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Answers in search of a question: 'proofs' of the tri-dimensionality of space

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Abstract

From Kant's first published work to recent articles in the physics literature, philosophers and physicists have long sought an answer to the question: Why does space have three dimensions? In this paper, I will flesh out Kant's claim with a brief detour through Gauss' law. I then describe Büchel's version of the common argument that stable orbits are possible only if space is three dimensional. After examining objections by Russell and van Fraassen, I develop three original criticisms of my own. These criticisms are relevant to both historical and contemporary proofs of the dimensionality of space (in particular, a recent one by Burgbacher, Lämmerzahl, and Macias). In general, I argue that modern "proofs" of the dimensionality of space have gone off track.

Keywords: Dimension; Space; Gravity; Kant; Stability; Orbits.

In Kant's (1746) first published work, *Thoughts on the true estimations of living forces*, he speculated that the dimensionality of space follows from gravity's inverse square law. Though he said more about dimensionality, especially in regard to the phenomenon of handedness, he never developed this line of thought further. But the very idea was revolutionary; for it is the first time that anyone tackled the question of spatial dimensionality from a physical perspective, as opposed to a mathematical or conceptual perspective. Kant's conjecture instigated a long tradition, continuing to

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this day, of philosophers and physicists trying to show or explain why space is three dimensional. Their efforts expand on Kant's claim and typically probe areas of physics besides Newtonian gravitational theory.

Perhaps the most famous paper in the modern tradition is that by the eminent physicist Ehrenfest. Ehrenfest (1917, p. 400) points out in his brief introduction that the very question "Why has space just three dimensions?" perhaps has "no sense" and can be "exposed to justified criticism". Yet he says he will not pursue these matters in the paper, and instead will leave it to others to determine "what are the "justified" questions to which our considerations are fit answers". The "answers" are aspects of physical theory that pick out three spatial dimensions as singular, such as the oft-heard claim that stable orbits are possible only in three dimensions. Though there has been some discussion of the physics of his "answers," there has not been much, if any, philosophical scrutiny of what the proper questions are.

In what follows I critically evaluate the argument started by Kant and developed by Paley (1802), Ehrenfest, and others.[1] Although this type of argument has been advanced in many different areas of physics, the central version threading its way through all of this history is the one started by Paley. Paley concludes that there are or must be three spatial dimensions from an argument including—crucially—the premise that stable orbits are possible only in three dimensions. I challenge both the truth of this specific premise and the adequacy of the general argument as an explanation of dimensionality. The former challenge is obviously relevant only to the species of arguments invoking stability and orbits, whereas the latter is relevant to the whole family of arguments seeking to explain the three dimensionality of space via some "singular" feature of physics in three dimensions. I argue that, viewed as answers to the question that now motivates them, these answers fail and cannot be saved. Rather than conclude on a negative point, however, I would like to suggest that with very different background assumptions (say, from Kant's original perspective or from that of contemporary superstring theory) there might be something salvageable in these considerations.

1. Context

The idea that there might be more than three spatial dimensions is not very startling to the contemporary reader. Programs in quantum gravity and the popular science literature abound with speculation that the number of spatial dimensions may be anywhere between three and twenty-five. Some have even suggested the

[1] See, for instance, Barrow (1983), Büchel (1969), Burgbacher et al. (1999), Carnap (1924), Caruso and Xavier (1987), Hadamard (1923), Mariwalla (1971), Mirman (1986), Penney (1965), Tegmark (1997), Weyl (1922), and Whitrow (1955). For more references and some historical background, see Janich (1992) and Jammer (1993). Not all of these articles use stable planetary orbits in their argument. Among the claims one finds in this literature are: "the hydrogen atom has no bound states for $n>3$"; "the wave equation implies that high fidelity communication requires $n=3$"; "only in $n=3$ do the gravitational, electric and neutrino fields determine their fields with equal strength"; "atomic spectra imply that $n=3$"; "neutron diffraction experiments imply that $n \leq 5$."

number is a fraction or even complex, and still others that this number evolves with time.[2] It was not always this way. Though higher-dimensional objects were well-known in mathematics—Diophantus and Heron of Alexandria discussed objects such as "dynamocubos", a square multiplied by a cube—such objects were considered of, at best, as playthings of the imagination. Pappus (ca. 300 AD) counseled against working on such 'impossible objects', for he did not think one should waste time on the impossible. Pappian views dominated thought on dimensionality and can be found in ancient times through the work of Hermann Lotze, a 19th century German philosopher, and even today in van Cleve (1989). Against this background, it is striking that Kant of all people (given his later views on space and time) was significantly at odds with the prevailing wisdom of his time. For Kant thought not only that the inverse square law entailed the three dimensionality of space, but that God could have chosen instead an inverse cube law which in turn would have picked out a four-dimensional space, and so on.

The 20th century witnessed remarkable changes in the mathematics, philosophy and physics of dimensionality. Menger and Urysohn complete the development of the topological theory of dimension.[3] Kaluza and Klein famously use a fifth dimension to "unify" gravity and electromagnetism, an attempt motivating contemporary superstring theories. And the philosophers Carnap, Reichenbach (1956) and Poincaré (1898; 1913) (who himself made monumental contributions to the topology of dimensions), among others, advocated geometric and topological conventionalism and applied it to dimensionality. In none of these fields was three spatial dimensions metaphysically or conceptually mandated. The number three emerged for the conventionalists as simply the most convenient integer for science to use (where "convenient" is sometimes read so broadly as to strain the notion). Once this 'conventional' choice has been made, Reichenbach thought one might still try to explain it with physical arguments of the sort we will consider.

'Proofs' of the existence of three spatial dimensions using stable orbits and the like persist through these many advances more or less unchanged. If anything, these advances help this style of argument flourish in the 20th century and today. How one views these efforts, however, has changed in at least two respects. During the heyday of ordinary language philosophy, the arguments of Kant, Paley and Ehrenfest are conceived as showing that the number of spatial dimensions is *contingent* (see Swinburne, 1968). For those interested in conceptual analysis and metaphysical necessity, the idea that spatial dimensionality varies with physical law is surprising because physical law is such a weak kind of necessity compared with conceptual or metaphysical necessity. Meanwhile, for those for whom the number of dimensions is definitely up for grabs, such as string theorists, the potential surprise would occur if these arguments infused the number three with any kind of necessity at all.

Indeed, one might ask how is it that the stable orbits tradition has existed alongside the physics of extra dimensions, or for that matter, the physics of

[2] In order: Zeilinger and Svozil (1985), Ashmore (1973), Arkani-Hamed, Cohen, and Georgi (2001).

