INVESTIGATIONS INTO THE APPLICABILITY OF GEOMETRY

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Investigations into the Applicability of Geometry

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Abstract

Philosophical reflection about the sciences has persistently given rise to worries that mathematics, while true of its own special objects, is inapplicable to nature or to the physical world. Focusing on the case of geometry, and drawing on the histories of philosophy and science, I articulate a series of challenges to the applicability of geometry based on the general idea that geometry fails to fit (or correspond to) nature. This series of challenges then plays two major roles in the dissertation: it clarifies the ways in which the applicability of geometry poses a problem for two major 17^{th} century natural philosophers, *viz.*, Galileo and Leibniz, and it allows for the investigation of the relationship between geometric structures and nature by means of an investigation of the applicability of geometry.

I begin with the challenge pressed by some thinkers in the Aristotelian tradition that the results which geometry proves about its objects are false when interpreted as assertions about objects in nature. Despite the durable influence of this challenge and the Aristotelian theory of science which inspires it, I argue that Aristotle himself did not oppose the use of geometry in empirical inquiry, but rather offered an account of it. I then examine how Galileo takes on the objection that geometric results are false if understood as claims about nature in his *Dialogue Concerning the Two Chief*

World Systems. On my interpretation, Galileo argues the objection should be recast as the claim that there are no geometric points, lines, or surfaces in nature. This is an objection both Galileo and Leibniz take seriously in developing their new mathematical physics, although I argue that Galileo and Leibniz react to the objection very differently: Galileo *rejects* the objection as false and grounded on a misconception of the relationship between geometry and nature, whereas Leibniz *grants* the truth of the objection and tries to show that it is not damaging for the project of mathematical physics.

In defending the applicability of geometry, both Galileo and Leibniz help to develop and employ notions of approximation in the sciences. Their work highlights an important presupposition of approximations: that there must be determinate discrepancies between an object being approximated and its approximation. I conclude the dissertation with an argument that actual applications of geometry in empirical science require that there be determinate discrepancies between geometric structures and nature.

Contents

Acknowledgments

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To my mother
And hers
And to the memories of my father
And his

Chapter 1

Challenging the Applicability of Geometry

In the history of philosophical reflection about the sciences there have been persistent worries that mathematics, while perhaps both applicable and true when discussing its own special subject matter, is nonetheless inapplicable *to the physical world* or *when it comes to nature*. These worries have been most severe in the case of geometry, where the concern has been that although geometry furnishes us with a collection of truths about its special objects—points, lines, curves, *etc.*—nonetheless geometry is inapplicable when it comes to natural things such as bodies, trajectories, and physical space.

These worries about the applicability of geometry have nearly always taken place against a background in which, as a matter of scientific practice, geometry was being applied routinely and (by the standards of the time) successfully in the study of nature. In ancient science the application of geometry was especially evident in astronomy, optics, and mechanics.[1] This remained the case into the 17^{th} century, during which the range of applications of geometry was expanded tremendously by the many researchers who attempted to give natural scientific explanations exclusively in terms of the size, shape, position, and motion of bodies; each of these fundamental notions was to be understood geometrically.[2] In the present day, we have inherited various questions from the 17^{th} century or earlier which have traditionally received geometric answers and often still do: What is the figure (*viz.*, shape) of the earth? What trajectory does the moon trace out in its orbit? In more recent times we have added further questions to these: What is the shape of DNA? And, perhaps most importantly: What mathematical geometry accurately captures spatiotemporal relations in the physical world?

In much of this work I will be concerned with questions of the applicability of geometry which come down to us from ancient and 17^{th} century science. This is especially the case in the following three chapters in which I discuss Aristotle, Galileo, and Leibniz. Like other scientists of their respective periods, Aristotle, Galileo and Leibniz all applied geometry in their scientific work. They form part of the background just mentioned in which geometry is routinely applied in the study of nature. On the other hand, each one developed conceptual resources which help us to articulate and deepen the inchoate worry that geometry might somehow be inapplicable with respect

[1]See especially Ptolemy's *Almagest* and Euclid's *Optics* (Ptolemy, 1998; Euclid, 1972).

