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Geometric Possibility: An Argument from Dimension

1. Introduction

What are the possible structures of space? I suspect that this question has no completely determinate answer, but nevertheless it's a question that deserves some consideration: an answer to it, even a somewhat vague one, would be relevant to the substantivalist/relationalist debate, as Belot's recent book Geometric Possibility (Belot, 2011) illustrates in detail. I'm sympathetic to the view that there is no completely determinate answer to this question, but many people (including me) have strong intuitions that would significantly influence even an indeterminate answer in fairly striking ways; intuitions that at least narrow down which structures are possible structures of space. For example, there are those like Maudlin (2007) and Nerlich (1994) who believe that distance relations between points must be mediated, and that distance must be defined in terms of the minimal length of path between two points (length of path being taken as primitive). Prima facie this view rules out any discrete space, and more[1], as representing a possible structure of space. Those who don't share this intuition, and allow distance relations to be unmediated, take the class of mathematical structures that represent possible structures of space to include structures ruled out by Maudlin and Nerlich. Belot has (perhaps vague) intuitions that being able to define a suitable notion of distance in a space marks a mathematical space as representing a possible way that space could be; he takes this to indicate that the class of metric spaces might be a plausible class to represent ways that space could be. So our intuitions influence at least the ballpark where our answers to this question lie, even if they don't lead to an exact answer. I think it is a worthwhile exercise to examine where some of our intuitions lead us in this regard. Here I propose to do this with an intuition that I have (and, I'll argue, that others have had) namely, that to be a structure with the right credentials to represent a possible structure of space that structure must have a determinate dimension.

An outline of what lies below is as follows: first I describe the methodology I take to be appropriate for addressing a question like this. I think a Bricker-style principle of plenitude on possible structures of space is part of the proper way to approach things here. But that principle won't work without something to give it more teeth, and I suggest that this is where intuitions about what makes a space "spatial" can come in. I discuss the plausibility of the intuition that space must have a determinate dimension—arguing that it's not an uncommon intuition, and examine various ideas people have had about how to cash out a notion of dimension for spatial structures; I then present and explain the three classic dimension functions. Next I turn to examples of mathematical spaces that seem on the face of it to be plausible candidates for representing space. These examples are due to the Russian

[1] See *Geometric Possibility* p.26 for other structures ruled out by this intuition.

topologist Fedorchuk, and they are of interest here because of their pathological dimension properties. I discuss the implications of these pathological spaces for our conception of dimension and of space, and argue that we are led by considerations of dimension to the view that something roughly like the class of metric spaces, or even a fairly natural subclass of this class serves to represent the class of possible spaces.

A cautionary remark is in order for the metaphysically faint of heart: when asking the question which class of mathematical spaces represents ways that space could be I am presupposing a brand of metaphysical possibility that goes beyond physical possibility: I don't think an adequate answer to the question is simply whichever mathematical space is presupposed by the correct physics or by the best confirmed physics. For example, I (along with Belot) take it that even though space is not Euclidean, nevertheless it is true that it could have been. And I think it could have had other quite different geometric structures too. If you agree, then I think there is an interesting argument to be had below. Of course some readers will take this presupposition of a notion of metaphysical possibility to be simply unacceptable. Those readers who balk at such notions will not find the main philosophical thrust of what follows convincing, but at the very least I hope they will find it interesting.

2. Plenitude and its role in reasoning possible geometry

In *Geometric Possibility* Gordon Belot suggests that it is to be taken as obvious that at least Euclidean three space is a possible structure that space could have, and he thinks it reasonable to suppose that "the class of mathematical structures that represent possible spatial geometries correspond to some suitable natural generalization of Euclidean geometry" (p.9). These two claims have considerable intuitive appeal, and this starting point has some precedent in the literature, (see Bricker, 1991, and 1993). But how we proceed from them to try to shed some light on the class of possible structures of space is not so clear.

One place to look is Bricker's approach to answering the slightly different question of how we know what the space of logically possible structures is in "Plenitude of Possible Structures" (Bricker 1991). According to Bricker our knowledge (of modal facts—say of the class of possible geometries of space) "starts from our theorizing about the actual world, and is extended, in accordance with the demands of plenitude, by the results of mathematics." (p.608, 1991) The constraints laid down by theorizing about the actual world for Bricker require only that if a structure plays or has played an explanatory role in our theorizing about the world then that structure is logically possible. But, as Bricker points out, we think that all sorts of structures are possible despite their never having played such an explanatory role; it is in our search for these structures that a principle of plenitude guides us according to the dictum that there are no gaps or, more fundamentally, unnatural boundaries in logical space. If Euclidean geometry of three dimensions is a structure that has played an explanatory role and so is possible, the principle of plenitude will guide us to some intuitively natural generalization of that structure: Perhaps the class of all Euclidean geometries of any finite (and perhaps of infinite) dimension, or perhaps the class of all metric spaces of three dimensions. Here, as elsewhere according to Bricker, mathematics plays the role of filling up logical space plenitude-wise. But what are the criteria by which mathematics should fill up logical space? For Bricker, there are classes of mathematical structures[2] and the boundaries in logical space are determined by these classes. Some such classes are natural while others are not. If a class serves as a principle object of study for some major area of mathematics then that class is natural. Boundaries in logical space are natural if they are given to us by natural classes or unions of natural classes. Thus groups and topological spaces are structures that form natural classes, and so natural boundaries, in the logical space of structures. The precise principle of plenitude that Bricker argues for is the *Plenitude of structures*: Suppose S is a class of logically possible structures. Any structure belonging to any natural generalization of S is logically possible. (Bricker 1991 p. 617.)

How might one use this principle of plenitude to guide thinking about our question: which class of mathematical structures represents the possible geometries of space?[3] Start with the mathematical structures that have been (explanatorily) used to theorize about space then consider every natural generalization of that structure, the class of structures this gives you will be the class of possible geometries of space. Classes that serve as principle objects of study for some major area of mathematics are to constrain our generalizing. If we start with Euclidean three space we can see that this method certainly is one way to cash out the intuition that the class of possible geometries of space is represented by some suitable generalization of Euclidean geometry. But just what is this suitable generalization? Generalizations, even natural ones, can go every which way, and can go very far: One generalization of Euclidean three space leads us to three dimensional metric spaces, another, n-dimensional metric spaces, yet another, three dimensional topological spaces, but why not n-dimensional topological spaces, or continuous metric spaces, metric spaces in general, or topological spaces in general? How far do we go when considering "any natural generalization"? And what guides us in knowing when and where to stop? Nothing in Bricker's account so far helps very much. Perhaps it is obvious that some of these are not natural according to Bricker. But without adding to the view something with enough teeth to stop the generalizing at some point we end up including some mathematical structures that do not appear to have the right credentials to represent structure that is in anyway *spatial*. Belot also articulates this worry: if we think of Euclidean geometry as a structure with points and finitely many relations defined on those points and generalize accordingly, to any structure

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[2] And entities according to Bricker, but that need not concern us here.
[3] It's implicit in Bricker's paper that he thinks we can use the principle of plentitude to advance our understanding of an answer to our question. On route to the Principle of Plenitude he asks us to consider the class of logically possible spatiotemporal structures, and he takes it to be clear that this class would contain both discrete as well as continuous space-times. Given this he argues that, since no natural class of structures would contain both, we must at least take the class of logically possible structures to contain unions of natural classes.

with finitely many relations defined on its set of points we are clearly including structures that are in no way spatial (Belot 2011, p.11).