[3] For an authoritative discussion of the history of the proof of the invariance of dimension theorem and the development of the Urysohn–Menger theories of dimension, see Johnson (1979, 1981).

spacetime. Regarding the latter, relativity seems to teach that there is not really space and time, but rather spacetime. The question should then be, how many dimensions does spacetime have? The question would then be whether the proofs can be extended to the relativistic domain. If so, then we have a proof that spacetime is four dimensional. If relativistic effects destroy the argument, however, then the most we can conclude is that in the classical limit spacetime must appear four dimensional. The same goes for superstring theory. Proofs of the three dimensionality of space and superstring theory appear to conflict, one saying there need be three dimensions and the other saying there need be more. But most recent efforts to prove space three dimensional carefully amend their claims so as not to rule out superstring theory. The claim is that physics should appear three dimensional at some non-fundamental level, but this leaves open the possibility of higher dimensions at fundamental levels.

2. Kant, Gauss and gravity

It is not hard to fill in the details of what Kant must have had in mind when he said the inverse square law determined the dimensionality of space to be three. Indeed, it will be useful for our later discussion to flesh this comment out, noting the connection between Newton's inverse square law for gravity, the dimensionality of space, and Gauss' law. Having written before Gauss was born, Kant of course did not use Gauss' law. Nor did he need it. My digression through Gauss' law is motivated by the fact that it just falls out from Kant's reasoning and will be important to us later—not by a desire to reconstruct what Kant actually thought.

Gauss' law is the statement that the total flux through a closed surface of any shape with mass (or charge) m inside is equal to the change of net mass (or charge) enclosed anywhere in that surface, divided by a constant. It is of central importance in many areas of physics. To understand the general idea of the law, consider a tube with water flowing through it. Suppose the water has density ρ and is traveling with velocity $\boldsymbol{v}$. Consider an imaginary surface S cutting the tube and some sub area of it, ΔA. How much water will cross ΔA in time Δt? The answer is trivial: the cylinder with size $v\,\Delta t\,\Delta A$ will pass through ΔA, so multiplying this by the density, we have $\rho v\,\Delta t\,\Delta A$. If we now divide out Δt we find the rate of flow, or flux, of the water: $\rho v\,\Delta A$. Generalizing to surfaces not normal to the velocity of the water and also summing over all faces ΔA while taking the limit, we obtain

$$\Phi = \int_S \rho \boldsymbol{v} \bullet \boldsymbol{n}\,\mathrm{d}A,$$

where $\boldsymbol{v} \bullet \boldsymbol{n}$ is the part of the velocity pointing normal to S. If we let $\boldsymbol{F} = \rho \boldsymbol{v}$, and $\boldsymbol{A} = \boldsymbol{n}A$, where $\boldsymbol{n}$ is normal to the surface, we arrive at the familiar expression

$$\Phi = \int_S \boldsymbol{F} \bullet \mathrm{d}\boldsymbol{A}$$

for the flux, where $\boldsymbol{F}$ and $\boldsymbol{A}$ are vectorial quantities. If we have a faucet with water flowing out and enclose it within an imaginary surface of arbitrary shape, the amount of flux Φ equals the amount of water crossing this surface. Gauss' law is then the claim that this flux is equal to the source times a constant when the surface is closed around the source. Gauss' law (for water) is intimately bound up with the idea that the amount of water is conserved, for it ties the amount of water passing through a closed surface to the changing amount of water inside that surface.

In the case of gravity, there is not some "stuff" that plays the role of water. We are instead talking about a vector field and its source is not a faucet but a massive body. The Newtonian gravitational force between two bodies is of course

$$\boldsymbol{F}_{\mathrm{g}} = \frac{Gm_1m_2}{r^3}\boldsymbol{r},$$

where m_1 and m_2 are the masses of the bodies, G is Newton's constant (with dimensions of spatial volume, mass and time) and $\boldsymbol{r}$ is the unit distance vector between the two bodies. We can still define a kind of flux, though it is no longer a flow in the intuitive sense. We simply substitute the vector field $\boldsymbol{F}$ by the gravitational field strength

$$\Phi_{\mathrm{g}} = \int_S \boldsymbol{g} \bullet \mathrm{d}\boldsymbol{A}$$

where $\boldsymbol{g}$ is the gravitational field strength, with $\boldsymbol{g} = \boldsymbol{F}_{\mathrm{g}}/m$. The gravitational flux is, loosely, the amount of gravitational influence a source has. Given one mass a distance $\boldsymbol{r}$ away from another, $\boldsymbol{g}$ encodes how much acceleration the force from one body exerts on the other. The flux Φ_{g} is now the amount of "strength" passing through the surface. One then makes a kind of hunch that gravitational flux will be conserved, which is equivalent to the idea that Newton's law expresses conservation for the number of field lines emerging from a mass source. Gauss' law becomes

$$\Phi_{\mathrm{g}} = -4\pi G \sum_i M_i,$$

where the sum is over the various mass sources inside the surface and the minus sign arises from the attractive nature of gravity. This hunch is spectacularly well confirmed. The gravitational version of Gauss' law is crucial to explaining a number of observed effects. The most familiar are that the force produced by a solid sphere of matter is the same at the surface of the sphere as it would be if the matter were concentrated at the source, originally proved by Newton. It is also essential in allowing that a spherical shell of matter produces no gravitational field inside. Gauss' law in both electromagnetism and gravity is confirmed to an extraordinary accuracy.

To see the connections among the force law, Gauss' law and dimensionality, let us look at an especially simple case. Suppose we have a single massive point source and it is enclosed by successively larger spheres centred upon this source, as in Fig. 1. The surface area of such spheres increases as r^2, of course. Suppose we want flux conservation. How can we get this if the surface area spreads as r^2? Simple: we demand that gravitational force is emitted as a spherical expanding field but that the

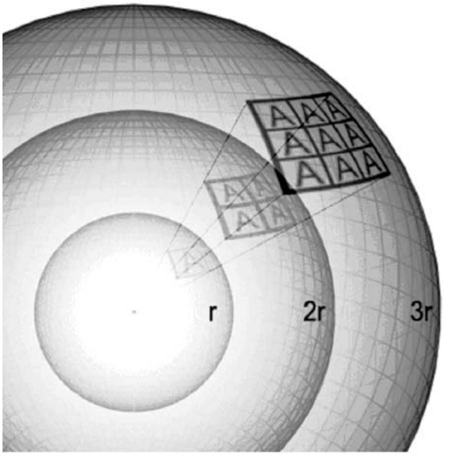

Fig. 1. Massive point centered inside concentric spheres of increasing radius.

magnitude of this field decreases by r^{-2} to cancel the increase in area. Thus we need an inverse square field.