[2]I am referring to the adherents of the mechanical philosophy, various versions of which were defended by Galileo, Descartes, and other scientists. That Descartes understood the fundamental notions of the mechanical philosophy geometrically is illustrated well to his response to Desargues' concern that he had given up geometry in favor of other studies; Descartes replied in 1638 that "if [Desargues] cares to think about what I wrote about salt, snow, rainbows, etc., he will see that my entire physics is nothing but geometry" (Descartes, 1991, p. 119). For the original French text see (Descartes, 1964-1976, Vol. II p. 268).

to nature. We can therefore ask regarding each of these three thinkers: What challenges to the applicability of geometry in the study of nature arise within his thought and work? Moreover, Aristotle, Galileo and Leibniz all developed philosophical views according to which geometry plays various important roles in natural science. So we may pose an additional question concerning each of them: What positive solution did he (or could he) offer in order to meet the challenge to the applicability of geometry which arises for him? In the following three chapters I will give answers to these questions as they concern Aristotle, Galileo and Leibniz in turn.

In this first chapter I want to set aside consideration of any positive solutions to worries about the applicability of geometry and focus directly on the worries. My aim is to develop a series of challenges one might make to a natural scientific theory which applies geometry, later members of which tend to be more articulate and better developed than previous members. I do not claim that these challenges somehow all represent the very same underlying worry, although they do have a strong family resemblance. Thus it can sometimes be difficult to tell whether a given thinker is raising one or the other of the challenges, or even more than one at the same time. I will use the thought of Aristotle, Galileo, and Leibniz in posing these challenges, but that does not make any one of the challenges particularly Aristotle's or Galileo's or Leibniz's. Rather, I hope that by grounding the discussion in these and other historical figures I will be able to articulate challenges to geometry that are richer in virtue of their sensitivity to the history of philosophy and scientific practice.

I wish to be clear right from the beginning that my ultimate goal is not to call the applicability of geometry to the physical world into doubt. While I believe it is worthwhile for its own sake to elucidate the family of challenges I am about to discuss, I do not intend to use any one of them in an attack on geometry's applications. Rather, getting clear on these challenges to the applicability of geometry serves two methodological purposes. Firstly, taken as objections to any particular application of geometry, the challenges highlight certain theoretical and practical problems for the working scientist. Especially when I am discussing historical figures, much of my aim will be to see how those figures grapple with the problems and argue that they can be overcome. I believe this illuminates the issue of how scientists over time have become more able and sophisticated in their applications of geometry, and it thereby illuminates what such ability and sophistication amounts to. Secondly, because the challenges to the applicability of geometry I investigate concern a failure of correspondence between geometric objects and the physical world, they provide a conceptual link between the question of the applicability of geometry and the related but separate question of the relationship between geometric objects and physical reality. In the final chapter of this work I will use that link to argue that the applicability of geometry imposes a substantive constraint on the relationship between geometric objects and the physical world.

1.1 Sphaera planum in puncto non tangit

I begin with an as yet inarticulate worry that geometry is somehow inapplicable to the physical world. An early and highly influential source of this worry is to be found in Aristotle's *Metaphysics* B, where Aristotle writes:

> . . . [A]stronomy. . . cannot be dealing with perceptible magnitudes nor with

> this heaven above us. For neither are perceptible lines such as the geometer speaks of (for no perceptible thing is straight or curved in this way; for a hoop touches a straight edge not at a point, but as Protagoras said it did, in his refutation of the geometers), nor are the movements and complex orbits in the heavens like those of which astronomy treats, nor have geometric points the same nature as the actual stars.[3]

For the time being I would like to bracket the question of how to interpret the significance of this text for the understanding Aristotle's own views and focus instead on just what position is being attributed to Protagoras.[4] Protagoras is supposed to have said that a hoop does not touch a straightedge at a point, and the context suggests that what is at issue are perceptible straight lines and curved lines. The claim seems to imply that if we were to take a straight ruler and lay it against a circular sheet of paper, we would see that the ruler and the paper do not contact each other at just one point. This is deemed to be a refutation of geometry since the latter tells us that a circle and a line tangent to it intersect at exactly one point. It is noteworthy that no claim is made about what we *would* see if we were to put ruler and paper together, although from personal experience I think the likely alternative is that we see the ruler and paper touching each other along a very short line.[5] Other salient

[3]All citations of Aristotle refer to (Aristotle, 1984a, 1984b). I will follow the practice of giving the Bekker numbers in addition to the page numbers in the volumes just cited. The text just cited appears in the *Metaphysics* at 997b30-998a10 (Aristotle, 1984b, p. 1576).

[4]I concur with Lear (1982) that this stretch of *Metaphysics* B is part of a discussion of the difficulties facing Platonistic views and does not express Aristotle's own position. For Lear's treatment of this issue, see especially §2 of (Lear, 1982). Nonetheless, later Aristotelians I will discuss presently did use this passage from *Metaphysics* B as inspiration for their challenges to the applicability of geometry.