The principle of plenitude is part of what we need to help us make headway on our question, but it needs to be supplemented with something that guides us through the logical space of structures picking out from within them a natural class of "spatial" structures. And it seems that intuitions about what makes a space "spatial" are what help here.

Belot, starting with the idea that the class of spatial geometries should be a suitable generalization of Euclidean geometry, argues that at least on various natural ways of understanding Euclidean geometry, we see a pattern emerge when we pay close attention to our intuitions, "An intuitively plausible sufficient condition is that the structure in question supports a natural notion of distance between points" (p.11). Belot suggests that the class of spaces that has the best claim to representing the possible structures of space is the class of metric spaces[4]. He thinks that there is no defensible reason to take a smaller class arguing that one would be hard pressed to find good grounds to argue for a sub class of the class of metric spaces; for any criteria one would pick, one would find oneself arbitrarily casting out spaces as much as arbitrarily including spaces.

Belot certainly recognizes that for each characterization of Euclidean geometry he considers there will be the temptation to include some structures in which a reasonable notion of distance is not definable; he points to projective geometry and symplectic spaces as examples. That is, there are, according to his intuitions, spaces that might have the right kinds of credentials to represent a possible space even if they don't support a notion of distance. Now it seems, given the nature of our question, that we have to live with some vagueness with respect to an answer, but there certainly does seem to be something intuitively appealing about the class of metric spaces as our answer. What I want to do here is suggest that our intuitions about the possible structure of space are richer than just those that focus on distance, and that we should consider embracing these intuitions. When we do we are led to something roughly like the class of metric spaces or a fairly natural subclass of these. In particular, depending on how we understand the notion of dimension, we are led either to the full class of metric spaces, or to the class of separable metric spaces as the class that represents the possible geometries of space. Looking at considerations of dimension gives us some reason for why it is the

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[4] A space, X, is a metric space if there us a function d: $X \times X \to \mathbb{R}$ Such that (i) $d(x,y) \geq 0$ for all x,y in X (and $d(x,y) = 0$ if and only if $x=y$) (ii) $d(x,y) = d(y,x)$ for all x,y in X and (iii) The triangle inequality holds: $d(x,y) + d(y,z) \geq d(x,z)$ for all x,y and z in X. It is worth remarking here that Belot discusses the question of the possible geometries of space in the first chapter of his book, but nothing in the rest of the book hinges on the reader agreeing with him in taking the extent of possible geometries to be represented by the class of metric spaces. It's more than enough for his purposes in the rest of the book, which are to assess the extent to which a relationalist about space can match the substantivalists claims about geometric possibility, that we simply think space could have had the structure of any of the geometries of constant curvature.

class of metric spaces, or some subclass of them, that are the possible ways space could be; reasons that one may not have ever realized if one thought only about distance.

3. Topological spaces

If we think about possible features that a structure ought to have in order for that structure to represent a way that space could be distance comes to mind, but that is certainly not the only feature that comes to mind. There are topological features that would appear to mark a mathematical space as representing features of physical space. Topology is often described by philosophers as rubber sheet geometry, and topological invariants of spaces are those features that remain the same under homeomorphism, or less precisely, but perhaps more intuitively, under continuous deformation. You can take an elastic enough ball and deform it into a cube by stretching and squeezing alone, but to deform it into a cube with a hole right through it no amount of squeezing and stretching will suffice, you'll have to pierce it all the way through. More generally: take any two points, a and b, of a perfectly elastic sheet and now stretch and squeeze the sheet as much as you like, there are relations, and they appear to be intuitively spatial relations, between the points that will remain under any stretching or squeezing. And these relations are the ones encoded by the topology of the space, and there are plenty of them. For example, suppose one of those points, say b, lies in an open set B (say the inside of a circle drawn (before the stretching began) around b), and the other point, a, is such that no matter how small a closed figure you draw around it, there are points in B that lie inside the boundary of that closed figure, this relation between the point a and the set of points B will still hold no matter how much you squeeze and stretch the space (such points, once we shore up the definitions, are limit points of B).

There is a sense in which these topological features do have to do with what we might want to call an absolute notion of distance. By piercing the cube with a hole you have made it the case that points that used to lie "close together" in the cube now lie "far apart". Of course by stretching the ball enough you'll make it such that points that used to be 1cm apart are now 100cm apart, but there's an intuitively clear notion of "close-by-ness" that remains unchanged by the stretching but that is changed by the piercing. And points that are limit points of sets (but not members of those sets) are points that intuitively lie arbitrarily close to (but not in) those sets. Again, this notion of arbitrary closeness remains unchanged by stretching or squeezing. Are the relationships described here relationships of distance? They are definable in topological spaces, even in non-metrizable topological spaces, and so are not exhaustively characterised simply by the axioms of a metric space[5]. Certainly those who take the metric space axioms as defining distance wont count them as

[5] This absolute notion of distance is captured in a metric space if we use the metric to induce a topology on the space defining the open sets of the space to be the interiors of balls of arbitrary radius given by the metric centered on arbitrary points of the space.

distance relationships. But whatever we call them they are at the very least spatial relationships, and they are spatial relationships that are encoded by the topology of the space.

There is a sense, then, in which topology can be seen as characterizing spatial features, and even distance or closeness relations depending on whether you are willing to call them that, of a more general kind than the metric. Once we define a topology on a set of points we have a way of determining, for any point and subset of the topological space whether that point is a limit point of the subset; we have a way of characterizing the notion of closeness described above. Such spaces are not metric spaces, since they don't have a distance function defined on them satisfying the metric space axioms. Nor are all such spaces metrizable. To be metrizable there must be a metric definable on the space that induces the topology in the sense that the open sets determined by the metric coincide with the open sets of the topology. And not every topological space is metrizable in this sense, and so not every topological space is one in which we can define a global or even local notion of distance that satisfies the metric space axioms that is compatible with the topology of that space.[6]

Describing topological spaces the way I have done here highlights their "geometric" or "spatial" character, and just as metric spaces can be wild and weird[7] topological spaces can be too.[8] But there's a lot more topological structure that one can impose on a topological space before arriving at a space on which you can define a metric compatible with the topology, and a lot of this structure seems to be "spatial" too. The separation axioms are axioms that also appear to be encoding spatial information: A space is *Hausdorff* if any two distinct points lie in distinct neighborhoods, and a space is *regular*, just in case any point and closed set not containing it lie in disjoint neighborhoods. These relationships will clearly be invariant under continuous deformations, and their definitions seem to be giving us "spatial" information: they give us information about closeness relations between points and neighborhoods of points of space, even though they aren't giving us information about metric distance. One can have topological spaces that are locally homeomorphic to R^n, that is, in the small they are topologically just like R^n, but in the large they are very different, and are not metrizable.

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[6] Topologists will often take a metric space to be a metrizable space X together with a specific metric that gives the topology of X (see Hocking and Young (1988), p.11, Dugundji (1966), p.183, Steen and Seebach (1995), p.34, and Munkres (2008), p. 120, although see Engelking (1989) p.247 for an example of a topologist who doesn't do this). Clearly one *could* define a metric on any set of points regardless of a given topology on that set, since one can just assign, for example, the discrete metric to any point set. But a set with a topology and a metric not compatible with that topology would have some strange features: it could happen that a point is arbitrarily close to a set, in the sense of being a limit point of that set according to the topology, but very far away from all members of that set according to the metric.

[7] Just read Chapter 1 of Belot's *Geometric Possibility* to get a sense of how wild and weird.