In slightly more detail, the outward flux from a sphere is

$$\Phi_{\rm g} = -\oint_S \boldsymbol{g} \bullet {\rm d}\boldsymbol{A} = -\int_S g\,{\rm d}A = -gS$$

(Here we can use g rather than $\boldsymbol{g} \bullet \boldsymbol{n}$, the component of $\boldsymbol{g}$ normal to the surface S, because we are assuming the field is radial and S is spherical.) Now, assume that the gravitational acceleration is given by $g = GM/r^2$ and that the surface area is given by $S = 4\pi r^2$. Plug in these values into our definition of flux

$$\Phi_{\rm g} = -gS = -\left(\frac{GM}{r^2}\right)4\pi r^2 = -4\pi GM$$

and we arrive at Gauss' law. Textbooks commonly derive the form of the force law from the assumption of Gauss' law, but as above, it is trivial to derive Gauss' law once one has the force law. One of course needs various other assumptions; most notably, that the force is emitted radially from the source and that space is isotropic. But with Euclidean space as a kind of background presupposition (which we will remove later), isotropy is implied and radial emission is natural (because of the usual correspondence between dynamical and spatiotemporal symmetries). With these assumptions in place, one can see that any field other than an inverse square field would entail that the r^2's do not cancel and Gauss' law would not hold. If we had, say, an inverse cube field, then the flux would decrease with increasing r ($r^{-3}r^2 = r^{-1}$). For any field other than an inverse square field, the flux would depend on the distance of the surface from the source.

How do dimensions fit in? In the above reasoning, we implicitly assumed something about the dimensionality of space, for we held that the force radiates from the source as a sphere and not a hypersphere or a circle, etc. The source thus radiates into a three-dimensional space rather than some other space. Jump up one dimension and suppose the force still emerges from the point radially and into an isotropic space. The surface area of the sphere $S = 4\pi r^2$ is now replaced with that of the hypersphere $S = 2\pi^2 r^3$. In this case, however, an inverse square field no longer cancels out the r-dependence and an inverse cube field would be needed for such a result.

Of course, this kind of reasoning must be dramatically complicated when we turn to the curved geometries and non-trivial topologies allowed by general relativity. No longer can we assume that space is flat and isotropic or that the topology is Euclidean. And no longer will Gauss' law in general be globally true—a fact related to the lack of global energy conservation in general relativity. To see the point quickly, suppose that space itself is curved as a hyper-sphere. Then as the force from a mass source spreads, it will eventually come back on itself: the lines of force will interfere with one another. In a variably curved spacetime, the lines of force will be enormously complicated. No surprise, then, that Gauss' law is not in general globally true in general relativity.[4] One can still find constraints on dimensionality in general relativity, but if they are exact they will be heavily solution dependent. Perhaps one could get something slightly more general if one imposed restrictions such as that the equations for matter contain only differentials of second order or less and that the geometry be one with various symmetries. Alternatively, one might abandon any aspirations for such proofs and merely view these proofs as relevant in the weak-field approximation, where Newtonian theory holds. In this case, one would adopt the same attitude toward general relativity as modern 'proofs' adopt toward superstring theory and the like (mentioned above).

Returning to Kant and classical physics, we have seen that Kant's claim that the dimensionality of space is related to the inverse square law is, left at that, plainly true. They are intimately connected, once one has the necessary assumptions about forces and the nature of space in place. Kant himself, in the *Estimation*, claims a particular direction of dependency: that "the three-dimensional character seems to derive from the fact that substances in the existing world act on each other in such a way that the strength of the action is inversely proportional to the square of the distances".[5] One might ask why dimensionality follows from the inverse square law and not vice versa. We will return to this question after discussing the modern arguments.

[4]For a spherically symmetric distribution of matter, Birkhoff showed that Einstein's equations have a unique solution, namely, the Schwarzschild solution. A corollary of this is a version of Gauss' law: in such a solution the acceleration of a mass shell in a dust-filled universe depends only on the matter inside the shell.

[5]The translation is from an English translation of the *Estimations* forthcoming by Eric Watkins. I am grateful to Professor Watkins for letting me use an early copy.

3. The stable orbits argument

Kant's suggestion is really just that, a suggestion, as opposed to a developed argument for the three dimensionality of space. Starting with Paley (1802), however, others picked up the idea that a physical argument could be provided for explaining the dimensionality. Though the physical phenomena vary from argument to argument, by far the most common is the one that begins with our observation of various stable planetary orbits (see, for instance, Barrow, 1983; Büchel, 1969; Ehrenfest, 1917; Tangherlini, 1963; Tegmark, 1997; Whitrow, 1955). The idea, in a nutshell, is that the existence of stable planetary orbits explains in some sense the three dimensionality of our world. We will talk about what sense of explanation they are looking for with such proofs in Section 5.2. Here I just want to present the argument. Its main claim is that stable orbits are possible only (or almost only) in three spatial dimensions. Perfectly circular orbits where the attractive force is exactly compensated by the necessary centrifugal force are possible in all spatial dimensions. But circular orbits, the argument goes, are extremely unlikely. The slightest perturbation destroys the orbit. In our world, we have remarkably stable elliptical orbits. All manner of forces constantly perturb these objects yet still they (for the most part) continue in elliptical orbits. We can only have these orbits in three dimensions, and this (somehow) explains why there are three dimensions.

In what follows, I will focus almost exclusively on the stable orbits arguments, as I believe it is representative of the remainder of the arguments and also the most common. Virtually every general point I have to make about this argument holds also, so far as I can see, with the other arguments mentioned in footnote 1 as well. I will follow Büchel's version of the argument because it is a little more sophisticated than some textbook versions of the same claims, but not as cumbersome as Ehrenfest's or as quick as Barrow's. Let us now look at Büchel's argument in more detail.

We begin with the Poisson–LaPlace equation for a gravitational potential:

$$\frac{\partial^2 V}{\partial x_1^2}+\frac{\partial^2 V}{\partial x_2^2}+\frac{\partial^2 V}{\partial x_3^2}+\cdots+\frac{\partial^2 V}{\partial x_n^2}=k\rho. \tag{1}$$

Here V is the potential and ρ is the density of the matter field. Note three features of this equation, all of them important to what follows. First, we are assuming it valid in all spatial dimensions n. Second, it is Gauss' law, which we saw above; i.e., this is the "differentiable" form of Gauss' law written for a scalar potential using the identity $\boldsymbol{g}=-\nabla V$. Third, the electrostatic potential can be described the same way, so just as with Kant's argument, what can be said for gravity can be said for electricity (relevant for the analogous argument using stable atoms).

The solution of (1) is

$$V=\frac{-C}{r^{n-2}}$$

and the force is defined as

$$F = \frac{(n-2)C}{r^{n-1}}.$$

C is an arbitrary constant and r is the distance to any point in the field. We are thus assuming that the inverse square force is not true in all spatial dimensions; rather, in two dimensions it is inverse r, in three dimensions it is inverse r squared, etc.

For Büchel, a stable orbit is one where r alternates for all time between its perihelion (minimum) value r_1 and aphelion (maximum) value r_2. He considers the two-body problem, with a body describing a central orbit around another body. Let the mass of the orbiting body be m. Its angular momentum M is constant and equal to

$$M = mr^2\dot{\varphi}, \tag{2}$$

where φ is the azimuthal speed.

