to space" (Helmholtz, 1977, 39-40). To use a contemporary gloss, the problem of space was the problem of demarcating the candidate *physical* geometries; demarcating which mathematical geometries provide possible descriptions of physical space.

This depends, of course, on the recognition that a plurality of geometries exist. As is well known, the classical non-Euclidean geometries — geometries with either a constant negative or a constant positive curvature — were the climactic result (especially with the work of Lobachevsky and Bolyai around 1830) of centuries of failed efforts to derive Euclid's parallel postulate from his other postulates.[2] Proliferating the number of mathematical geometries further, Riemann developed the general notion of a variably curved geometry in his 1854 Inaugural Dissertation.[3] The question thus arose: did all of these geometries present a possible description of physical space?

Helmholtz tackled this problem head on in the late 1860s. According to Helmholtz, the very possibility of physical geometry depends on the fact of the free mobility of rigid bodies:

[1]For a classic discussion, see Torretti (1978) §3.1.
[2]For discussion see, for example, Gray (2007) §§9, 10.
[3]Published as Riemann (1868).

we need things like rulers — objects that behave as standards of length — which can be moved around space without changing their dimensions, thus allowing us to measure spatial magnitudes. He then went on to give an argument showing that the only mathematical geometries which have the appropriate congruence structure to represent such free mobility are Euclidean geometry and the geometries of constant positive or negative curvature. Furthermore, such geometries must have the familiar quadratic ("Pythagorean") metric. Thus we have a kind of transcendental argument to the effect that, if we have any kind of geometrical description of space at all, it must employ a geometry of constant (zero or non-zero) curvature with a Pythagorean metric. This rules out the variably curved geometries developed by Riemann as well as any geometries with a non-Pythagorean metric.[4] Helmholtz's solution to the problem of space achieved widespread consensus by the end of the century, especially once Sophus Lie had provided a rigorous version of the mathematical part of Helmholtz's argument using his theory of continuous transformation groups. Poincaré, in particular, agreed with Helmholtz that the candidate descriptions of physical space were given by the geometries of constant curvature, and that Riemann's variably curved geometries "so interesting on various grounds, could never be… purely synthetic, and would not lend themselves to proofs analogous to those of Euclid" (Poincaré, 1952, 56).

Let us consider a little more closely the role of the *postulate of the free mobility of rigid bodies* in Helmholtz's solution to the problem of space. If there were no bodies that could be moved freely without changing their dimensions — so the argument went — we would not be able to measure spatial magnitudes at all, and so would not be able to develop any kind of physical geometry. Notice, however, that in talking about the free mobility of rigid bodies, we are considering a kind of transport which leaves *metric* properties (distances and angles) unchanged. We might attend instead to a notion of *affine* transport: a motion which moves an object entirely along parallel lines (or "autoparallels" — the straightest lines). But with this in mind it quickly becomes apparent that moving an extended rigid object in such a way that all of its parts move along parallel lines *is only possible in flat Euclidean geometry.*

To see this, consider the constant positive curvature instantiated on the two-dimensional surface of a sphere. Imagine that there are tiny ball bearings that can roll around the surface, and imagine that, at a particular time, four of these ball bearings form a small square configuration. Now note that, unless we intervene, *the ball bearings will not maintain their square configuration as they roll around the sphere.* Each ball bearing will follow a geodesic, and those geodesics will either converge or diverge. If instead of a configuration of ball bearings, we imagine a small extended shape — made from some sort of slippery rubber material, say — that can sit flush on the sphere and is free to slide around the surface, we can see that elastic tensions will occur as it moves, tensions which will become stronger if it moves faster. (As the different parts of the shape will tend to follow their own geodesic inertial trajectories, the internal forces of cohesion will have to work to hold it in its equilibrium configuration as it moves.) Indeed, by carefully measuring such elastic tensions we would have an empirical method of measuring velocity relative to the sphere, and thus a

[4]The general family of non-Pythagorean metrics are now called Finsler metrics. See Chern (1996) and Coleman and Korté (2001) §4.5.

novel way to measure its curvature.

As noted at the outset, this is a strikingly simple observation. Nevertheless, it also has striking consequences. Perhaps the most remarkable consequence is that absolute motion — motion relative to space itself — is detectable in any space with a constant non-zero curvature. This point is spelled out and applied to the debate between substantivalist and relationalist attitudes to space by Graham Nerlich.[5] Nerlich notes that Leibniz's "boost" argument, that moving the entire material universe in absolute space would not make any empirical difference, *assumes that space is Euclidean.* (To my knowledge, Nerlich is the only commentator who has observed that the original (Galilean) principle of relativity is violated in the classical non-Euclidean geometries.)

The recognition that we could build a kind of "absolute motion detector" if we lived in a non-Euclidean space does not undermine Helmholtz's argument that the free mobility of rigid bodies demarcates the geometries of constant curvature. It is still true that a body of given dimensions can be placed in any part of space (i.e. independently of position) just in case the curvature of that space is not variable. (Helmholtz discusses the intuitive example of the difference between the surface of a sphere and the surface of an egg. On a constantly curved sphere, any "piece" of the surface will fit anywhere else, but this is not true on the variably curved surface of an egg.)[6] If one simply posits that a certain class of bodies are ideally rigid — made from a totally inflexible and unbreakable material — then it is still true that those bodies could be moved in spaces of constant curvature, but not, in general, in spaces of non-constant curvature. Nevertheless, the recognition that absolute motion is detectable in any constantly curved space does add a new spin to the notion of "free transport of rigid bodies", for it is only in a space described by flat, Euclidean geometry that such transport is truly "free". For present purposes, the point I will focus on is that in a space with a constant non-zero curvature, there will be an absolute state of rest: there will be a unique frame of reference in which stationary free bodies maintain their relative distances from one another and are not subject to internal elastic tensions.