[8] For a great and accessible survey look at *Counterexamples in Topology* (Steen and Seebach, 1995)

So topological spaces with rich enough structure on the face of it seem to be natural candidates for representing spatial structure. If so, and if we think that there are non-metrizable spaces in which the structure is rich enough then we may be led to a larger class of spaces than the class of metric spaces by a Bricker style principle of plenitude.

I'll argue below that there are fairly plausible considerations that can be brought to bear to rule out at least some classes of non-metrizable, but otherwise prima facie "space-like" spaces, as counting as representing ways that space could be, and that these considerations come from thinking about dimension rather than just distance. For the argument to work we need to think that it is at least intuitively plausible that for a mathematical space to count as a possible way for space to be it should have some (possibly infinite) dimension. That to be a possible way that space can be requires that that space has some determinate dimension, and that there should be determinate facts about the space in virtue of which it has the dimension it does. If you agree, then there are examples that can be used to illustrate that the class of metrizable spaces, or perhaps a fairly natural looking subclass of the class of metrizable spaces gives us all the possible ways that space could be. The examples and the arguments are in sections 7 through 9 while the required definitions of dimension are in section 6. First I want to try to motivate the idea that thinking that space must have a determinate dimension is a reasonable view.

4. Could space fail to have a determinate dimension?

Can we imagine space that doesn't have a determinate dimension? Many philosophers, physicists and mathematicians appear to have had the intuition that physical space must have some determinate dimension. In fact there has been a long and lively debate that has flourished at various times since the Greeks about whether or why space has three dimensions. An excellent discussion of which can be found in the first of Dale Johnson's two papers on the history of topology (Johnson 19xx and 1979). The earlier participants tended to want to rule out the possibility of a space with anything but three dimensions. Thus Ptolemy purportedly argues that space has to have three dimensions, for it is impossible to draw a line perpendicular to three mutually perpendicular lines (Thomas, 1941). And Aristotle expresses an interest in dimension in various places, but in "On the Heavens" he makes his commitment to the three dimensionality of space quite explicit: "Of magnitude that which (extends) one way is a line, that which (extends) two ways a plane, and that which (extends) three ways a body. And there is no magnitude besides these, because three dimensions are all that there are, and thrice extended means extended all ways." (McKeon, 1941, I.1, 268a4-13, 268a20-268b5).

In the seventeenth century we find the following kinds of remarks, this one from John Wallis in his *A Treatise of Algebra, Both Practical and Historical* (1685),

"For whereas Nature in propriety of Speech, doth not admit of more than Three (Local) Dimensions, (Length, Breadth and Thickness, in Lines, Surfaces and

Solids;) it may justly seem very improper, to talk of a Solid (of three Dimensions) drawn into a Fourth, Fifth, Sixth, or further Dimension.

A Line drawn into a Line, shall make a Plane or Surface; this drawn into a Line, shall make a Solid: But if this Solid be drawn into a Line, or this Plane into a Plane, what shall it make? a *Plano-Plane*? That is a Monster in Nature, and less possible than a *Chimera* or a *Centaure*. For Length, Breadth and Thickness, take up the whole of Space. Nor can Fansie imagine how there should be a Fourth Local Dimension beyond these Three."

But by the latter part of the eighteenth century mathematicians and philosophers, while still confident of the actual three dimensionality of space are willing to countenance the possibility of space having a dimension other than three. And more recently, Ehrenfest (1917) and Russell (1897) and still more recently Barrow (1983), Buchel (1969), and Callender (2005) all contributed to the debate concerning the three dimensionality of space. Russell in his dissertation "An Essay on the Foundations of Geometry" claims that "we obtain, as an a priori condition of geometry, logically indispensible to its existence, the axiom that *Space must have a finite integral number of Dimensions*", (p 161) for otherwise positions would not be definable uniquely and exhaustively by a finite number of relations.[9] And his discussion here is not simply about geometry as a branch of pure mathematics, for him evidence about the actual number of dimensions that physical space has must come from experience, but the possible answers to the question of the dimension of physical space are constrained to be some natural number or other. Others, such as Barrow, Buchel and Ehrenfest have suggested that the three-dimensionality of our world is explained by the existence of stable planetary orbits.[10] The intuition that physical space has some determinate dimension runs deep.[11]

5. A canvassing of intuitions about dimension

[9] For Russell, here, since positions in space exist only in relation to other positions, and in order to specify a position enough relations must be given in order to determine its relation to any new position, the number of relations so required constitute the dimension of the space.

[10] The kinds of arguments for the three dimensionality of space on the grounds that it is required by for the existence of stable planterary orbits require only the commitment that space have three dimensions because of the nature of the actual laws of physics. Proponents of such arguments might think it possible for space to have come other number of dimensions, but they also might think it possible that space have no determinate dimension at all. My hunch is that this is not the view of most of those described above, but if you are in that camp I am afraid the argument below won't be convincing. For those, like me, who think that space must have some determinate dimension or other the argument will have more force. Thanks to Gordon Belot for discussion on this.

[11] I don't want to claim that we know a priori that space must have a determinate dimension, rather I am just trying to motivate the claim that thinking that substantival space must have a determinate dimension is an a intuition shared by many.

Presumably our intuitions about dimension come in part from our informal (and formal) geometric training and in part from our experience of space and objects in space.

When set the task of mapping a region of our space the temptation to pick three axes and locate points of space via their coordinates using ordered triples of reals is overwhelming. If we were asked to map points on a plane we'd just use two axes, and use ordered pairs to locate points, and for points on a line just one axis. Thus we are tempted to think that this choice of the number of axes captures some property of the space we are mapping, and we are tempted to think that this property is its dimension. And this does seem to partially underlie our intuitive notion of the dimension of space, but it runs into difficulties when we think of space filling curves of the kind first championed by Peano.

Another source of our intuitions about the dimension of space comes from thinking about objects in space and their extension and boundaries. A ball seems to "take up space" while its boundary, the surface of the ball, lacks that "depth" and in that respect seems similar to a plane. And we extrapolate to other geometric objects such as circles and their circumferences or lines and points. These dimensional features of objects seem to be related to our concept of the dimension of space. It is natural, especially so if we are substantivalists, to think that the dimensional properties of objects that are sketched out in this way arise from the dimensional properties of the parts of space that they occupy. Another source of our intuition, which may be a development of the previous one, is that when we start thinking about things in R^1, $R^2$and R^3, a plane bisects space, while a line bisects a plane and a point bisects a line. And these relations seem to be relations that hold in virtue of the dimensions of the relata.

Clearly these intuitions come in part from our geometric schooling, the third more obviously so than the first two. It is certainly not clear how concrete our intuitions about dimension would be without some geometric schooling. But what seems likely is that one doesn't need much formal training in geometry, and perhaps one only needs to start thinking about geometry quite informally[12] before one starts to have fairly robust and determinate intuitions about the dimension of objects (or their parts, such as their surfaces and boundaries) that we see and the space that we live in. It seems likely too that these intuitions come from the kinds of considerations I have just outlined.

That something like these intuitions underlie our conception of dimension can be further confirmed by sampling some of the conceptions of dimension one sees being suggested throughout history. Just as the debate over the three dimensionality of space has participants going at least all the way back to the Greeks the Greeks also seem to have had fairly robust intuitions about what dimension was.

