Back to the Problem of Space

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1 Introduction

The main idea of this paper is remarkably simple. The idea is that geodesic deviation, familiar from the theory of general relativity, is a physically significant phenomenon 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 of the major figures who engaged with the philosophy of geometry in the nineteenth century failed to recognize that *absolute motion is detectable in any space of constant non-zero curvature*. I will examine the implications of this for the main positions in the philosophy of geometry that arose in this period: *geometrical empiricism* and *geometrical conventionalism*, typically attributed to Helmholtz and Poincaré respectively. I will argue that the distinction between Helmholtz and Poincaré in this regard, which was never on very firm ground in the first place, is further weakened. I will also suggest that the way in which the choice of a physical geometry remains a "convention" in this context is quite unlike the convention of, say, choosing a system of measuring units, despite Poincaré's suggestions to the contrary.

2 The problem of space

I want to begin by reviewing what I take to be the central problem of physical geometry in the nineteenth century: the "problem of space" (or the "Helmholtz/Lie problem of space"). This is the problem of identifying which features of geometry, regarded as a particular field of mathematics, allow it to be used as a description of physical space. To put it another way, the problem of space is 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 mathematical geometries exist. (For most of its history, geometry was just Euclidean geometry, which was regarded as nothing other than a systematic description of physical space.) 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. Proliferating the number of mathematical geometries further, Riemann then developed the general notion of a variably curved geometry in his 1854 Inaugural Dissertation. The question thus arose: did all of these mathematical 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: 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 the geometries of constant 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 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.[1] 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 1905, 56).[2]

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 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

[1] The general family of non-Pythagorean metrics are now called "Finsler" metrics [ref].

[2] Poincaré, H. *Science and Hypothesis.* London: Dover, 1905.

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 together as it moves.) Indeed, by carefully measuring such elastic tensions, we would have a kind of empirical method of measuring the curvature of the sphere.

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 relativist attitudes to space in Nerlich (1979).[3] Nerlich notes that the Leibniz shift 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" in a non-Euclidean geometry 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 constant. (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.) 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 one frame of reference in which stationary free bodies will maintain their relative distances from one another and not be subject to internal elastic tensions.

3 Geometrical conventionalism

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

[3]Nerlich, G. "What Can Geometry Explain?" *The British Journal for the Philosophy of Science* 30(1): 1979.

One possible answer is that, for a geometrical empiricist — someone who thinks that the question of the correct geometry is to be settled by experiment — this insight 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. Thus, for someone inclined to think that physical geometry can be determined empirically, the detectability of absolute motion in a space of constant non-zero curvature would provide a new experimental probe. And if Helmholtz is taken as the standard bearer of geometrical empiricism (a point which I will return to later on), it's plausible that this would have been his response.

What about a geometrical conventionalist? Geometrical conventionalism — principally advanced by Poincaré — can be understood as the claim 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 and more convenient than another. Thus, where geometrical empiricism claims that the empirical facts will determine the one true geometry, geometrical conventionalism claims that a plurality of geometries can always be made to cohere with the facts. Thus Poincaré writes "One geometry cannot be more true than another; it can only be more convenient" (59).[4]

We have seen that absolute motion is a phenomenon that only occurs in a space of constant non-zero curvature, and could even provide a way to measure the value of that curvature. This sounds 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" (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

[4]For the remainder of this paper, a page number without a further citation refers to to Poincaré 1905.

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. Hence it seems that, 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" (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 it turns out, Poincaré explicitly addresses the question of whether or not *the principle of relativity might be satisfied in one geometry but violated in another*. 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" (87). Noting that this law has been upheld by all of our 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. 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 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;" thus, "If the law is true in the Euclidean interpretation, it will be also true in the non-Euclidean interpretation" (89).

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

> For the mind to be fully satisfied, the law of relativity would have to be enunciated as follows:– The sate of bodies and their mutual distances at any given moment, as well as the velocities with which their 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. (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" (90). 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" (90). 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" (91). Because the detectability of absolute rotation does not point to an empirical difference between Euclidean and Lobachevskian geometry, Poincaré feels secure in putting the matter to one side. But his reasoning here is striking insofar as it indicates that he may well have been troubled by the case of absolute *linear* motion, which is *only* detectable in non-Euclidean geometry.

However, we are not done yet. In arguing against the idea of absolute space, Poincaré returns to the matter of absolute rotation and makes clear this is something *that he also regards as conventional*. 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" (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." (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 meaning. There is nothing more in one than in the other." (131)

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 which could naturally be described as Lobachevskian, that is as having a small negative curvature. Let us also imagine that the inhabitants of this universe have, like us, developed the practice of using Euclidean geometry but have recently 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. They 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 be unobservable.) 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 does not, after all, appear to be significantly different from the cases he is aware of. Where does this leave us?

4 The price of inconvenience

What the absolute motion detector helps to make vivid is the kind of "inconvenience" that would be at issue if we were to choose an inappropriate geometry. This is significant because, in the nineteenth century context, 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 has many similarities to deciding whether or not the Earth is rotating. In some sense, we *can* make the inconvenient choice, but it will be at the cost of introducing obviously arbitrary (and empirically ungrounded) elements in the rest of our physics.

Now, it would hardly be contentious to claim that Poincaré erred in assuming that Euclidean geometry would always be preferred. However, such a criticism of Poincaré 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).[5] 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 in a more straightforward way, just within the realm of classical physics.