3 Did Anyone Know?

Although it is not difficult to appreciate the fact that absolute motion is detectable in a space of constant non-zero curvature, I claim that no one in the nineteenth century showed any awareness of that fact. This is, at a first pass, a rather startling claim to make. How could it be that the luminaries grappling with physical geometry in the nineteenth century overlooked something that can appear relatively elementary to us now?

Let us first examine whether our nineteenth century protagonists at least came close to recognizing the detectability of absolute motion. Beginning with Helmholtz, we find the following short paragraph, for example, in "On The Origin and Meaning of Geometrical Axioms":

> It still remained to be seen whether the laws of motion as dependent on moving

[5]See Nerlich (1979) and Nerlich (1991).

[6]See Helmholtz (1876) 305.

> forces could also be consistently transferred to spherical or pseudospherical space. This investigation has been carried out by Professor Lipschitz of Bonn. It is found that the comprehensive expression for all the laws of dynamics, Hamilton's principle, may be directly transferred to spaces of which the measure of curvature is other than zero. Accordingly, in this respect also the disparate systems of geometry lead to no contradiction. (Helmholtz, 1876, 313-314)

Helmholtz here cites two papers, the most relevant of which is Lipschitz (1872), "An investigation of a Problem of the Calculus of Variations in which the Problem of Mechanics is Contained." However, what Lipschitz demonstrates there is limited to what Helmholtz claims on his behalf: Hamilton's principle (and the general resources and methodology of Hamiltonian mechanics) can indeed be applied within spaces of non-zero curvature. But Lipschitz does not study the motions of bodies, rigid or otherwise, in such spaces (something which can indeed be done by writing down the appropriate Hamiltonian).[7] Thus Helmholtz's appeal to Lipschitz is merely to make the modest point that there is no *contradiction* in attempting to study mechanics in spaces of constant curvature. If this is the closest Helmholtz came to recognizing the detectability of absolute motion in non-Euclidean geometry, it is evidently not very close at all.

Turning to Poincaré, it turns out that he came much closer. Poincaré directly engaged with the question of whether or not some version of the principle of relativity *might be satisfied in one geometry but violated in another.* In *Science and Hypothesis*, the "law of relativity" that he initially considers is the following: "the state of the bodies and their mutual distances at any moment will solely depend on the state of the same bodies and on their mutual distances at the initial moment, but will in no way depend on the absolute initial position of the system and of its absolute initial orientation" (Poincaré, 1952, 87). Noting that this law had been upheld by all experiments on the supposition that space is Euclidean, Poincaré quickly dismisses the idea that the law might be violated if we supposed instead that space was Lobachevskian (having a negative curvature). The only way of changing an absolute position without at the same time changing some relative position is to change the absolute position of the entire material universe, and that is not something that can be observed: "All that our instruments, however perfect they may be, can let us know will be the state of the different parts of the universe, and their mutual distances" (Poincaré, 1952, 89). Thus, if this law of relativity "is true in the Euclidean interpretation, it will be also true in the non-Euclidean interpretation" (ibid).

However, it will not have gone unnoticed that Poincaré's law of relativity only concerns absolute position (and orientation), not absolute velocity.[8] This is something he turns to address explicitly:

[7] A study of motions in non-Euclidean geometry was carried out by Wilhelm Killing, see in particular Killing (1885). Even there, however, there is no derivation of the fact that bodies would experience elastic tensions when moving in such a space.

[8] In other places, Poincaré gives a more straightforward statement of the Galilean relativity principle; see for example Poincaré (1946) 300. For some helpful remarks concerning Poincaré's use of expressions corresponding to "the principle of relativity," "the principle of relative motion" and "the relativity principle," see Darrigol (1995) 4.

> For the mind to be fully satisfied, the law of relativity would have to be enunciated as follows:– The state of bodies and their mutual distances at any given moment, as well as the velocities with which those distances are changing at that moment, will depend only on the state of those bodies and their mutual distances at the initial moment, and on the velocities with which those distances were changing at the initial moment. But they will not depend on the absolute initial position of the system nor on its absolute orientation, nor on the velocities with which that absolute position and orientation were changing at the initial moment. (Poincaré, 1952, 89-90)

Once we have included references to velocities, Poincaré notes that this law, "does not agree with experiments — at least, as they are ordinarily interpreted" (Poincaré, 1952, 90). And the reason that this law does not agree with experiments is because of *rotation*. If we were to be transported onto a planet "the sky of which was constantly covered with a thick curtain of clouds", we would still be able to detect the planet's rotation by, for example, "repeating the experiment of Foucault's pendulum" (ibid). The experimental detectability of absolute rotation is, according to Poincaré, "a fact which shocks the philosopher, but which the physicist is compelled to accept." Indeed, "We know from this fact Newton concluded the existence of absolute space." Poincaré does not see things this way, and he will go on to explain why. But for now, he can simply make the observation that "the difficulty is the same for both Euclid's geometry and for Lobachevsky's" (Poincaré, 1952, 91). Because the detectability of absolute rotation does not point to an empirical difference between Euclidean and non-Euclidean geometry, Poincaré feels secure in stating that for now he "need not therefore trouble about it further" (ibid). But his reasoning here is striking because it is evidence that he was genuinely unaware of the detectability of absolute *linear* motion, which is *only* detectable in non-Euclidean geometry.