[5]Some translators have thought it so obvious that this was Protagoras's view that they put it in the translation. Kathleen Freeman translates as follows: "Protagoras, arguing against the definition of the mathematicians and appealing to perception, used to say that the tangent touched the circle not at a point but along a line" (Freeman, 1957, p. 126). My colleagues who read Greek assure me that there is no basis for adding the phrase "but along a line" to be found in Aristotle's text. It

possibilities would be that ruler and paper cannot be made to touch, or that ruler and paper touch over a so-called indivisible line, *viz.*, an extremely small line of non-zero length which nonetheless cannot be divided into smaller lines. I do not believe we have any textual evidence that will help us settle the question of which among these possibilities conforms to Protagoras's opinion.

Although I am inclined to think that Protagoras's opinion about the true contact of a hoop with the straightedge remains something of a mystery to us, Averroës shows no hesitation in making the following statement in his commentary to *Metaphysics* B:

> The geometer in fact demonstrates that the straight line touches a circle in a point: but a sensible line does not touch a sensible circle in any other way than in a line. And he similarly posits that a sphere touches a surface at a point: but a sensible sphere does not touch a sensible surface in any other way than in a surface. And therefore Protagoras refuted the geometers in the sophistical manner and said that their science was false...[6]

It is clear from this passage that Averroës takes Protagoras's opinion to be that the hoop and the straightedge touch in a line, though of course that does not settle the issue. Now, it is ultimately not so important for our purposes whether Protagoras himself held that the hoop and straight edge touch in a line or in some other way (just so long as their contact is not at a point). Much more important is what the tradition of commentary on Aristotle and Aristotelian philosophy made of the view

is also clear from Averroës' commentary on *Metaphysics* B that Averroës reads the objection to be that a circle and a line do not touch at a point but rather in a line. Again, this does not strictly follow from the text. We will turn to Averroës' commentary presently.

[6]See comment 8 of Averroës' commentary to *Metaphysics* B in (Aristotle, 1550 – 1552, Vol. 8 p. 22 *et verso*). The translation from the Latin is mine. Here is the original: "Geometer enim demonstrat quod linea recta tangit circulum in puncto: sed linea sensibilis non tangit circulum sensibilem, nisi in linea. Et similiter ponit quod sphaera tangit superficies in puncto: sed sphaera sensibilis non tangit superficiem sensibilem, nisi in superficie. Et ideo Protagoras sophista redarguebat Geometras, et dicebat quod scientia eorum est falsa..."

attributed to Protagoras. That a sensible circle touches a sensible line not at a point but along a line—or even more so that sensible sphere touches a sensible plane not at a point but over a surface—became stock examples trotted out in order to illustrate how the natural or sensible world deviates from what geometry asserts. One can see this already in the passage from Averroës just given, but one finds it also elsewhere in Averroës,[7] in Aquinas,[8] and in the writings of other Aristotelians. Indeed, for a number of later thinkers working within a broadly Aristotelian framework, the Protagorean claim that a sensible sphere contacts a sensible plane not at a point but in a surface approached the status of a commonplace.[9]

Just to deny that a sensible sphere touches a sensible plane at a point is not yet to raise much of an objection to the use of geometry in the study of nature. Since it is a theorem of (Euclidean) geometry that a plane tangent to a sphere intersects the sphere in exactly one point, it does seem to follow that on the Protagorean view, geometry misrepresents spheres and planes in nature in at least that one respect. Moreover, if we were to infer from the geometric theorem about spheres and planes that, for example, the contact of a plane with a spherical planet occurs at a point, we would be making a bad inference. Nonetheless, we might hold out hope that the apparent failure of geometry to capture the facts of nature might be isolated to a small

[7]Cf. for example Averroës' commentary on *De Caelo* (Aristotle, 1550 – 1552, Vol 10, p. 11 *verso*).

[8]Cf. Aquinas's *Division and Methods of the Sciences*, where Aquinas writes: "Thus, the judgment about a mathematical line is not always the same as that about a sensible line. For example, that a straight line touches a sphere at only one point is true of an abstract straight line but not of a straight line in matter..." (Aquinas, St. Thomas, 1986, p. 78).

[9]Two such thinkers I will discuss at greater length here are Alessandro Piccolomini and Benedict Pereira.

handful of propositions concerning circles, spheres, lines and planes. For safety's sake, these propositions could simply be avoided when reasoning about the natural world. If it can be made to stick, the more forceful objection would be the one according to which the theorems of geometry are generally false when they are understood as claims about the natural world. That would be harder for the natural scientist merely to work around. I suspect the more forceful objection is often implicit in the use of the contact of the sphere and the plane as a *stock* example; to bring up the example is already to suggest that many further propositions of geometry also deviate from what happens in nature or in sense perception. But it takes some further work to develop and articulate the more general, hence more forceful challenge we are aiming at.