Büchel then makes the approximation that at "extreme distances from the central body $\mathrm{d}r/\mathrm{d}t = 0$" (p. 1223). When the velocity vanishes, the kinetic energy becomes

$$T = \frac{M^2}{2mr^2}.$$

He then invokes the conservation of energy, $T + V =$ constant, to write:

$$\frac{M^2}{2mr_1^2} - \frac{C}{r_1^{n-2}} = \frac{M^2}{2mr_2^2} - \frac{C}{r_2^{n-2}}.$$

Using Eq. (2), the centripetal force in a circular orbit is

$$F_{\mathrm{c}} = \frac{M^2}{mr^3}.$$

We observe that for an eccentric orbit, the attractive force must be less than the centripetal force at perihelion; otherwise the planet would crash into the other mass. Similarly, at aphelion, the attractive force must be greater than the centripetal force; otherwise the planet would escape to infinity. Hence, we impose two conditions: (a) at r_1, $F < F_{\mathrm{c}}$ and (b) at r_2, $F > F_{\mathrm{c}}$. We can then substitute these values into our statement of conservation, obtaining an inequality because we are replacing something small by something smaller and something large by something larger. Massaging a bit, we obtain:

$$\left(\frac{M^2}{mr_1^2}\right)\left[\frac{1}{2} - (n-2)^{-1}\right] < \left(\frac{M^2}{mr_2^2}\right)\left[\frac{1}{2} - (n-2)^{-1}\right].$$

Remembering that $r_2 > r_1$, this condition rules out any elliptic orbit for $n \geqslant 4$.
The other proofs begin the same way. Ehrenefest begins with the Poisson equation, finds the equations of motion, and then looks for trajectories for which $\dot{r}$ has real and alternatively positive and negative values. If this is not the case, then the planet must either crash into the one it is orbiting or escape to infinity. Like Büchel, he finds that in $n \geqslant 4$ there can be no stable orbits; however, unlike Büchel he finds that there can

be stable orbits in $n = 2$ but these orbits are not closed and lack other desirable properties. Barrow (1983) instead invokes the following two conditions as necessary without comment:

$$r^3 F(r) \to 0 \quad \text{as} \quad r \to 0$$

$$r^3 F(r) \to \infty \quad \text{as} \quad r \to \infty.$$

These can be reproduced from some standard textbook calculations.

In sum, assuming that the Poisson equation is valid in all dimensions and that our world contains stable orbits, the argument to an inverse square law can be read as an argument for three spatial dimensions. Tangherlini (1963) makes the same kind of argument but with a general relativistic analysis in the Schwarzschild solution (more on which later).

Proponents of the stability argument do not seem terribly worried by $n<3$. Negative dimensions are impossible according to any of the usual ways of understanding either topological or metrical dimension. Furthermore, there are no orbits in $n = 1$, so the question is really about excluding $n = 2$. This rejection of two spatial dimensions is done in a variety of ways. Ehrenfest and Büchel exclude it because they want the gravitational potential to vanish at infinity, but it does not in $n = 2$. Ehrenfest also mentions Bertrand's theorem at this point, though he does not spell out its role in the argument. One can easily see how it could be relevant, however. This theorem states that bounded trajectories are closed only under a central force when the force law is proportional to the distance between objects or inversely proportional to its square. Assuming the orbits must be closed, this would help us exclude $n = 2$ (though see Section 5.1). Whitrow (1955), Hawking (2001) and Tegmark (1997) dismiss $n = 2$ because biological organisms would face all sorts of insurmountable topological problems; $n = 2$ worlds are "too barren to contain observers" (Tegmark, 1997, p. 70). The idea here is an anthropic one that we will come across again. Another remark one sees is that there is no gravitational force in general relativity for $n<3$, but this assumes we are working in a general relativistic setting rather than the Newtonian one in the proof.

4. Previous objections

Before getting to my own objections, let us look at two others that have been made to the stability argument.

4.1. Russell's objection

In his dissertation-turned-book, Russell holds that there are three a priori principles that ground geometry. One of them is that there are a finite number of spatial dimensions; the exact number, he thought, is contingent and known a posteriori. In making this claim, Russell argues, and van Fraassen (1985) echoes (p. 136), that there might be a small inaccuracy in Newton's law of gravity that remained undetected, but that a small inaccuracy in space being three dimensional would not go undetected.

> The limitation of the dimensions to three is... empirical; nevertheless, it is not liable to the inaccuracy and uncertainty which usually belong to empirical knowledge. For the alternatives which logic leaves to sense are discrete—if the dimensions are not three, they must be two or four or some other number—so that small numbers are out of the question. Hence the final certainty of the axiom of three dimensions, though in part due to experience, is of quite a different order from that of (say) the law of gravitation. (Russell, 1897, p. 163).

From this argument we are supposed to conclude that Kant's argument and those like it cannot succeed.

Though I feel I understand what Russell might have tried to get at with this objection—I think he is reaching toward an objection I mount later—as it is stated I cannot understand it. True, G, Newton's constant, might differ slightly from what we think it is, or r^2 might really be $r^{2.00001}$.[6] These variables can take on a value in the range of the real numbers, whereas the topological or metrical dimensionality can only take on a value in the range of the natural numbers. But if read as the simple claim that we are always more certain of the values of natural-numbered quantities than real-numbered quantities, I am not sure I see a reason to believe this. I have more reason to think the circumference of a circle divided by twice the radius is 3.1415... than that the number of species on the earth is 5 million. Why should the difference between a countable versus an uncountable number of ways of being wrong show up at the level of epistemic warrant?

Van Fraassen interprets Russell as making the point that the topological properties like dimension are more fundamental than the metric properties described by gravitation. He writes

> Dimensionality is not a metric but a topological feature of space. Hence, the features of the physical world pointed out are simply not basic enough to shed much light on the dimensionality of space. (van Fraassen, 1985, p. 136)

Again, at least prima facie, the argument is not compelling. To be sure, mathematically, the spacetime metric requires a manifold with topological structure; in a formal sense the topology is more fundamental than the metric. But that does not mean that they are not on a par physically or metaphysically. There are many senses of 'fundamental,' and an argument is needed to show that we are more certain about the more mathematically fundamental. Coming from van Fraassen interpreting Russell, this point is ironic. Both philosophers are spacetime relationists, i.e., those wanting to found all spatiotemporal features of the world on distance relations

[6] In 1894 Hall, measuring the power law $F \propto r^n$ from the excess precession of Mercury, found $n = -2.00000016$. This would have eliminated the need to account for the motion of Mercury with a new theory; however, such a value for n would have been inconsistent with other motions in the solar system.

among events. Yet from this perspective one usually gets the topology from the distance relations between objects, so the metric is more basic than the topology.

4.2. Why gravity?

Why does the gravitational force dictate the dimensionality and not (say) the strong force? This is a question Abramenko (1958) asks. Indeed, we cannot run the same sort of argument with the strong force, the force binding the nucleus together that acts on gluons and quarks. In gravity and electromagnetism, the strength of the field decreases like r^{-2} a distance r away from a charge or mass source. The strong force, which is transmitted via eight gluons, does not behave this way. The force between color charges (quarks and gluons) does not decrease with distance, nor is color flux conserved. But it is a fundamental force, too, so why does not it have some "say" in the matter of dimensionality?