This brings us back to the supposed distinction that we started with, between empiricism and conventionalism. The representatives of these two positions are typically taken to be Helmholtz and Poincaré respectively, but it quickly becomes evident that the distinction between them is not so easy to make out. Helmholtz, for his part, clearly acknowledges key tenets of the conventionalist position. 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 makes 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" (Duerr and Ben-Menahem 2022, 167).[6]

For Poincaré too, geometrical conventionalism was never just about geometry. Physical geometry can only be construed as conventional insofar as the principles of mechanics can also be regarded as conventional: if we change geometry, we must at the same time change our laws concerning the displacement of "rigid bodies" and the propagation of light, and so

[5]Helmholtz, H. "Epistemological Writings." (P. Hertz and M. Schlick 1921 Centenary Edition). *Boston Studies in the Philosophy of Science* XXXVII. 1977.

[6]Ben-Menahem, Y. and P. Duerr, "Why Reichenbach wasn't entirely wrong, and Poincaré was almost right, about geometric conventionalism," *Studies in History and Philosophy of Science* 96: 154–173, 2022.

on. As Poincaré makes perfectly clear, on his view the principles of mechanics "share the conventional character of the geometrical postulates" (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" (58). He also makes clear that the geometrical conventions we settle on will be highly non-arbitrary:

> the principles of geometry are only conventions; but these conventions are not arbitrary, and if transported into another world (which I shall call the non-Euclidean world, and which I shall endeavour to describe), we shall find ourselves compelled to adopt more of them. (xx)

With this in view, it may seem all the more surprising that Poincaré felt so convinced that Euclidean geometry had "nothing to fear from fresh experiments" (84). Poincaré devoted sustained attention to the processes by which we may have come to develop our spatial intuitions. In particular, thanks to our ability to move external objects and move our own bodies, we came to articulate laws of displacement — the laws which are, according to Poincaré, "the object of geometry" (73). Poincaré also paints a vivid picture of how it would be possible to exist in a world in which the actual laws of displacement would have naturally brought us to use Lobachevskian geometry.[7] So given that he saw experimental facts as guiding us towards a highly non-arbitrary choice of geometry, why did he remain so confident that we would never sacrifice Euclidean geometry? His explicit remarks in this direction are as follows:

> One geometry cannot be more true than another; it can only be more convenient. Now, Euclidean geometry is, and will remain, the most convenient: 1st, because it is the simplest, and it is not so only because of our mental habits or because of the kind of direct intuition that we have of Euclidean space; it is the simplest in itself, just as a polynomial of the first degree is simpler than a polynomial of the second degree; 2nd, because it sufficiently agrees with the properties of natural solids, those bodies which we can compare and measure by means of our senses. (59)

The first reason that Poincaré offers — that Euclidean geometry is "simplest in itself" — is reminiscent of the idea that the orbits of the planets should be circular rather than elliptical because circles are intrinsically simpler than ellipses. But this kind of simplicity can also be viewed as merely a special case brought about by setting certain parameters to zero. There is no obvious reason to think that such special cases are more likely to occur in nature (if anything the opposite). The second reason that Poincaré offers — that Euclidean geometry aptly describes bodies that we can measure "by means of our senses" — is also not particularly compelling. Of course, given that the world as we experience it (at our length scale) is well described by Euclidean geometry, it seems extremely unlikely that we would abandon Euclidean geometry as it applies in a rough and ready, everyday kind of way. But that is hardly the issue that someone like Helmholtz, for example, is addressing when he thinks that we will simply have to wait and see what accurate measurements of the large scale structure of space might reveal about geometrical curvature. Even if we had discovered

[7]See his discussion of what is now known as the "Poincaré sphere", pp. 75-79.

that the parallax of distant stars is positive, and even if we had thereby come to adopt the claim that space is, after all, Lobachevskian, this would hardly undermine the practice of using Euclidean geometry in our handling of blocks and slabs and the like. Hence we are pushed back to the following thought: Poincaré felt sure that giving up Euclidean geometry would always be too big a leap given the availability of an alternative of, say, giving up the claim that light propagates in straight lines. If this is right, I think that the recognition that absolute motion is detectable in spaces of constant non-zero curvature would have likely shifted this balance. That is, it would have likely shifted Poincaré's level of confidence that Euclidean geometry had nothing to fear from fresh experiments.

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.

In concluding, it should be noted that there is a different tack that one might take with a view to the absolute motion detector: to deny, on a priori grounds that physical space *could* have a non-zero constant curvature. If absolute motion is regarded as impossible in principle — if space is not the *kind of thing* that objects can move with respect to — then this idea can be leveraged as a metaphysical argument against the possibility of space having a constant non-zero curvature. Here one can recall Leibniz's arguments in favor of relationism.[8] So another way that some of our nineteenth century protagonists might have been inclined to respond to the idea of an absolute motion detector would have been to argue that space must be Euclidean after all. Given that the classical solution to the problem of space had already restricted the candidate physical geometries to those of constant curvature, the denial of the metaphysical possibility of absolute motion would then bring us to the sole option of flat, Euclidean geometry.

Although such an argument may well have been articulated, I doubt that it would have won many adherents. This kind of metaphysical argument would have had to swim upstream against the more empiricist currents that were on the rise in the second half of the nineteenth century. Beyond this, there was great interest in this period in examining the notion of the ether, especially the supposed electromagnetic ether. And of course, electromagnetism was understood to imply that there *was*, in fact, a privileged reference frame — a reference frame in which the propagation of electromagnetic effects was isotropic. In combination with the rising empiricist tendencies and distrust of a priori arguments, I suspect that most philosopher-physicists of the time would have regarded the absolute motion detector as a coherent conceptual possibility, not to mention an enticing experimental probe of the actual curvature of space.

[8]As noted above, Nerlich (1979) has observed that Leibniz's relationist arguments sometimes implicitly *assume* that space is Euclidean.