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[12] And perhaps watching some Star Trek

Among those who noted that consideration of extension and boundaries of figures leads to a characterization of dimension were the Pythagoreans: The following remarks in Speusippus assign numbers to different geometric objects: "For 1 is a point, 2 is a line, 3 is a triangle and 4 is a pyramid; ….in surfaces and solids there are the elements--- point, line, triangle, pyramid,…..The same result is seen in their generation." (Thomas 1939, p. 80). And Aristotle in his Metaphysics attributes the following to the Pythagoreans: "Some think that the limits of bodies, such as surface and line and point or unit are substances, rather than body and the solid." Metaphysics, VII.2, 10286b16, and "There are some who, because the point is the limit and end of a line, the line of a surface and the surface of a solid, hold it to be inescapable that such natures exist" Metaphysics XIV.3, 1090b5 (quoted in Guthrie 1962, pp. 259) In addition, Euclid's first six definitions of book 1 of his <u>Elements</u> explain the bounding relation between points, lines and surfaces.

In Nicomachus of Gerasa there is an explicit statement of the relation between boundaries and dimension: "Indeed, when a point is added to a point, it makes no increase, for when a non-dimensional thing is added to another non-dimensional thing, it will not thereby have dimension…..For dimension is that which is conceived of as between two limits. The first dimension is called "line" for line is extended in one direction. Two dimensions are called "surface", for a surface is that which is extended in two directions. Three dimensions are called "solid", for a solid is that which is extended in three directions. …. The point, then is the beginning of dimension, but not itself a dimension, and likewise the beginning of a line, but not itself a line; the line is the beginning of surface, but not surface, and the beginning of the two-dimensional, but not itself extended in two directions. Naturally, too, surface is the beginning of body, but not itself body, and likewise the beginning of the three-dimensional but not itself extended in three directions" (quoted in Guthrie 1962, p. 261)

One also finds evidence of an intuition about dimension that comes from the generation of elements of a higher dimension from elements that are intuitively of a lower dimension, the so-called fluxion theory: so from Aristotle, thought by some to perhaps be an attribution to the Pythagoreans: (On the Soul, I.4) "For they say that the movement of a line creates a plane and that of the point a line; and likewise the movements of units will be lines." And in Sextus Empiricus (1936) p.346-349 "But some assert that the body is constructed from one point; for this point when it has flowed produces the line, and the line when it has flowed makes the plane, and this when it has moved towards depth generates the body which has three dimensions."

We have here an array of intuitions regarding the kinds of properties of objects and space that form part of our intuitive conception of the dimension of space. If it is the case that dimension is as central a notion to space as I have been suggesting then we ought to be able to characterize more precise notions of dimension that accord with these intuitions. This is one thing that the rise of dimension theory in the twentieth century did, and it is to the characterizations of dimension that resulted that we turn to next. If we are to evaluate candidate classes of mathematical spaces according to their dimension properties these are tools with which we can do it.

6. Mathematical (topological) characterizations of dimension

Dimension Theory is a branch of mathematics originally completely confined within topology that blossomed in the early 1920's and flourished for the next 15 years. During that time a well-developed theory of dimension for separable metric spaces was developed.[13] Since the 1950's dimension theorists have extended the theory to larger classes of spaces. Abstractly the problem is to come up with a function from spaces to numbers such that the value of the function for a given space is taken to be the dimension of that space. Such a function would be a dimension function. But there have to be some criteria of adequacy that such a function must satisfy in order to deserve to be called a dimension function. The most natural criterion is that the value of the function for R^n for any n must be n: the function must give the right dimension for our familiar Euclidean metric spaces. A function that gives the "wrong" number for R^n is surely not a function whose values reliably reveal the dimension of a space.[14] A second, probably less natural, but still quite natural, criterion is that the dimension function is a topological invariant: given two spaces X and Y that are homeomorphic, the value of the dimension function for X and Y should be the same.[15] But beyond these natural conditions there would seem to be considerable leeway. For the dimension theorists of the early part of the twentieth century the value of the dimension function for a given space must be -1, a natural number, or infinite. Beyond these two conditions the motivations for the three classic dimension functions, small inductive dimension, large inductive dimension, and covering dimension, that came out of the work of the dimension theorists of the first half of the twentieth century, seem to be the kinds of intuitions outlined in section 5 above.

The motivating idea for small inductive dimension comes straight from the second of the geometric intuitions outlined above: from considering the relation between the dimension of a boundary of an open set[16] in a space, and the dimension of the open set itself in Euclidean space. In R^2 the boundary of an open ball is a circle the dimension of which is one less than the ball. More generally, in R^n the boundary of a ball is a surface whose dimension is n-1. And the dimension of a point should be zero. So it is natural to start with the dimension of the most basic set—the empty set—stipulate that its dimension is -1, and then inductively define the dimensions of

[13] A space X is separable if it has a countable dense subset, that is, there is a countable subset, A, of X such that the closure (in X) of A is X. The rationals comprise a countable dense subset of the reals.
[14] That the dimension of R^n is n is sometimes referred to as the fundamental theorem of dimension theory.
[15] Certainly this is natural if we believe topology is the right part of mathematics in which to seek the correct account of the dimension of space, that is, if we think that dimension is a topological notion rather than a metric notion or some other notion.
[16] The boundary of a set is just the closure of that set (which you get by taking the union of the set and all its limit points) minus the interior of that set, and is notated here by δV

sets in terms of the dimensions of their boundaries from there. The dimension of a point, whose boundary is the empty set should be zero, the dimension of a set whose boundary is a point should be 1, and so on.

Thus we get the recursive definition of the small inductive dimension, ind, of a space[17], X, as follows:

> SI1. ind X = -1 iff X = ∅
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> SI2. ind X ≤ n, where n = 0,1,2,…, if for every point x of X and each neighborhood V ⊂ X of x there exists an open set U⊂V such that x∈U⊂V and ind δV ≤ n – 1.
>
> SI3 ind X= n if ind X ≤ n and ind X >n-1
>
> SI4 ind X = ∞ if ind X > n for n + -1,0,1,….

This definition of small inductive dimension was formulated by Urysohn and Menger in the 1920's.

The large inductive dimension, Ind, of a space is the second of the three classic dimension functions, and is closely related to the small inductive dimension. The definiton is due to Cech in the 1930's and is often referred to as Brouwer-Cech dimension. (Engelking ,1995, p. 42) since it is closely related to Brouwer's *Dimensionsgrad* function, defined in terms of cuts. Again the motivating factor is an inductive definition exploiting the relation between the dimensions of open sets and their boundaries in Euclidean space, but the second part of the recursive definition differs in a crucial way. Instead of considering the boundaries of open sets containing points, we are interested in the boundaries of open sets containing neighborhoods of closed sets.

> LI1. Ind X = -1 iff x = ∅
>
> LI2. Ind X ≤ n, where n = 0,1,2,…, if for every closed set A ⊂ X and each open set V⊂ X which contains the set A there exits an open set U ⊂ X such that A ⊂U⊂V and Ind δV ≤ n – 1.
>
> LI3 IndX= n if Ind X ≤ n and Ind X >n-1
>
> LI4 Ind X = ∞ if Ind X > n for n + -1,0,1,….

The definition of the covering dimension, dim, of a space was first given by Cech also in the 30's. It goes by a number of names in the mathematical literature: it's also called "Cech-Lebesgue dimension", "covering dimension", and even occasionally "topological dimension". Intuitively it comes from thinking about how we could tile a space with open sets in such a way that we minimize the number of tiles any point

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[17]This is the definition for regular spaces: A regular space is a space for which given a closed set A and a point b not in A, there are disjoint open sets OA and Ob containing A and b respectively. This is just a space that satisfies T3 of the topological separation axioms. For definitions of ind, Ind and dim see for example *Theory of Dimensions Finite and Infinite,* Engelking (1995). For the separation axioms see any standard text in general topology, for example, *General Topology*, Munkres (2008).

lies in. For example imagine tiling a wall, if you have an unlimited supply of tiles with no constraints on how the covering goes it's easy enough, but if the constraint is to cover the wall so that you minimize the number of tiles covering any given point you'll find that you can't avoid some points being covered by three tiles. If you can't tile a space with open sets without having some point lie in at least three of those open sets then the dimension of the space is two.