Remember, firstly, that any force law can live in any dimensionality. There is no problem with having inverse cube laws operate in a three-dimensional space or inverse square laws operate in a 13-dimensional space. The relationship among forces, strengths and the nature of space just is not as "tidy," in the sense that non-radial forces, non-isotropic space, and so on would be needed. So the problem is not that the color force is picking out one dimensionality and gravity another; if that were so, we would have to go with the larger dimensionality so as to "fit" both forces. The question is, instead, why go with the dimensionality that makes for a tidy package with gravity than one that makes for a tidy package with the color force?

To this I can imagine two answers. The first is that, at least in the case of the color force, its relationship with distance is very complicated and it is hard to imagine any tidy relationship with space. The second is that even if one could devise such a package, Ehrenfest and especially Kant might point out a reason to favor the gravity package over others; namely, gravity and only gravity is a universal force, whereas the strong interaction occurs only between colored particles, the electromagnetic interaction occurs only between electrically charged particles, and the weak interaction occurs only between quarks, leptons and electroweak gauge bosons. Gravity, being the only universal force, gets to "decide."

In any case, many similar arguments do not appeal to gravity (cf. footnote 1). One can imagine repeating this question about any physical feature picked out as special by one of these arguments. But if the complaint is a general one, namely, that these features are too "contingent" to play the role assigned to them, then I think they reduce—like Russell's—to an objection we will soon meet.

5. Objections

5.1. Too simple

A major empirical premise of the stable orbits argument is that some actual phenomena are correctly classified as stable orbits. The argument has force only if the stability it refers to is what we actually observe. If the conditions on stability are wildly unrealistic then the proof may be impeccable, as far as it goes, but be irrelevant to our world. Before tackling the argument form itself, I would like to question whether this oft-repeated claim, namely, that stable orbits are possible only in three dimensions, is even remotely established.

There are a number of places where one might worry; but broadly speaking, the worries will be that what is meant by 'stable orbit' is either too strong or too weak. If Poincaré recurrence counted as completing an orbit, the criterion would clearly be too weak. But the proof could also be too strong: for instance, if the proof only worked for strictly periodic orbits, then the planet Mercury's orbit would not count as a stable orbit, for it travels a slow precessional ellipse that deviates slightly from elliptical and whose properties evolve over time. If all the other planets, moons, etc. also had orbits that evolve like Mercury's over time, but just less so, then the argument would not apply to anything in the actual world. Hence one could not appeal to empirically observed stability in our universe, for none of the paradigms of observed stability would count as stable.

How do Büchel's proof and the others understand stability? They assume stable orbits are always between $r_{\min}$ and $r_{\max}$. Stability amounts to having the properties of permanence and boundedness: the orbit is permanently bounded by $r_{\min}$ and $r_{\max}$. In the literature concerned with the stability of the solar system, this conception of stability is considered absurdly weak, for consistent with this definition of stability an orbit may execute all manner of weird behavior and still be between $r_{\min}$ and $r_{\max}$. For this reason, modern discussion of stability uses conceptions such as Liapunov stability, structural stability, KAM stability, and so on.

From a modern perspective, one would certainly hope for stronger proofs than Büchel's. For present purposes, however, I am more concerned that even Büchel's weak sense of stability is too strong. Why? Real orbits are not permanently bounded. In the real world, presumably everything above the molecular scale that we call an orbit actually had a beginning. Since the equations are time reversible invariant, that is equivalent to them having an end point. Viewed backwards in time, the actual history is one where each planet either crashed into another mass or escaped into infinity (or Big Bang singularity). By ruling these out, we are ruling out the actual history. One might think we could add entry or exit paths onto the orbits between $r_{\min}$ and $r_{\max}$, but if the orbits are otherwise exactly the same, then it is clear this violates classical mechanics' determinism.

All we really have evidence for are orbits of less than 15 billion years, so the assumption that the orbits are eternally between $r_{\min}$ and $r_{\max}$ seems too strong. When one considers dimensions other than three, it is not as if *all* initial trajectories crash into the central planet or begin their journey to infinity *right away*. For some initial conditions one will find 'orbits' that crash or escape much later. So we need some confidence that 'much later' does not equal 15 billion years for some initial conditions. The proofs do not warrant this confidence. One can imagine future proofs to the effect that unstable orbits are generic in the relevant solution space in dimensions apart from three, but the existing proofs are far from establishing this; and such proofs still would leave open the possibility that we live in (say) five dimensions but with an atypical initial condition.

Likewise, one might also worry about any proof invoking Bertrand's theorem, as Ehrenfest seems to in ruling out stable orbits in two dimensions. Bertrand's theorem is usually quickly glossed as saying that the only bounded trajectories of a central force problem that are closed are those described by a force that is proportional to the distance between bodies or a force inversely proportional to the square. The gloss overstates matters. Bertrand's theorem in fact proves that those two force laws are the only bounded trajectories that close *for every initial condition*. But there are still an infinite number of closed trajectories with other force laws possible; it is just that for these other force laws the trajectories do not stay closed under substitution of arbitrary initial conditions. An inverse fifth force with zero total energy and nonzero angular momentum leads to a circle through the area of force, for instance.[7] Since we certainly do not observe that the closed orbits in our world are also closed given *any* initial condition, proofs crucially invoking Bertrand's theorem are again too strong.

Finally, when we move to general relativity and non-Euclidean spacetimes, everything changes once again—as in the Kantian case. In this case too, proofs about stability and orbits will be solution dependent. Here it is important to point out, however, that Tangherlini (1963) does indeed extend the kind of proof we are discussing to a Schwarzschild solution, the natural solution to use for our Keplerian orbit problem. Of course, this solution and the corresponding proof crucially rely on the assumptions of a static and spherically symmetric metric, so the proof is only as good as the solution is an approximation. In fact it is a very good approximation to certain systems (e.g., Mercury–Sun system) and a poor one to others (e.g., the global spacetime). As before, if the intention of the proof is to base it on fundamental physics, we would want to use the metric closest to the actual metric of the actual world. If, by contrast, the intention is only to show that in certain regimes the proof obtains, one may be satisfied with such a proof.

Despite these misgivings, there are other proofs dealing with other phenomena. And we have not uncovered any in principle reason for thinking future arguments about stable orbits and dimensionality could not be convincing. And there really is a difference between three dimensions and the rest regarding at least the simple stability argument. So let us continue, under the assumption that physics can indeed show that stable orbits are possible only in three dimensions. What do we make of the argument in that case?

5.2. *Is tri-dimensionality explained?*

Let us begin by immediately distinguishing two questions blurred in most of the spatial dimensions literature:

(1) How many spatial dimensions are there?
(2) Why are there *that many* spatial dimensions?

[7] This point is made in a Letter to the Editor, by Martin S. Tiersten (*American Journal of Physics*, 70, 7, July 2002, 664).