Our next question, then, is how it could be possible for figures such as Helmholtz and Poincaré to have missed this fact. To begin to answer this question, we should note that the absolute motion detector is, in essence, measuring *tidal forces* due to *geodesic deviation*. These notions, which are central features of the theory of general relativity, can be illustrated via one of Einstein's well-known thought experiments. In the "Einstein elevator," we are to imagine a person inside a sealed, windowless container trying to ascertain (perhaps with a looming sense of panic) whether they are floating in empty space, far from other matter, or free falling in a gravitational field. From their perspective the two situations can appear identical: the person themselves, along with any other objects along for the ride, are floating weightlessly. This observation, that free falling in a homogeneous gravitational field is equivalent to moving inertially in empty space, is the *equivalence principle*, and it is something that Einstein once described as the happiest thought of his life.[9] The caveat that the gravitational field is homogeneous is important: someone free falling in a non-homogeneous gravitational field, like our own terrestrial gravitational field, could work out that they were not floating in empty space because the objects around them would be slowly drifting towards one another. In falling towards the center of the Earth, they are all approaching the

[9] Janssen et al. (2002) 265. For detailed discussion of the equivalence principle (or the several principles that have been called by that name) see Norton (1989) and Lehmkuhl (2021).

same point.

This drift effect is a manifestation of the phenomenon that we are after: tidal forces due to geodesic deviation. When objects move inertially they follow spacetime geodesics — they move in straight lines at a constant speed. This is simply a statement of Newton's first law of motion, and in the setting appropriate to classical mechanics, spacetime geodesics are just the four-dimensional equivalent of Euclidean straight lines.[10] However, in the variably curved spacetime described by general relativity, these geodesics can bend towards or away from one another. (That is, after all, just another way of saying that the spacetime is *curved*.) "Tidal forces" are the (apparent) forces that one might appeal to in order to explain the fact that objects are drifting closer together or further apart as a result of such geodesic deviation.

The crucial point for present purposes is that tidal forces also occur in spaces with *constant* non-zero curvature. And the fundamental reason why the absolute motion detector was overlooked in the nineteenth century, I contend, is that these notions — tidal forces and geodesic deviation — were not understood until the twentieth century; until after the development of general relativity.

To put this in its full context, we need to begin with the surprising fact that the notion of *parallelism* was absent from Riemann's original conception of a variably curved manifold in 1854, and remained absent in the subsequent development of Riemannian geometry for more than half a century (all the way up until Levi-Civita's reinterpretation of covariant differentiation in 1916). In the intervening time Riemann's innovations were in fact much more closely tied to *algebraic* developments, especially in the context of the absolute differential calculus developed by Ricci and others, than to work in geometry. In Ricci's hands, Riemannian notions were intentionally treated in a formal, abstract way.[11] When Einstein and his former classmate Marcel Grossman first began working with Riemannian geometry, they were, in a sense, starting down a path of bringing the algebraic tools of the absolute differential calculus *back* to Riemann's original geometrical setting. However, neither Einstein nor anyone else understood the full geometrical significance of these mathematical tools at that time.[12] And so it was that with the completed formulation of general relativity in 1915 there was a sudden surge of interest in understanding the absolute differential calculus from a less abstract (algebraic) and more intuitive (geometrical) perspective.

The peculiar nature of this situation was noted immediately by those who took up this task. Gerhard Hessenberg began his paper on the topic in the following way: "Due to the importance that the theory of quadratic differential forms has recently acquired for the

[10] At least, in typical formulations of classical mechanics, putting aside more exotic formulations such as Newton-Cartan theory.

[11] Thus the Christoffel symbol and the Riemann tensor, for example, did not initially have the geometrical associations they were to later acquire (see Goodstein (2018) 141, notes 24 and 27.) Indeed, the Riemann tensor was not even called a "curvature tensor" until after general relativity (see Reich (1992) 102).

[12] In the mathematical section of the initial "*Entwurf*" formulation of general relativity in 1913, Grossmann himself wrote: "I have purposely not employed geometrical aids because, in my opinion, they contribute very little to an intuitive understanding of the conceptions of vector analysis." (Einstein and Grossmann (1913); see Klein et al. (1995) 325.)

theory of relativity, the question of whether and how the extensive and cumbersome formal apparatus of this theory can be simplified, if not circumvented, is gaining new importance." (Hessenberg, 1917, 187). In a similar vein, Levi-Civita wrote in his paper:

> The mathematical development of Einstein's grandiose conception (which finds in Ricci's absolute differential calculus its natural algorithmic instrument) utilizes as an essential element the curvature of a certain four-dimensional manifold and the Riemann symbols relative to it. Encountering these symbols — or, rather, continuously using them — in questions of such a general interest, led me to investigate whether it would be possible to somewhat reduce the formal apparatus commonly used to introduce them... A refinement in this regard is actually possible, and essentially constitutes sections 15 and 16 of the present paper, which, initially conceived with only this purpose, gradually expanded to make some room for the geometric interpretation too. (Levi-Civita, 1916, 173)[13]

Levi-Civita went on to express his surprise that the proper geometrical interpretation of these Riemannian notions was not developed by Riemann himself: "At first I thought I would certainly find it in Riemann's original works... but there is barely an embryo of it." The mathematician Guido Castelnuovo, writing to Levi-Civita after reading his paper, was similarly surprised that the treatment of parallelism in a Riemannian manifold had remained obscure for such a long time: "The simplicity of the results ensures that you are dealing with a *natural* concept in differential geometry, and it is strange that experts in this branch of mathematics had missed it until now."[14] The fundamental insight that general relativity prompted Levi-Civita to uncover was that covariant differentiation represented parallel displacement. This in turn made room for the affine interpretation of the Riemann tensor, not to mention the full notion of "spacetime curvature" that is now associated with general relativity. Returning to our main theme, it was this development which similarly led to the concept of geodesic deviation and the associated phenomenon of tidal forces. Here is the point made by John Stachel:

> Einstein's introduction of the metric tensor field as the mathematical representation of both the chrono-geometry of space-time and the potentials for the gravitational field did not carry with it most of the geometrical implications that we take for granted today. Insofar as it did carry geometrical implications, notably in fixing the geodesics of the manifold, this had to do with the interpretation of geodesics as shortest paths (or rather longest, for time-like paths...) in space-time. The interpretation of geodesics as the straightest paths in space-time, more important for the understanding of the gravitational field — in particular, the interpretation of the Riemann tensor in terms of the equation of geodesic deviation — had to await the work of Levi-Civita and Weyl on parallelism... Curvature, in other words, was given the Gauss-Riemann interpretation, rather than the interpretation as the tendency of geodesics to converge (or diverge), leading to its association with tidal forces. (Stachel, 2007, 437)

[13] Partial English translation in Renn et al. (2007), 1081-1082.
[14] Castelnuovo to Levi-Civita, 19 April 1917; quoted in Goodstein (2018), 118.