CL1 dim X ≤ n where n = -1,0,1,2,..., if every open cover of the space X has a finite open refinement of order ≤ n.

CL2 dim X = n if dim X ≤ n and dim X >n-1

CL3 dim X = ∞ if dim X > n for n + -1,0,1,....[18]

A mathematical example might help to illustrate further: suppose we have an interval of the real line, say [0,1]. We can cover this with a number of open sets, say [0,1) and (0,1], but suppose we are interested in a cover that minimizes the number of open sets that any point on the line lies in. Clearly we can't do it with fewer than two open sets, and some points will have to lie in both. This means, according to the definition of dim, our line has dimension 1. If we try the analogous construction for a disk in the plane we will find that the minimum number of open sets that some points lie in will be three, thus the disk has dimension two.

Covering dimension seems quite different from ind and Ind. But the tiling principle on which it is based is a natural way to think about the dimension of space even though it took until 1910 for anyone to think about dimension this way[19]. Lebesgue appears to have been the first to think of dimension in this way.

These are the three classic dimension functions, and they track topological properties of spaces. One might be tempted to ask why topology is the right place to look for the relevant conception of dimension for space; for one can give metric characterizations of dimension in a number of ways. There are a number of reasons to focus on these topological notions. First there is precedent: the mathematicians working on the problem of the invariance of dimension took topology to be the natural place to seek definitions of dimension; when one looks at the concerns they were motivated by it seems natural to interpret them as being motivated about the dimension of space, and not simply the dimensions of mathematical spaces as entities worth considering for their own sake. Historically the problem that motivated the development of dimension theory was Cantor's proof that there is a 1-1 correspondence between RxR and R. This caused concern about the legitimacy

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[18] A collection, A, of subsets of a space X has an ***order*** m if some point of X lies in m elements of A, and no point of X lies in more than m elements of A
Given a collection, A, of subsets of X, a collection B is a ***refinement*** of A if for every element B_t of B there is an element A_s of A such that $B_t \subset A_s$

[19] For a history of these definitions of dimension see D.M Johnson ("The Problem of the Invariance of Dimension in the Growth of Modern Topology" parts I and II). Lebesgue and Brouwer had a fairly acrimonious dispute about who first had satisfactorily proved the invariance of dimension. Johnson's history describes it well, and he illustrates the problems with Lebesgue's proof that uses the tiling principle.

of the notion of dimension of space itself amongst mathematicians, and work started in earnest trying to answer the problem of the invariance of dimension: if we have a continuous function with continuous inverse from R^n to R^m does that mean that n=m? Topological methods were those sought and found adequate for proving dimensional invariance.

At root the tiling principle is topological in character, and so are the three classic dimension functions. And they are all closely tied to our intuitions about what dimension is. So we *can* find topological notions of dimension that satisfy the criteria of adequacy we've laid down for a notion of dimension that fit with our intuitive conception of dimension. Of course for the purposes of this paper it is natural to consider a notion of dimension that applies more broadly than just to metric spaces. If one thinks that there really are compelling reasons to take topology as describing spatial structure, then one might think there are non-metric spaces that have the right kinds of credentials to count as ways that space could be. If so, and if dimension properties of spaces factor into this question we need to look beyond metric characterizations of dimension.

It's these kinds of reasons that motivate me to focus on topological notions of dimension here. What I'll turn to next is whether they can help us get any traction on the question of the structure that space could have. In particular, we will look at what might seem to be an intuitively natural generalization of Euclidean space: topological manifolds.

7. Topological manifolds with non-coinciding dimensions

Topological n-manifolds are topological spaces that are Hausdorff, that is, that for each pair of distinct points x and y there are disjoint open sets that contain x and y respectively; and any n-manifold is locally homeomorphic to R^n: every point of M has a neighborhood (lies in an open set) that is homeomorphic to R^n. A topological manifold thus defined may not be metrizable[20]. But topological n-manifolds are trivially locally metrizable since they are homeomorphic to R^n. In virtue of this mathematical structure these are spaces that represent a fair degree of "spatial" structure. As such they seem to be ideal examples of spaces that fall outside the class of metric spaces but appear to have fairly strong credentials for representing a way that space could be: there is no global notion of distance, but there is a local notion, and locally they couldn't be more like R^n, they are a generalization of Euclidean space; are they a natural generalization? The study of their properties forms a subdiscipline within topology, so they at least bare the Bricker mark of the natural.

Why take this definition of a manifold? Some texts, for example Munkres, add further structure to the definition of a manifold. If we add that the manifold has to

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[20] A space (X,T) is ***locally metrizable*** if each point lies in a neighborhood that is metrizable in the subspace topology. One might wonder about the conditions under which local metrizability gets one metrizability; a sufficient condition for this is if the space is compact and Hausdorff. See Munkres (2008)

be second countable (as Munkres does) or paracompact we are adding considerable structure beyond the definition we have here.[21] A paracompact manifold is metrizable, and a second countable manifold is metrizable and embeddable into a finite dimensional Euclidean space.[22] If we are considering spaces as candidates for representing possible ways that space can be then in adding this structure to our definition of a manifold we are already assuming that metrizability, or even embeddability into a finite dimensional Euclidean space[23] is a requirement of being a "spatial" space. For our purposes it is worth exploring whether without structure that guarantees that we can define a metric (compatible with the topology) on the manifold there are reasons to rule out such manifolds as representing a possible way that space can be. And I am interested in whether there are reasons that come from considerations that have to do with dimension rather than metrizability or embedability in finite dimensional Euclidean space. So we'll consider the more general definition of a topological manifold.

There are a number of open questions about the dimension properties of topological n-manifolds. While manifolds are Hausdorff, normal manifolds satisfy a stronger separation property; for a *normal* manifold, for each pair of disjoint closed sets A and B, there are disjoint open sets that contain A and B respectively. It's known that for normal topological n-manifolds, M, $n = \text{ind}M \leq \text{dim}M \leq \text{Ind}M$. And it is known that once we impose paracompactness on our manifold, M, because we end up with a metrizable and locally separable (since locally Euclidean) manifold, it ends up being separable, that is, it has countable dense subset (see Fedorchuk 1995, 152). And for all separable metric spaces the classic dimension functions ind, Ind, and dim coincide. But it is not known under precisely which conditions ind, Ind and dim coincide or diverge for topological manifolds. And it seems that dimension properties can get fairly wild for spaces that might seem like one natural way to generalize Euclidean space.

V.V. Fedorchuk, the Russian topologist, is famous for constructing spaces that have non-coinciding dimensions. His examples of manifolds with non-coinciding dimensions are constructed under the assumption of additional set theoretic axioms, and this is not atypical: in general topology it is common to construct counterexamples using consequences of the constructability axiom. In Fedorchuk (1993) he shows, for any pair of integers m and n such that $4 \leq n < m$, how to construct a countably compact perfectly normal hereditarily separable differentiable n-manifold M^n_m with $n = \text{ind}\, M^n_m < m = \text{dim}\, M^n_m < m+n-2 = \text{Ind}\, M^n_m$, under the assumption of Jensen's diamond principle (a consequence of the axiom of

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[21] A space, X, is paracompact if every open covering of X has a locally finite open refinement that covers X. A space X is second countable if it has a countable basis for its topology. Any second countable manifold is paracompact, although the converse doesn't hold.