The first question merely tries to figure out the right number; the second tries to answer why it is that number. The second question takes the number of spatial dimensions as a contingent feature of the universe, one needy of explanation.

Virtually every participant in this tradition seeks to answer (2) rather than (1). Thus Barrow (1983, p. 337) talks of ''explaining why the world has three dimensions'', Whitrow (1955) very title asks this question, Penney (1965, p. 607) hopes to present a ''raison d'être'' for the observed dimensionality... of the physical world'' (1607), and so on. Probably Tangherlini states the claim clearest among non-philosophers. He wants to take the claim 'there shall exist stable orbits', make it a principle of *n*-dimensional dynamics and gravitation theory, and then show that ''the proposition that space is three dimensional becomes a theorem, rather than an axiom'' (639) of such physics. The idea underlying all of this work is admirable enough: take something that seems to be brute or necessary in our theoretical framework and show instead that it is contingent and its value can be derived from this framework.

There are many ways of answering question (1). For instance, one might look to fundamental physics the way Poincaré and Reichenbach do. Poincaré and Reichenbach, being conventionalists, thought that there was no fact of the matter regarding dimensionality until one had made various definitions and assumptions. One could fit the phenomena, in principle, in any number of dimensions. The question is which number is most convenient for science. Both answer three, Poincaré due to interesting group theoretical considerations and Reichenbach due to a desire to satisfy the principle of action by contact, which he felt satisfiable only in three dimensions. (Albert (1996) recently turns the Reichenbach argument on its head in the quantum realm.) Today, string theorists and others are arguing that the most natural dimensionality is 10 or 11, since these dimensions allow for the existence of various symmetries and also allegedly unify and explain the existence of the forces. Note that one need not be a conventionalist to make these kinds of arguments. Naturalness, simplicity, consilience, unification, etc., might be marks of truth rather than marks of convenience. Even the conventionalists were not so conventionalist about spatial dimensionality—Poincaré suggests that biology might 'hard-wire' us into believing in three spatial dimensions.

I want to grant that the stability argument and Kant's considerations might play some role in answering question (1). After all, in Euclidean space at least, we do have a very tidy set of relationships, having the force expand isotropically through the space, flux conservation, the orbits being stable under perturbation, etc. Surely any other force laws, etc. would greatly complicate our physics. Therefore, this argument may help us determine which dimensionality is the most natural one given the phenomena and explanations of other phenomena. The same goes for many of the other arguments.[8]

[8]For instance, Ehrenfest pointed out a fascinating connection, namely, that there is a dualism between rotation and translation in three and only three dimensions: both are defined via three characterizing numbers. In two dimensions, there is only one type of rotation and two types of translation; in four dimensions, there are six types of rotation and four translations, and so on. Yet in three dimensions there are three types of rotation and three types of translation. This deep geometrical fact has many repercussions throughout dynamics.

But, as Reichenbach states,

> one can proceed, after recognizing that the three-dimensionality is a physical fact, to the question of its explanation, i.e., one can now search for a cause of the three-dimensionality of space (pp. 279–280).

We can move from question (1) to (2). Being no stranger to stability arguments like the one we are considering, Reichenbach in fact gives it as an example of an answer to (2):

> Such a proof might read: if space has n dimensions, and it is a general law of nature that the attraction between masses varies inversely with the $(n-1)$th power of their distance, then the dimensionality of space must be $n=3$, since otherwise the motion of the planets and also the arrangement of the stars would not be stable (Reichenbach, 1956, p. 280).

Though unsatisfied with such arguments to date because they were not generally relativistic, he endorses the general line. He then comments on this strategy:

> The three dimensionality would thus be recognized as a logical consequence of certain fundamental properties of matter, which in turn would have to be accepted as ultimate facts. Any other attempt at explanation would be vain. The three dimensionality of space cannot be maintained as an absolute necessity; it is a physical fact like any other, and therefore subject to the same kind of explanation (Reichenbach, 1956, p. 280).

Let us suppose that the arguments succeed in showing that there cannot be stable orbits in $n>3$; and furthermore, that the laws we have formulated are *the* laws for any dimension; and furthermore, that stability in this argument is neither too strong nor too weak to count known phenomena, like the moon's orbit, as stable. Can the stability argument answer the why question?

There is a strong feeling—which I think Russell, van Fraassen and Abramenko were all expressing—that stability is just the wrong kind of feature to use to explain why space is three dimensional. One wants to parody the argument by appealing to something like the fact that separating inter-locked three-dimensional iron rings is challenging to human beings in three dimensions, but it would not be challenging in four dimensions or more:

> If space has n dimensions, and it is a general law of nature that closed inter-locked n-dimensional 'rings' of matter cannot be separated by creatures such as human beings, then the dimensionality of space must be $n=3$, since otherwise separating inter-locked rings wouldn't be challenging in our world.

The feeling is that stability, like the challenge of separating rings, is simply not a deep enough feature to explain dimensionality; if anything, these facts are symptoms of the dimensionality.

Let us look more closely at the argument:

1. Space has n dimensions.

2. It is a general law of nature that the attraction between masses varies inversely with the $(n-1)$th power of their distance.
3. If proposition 2 is the case, then planets are stable only if $n = 3$.
4. The planets are stable.
5. Therefore, it is/must be the case that $n = 3$.

I take it that we want the 'must be' reading in 5 to get an answer to question (2) rather than (1) at the beginning of this section. The 'is' reading just adds stability to the pot of evidence we have for thinking space is in fact three dimensional. Stability would give Hempelian confirmation of the hypothesis that space is three dimensional, but it would not explain the dimensionality any more than finding a raven that is black explains why all ravens are black. To get an explanation we need the 'must' reading in 5, but for that we need premise 4 to read that the planets must be stable. In Reichenbach's terminology, we need to treat his ultimate facts as absolute necessities; in Tangherlini's terminology, it must be an axiom that the planets are stable. The obvious question is, why treat stability, which is not a corollary of any fundamental law, as necessary or axiomatic? Surely there did not *have to be* stable orbits.

It is not surprising that at this point one sees appeals to the dubious anthropic argument made in the literature. Hawking and Barrow, for instance, have provided anthropic arguments that "explain" the three dimensionality of space. Hawking, and to some extent Tegmark and Whitrow, have said that life is only compatible with three dimensions, so the fact that we are around now explains why there are three dimensions. But even if three spatial dimensions are a necessary condition of life, it does not follow that there must be three dimensions. It only follows that there *are* three dimensions. Unless one adopts the vaguely theological, strong anthropic principle and holds that there must be life, we would not get an explanation of why there are three dimensions. See Worrall (1996) for a critique of anthropic reasoning in many of its forms.