To sum up, I have argued that the fundamental reason why no one in the nineteenth century showed any awareness of the possibility of detecting absolute motion in spaces of constant non-zero curvature is that the crucial notions of geodesic deviation and tidal forces had not yet been developed at this time.

Beyond this, there was not yet a proper study of four-dimensional spacetime geometry and, in particular, an appreciation of the connection between inertial trajectories and affine geodesics. In brief: the study of spacetime geometry in the sense with which we are familiar today began with Minkowski's geometrical reinterpretation of special relativity. Although Einstein did not immediately appreciate Minkowski's contribution, he later saw it as an essential component in his development of general relativity.[15] As part of the geometrical unpacking of Riemannian geometry that came in the years after 1915, Weyl showed that affine structure could be defined independently of (and prior to) metric structure. In this way, it became possible to recognize that four-dimensional affine geodesics represented inertial trajectories. With all this in view, the kinematical differences between Euclidean and non-Euclidean geometries are relatively easy to see. But the appreciation of four-dimensional spacetime geometry and the physical significance of affine geodesics are, of course, highly non-trivial developments, and this helps to make more intelligible why something in this domain which strikes us as fairly straightforward now may have been completely out of sight to figures in the nineteenth century. To them, the fundamental notions of physical geometry were always *metrical* notions. As we have seen, the most immediate point that one may be inclined to note concerning the physical significance of metrical notions in the constant (zero and non-zero) curvature geometries is that congruence transformations corresponding to the "free mobility of rigid bodies" occur in *all* of them.

4 Some Counterfactual History

I now want to consider the counterfactual question: what might have been different if our nineteenth century protagonists had recognized that absolute motion is detectable in spaces of constant non-zero curvature?

Let us begin with a rough characterization of different positions in the philosophy of geometry. A *geometric empiricist* is someone who regards physical geometry as analogous to other physical theories, such as theories of heat or electricity. The question of the correct geometry is thus to be settled by experiment, and the actual value of the curvature of space, zero or non-zero as the case may be, is an empirical question. A *geometric conventionalist*, by contrast, is someone who thinks that choosing between alternative physical geometries is analogous to choosing between alternative systems of units (e.g. metric or imperial) or choosing between alternative coordinate systems (e.g. Cartesian or polar). Any of these choices will be, in some sense, equally valid, but one may prove to be simpler or more convenient than another. Where a geometrical empiricist claims that the empirical facts will determine the one true geometry, a geometrical conventionalist claims that a plurality of geometries can always be made to cohere with the facts.

[15] For detailed analyses of Einstein's path from special to general relativity, see Renn et al. (2007) and Janssen and Renn (2022).

These two positions — geometrical empiricism and geometrical conventionalism — are typically associated with Helmholtz and Poincaré respectively, but as will become clear the rough characterizations that I have sketched so far will prove inadequate to their actual views. I will deal quickly with the crude empiricist before spending considerably more time with the conventionalist, and in particular Poincaré himself. I return to the real Helmholtz, and reflect on how far his supposed empiricism was from Poincaré's conventionalism, in the conclusion.

The likely response of a geometrical empiricist to the absolute motion detector is clear enough: for them, it would have indicated a new way to measure the geometry of space. In the context of the nineteenth century, the curvature of space certainly seemed to be zero within the range of experimental error. But as non-Euclidean geometry gained acceptance as a genuine physical possibility, Gauss arranged to measure the internal angles of a triangle formed by three mountain peaks to see if he could detect any deviation from Euclidean expectations. Later, both Helmholtz and Poincaré entertained the idea that measuring the angles of astronomical triangles would be the best way to probe the curvature of space.[16] So for someone inclined to think that physical geometry can be determined empirically, the absolute motion detector would presumably have been regarded as pointing to a novel way to measure the geometry of space.

What about a geometrical conventionalist? At first sight, the absolute motion detector might sound like a straightforward refutation of geometrical conventionalism. However, note that we didn't need the detectability of absolute motion in order to find a way to measure the geometry of space. The most obvious way to do that is simply to measure the internal angles of large triangles. Poincaré explicitly considers an imagined situation in which we encounter a violation of Euclidean geometry when measuring the parallax of distant stars, but then claims that we would really have a "choice between two conclusions: we could give up Euclidean geometry, or modify the laws of optics, and suppose that light is not rigorously propagated in a straight line" (Poincaré, 1952, 84). Of course, if we have *stipulated* that the propagation of light sets the standard of straightness — if we have defined "straight line" to mean the path of a ray of light — we cannot then *empirically test* that stipulation. We could elect some other standard of straightness instead, but the criterion for such a choice, according to Poincaré, is not the *truth* of the matter (whether this or that phenomena *actually* instantiates straightness), but rather the *convenience* of the choice. One definition of straightness will make our physics simpler, whereas another will make it more complicated. The fact that we can make a choice — that we *must* make some choice — concerning which physical phenomena instantiate the geometrical notion of straightness is the crucial point. Just as we must settle on a system of units, Poincaré argues, we must also settle on certain geometrical conventions.

It is helpful for Poincaré's argument that, at least at a first pass, it seems plausible that we might give up the claim that light propagates in a straight line. Making that shift doesn't seem like it would immediately cause chaos in the rest of our physics.[17] Hence it seems that,

[16] See Poincaré (1952) 83-84, and Helmholtz (1977) 18. In fact, the measurement of stellar parallax was already discussed by Lobachevsky himself; see Bonola (1912) §45.