[22] The Smirnov Metrization theorem states that a space is metrizable iff it is paracompact, Hausdorff and locally metrizable. (see for example, Munkres, p. 261) Local metrizability of a manifold follows trivially from it's local Euclidean structure.

[23] Embeddability into a finite dimensional Euclidean space is something one could try to argue is a constraint on the possible spaces, but this seems too strong: couldn't be infinite dimensional?

constructibility). And under the Continuum Hypothesis, a weaker assumption than the Jensen diamond, he shows in Fedorchuk (1995) how to construct, for any integers n and m such that $4 \leq n < m$, a perfectly normal hereditarily separable differentiable n-manifold $M^{n,m}$ of dimension: $m-1 \leq \dim M^{n,m} \leq m < m+n-3 \leq \mathrm{Ind}\, M^{n,m} \leq m+n-1$. Both examples require constructing a manifold via taking the limit of an inverse spectrum of spaces. These spaces are constructed by embedding the product of $M_1 \times I^{n-4}$ into the sphere S^{n-1} that bounds the closed ball, B^n.
One then partitions the closed ball, $B^{n,}$ using features of this embedding and constructs a quotient space from that partition. Using this quotient space one constructs an inverse spectrum of spaces, endowed, via CH or Diamond with a topology and differential structure. And the limit of the resulting inverse spectrum is a space in which the differentiable n-manifold with pathological dimensions lies as a subspace. They are mathematically "wild" examples, which are the results of "wild" constructions, but they are differentiable n-manifolds nonetheless, and so each is locally diffeomorphic to Euclidean n-space.

What is the significance of these examples for the question of the possible structures of space? Ignoring for the moment the question of the use of the Jensen Diamond principle or the Continuum Hypothesis, if we agree that a mathematical space has to have a determinate dimension in order to count as a possible way that space could be, and we agree that there is no one of the classic dimension functions that is the correct one, and that coincidence of ind, Ind, and dim is the mark of a space having a determinate dimension, then prima facie it would seem that we have an argument from these examples that at least various differential (topological) manifolds do not represent ways that space could be, and that the class of differential n-manifolds is not a suitable class to include in the union of classes that provide the plenitude of possibilities amongst spatial structure. This is the kind of argument I think one should make. But to defend it we'd need to be committed to each of the premises. In the next section I will discuss the question of whether one of the dimension functions is the correct one, and whether coincidence of the dimension functions should be seen as a mark of a space having a determinate dimension. I will take up the issue of additional set theoretic assumptions briefly afterwards.

8. Is one of dim, Ind and ind the correct characterization of dimension?

Perhaps it is a mistake to think that coincidence of all three of the classic dimension functions is what matters for a space to have a determinate dimension. Maybe just the coincidence of two is required, or perhaps only one of ind, Ind, and dim is *the* correct characterization of the dimension of space. I think this is a mistake. As I suggested earlier there are intuitions about what grounds the dimension of space, and dim, ind and Ind provide ways to flesh out these intuitions. But no one of these intuitions appears to be the univocal one. There doesn't seem to be any clear way to argue for one or two of these definitions as being right ones. However, mathematicians will make remarks such as the following passage from Engelking "The three dimensions coincide for the class of separable metric spaces….In larger classes of spaces the dimensions ind, Ind, and dim diverge. At first, the small inductive dimension ind was chiefly used; this notion has a great intuitive appeal and leads quickly and economically to an elegant theory. The dimensions Ind and dim played an auxiliary role and often were not even explicitly defined. To attain the next stage of development of dimension theory, namely its extension to larger classes of spaces, first and foremost to the class of metrizable spaces, it was necessary to realize that in fact there are three theories of dimension and to decide which is the proper one. The adoption of such a point of view quickly led to the understanding that the dimension ind is practically of no importance outside the class of separable metric spaces and that the dimension dim prevails over the dimension Ind" p.vii, Engelking (1995). Certainly dim appears to be the topologists favorite of the three classic dimension functions. It is often presented as *the* correct notion of dimension in modern texts on general topology. But the kinds of considerations that lead mathematicians to argue for this aren't deriving from a consideration about the nature of space and its dimension, but from simplicity and elegance of results. I now turn to a detailed discussion of why at least Engelking thinks that this is the right way to think of dimension. To do so involves looking at some of the results of the classic dimension theory of the 20's and 30's and how they have been extended. [24]

8.1 The canonical dimension theory and its extensions

The criteria of adequacy for a dimension function described in section 6 are remarkably minimal. It's not hard to think of other intuitively desirable properties that a dimension function should have if it purports to be tracking the dimension properties of physical space. For example, it would be nice to have a logarithmic law for the dimension of products of spaces, that is, to have the dimension of the product of any two (non-empty) spaces to be equal to the sum of the dimensions of those spaces. It seems natural to think of taking products of two spaces as a mathematical representation of drawing a point along a direction to get a line, or a line along a direction to get a surface. To have each such procedure generate an additional dimension is a special case of the logarithmic law. Similarly for subspaces of a space it would be nice for monotonicity to hold: to have the dimension of any subspace of a space, X, be less than or equal to the dimension of X. It would be odd indeed if one could restrict oneself to a particular subspace of a space and thereby be in a higher dimensional space. Another requirement on subspace behaviour, a requirement that seems natural enough on the face of it, would be an intermediate dimension theorem for a space of the following kind: that a space of dimension n contain subspaces of dimension k for all $k \leq n$. Further, there is dimension behaviour that would be natural enough to expect when building new spaces out of old by taking unions of spaces. Sometimes this procedure should result in higher dimensional

[24] In the following subsection is a summary of various results about each of dim, ind and Ind in classes of topological spaces. These results are interesting in their own right, but the reader keen to get to the philosophical discussion might want to skim this subsection and move on to the next one.

spaces, such as when we take the union of the rationals and the irrationals, but in other cases, for example, when one is dealing with unions of closed subspaces of a space, (say, by taking the union of two closed subspaces of the reals) one would expect that the dimension of the union would not exceed the larger of the dimensions of the two spaces.

How do ind, Ind and dim fare according to these criteria? It depends on the setting in which you ask the question. For the class of separable metric spaces (for which dim, ind and Ind all coincide) we get quite good behaviour. We cannot achieve a logarithmic law for products, but we can get the next best thing: For every pair, X, Y, of non-empty separable metric spaces ind(XxY)≤ indX + indY (Engelking, p.35) But the inequality cannot be replaced by equality: there are separable metric spaces X such that ind X=IndX=DimX=1 and ind(X x X)=Ind(XxX)=dim(XxX)=1. One such example is Erdös Space: let H_0 be the subspace of Hilbert space[25] consisting of all points with rational coordinates. Here the small inductive dimension of H_0, and in fact of any finite or countable product of H_0, is equal to 1. (Engelking, p.36). Subspaces are also well-behaved with respect to dimension in the class of separable metric spaces: monotonicity holds for each dimension function, and the further requirement, that a space of dimension n contain subspaces of dimension k for all k≤n, holds for separable metric spaces of finite dimension.[26] In addition, the following sum property holds: If we have a separable metric space, X, that can be represented as a countable union of closed subspaces each of which has a dimension no more than n then X has dimension no more than n

In the larger class of metrizable spaces it's known from the Katetov-Morita Theorem that values for dim and Ind coincide, while there are metrizable spaces for which ind diverges from dim and Ind.[27] For ind, the sum theorem fails to hold: one can construct a metrizable space of small inductive dimension one by taking the union of just two closed subspaces with small inductive dimension zero[28]. But for Ind and dim we get dimension behaviour that fares just as well according to the criteria above as we had for separable metric spaces: we still have the product inequality, and we have both monotonicity on subspaces and the stronger requirement of there being subspaces of a space X of Ind and dim n for any natural number ≤ n (for finite dimensional spaces). We also have the countable sum property for Ind and dim.