Now I suspect that a positivist/empiricist of Reichenbach's stripe may not be bothered by this kind of criticism: he was not one of physical necessity's greatest fans and surely did not think it necessary for scientific explanations. In his eyes, such a proof reduces two phenomena to one phenomenon by linking dimensionality to stable orbits, and that is presumably all one can hope for. Nor would Tangherlini be over impressed; for him it is simply a matter of whether adding 'there must be stable orbits' to the fundamental laws of physics is worthwhile. Should such a claim be a theorem or an axiom?

This question is naturally approached via Lewis' idea of the Best System. Consider all of the deductive systems whose theorems are all true. About these systems Lewis writes:

> Some are simpler, better systematized than others. Some are stronger, more informative than others. These virtues compete: an uninformative system can be very simple, an unsystematized compendium of miscellaneous information can be

very informative. The best system is the one that strikes as good a balance as truth will allow between simplicity and strength (Lewis, 1994, p. 478.).

Reichenbach/Tangherlini's suggestion, put in this context, is that the proposition that there exist stable orbits is an axiom of the Best System. The suggestion is a bit peculiar since the proposition expresses a matter of particular fact rather than a generalization. But such an extension is not without precedent.[9]

In this case it seems clear, however, that Reichenbach/Tangherlini's suggestion would not make it into the Best System as an axiom. Compared to Einstein's equations, Dirac's equation, etc., the claim that there exist stable orbits seems very weak. The proposition that there are stable orbits is not very strong. Arguably we get the three dimensionality of space, but that is about it. Furthermore, to actually infer truths about the world from such a generalization it will need to be vastly more complicated than its present form. After all, not just anything can get into a stable orbit with anything else. So if the generalization is to be true, one will have to restrict its scope quite severely. This move will have costs, both in terms of the simplicity of the generalization and in terms of its strength. For these kinds of reasons I do not believe stability is powerful or simple enough a fact to merit inclusion in the Best System. Read this way, the stable orbits explanation of dimensionality is not guilty of a fallacy or a serious methodological sin; it is 'wrong' only because it is an unfortunate way of organizing one's knowledge.

5.3. *Modal mayhem: what is 'physics of $N>3$'?*

Let me now turn to what I think is the most fundamental worry about proofs like the one we have examined. In the brief introduction to his 1917 paper, Ehrenfest asked "What is meant by 'physics' of R^4 or R^7?" He clearly recognized the importance of the question, but he left it to others to consider. It is obvious why this question is important. We only get the result that three dimensions are special if we hold some aspects of our laws of physics fast and vary the dimensionality. To say that stable orbits are possible only in $n<4$ we need a theory of gravity, etc., in 4, 5, 6.... If we allow every physical feature to vary with the dimensionality—as Poincaré (1913) allows—it is hard to see how anything could turn out physically special. Most advocates of this type of argument explicitly endorse assumptions about what laws operate in higher dimensions. In fact, Barrow and Tipler (1986) espouse a "principle of similarity": the assumption that the laws of physics in worlds of other dimensionality are as similar to ours as possible. This kind of assumption finds its way into Büchel's proof at the first step: if we did not assume the LaPlace–Poisson equation is valid for all n in the proof, it would immediately collapse. The other worlds are similar to ours in holding LaPlace–Poisson steady while letting the force law vary. If instead we assumed that anything goes in each dimension, then we could

[9]Following Boltzmann and others I defend such a move for the cosmological low entropy initial condition required by thermodynamics; see Callender (2004).

simply assume there are stable orbits in any n and then solve for the remaining n-dimensional laws that allow stable orbits.

Recent work in physics has more or less proceeded in this fashion without appreciating the general point. Burgbacher, Lämmerzahl, and Macias (1999)—henceforth BLM—for instance, show that there *are* stable hydrogen atoms in higher dimensions, contrary to the Ehrenfest result against stable hydrogen atoms (see also Caruso & Xavier, 1987). BLM's argument applies to stable planetary orbits too, an application that they explicitly endorse. Let us review their reasoning, for I think seeing it will make the general lesson emerge with ease.

Working with electrodynamics and stable atoms, BLM claim that in all dimensions stability requires that the electrostatic potential be proportional to r^{-1}. They *begin* with the assumption that there is stability (described by a potential proportional to r^{-1}) in all dimensions. Then they work backward, as it were, to discover what laws would be needed to give r^{-1} potentials in all dimensions. For $n>3$, this leads to a modification of Maxwell's equations, yet the solutions of these new equations have the same structure as for $n=3$ and the force between charges is the same as in $n=3$. The main differences, however, are that the modified Maxwell equations do not lead to a Gaussian law for charges and in even higher dimensions lead to non-local equations.

As they note, the same reasoning can be used for stable orbits in the case of gravity, and hence, our case. Again, they hold fast stable orbits and an r^{-1} potential—this time the gravitational potential. They then work backwards and find that they are forced to modify the Poisson–LaPlace equation for the Newtonian potential in the same way as they had to modify that equation for the electrostatic potential. As the Poisson–LaPlace equation is simply Gauss' law, Gauss' law is broken in this case too. General relativistically, their changes would entail slightly modifying Einstein's equations in higher dimensions.

BLM do not deny the validity of the Ehrenfest-style argument. Rather, they are essentially making the classic move of turning Ehrenfest's *modus ponens* into a *modus tollens* argument. We have an inconsistent set: {(a) Gauss' law/Poisson equation, (b) stability (r^{-1} potential), (c) stability in $n>3$ dimensions}. Ehrenfest holds the first two members fast and denies that there could be stability in $n>3$ dimensions. BLM holds the last two fast and denies Gauss' law and related higher-level equations. They write, "we think that the specific expression for the force between charged particles and the stability of atoms are of more basic physical importance than the validity of Gauss' law" (633). They do show that Gauss' law is valid for a quantity related to the field strength, but it is not valid for the field strength itself.

	BLM	Ehrenfest
Assume	There is stable motion in $n>3$ described by $1/r$ potential	Gauss' law/Poisson equation
Then	Modify Gauss/Poisson equation	There is no stable motion in $n>3$

We have a kind of Quine–Duhem problem in action. How are we to arrange these premises? If we view Gauss' law as basic and important, then we will not be willing to modify Maxwell's/Poisson's/Einstein's equations so as to obtain stable motion. If we are willing to sacrifice Gauss' law as BLM are, then we can get stable motion. How are we to adjudicate this debate?

One might think that if the battle were between a fundamental law and a non-fundamental one, then an argument could be made that the fundamental law should win, that fundamental laws should not be varied with dimensionality. If one anointed Gauss' law fundamental, then one would go with Ehrenfest; if one anointed the form of the potential fundamental, then one would go with BLM. What is nice about this case is that neither I nor people I have talked to about it have any idea which way to go. Clearly Barrow and Tipler's principle of similarity does not help here. In the classical case, we saw how one could get the form of the force law (and hence the form of the potential too) from assuming Gauss' law; alternatively, we saw how one could get Gauss' law from the form of the force law (and hence potential). The assumptions to get from one to the other are equally mild, so one lacks strong intuitions about which way to go.