[17] Note that, prior to relativity theory, there was no intimate connection between a light ray and a spacetime

in the scenario that Poincaré is imagining, we might genuinely have to weigh up which of the two descriptions is more convenient. Poincaré himself, with ill-fated confidence, declares: "It is needless to add that every one would look upon this solution [giving up the claim that light propagates in a straight line] as the more advantageous" (Poincaré, 1952, 84).

Our question now is whether there is an essential difference between the absolute motion detector and the more straightforward idea of the measurement of stellar parallax. As we have seen, Poincaré engages with the question of whether or not the principle of relativity might be satisfied in one geometry but violated in another, arriving at the observation that a principle of relativity which includes references to velocities is false in full generality because of the experimental detectability of absolute rotation (at least insofar as our experiments "are ordinarily interpreted" (Poincaré, 1952, 90)). However, given that absolute rotation is similarly detectable in both Euclidean and non-Euclidean geometry, Poincaré puts the matter to one side. Now, I have claimed that Poincaré simply did not know about the experimental detectability of absolute *linear* motion in non-Euclidean geometry. Combined with the reasoning he offers for ignoring the detectability of absolute rotation ("the difficulty is the same for both Euclid's geometry and for Lobachevsky"), this might suggest that Poincaré would have regarded the absolute motion detector as genuinely threatening to his conventionalist stance. But we are not done yet. When Poincaré later returns to his argument against the idea of absolute space, he makes clear that, from his perspective, the claim that a frame of reference is undergoing rotation *is also a convention.*

According to Poincaré, the inhabitants of a permanently cloudy planet would be able to account for inertial effects by regarding centrifugal and coriolis forces as real rather than pseudo (or "inertial") forces. The inhabitants of this planet would have to reckon with the fact that these forces increase with distance and disrupt the isotropy of space ("They would see, for instance, that cyclones always turn in the same direction, while for reasons of symmetry they should turn indifferently in any direction" (Poincaré, 1952, 130)). But Poincaré argues that they could maintain a commitment to the symmetry of space and the principle of relativity by supposing the existence of an all-pervading ether with particular mechanical properties. In short,

> they would invent something which would not be more extraordinary than the glass spheres of Ptolemy, and would thus go on accumulating complications until the long-expected Copernicus would sweep them all away with a single blow, saying it is much more simple to admit that the Earth turns round. Just as our Copernicus said to us: "It is more convenient to suppose that the Earth turns round, because the laws of astronomy are thus expressed in a more simple language," so he would say to them: "It is more convenient to suppose that the Earth turns round, because the laws of mechanics are thus expressed in much more simple language." (Poincaré, 1952, 130-131)

Granting that this simpler description would quickly win people around, Poincaré nevertheless insists this is a conventional choice: "these two propositions, 'the Earth turns round,' and, 'it is more convenient to suppose that the Earth turns round,' have one and the same

geodesic. This is, of course, a crucial point in the context of this paper.

meaning. There is nothing more in one than in the other." (Poincaré, 1952, 131)[18]

Recall, however, that unlike absolute rotation, the absolute motion detector identifies an effect that would only occur if the curvature of space was non-zero. How might Poincaré have attempted to accommodate it? Let us imagine a universe with a very small negative curvature and with inhabitants who, like us, have developed the practice of using Euclidean geometry, but who, unlike us, have detected what seems to be a privileged rest frame. They have observed that extended objects moving at a high velocity relative to this frame register elastic tensions, and that several such objects drift apart from one another when they seem like they should be moving inertially. The inhabitants of this universe have also observed that these effects become more and more pronounced as the relative velocity increases. Perhaps, in addition, they have detected that the parallax of distant stars is positive. In the face of this, could they nevertheless maintain a Euclidean description of their world?

It seems that they could, and in much the same way as the inhabitants of Poincaré's cloudy planet could avoid describing their reference frame as rotating. The inhabitants of our Lobachevskian universe could appeal to the existence of an all-pervading ether, motion relative to which caused objects to repel one another. If they had also detected that the parallax of distant stars was positive, this could be attributed to the particular effect of the ether on the propagation of light. We can imagine, of course, that an eventual geometrical Copernicus — as we might call him — would eventually sweep away such an ether hypothesis by observing that it is much simpler to admit that the universe is Lobachevskian. And yet it still seems available to Poincaré to claim that the two propositions, "the universe is Lobachevskian" and "it is more convenient to suppose that the universe is Lobachevskian" have the same meaning; that there is nothing more in one than in the other.

What about the reverse scenario? Can we describe our own world, which *lacks* any measurable absolute velocity, as if it were Lobachevskian? We would have to posit the existence of an ether which systematically hides a preferred rest frame from us: as objects moved, the tidal forces associated with motion through Lobachevskian space would have to be exactly compensated for by the attractive effect generated by moving through this ether. (This brings to mind the original explanations of the negative result of the Michelson-Morley experiment, with Lorentz (and others) arguing that motion through the electromagnetic ether contracted measuring instruments in just the right way so as to render such motion unobservable.)[19] So, once more, we have two alternative descriptions, one using Euclidean geometry and one using Lobachevskian geometry, and as before the choice between these alternatives can be deemed, in some sense, a matter of convenience.

I think that it is certainly plausible that this would have been Poincaré's response. The upshot seems to be that the absolute motion detector was not, after all, significantly different from the cases he was aware of. Where does this leave us?

[18]This remark points to Poincaré's underlying commitment to the denial of absolute space. I return to this point, and to Poincaré's more considered discussion of the Earth's rotation three years later in *The Value of Science*, in section 6 below.

[19]For Poincaré's own discussions of Lorentz contraction, see for example Poincaré (1946) 305-308 and 415-416.