If we start looking at the properties of our dimension functions in yet larger classes of spaces we start to see more divergence of behaviour, and some behaviour that

[25] Hilbert Space is the set of all infinite sequences $\{x_i\}$ of real numbers such that the series of sums of their squares converges.
[26] For separable metric spaces of infinite dimension things are more complicated: under the continuum hypothesis one can construct infinite dimensional separable metric spaces in which every subspace has dimension 0. (see Engelking 1995 p.65)
[27] Roy's space is a completely metrizable space, X, such that indX=0 while dimX=IndX=1. (see Roy 1968, and Engelking 1995 p.163 and p.220)
[28] See Engelking 1995, p.227

seems to be really quite bad, at least according to the criteria above. Focusing on products in the class of normal spaces, for example, there are compact X and Y, such that ind X=Ind X=1, ind Y=Ind Y=2, but for which Ind (XxY)≥ind (XxY)≥4. There is also a normal space Z such that Ind Z=0, but for which Ind (ZxZ)>0. In hereditarily normal spaces, normal spaces all of whose subspaces are also normal, things still look unsettling: in hereditarily normal compact spaces whether there exist X and Y such that Ind (XxY)> Ind X+Ind Y is open. Moreover, under CH there are cases of locally compact perfectly normal spaces, Z, such that Ind Z=0 and Ind (ZxZ)>0 (Engelking 1995, p.136).

Turning to subspace behaviour, for X and Y both hausdorff and normal, if X is closed in Y, X is strongly paracompact, or Y is metrizable, then monotonicity holds. And while monotonicity holds for both Ind and dim for closed subspaces of normal spaces, there are hereditarily normal spaces in which monotonicity fails for Ind and dim, though interestingly not for ind. That is, there are hereditarily normal spaces, X, with dim X = Ind X = n, but which have subspaces of large and of covering dimension greater than n. There are even hereditarily normal spaces, X, such that dimension dimX =IndX =0, but which contain, for every natural number n, a subspace A_n with IndA_n=dim A_n=n (See Engelking 1995, p.135).

It is for reasons such as this that Engelking says the dimensions dim and Ind give us dimension theories that are "poorer and less harmonious than the dimension theory of separable metric spaces" (Engelking 1995, p. 127). To reign in Ind and have dimensional behaviour for subspaces and sums that looks closer to that of separable metric spaces, we have to restrict ourselves to strongly hereditarily normal spaces.[29] Every subspace, M, of a strongly hereditarily normal space X is such that IndM ≤ IndX. We get a counterpart to the other desirable property of subspaces of intermediate dimension for closed subspaces (actually of any normal space): If X is a normal space with large inductive dimension, n, greater than or equal to 1, then for every integer k less than n there is a closed subspace of X for which Ind = k. And if a strongly hereditarily normal space can be represented as a union of countably many closed subspaces each of which has dimension less than or equal to n, then the space itself has dimension less than or equal to n. Finally, if X and Y are both strongly hereditarily normal and compact we have that Ind(XxY) ≤ IndX+IndY.[30]

While the behaviour of Ind is moderately good in the class of strongly hereditarily normal spaces the behaviour of dim seems to remain moderately good in a slightly

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[29] Strongly hereditarily normal spaces are T_1 (that is, a space, X, such that for every pair of distinct points, a, b in X, there exists an open set A that contains a but not b, and there exists an open set B that contains b but not a) and for every pair U, V of separated subsets of X there are disjoint open sets W and Y such that U⊂W and V⊂Y and each of W and Y can be represented as the unions of point-finite families of open F_σ-sets. (A family of sets is *point finite* if any point of the space lies in just finitely many members of the family, and *F_σ-sets* are sets that can be represented as countable unions of closed sets)

[30] Dimension theorists long recognized that restricting the class of normal spaces, and looking at dimension properties of Ind in this restricted class might reveal less pathological behaviour of Ind. Various restrictions have been proposed; a discussion of the results obtained can be found in Engelking (1995), chapter two.

larger class of spaces. The countable sum theorem for dim holds in normal spaces. Monotonicity holds for dim (as for Ind) for closed subspaces of normal spaces; it also holds for some other kinds of subspaces of normal spaces[31], but it doesn't hold generally. Interestingly, unlike Ind, the further property for closed subspaces of intermediate dimension does not hold. In fact for every integer n greater than 1 there are compact normal spaces with dim=n whose closed subspaces have either dim less than or equal to zero or equal to n. Monotonicity holds generally for dim in strongly hereditarily normal spaces. And for products we have dim (XxY) $\leq$ dimX+dimY provided X is either compact or metrizable (and nonempty), Y is normal (and non empty), and the product, XxY, is normal.

8.2 The philosophical relevance of these results

What should we say of the relevance of this behaviour to the question of which one of, or indeed whether one of, the dimension functions is the best? Implicit in Engelking's discussion is something roughly like the following argument. The classic dimension functions agree for the class of separable metric spaces, and this gives us the canonical dimension theory: we take the theorems about dimension provable in that class as representing the canonical theory of dimension. These theorems include those described for separable metric spaces above, but also some others. But once we notice that these different dimension functions start to disagree outside this restricted class of spaces we have to decide how to understand the import of this disagreement. For Engelking it is "necessary to realize that in fact there are three theories of dimension and to decide which is the proper one." For him the relevant argument here appears to be as follows. To evaluate how good a dimension function is we observe its behaviour as we increase the class of spaces under consideration. We look at how many of the dimension theorems that hold in separable metric spaces also hold for a given function in the larger class. The dimension function that gives out last, the one that can get us closest to the canonical theory in the largest class of spaces, is the correct dimension function. Engelking says it is dim that fares best in *this* regard, and about this he appears to be right[32]

But it is not clear that the divergence of the dimension functions should be understood as a call for deciding which is the *right* function. Nor is it clear, even if one wanted to argue that one function is the right function, that Engelking's procedure is a well motivated one. And both of these worries can be motivated by the same kinds of considerations. Our intuitions about dimension come from the

[31] In particular, it holds for subspaces that are strongly paracompact, and for some others, see Engelking (1995), p.176.