The problem, I submit, is simply that there is *no background theoretical framework* that can help us answer this question. Both Ehrenfest and BLM are extending some $n = 3$ physical laws to higher dimensions and not extending some others. Absent a developed physical theory that takes dimensionality as contingent and offers principled physical constraints on what can happen in different dimensions, there seem to be no standards for knowing which laws hold in what dimension. Which is more fundamental, a r^{-1} potential in higher dimensions (and thus stable orbits there) or Gauss' law in higher dimensions (and thus no stable orbits there)? There is no scientific theory of this, and only vague intuitions fill the vacuum.

It is useful to compare and contrast this case with the following example of an 'explanation' of a fundamental constant. There is a school of quantum cosmology that offers explanations of why the speed of light is what it is, why Ω, the critical density of the universe, is what it is, why neutrino masses are what they are, and so on (see, e.g., Vilenkin (1995)). How does one explain why c is 300,000,000 m/s? In short, one posits an ensemble of worlds, puts a probability metric over all these worlds and then derives that most of these worlds—or most of the ones with humans in them—have light traveling close to the speed it travels here. Wildly speculative? Yes. Scratching an itch to explain that would be better off unscratched? Yes. But despite these problems, this explanation of c is better than present explanations of tri-dimensionality in at least one respect: at least one is explicitly positing a theory in which what we formerly took to be a fundamental constant is allowed to vary with world and there are clear constraints on this. By contrast, Ehrenfest, Büchel, Barrow and Tipler, BLM, and so on, are implicitly offering theories of higher-dimensional worlds wherein dimensionality is not a constant but a variable. Unlike Vilenkin, however, they are not dressing up this theory with details; nor do they even acknowledge that this is what they are up to (apart from citing vague principles like the "principle of similarity" above). To explain brute facts in one theory, one needs another theory. Without a developed theory, all we have are our intuitions and various vague recipes for determining closeness to worlds.[10]

BLM and Caruso and Xavier do not learn the lesson of their success. For after criticizing the Ehrenfest-style argument, BLM then claim to "have proven by a spectroscopic experiment that our space is three dimensional" (631) and Caruso and Xavier claim that nuclear diffraction experiments imply that $n \leqslant 5$. The idea is that the atomic spectra and diffraction experiments, not stability, have implications for the dimensionality of space. They "fix" Ehrenfest's methodology by focusing solely on laws that they believe do not show "singular aspects" concerning $n = 3$. That is, they look for laws that they believe are indifferent to the dimensionality of space, e.g., the laws of classical thermodynamics. This way, as they see it, they do not build in the answer to their question. Though ingenious, the argument falls to the same objection used above against Ehrenfest. Their argument is an improvement over Ehrenfest's because they seek to use laws and relationships that do not vary with dimension. But still, to get their conclusion, *some* physics is of course used, *some* assumptions are made, and these assumptions may not be legitimate in higher dimensions, e.g., BLM's assumption that in every dimension the lowest series contains only transitions with $l = k = 0$ or Caruso and Xavier's assumption that classical thermodynamics is valid in all n. In the absence of a successful theory that takes dimensionality to be variable, there is no way to know if these assumptions are warranted.

Are there theories that might explain the number of dimensions? On the horizon there may be. Arkani-Hamed, Cohen and Georgi, for instance, propose a dynamical theory of dimension creation that would, I suppose, take dimensionality as something to be appropriately explained via the dynamics of physical processes. But such theories are far too speculative for us to have much faith in today.

6. Conclusion: what questions are "justified"?

Ehrenfest wrote that he did not know if the question, "why does space have three dimensions?" is a "justified" question. In the preceding sections, I have argued that at least according to the dominant contemporary way of understanding this question (question (2) of Section 5.2), it is not a good one to ask. The problem has been that these proofs have substituted intuitions about higher-dimensional physics where one needs a theory.

Despite this problem, some of the "answers" provided by Ehrenfest and others are quite interesting. I submit that their interest lies primarily in their relevance to

[10]Realists about a similarity metric among metaphysically possible but physically impossible worlds may insist that there is still a fact of the matter about which world is closest to this one. In Lewis' system there presumably would be a fact of the matter at dispute between Ehrenfest and BLM. But notice that that metric is one weighting laws differently than matters of fact, so unless we could determine which is the fundamental law we will likely be epistemically closed to the solution even if there is a fact of the matter at issue.

answering question (1) of Section 5.2, the question of determining what in fact the number of spacetime dimensions is. Noticing connections between various disparate phenomena is the lifeblood of science. These connections help us determine the overall best systematization of the world, part of which is a commitment to a particular dimensionality.

Let me close by noting how our understanding of the question, why does space have three dimensions? depends crucially on what background assumptions are in play. Here are two examples.

First, return to the instigator of this whole project, Kant. Kant wrote,

> substances in the existing world… have essential forces of such a kind that they propagate their effects in union with each other according to the inverse–square relations of the distances,… [and] that the whole to which this gives rise has, by virtue of this law, the property of being three-dimensional.

Why did he think that the laws and forces determine the dimensionality and not vice versa? The answer is that early Kant was a kind of spatial relationist. He thought that substances and their forces were primary and that spatial properties were derivative upon the relationships among the substances and their forces. From this perspective, Kant's gravitational argument and stable orbits arguments are natural ones to make. After all, relationists owe us a story about how the various spatiotemporal properties—e.g., orientability, dimensionality, etc—are grounded in the relationist base (whatever it is). Stepping back from the details of Kant's argument, one can think of an argument like his as an attempt to get a spatiotemporal feature from a dynamical one, as a way of getting a topological feature of spacetime from dynamical features of matter. Understood as 'why does space have three dimensions, given relationist basis B?' the question is certainly a good one and Kant's answer is not an unreasonable one.

Second, fast-forward roughly 350 yr and consider the closely related question: why does spacetime *seem* to have four dimensions? In the mouth of a superstring theorist, or any other heir of Kaluza–Klein theory, the question assumes that spacetime is fundamentally more than four dimensions. With these theories as background, the amended question is again certainly a good one to ask. Recall that the original five-dimensional (Kaluza, 1921) unification of gravity and electromagnetism did not have anything like what we would now call a "compactification" mechanism—a process that in a certain limit approximates a four-dimensional spacetime. Kaluza's original model had little to say to our question. Only later with Klein, and then Einstein and Bergmann, did one get the idea of "rolled up" compactified dimensions (for the original papers, see Appelquist, Chodos, & Freund, 1987). These schemes, and others involving ingenious new ways of "hiding" extra dimensions (e.g. Giddings and Thomas, 2002), continue in contemporary physics. They are the answers to the new question. Ehrenfest-like demonstrations of the "singular" nature of four dimensions are only relevant in the sense that they motivate providing such a mechanism in the first place. Suppose it is true that one can only have stable orbits in four-dimensional spacetime. Then as we move from below the Planck scale to the classical scale, the stringy analog of matter had better behave more and more classically and its ten-dimensional space had better look more and more like three dimensions to such matter if it is to account for stable orbits and the like.

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