Two thinkers who help to develop the challenge are Benedict Pereira (1535 - 1610) and, partly in response to Pereira, Galileo. Pereira was a Jesuit Aristotelian philosopher one generation older than Galileo who occupied several chairs at the Collegio Romano in Rome, including the chair in physics.[10] In his major philosophical work *De Communibus Omnium Rerum Naturalium Principiis*, Pereira attacks the applicability of geometry along Protagorean lines, with the difference being that Pereira is more general and more explicit in raising his objection:

> ...[T]he properties of quantity which are to be demonstrated by the mathematician do not match up with respect to substance, but *per se*, as being divisible, commensurable, proportional, equal, or unequal: similarly a triangle has three angles equal to two right angles, a sphere cannot touch a plane except at a point, and other things of this kind which without any substance considered *per se* are to be seen in quantity...[but in fact] no

[10] A.C. Crombie gives the official title of Pereira's chair as being "Physica (seu Philosophia Naturalis)". For more on Pereira, Galileo, and the major universities of Italy in their time, see (Crombie, 1977, pp. 63-64 *et passim*).

> triangle has three angles equal to two right angles, and no straight line touches a circle at a point, because they are in some substance...[11]

Pereira grants that the mathematician's theorems are correct in their way (namely, *per se*), but he is willing to deny those theorems quite generally as soon as they are understood to be claims about material bodies in nature.[12] Galileo gives us a less technical version of Pereira's position in his *Dialogue Concerning the Two Chief World Systems* by putting the following words into the mouth of Simplicio, the representative of the Aristotelians in the dialogue:

> ...[T]hese mathematical subtleties... are true in the abstract, but applied to sensible and physical matter they don't answer up. Because mathematicians may demonstrate well enough by their principles, for example, that *sphaera tangit planum in puncto*, a proposition similar to the present one. But when it comes to matter, things happen otherwise. What I mean about these angles of contact and ratios is that they all go by the board for material and sensible things.[13]

By now we have seen several informal or colloquial ways of saying what is wrong with geometry as applied in the study of nature: the natural world doesn't answer

[11]The translation is mine. Here is the original: "[A]ffectiones quae a Mathematico demonstrantur de quantitate, non ei conveniunt in ordine ad substantiam, sed per se, ut esse divisibilem, commensurabilem, proportionabilem, aequalem, vel inaequalem: similiter triangulum habere tres angulos aequales duobus rectis, sphaericum non posse tangere planum nisi in puncto, et alia eiusdem generis, quae sine ullius substantiae respectu per se spectantur in quantitate... [enim] neque triangulum habet tres angulos aequales duobus rectis, nec linea recta tangit circulum in puncto, quia insint in aliqua substantia... (Pereira, 1586, pp. 375-376).

[12]The passage from Pereira is made more difficult by the technical vocabulary he uses; we will consider the phrase "*per se*" at greater length in the discussion of Aristotle's *Posterior Analytics* in Chapter 2.

[13]See (Galileo, 1967, p. 203). I have deviated at various points from Drake's translation. Here is Galileo's original text: "[P]erché finalmente queste sottigliezze mattematiche, Sig. Salviati, son vere in astratto, ma applicate alla materia sensibile e fisica non rispondono: perché dimonsterranno ben i mattematici con i lor principii, per esempio, che *sphaera tangit planum in puncto*, proposizione simile alla presente; ma come si viene alla materia, le cose vanno per un altro verso: e così voglio dire di quest'angoli del contatto e di queste proporzioni, che tutte poi vanno a monte quando si viene alla cose materiali e sensibili" (Galileo, 1998a, p. 220).

up to or correspond to (*risponde*), or match up with or fit (*convenit*), the geometric theorems. The more formal way of analyzing this failure of correspondence depends on there being some way of interpreting geometric theorems as being about material bodies in the natural world. Let us suppose we can force the desired interpretation of a geometric theorem ϕ by writing "When it comes to physical and material things, ϕ." The failure of correspondence with respect to any given geometric proposition ϕ is simply the fact that, whereas ϕ is a result of geometry, when it comes to physical and material things, $\neg\phi$.