[32] The description of the results I have given above appear to confirm this: it seems that dim gives us something closer to the canonical results in the class of normal spaces than Ind, and certainly than ind, although for each of ind and Ind there are some situations even in this larger class of spaces for which they appear to fare better. There are hereditarily normal spaces where monotonicity fails for Ind and dim, but not for ind, and in normal spaces we have the theorem on intermediate dimension for closed subspaces for Ind but not for dim.

kinds of considerations described in section 4 and 5 above. In part it is from thinking about dimension in Euclidean geometry, in part it is by thinking about shapes and their boundaries. And it is these intuitions that motivate each of the three classic dimension functions. Once we recognize that they are not coextensive beyond the class of separable metric spaces that by itself is not a reason to think one or more of them is *wrong,* or that one of them is *right*. If they diverged for a space we had independent grounds to think had dimension, p, and the value of only one of them for that space was p, then this would be some reason to think that that dimension function was the correct one. For example, if they diverged for one of the classic three geometries, and one of them got the intuitively right value for the dimension of the space, then we could perhaps argue that our intuitions about the dimension of that space are strong enough to justify the correctness of the function that accords with them. And we would revise our view of the other functions and try to understand what was wrong about the intuitions that led us to them. But even in this case it is not obvious that we would do this.[33] But the situation here is not of that kind. It is not because we have independent grounds for a view about the particular dimension of various normal spaces and dim gets us this dimension. Rather, we don't seem to have independent grounds for a judgement about the dimension of these spaces, and all three dimension functions return a number for a space, but dim also allows us to prove something closest to the theorems we had for the separable metric spaces.

An alternative way of thinking about the situation we are faced with is to think that each of the dimension function characterizes some aspect of the dimension of a space, and that for the spaces in which they coincide the dimension of that space is the value of that function. When they diverge we take this as reason to be wary of the notion of dimension of that space. It's not a space in which our usual conception of dimension applies. Part of that conception seems to be applicable to that space, but other parts are not. Thus rather than taking this as evidence that one of the dimension functions is correct, we could take it as evidence that there is something pathological about the space. We can ask and answer questions about the dimension of the space, but the kinds of answers we get suggest that the space itself does not have anything like "normal" spatial dimension properties.

Which of these arguments is the right one in this context? I don't see a knock down argument in either of them, but I think the latter one is at least as natural a way to think of things as Engelking's, even from the perspective of the pure mathematician, and is a more natural way to think of things when we are facing the question of which class of mathematical spaces has the right credentials to represent the class of possible physical spaces. Here it seems that one's intuitions about what grounds the dimensional properties of physical space seem to have a role to play, and the role they play seems to legitimize each of the dimension functions equally. Absent any confirmation that dim gets things *right* in the largest class of spaces I think we have no reason not to stick to our intuitive conception of dimension, a conception

[33] We might somehow revise our view of the dimension properties of the space itself.

that comes from our Euclidean or classical geometric intuitions. And so we should be guided by the coincidence of the dimension functions when using dimension to help us settle on a class of mathematical spaces that serve as representations of the way that space can be.

There is a different conclusion that I think the evidence so far could be used to help argue for: that is to argue that ind and Ind are in fact tracking the same features of our intuitive conception of dimension, but that Ind is preferable to ind. Their definitions are quite close, but Ind is better behaved than ind in the class of metric spaces (outside the class of separable metric spaces). And this is behaviour that we do have some independent grounds for thinking tracks dimensional properties of the spaces in virtue of the agreement of Ind and dim in this class of spaces. This isn't a line of argument I will pursue here, although I think one could make a case for it. But even if we were to make that case, and we were to endorse, with Engelking, that ind is "of practically no importance" we'd be left with Ind and dim. And the Fedorchuk examples still pose problems. If we were to take the coincidence of Ind and dim to be the mark of a mathematical space with determinate dimension the second of the Fedorchuk examples raises the same kind of problem. Here we have divergence of Ind and dim, and so we have an argument that this space fails to be a space with determinate dimension, and thus fails to represent a way that space could be. And this argument might have more force than any appealing to Fedorchuk's first example on the grounds that the space in the second example is constructed just with the assumption of CH

But even if we were to follow the topologists in their choice of favorite and simply endorsed the function dim as giving us a canonical notion of dimension what is striking about the first of Fedorchuk's two examples is that dim gives a value for the dimension of an n-manifold that is distinct from n. Surely, even if there are grounds for thinking dim correctly characterizes the notion of dimension, this by itself should make us uncomfortable about taking the full class of differentiable n-manifolds as representing ways that space could be, for in the kinds of examples we have been given we have n-manifolds, that is spaces locally just like Euclidean n-space for which the *right* dimension is not n.

9. Conclusions

These examples are all in the context of a set theory that goes beyond the naïve ZFC. Perhaps this is a reason not to take them seriously at all. In addition to the axioms of ZFC we need to either endorse the axiom of constructibility (which would be enough to generate both examples) or, if we are more selective, we'd have to endorse Jensen Diamond[34] (for the first) and the Continuum Hypothesis (for the second). These are

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[34] Jensen Diamond ($\diamondsuit$) asserts that There are sets $A_\alpha \subset \alpha$ for $\alpha < \omega_1$ such that $\forall A \subset \omega_1$, $\{ \alpha < \omega_1 : A \cap \alpha = A_\alpha \}$ is stationary. The sequence $\langle A_\alpha : \alpha < \omega_1 \rangle$ is called a $\diamondsuit$-sequence. (Given a cardinal of uncountable cofinality, κ, if $S \subseteq \kappa$, and S intersects every closed unbounded set in κ, then S is *stationary*.)

controversial set theoretic axioms, and there are set theorists who would take them to be unacceptable on the grounds that they have counterintuitive consequences.[35] Of course there are also set theorists who would argue for their endorsement on the grounds that their consequences are attractive. I don't propose to try to adjudicate this issue here, but a couple of remarks are in order: First, it is common practice for topologists to construct counterexamples within ZFC+CH. One thing that such examples show us is that indeterminacy in our concept of set can have far reaching consequences for other concepts that we are interested in (in this case even for what counts as a way space could be). And in principle the results of this practice could end up unearthing reasons to revise our set theoretic assumptions. The current almost universal acceptance of the Axiom of Choice among mathematicians is in part the result of exploring its usefulness in other mathematical contexts. So the use of these axioms in these examples should not be seen as automatically ruling them out as cases of interest. Moreover, it may be the case that examples of such manifolds with non-coinciding dimensions cannot be constructed in ZFC, but we don't know this: it is still an open question in dimension theory; and until this question is settled we can't rule out these kinds of examples even if we are set theorists wedded to ZFC.

So where do we stand? At the least we have an argument that there are spaces that seem to be natural generalizations of Euclidean geometry that upon closer inspection should not count as possible ways that space could be because they have pathological dimension properties at least under additional set theoretic assumptions beyond ZFC. Since it is not known whether we can construct similar pathological examples under just ZFC, we at least should be concerned about whether there are such examples. If we take having non-pathological dimensional properties as being a requirement on a mathematical space for it to represent space, in the sense that the notion of dim, ind and Ind should coincide, then modulo a Bricker style principle of plenitude that constrains us to the class of separable metrizable spaces as the possible ways that space could be, for at least in this class of spaces we know that the three notions of dimension agree. If this is the case, then considerations of dimension have led us to realize that the separability of Euclidean space is something that really looks like it is fundamental to the nature of space. If, on the other hand, we cast aside the intuitively natural ind, take coincidence of Ind and dim to be the mark of a dimensionally non-pathological space then since Ind and dim coincide for the full class if metrizable spaces we are led, endorsing again Bricker's principle of plenitude, to the conclusion that metrizable spaces represent the ways that space could be. As I suggested above, I think some argument could be made for this that goes beyond an appeal to the tastes of mathematicians. But I don't see strong reasons to prefer the latter option; for now I'll leave it as at least a possibility. Whichever of these options is ultimately the right one at the very least

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The Axiom of Constructibility (V=L) implies Jensen Diamond, and Diamond, in turn, implies the Continuum Hypothesis.

[35] See Maddy (1988a) and Maddy (1988b), but for a contrary view see Devlin (1977)

we have an argument that dimensional properties provide us with a means of getting some traction on the question of what the possible structures of space could be.

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