5 Conventionalism and Relationalism

At this point we need to get a little clearer on the thesis of geometrical conventionalism. The fundamental claim is that the metric is not objectively determinable. The facts do not and cannot settle the specific form of the metric; rather, we must establish a form of the metric by agreeing on a convention (even if that convention is non-arbitrary). What arguments can one muster to establish this conventionalist conclusion? Note that it is not sufficient to appeal to the idea that one can choose from among the constant curvature geometries so long as one is willing to alter the relevant aspects of the rest of one's physics. Poincaré does give this kind of argument as we have seen (in the imagined case of measuring a negative value for stellar parallax, for example, he argues that we could keep Euclidean geometry so long as we we are willing to assert that light does not travel in straight lines), but if that were the sole basis of his conventionalism, it would force the question of what's so special about geometry. The same kind of holistic argument can be applied to almost any aspect of any theory, and hence geometrical conventionaism would be at risk of collapsing into general theoretical holism.[20] To rescue it, we need some argument for why geometry is conventional other than in the (perhaps trivial) sense in which any aspect of any theory can be regarded as conventional.

An influential approach to this matter has been to argue that geometrical conventionalism is founded on a crucial fact about space: the fact of "metrical amorphousness".[21] From this perspective, geometrical conventionalism depends on a prior commitment to metric relationalism. David Stump has argued prominently that this is the basis of Poincaré's position,[22] observing that Poincaré recognizes a common basis for the different metric geometries that he is considering — the "amorphous continuum":

> In this continuum, primitively amorphous, we may imagine a network of lines and surfaces, we may then convene to regard the meshes of this net as equal to one another, and it is only after this convention that this continuum, become measurable, becomes Euclidean or non-Euclidean space. From this amorphous continuum can therefore arise indifferently one or the other of the two spaces, just as on a blank sheet of paper may be traced indifferently a straight or a circle. (Poincaré 1946, 235; see also 238)

This amorphous continuum is, in essence, a topological manifold. Poincaré does not say much regarding the exact ontological status of this underlying manifold,[23] but for present

[20]Cf. Weatherall and Manchak (2014), 246: "Ultimately... the attractiveness of a conventionalist thesis turns on how much one needs to postulate in order to accommodate alternative conventions. In some sense, one can be a conventionalist about anything, if one is willing to postulate enough — an evil demon, say." Although Poincaré has often been interpreted as relying on holist arguments (see Friedman (1999) 71-73), in fact he explicitly distinguished his view from general theoretical holism (see Stump (1989) 336-337).

[21]For the revitalization of this debate in the second half of the twentieth century, see Grünbaum (1968) and the responses in Friedman (1972), Glymour (1972) and Putnam (1974).

[22]Cf. Stump (1989) 335: "Poincaré's argument for his thesis of the conventionality of metric depends on a relationalist program for dynamics, not on any general philosophical interpretation of science... his arguments for the conventionality of metric do not depend on any global strategies such as general empiricism or Duhemian underdetermination arguments." See also Stump (1991) and Stump (2023).

[23]Poincaré writes: "The same questions which came up apropos of the truths of Euclidean geometry, come

purposes it suffices to note that he doesn't regard topological facts as conventional.[24] The topological continuum obviously lacks (by stipulation) intrinsic metric properties. Thus, to bring in metric structure, we need to apply something extrinsic. And it is here that we encounter relationalism: it is the *relations among material objects* that provides the basis for talking about the metric, and for distinguishing different metric geometries from one another. On Stump's interpretation, it is this stance on the nature of space that forces Poincaré's conventionalist conclusion in the specifically geometrical case. Hence geometrical conventionalism is rescued from collapsing into general theoretical holism.

It is clear, I think, that some such argument is required in order to preserve geometrical conventionalism as a distinct position. But for present purposes the most relevant point is that, *if* geometrical conventionalism depends on a commitment to relationalism, then the absolute motion detector takes on a heightened significance. Going back to Leibniz, as Nerlich (1991) has observed, some of the original arguments in favor of relationalism rely on the tacit premise that space is Euclidean. In the famed debate with Samuel Clarke, Leibniz is pressed with the following argument: "If space was nothing but the order of things coexisting; it would follow, that if God should remove in a straight line the whole material world entire, with any swiftness whatsoever; yet it would still always continue in the same place: and that nothing would receive any shock upon the most sudden stopping of that motion" (Leibniz and Clarke, 1956, 32). Leibniz's response is as follows:

> the fiction of a material finite universe, moving forward in an infinite empty space, cannot be admitted. It is altogether unreasonable and impracticable. For, besides that there is no real space out of the material universe; such an action would be without any design in it: it would be working without doing anything, *agendo nihil agere*. There would happen no change, which could be observed by any person whatsoever. These are the imaginations of philosophers who have incomplete notions, who make space an absolute reality. (Leibniz and Clarke, 1956, 63-64)

We now know that it is a fact particular to Euclidean space that it is always possible to find inertial trajectories which remain a fixed distance from one another. In geometries of non-zero constant curvature, however, it is *never* possible to find inertial trajectories which remain a fixed distance from one another. Hence the argument that absolute motion is absurd because it is entirely unobservable falls flat. Of course, few would fault Leibniz, or anyone else in the seventeenth or eighteenth century, for failing to anticipate non-Euclidean geometry. But a central concern of this paper has been to show how it was a crucial lacuna in the *nineteenth* century — once non-Euclidean geometries had become a central object

up anew apropos of the theorems of analysis situs. Are they obtainable by deductive reasoning? Are they disguised conventions? Are they experimental verities? Are they the characteristics of a form imposed either upon our sensibility or upon our understanding?" (Poincaré 1946, 239) However, he does not offer answers to these questions, limiting himself to simply observing "that the last two solutions exclude each other."

[24]It is noteworthy that Poincaré often puts aside the possibility of a *positively* curved space, limiting the viable options to just Euclidean or Lobachevskian geometry. Both these geometries are infinite, whereas a positively curved space is finite. It appears, then, that Poincaré tacitly concedes that such a (global, topological) difference is not, in the end, a matter of convention. For some further discussion of Poincaré's stance on topology, see Stump (1996).

of discussion — that the possibility of detecting absolute in a non-Euclidean space was not recognized. At any rate, we can now appreciate how the absolute motion detector presents a challenge to classical relationalism. And insofar as geometrical conventionalism depends on relationalism, it presents a challenge to conventionalism too.

6 The Price of Inconvenience

Even assuming that it is possible to maintain conventionalism independently of an underlying commitment to relationalism, the absolute motion detector makes vivid the kind of "inconvenience" that would be at issue if we were to choose an inappropriate geometry. One reason that this is significant is because in the nineteenth century the way in which it was imagined that we might discover that space was non-Euclidean was via measuring the parallax of distant stars. If positing that light doesn't propagate in straight lines does not require making dramatic changes in other parts of our physics, then holding a firm commitment to Euclidean geometry might not look like a particularly inconvenient choice. However, as we have seen, deciding on the appropriate geometry is not merely a matter of making the seemingly isolated decision concerning whether or not light propagates in a straight line. Rather, it has similarities to deciding whether or not the Earth is rotating. In some sense, perhaps we *can* make the inconvenient choice, but it will be at the cost of undermining the plausibility and coherence of the rest of our theoretical understanding of nature.

In fact, in *The Value of Science*, published three years after *Science and Hypothesis*, Poincaré returns to the question of the rotation of the Earth. Noting that some had misunderstood him as offering a "justification of Galileo's condemnation," Poincaré asserts that, on the contrary, the Earth's rotation has the same certainty for him as "the very existence of external objects" (Poincaré, 1946, 353). Although he still holds firmly to his rejection of absolute space, and thus continues to deny that it is a straightforward fact (rather than a convention) that the Earth rotates ("*in the kinematic sense*"), he argues that the far-reaching unifying power of adopting that convention — indeed, adopting the entire Copernican system — places it in a very special position:

> The intimate relation that celestial mechanics reveals to us between all the celestial phenomena are true relations; to affirm the immobility of the Earth would be to deny these relations, that would be to fool ourselves. The truth for which Galileo suffered remains, therefore, the truth, although it has not altogether the same meaning as for the vulgar, and its true meaning is much more subtle, more profound and more rich. (Poincaré, 1946, 353).

I have argued that the absolute motion detector brings out the fact that the "convention" of adopting a physical geometry is similar to the convention of adopting the Copernican system. Hence if Poincaré had known about the absolute motion detector, perhaps he would have been prompted to clarify that there is after all a subtle, profound, and rich sense in which the notion of truth does apply to physical geometry.

Another point to note concerns Poincaré's confidence that physicists would always prefer Euclidean geometry, come what may. Now, although it would hardly be contentious to claim that Poincaré erred on this point, such a criticism typically appeals to the much more radical shift brought about by the use of variably curved geometry in general relativity. According to Schlick's assessment: "The successes of Einstein's general theory of relativity, which sacrifices the validity of the Euclidean axioms, prove the error of Poincare's assertion, and it may be said with certainty that he would today gladly withdraw it in the face of those successes" (in Helmholtz (1977) 33, note 38). Poincaré can surely be forgiven for not foreseeing the general theory of relativity. But what the absolute motion detector helps to show is that Poincaré can be criticized for his rigid commitment to Euclidean geometry just within the realm of classical physics.

This brings us back to the supposed distinction between geometrical empiricism and geometrical conventionalism. Returning finally to Helmholtz's actual view, it is evident that he clearly acknowledged key tenets of the conventionalist stance. In the opening of his lecture, "On the Facts Underlying Geometry", he writes:

> in geometry we deal constantly with ideal structures, whose corporeal portrayal in the actual world is always only an approximation to what the concept demands, and we only decide whether a body is fixed, its sides fiat and its edges straight, by means of the very propositions whose factual correctness the examination is supposed to show. (Helmholtz, 1977, 39)

Here we find Helmholtz clearly articulating the very same issues that motivate geometrical conventionalism. And he made much the same point in "On the Origin and Meaning of the Geometrical Axioms":

> we have no criterion for the fixity of bodies and spatial structures other than that when applied to one another at any time, in any place and after any rotation, they always show again the same congruences as before. But we certainly cannot decide in a purely geometrical way, without bringing in mechanical considerations, whether the bodies applied to each other have not themselves both changed in the same manner. (Helmholtz, 1977, 24)

The application of basic geometrical notions is at the same time the application of basic mechanical notions. Inevitably, the two go hand in hand. If Helmholtz is seen as the standard bearer of geometrical empiricism, then the distinction between empiricism and conventionalism becomes very hazy indeed. As some recent commentators have put it, "the empiricist vs. conventionalist distinction turns out to be a false dichotomy" (Ben-Menahem and Duerr, 2022, 167). For Poincaré too, geometrical conventionalism was never just about geometry; on his view the principles of mechanics "share the conventional character of the geometrical postulates" (Poincaré, 1952, xx). Poincaré is also aware that the choices we make in geometry, just like the choices we make in mechanics, are "guided by experimental facts" (Poincaré, 1952, 58). Hence "the principles of geometry are only conventions; but these conventions are not arbitrary" (ibid).

At the end of the day, in the context of nineteenth century philosophy of geometry the lesson arising from the absolute motion detector is perhaps the following. If the distinction between geometrical conventionalism and geometrical empiricism ends up being more of a difference in emphasis — *the logical availability of an alternative*, on the one hand, or the fact that *the* *choice we make will be guided by experiment*, on the other — then perhaps it turns out that Helmholtz's emphasis had more to recommend it than Poincaré's.

Before concluding, it should be noted that there is a different response that would likely have arisen in the face of the absolute motion detector. If someone were disposed to think that absolute motion is impossible in principle — if space were thought of as not the kind of thing that objects can move with respect to — then that idea could be leveraged as a metaphysical argument against the very possibility of space having a constant non-zero curvature in the first place.

Take Russell's view in *An Essay on the Foundations of Geometry* (1897). According to Russell, space is a *form of externality*: a non-conceptual element of our conscious experience that serves as a necessary prerequisite for knowledge of a world of separate, mutually external objects. The comparison with Kant's account of space as a form of intuition is helpful here. For Kant, space and time, as the two forms of intuition, are necessary prerequisites for our experience of objects. A form of intuition is not the kind of thing that can stand in a relation to an object; it is what allows for objects to be discernible from one another and thus stand in possible relations. Russell's view is, in central respects, an adaptation of Kant's, replacing forms of intuition with forms of externality:[25]

> [Externality] must mean, in this argument, the fact of Otherness, the fact of being different from some other thing... So much, then, would appear to result from Kant's argument, that experience of diverse but interrelated things demands, as a necessary prerequisite, some sensational or intuitional element, in perception, by which we are led to attribute complexity to objects of perception (Russell, 1956, 62)

So construed, space is what allows for objects to stand in possible relations to one another. Space therefore cannot itself stand in a relation, in particular a spatial relation, to an object. It is on this basis that Russell argues that space is necessarily metrically homogeneous, i.e. that it must have a constant curvature: if space had a variable curvature, there would be distinctions between different parts of space and bodies could then stand in relations to those parts. But that is incompatible with the conception of space as a form of externality:

> Space would no longer be passive, but would exercise a definite effect upon things, and we should have to accommodate ourselves to the notion of marked points in empty space; these points being marked, not by the bodies which occupied them, but by their effects on any bodies which might from time to time occupy them. This want of homogeneity and passivity is, however, absurd; space must, since it is a form of externality, allow only of relative, not of absolute, position, and must be completely homogeneous throughout. To suppose it otherwise, is to give it a thinghood which no form of externality can possibly possess. (Russell, 1956, 152)[26]

[25] For a detailed discussion of the connection between Russell's notion of a form of externality and Kant's notion of a form of intuition, see Nunez (2024).

[26] Poincaré also talks of the "passivity" of space; see, for example, Poincaré (1946) 83.

Russell, along with everyone else, was unaware of the possibility of detecting absolute motion in a space of constant non-zero curvature. But had he come to recognize it, a plausible reaction he might have had at the time would have been to regard the absolute motion detector as refuting the possibility that space *could* have a constant non-zero curvature. Just as he regarded it as absurd for bodies to be related to positions in space (thus refuting the possibility of space having a variable curvature), he would have presumably seen it as absurd for bodies to *move* relative to space, let alone be affected by such motion. In that case, space would no longer be passive but would exercise a definite effect upon things; to suppose it otherwise would be to give space "a thinghood which no form of externality can possibly possess." Thus, if non-zero curvature of any kind (variable or constant) were ruled out, space would have been deemed necessarily Euclidean after all.

This brings us to a final figure in this story: the Belgian philosopher and mathematician, Joseph Delboeuf (1831–1896). As has recently been discussed by Fay (2024), Delboeuf had already been promoting a different argument for the claim that space must be Euclidean, based on the observation that only Euclidean space accommodates the notion of *similarity*. In a space with a non-zero constant curvature, expanding or shrinking an equilateral triangle, say, will change its internal angles; thus it is only Euclidean geometry that allows for scale invariance. Now, scale transformations (uniform expansions and contractions) are evidently unlike rigid motions (uniform translations and rotations) in some important respects. Firstly, scale transformations do not occur in nature in any obvious way; secondly, and more importantly, they do not seem necessary for the possibility of measuring spatial magnitudes (recall the role of the free mobility of rigid bodies in Helmholtz's solution to the problem of space). Perhaps for these reasons, neither Poincaré nor Russell were moved by Delboeuf's argument. However, independently of this, Delboeuf himself would surely have welcomed the recognition of the possibility of detecting absolute motion in spaces of non-zero constant curvature, and seen it as buttressing his claim that space must be Euclidean. And if Delboeuf had had the absolute motion detector in his arsenal, perhaps his argument would have been received quite differently by others as well.

7 Conclusion

I have argued that the detectability of absolute motion in spaces of constant non-zero curvature was out of sight to the major figures who grappled with physical geometry in the second half of the nineteenth century. To account for this, I noted that the central notions on which the absolute motion detector depends — the notions of tidal forces and geodesic deviation, or the physical significance of affine geodesics — were twentieth century innovations that were only developed in the aftermath of general relativity.[27] More fundamentally, I suggested that recognizing the possibility of the absolute motion detector depends on a shift from spatial to spatiotemporal thinking, and associating a straight line with an inertial trajectory.

I argued that if the idea of the absolute motion detector had been appreciated at the time, its

[27] Stachel (2007) argues that a central aspect of this history could have been otherwise. In particular, Stachel argues that the notion of an *affine connection* could have been developed in the nineteenth century, before the theory of relativity.

impact on the philosophy of geometry would have been widely felt. I sketched three responses that Helmholtz, Poincaré, and Russell (or Delboeuf) might have had. The first (empiricist) response would have been to simply accept that absolute motion might be detectable and might even provide a means of measuring the curvature of space. In this way, the idea of measuring absolute motion would not have been so different from the idea of measuring negative parallax. The second (conventionalist) response would have been to insist that the choice among the constant curvature geometries was conventional, but to acknowledge that choosing an "inconvenient" geometry would be like choosing the Ptolemaic system over the Copernican. The third (metaphysical) response would have been to regard the detectability of absolute motion as ruling out the physical significance of non-Euclidean geometries altogether. Just as many thought that the transcendental requirement of the free mobility of rigid bodies ruled out variable curvature, some would have seen the metaphysical impossibility of absolute motion as ruling out constant non-zero curvature, thus making the world necessarily Euclidean after all.

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