The completely general version of this objection against geometry in the study of nature would just be that for any theorem of geometry ϕ, when it comes to physical and material things, $\neg\phi$. The way in which Pereira and Galileo state the objection, it is unclear whether the objection is meant to be completely general. It seems likely that the objection supposes a failure of correspondence with respect to any proposition stating a precise ratio, including equality, between quantities; since many propositions of geometry do state precise ratios, that would already make the failure of correspondence quite significant. On the other hand, it seems far too extreme to suppose that all geometric theorems fail when interpreted as being about natural objects; consider theorems stating inequalities or stating suitably robust facts about incidence: *e.g.*, in a triangle, the angle opposite the larger side is larger;[14] if a line intersects one side of a triangle but does not intersect any of the three vertices of the triangle, then the line also intersects one of the other sides of the triangle.[15] Though

[14]This is Euclid, I.18 (Euclid, 1956, p. 283).

[15]This is the axiom of geometry named for Moritz Pasch.

it introduces an element of vagueness in the objection, let us use "Γ" for the set of geometric theorems with respect to which nature fails to correspond. To be faithful to the spirit of Pereira's objection, we may suppose Γ includes the various Protagorean propositions and propositions about precise ratios (including the claim that the angle-sum of a triangle equals two right angles). Otherwise it is left unspecified just which geometric theorems are in Γ. The challenge to geometry is then:

[**Protagorean Challenge**] For all $\phi \in \Gamma$, ϕ is a theorem of geometry, and when it comes to physical and material things, $\neg\phi$.[16]

What one would most like to know of course is just which theorems of geometry are in Γ, that is, informally, which ones "fail to correspond" in nature. In the next section, we will consider weighty reasons for thinking that Γ must really be empty, and that will lead us to our next challenge.

1.2 Geometric objects do not exist in nature

A typical feature of the thinkers who raise the Protagorean Challenge is that they want to grant the truth of geometric theorems and the validity of geometric demonstrations, at least as these are understood in geometry.[17] This is, in a way, a

[16]Here is a rough translation of the Protagorean Challenge into English for those readers who find the symbolism unhelpful: the challenge claims that each geometric theorem in a certain collection of such theorems asserts a falsehood when it comes to physical and material things. The advanced reader will see that the Protagorean Challenge was actually formulated without semantic terminology, but in any case the advanced reader has no use for the rough translation just provided.

[17]A possible exception here is Protagoras himself. Protagoras may simply have been trying to show how geometry can be reduced to contradictions. I call the challenge "Protagorean" rather than "Protagoras's" partly because Protagoras's own view of these issues remains obscure. At any rate, Protagoras's own view is arguably less important than the use to which his remark was put by later

virtue of the sort of position out of which the Protagorean Challenge is apt to be made: without proposing the outright rejection of geometry as false, it still attempts to call its applicability into question. However, one who raises the Protagorean Challenge while granting the validity of geometric demonstrations runs a very serious risk of contradicting himself. Part of Galileo's contribution to this debate was to expose that risk with great flair in the *Dialogue*.

Suppose someone claims that when it comes to natural and material things, it is not the case that a sphere touches a plane at a point. It follows from this claim that in nature there is a sphere and a plane which touch each other in some other way than a point. We can obtain a (putative) concrete example of this by considering the statement of another Aristotelian from renaissance Italy, Alessandro Piccolomini (1508 - 1579), who writes: "Even if celestial bodies are free of every imperfection and are perfectly round, nonetheless they cannot be touched in this way by a straight line without the contact comprehending some interval."[18] This suggests that if we want a concrete example, we may consider Mars: Aristotelians like Piccolomini should grant that Mars is perfectly spherical, and yet they should also grant that any plane we care to pick that is tangent to Mars shares not just a point with the surface of Mars but rather a whole surface.

Now let S be Mars and let P be a plane tangent to it (see Figure 1.1). Let p and p' be points in the intersection between S and P, and let o be the center of S. By a

philosophers such as Averroës and Pereira.

[18]The original text: "Siquidem, quamvis corpora caelestia ab omni labe immunia, sint perfecte rotunda ac tornata: non ob id tamen a recta linea, possent ita contingi, ut quadam intercapidine non contingantur" (Piccolomini, 1547, p. 20). See also Biringucci's translation at (Piccolomini, 1582, p. 38).

prior lemma we have that the line op and the line op' are orthogonal to P; the lemma is that a plane tangent to a sphere at a point is orthogonal to the line containing the center of the sphere and the given point. However, there is exactly one line l that is orthogonal to P and contains o. Therefore line op and line op' are in fact the same line l. Moreover, there can be at most one point in the intersection of line l and plane P orthogonal to l. Therefore $p = p'$, *i.e.*, S and P intersect at just one point. If we continue to insist that $p \neq p'$, we are contradicting ourselves.

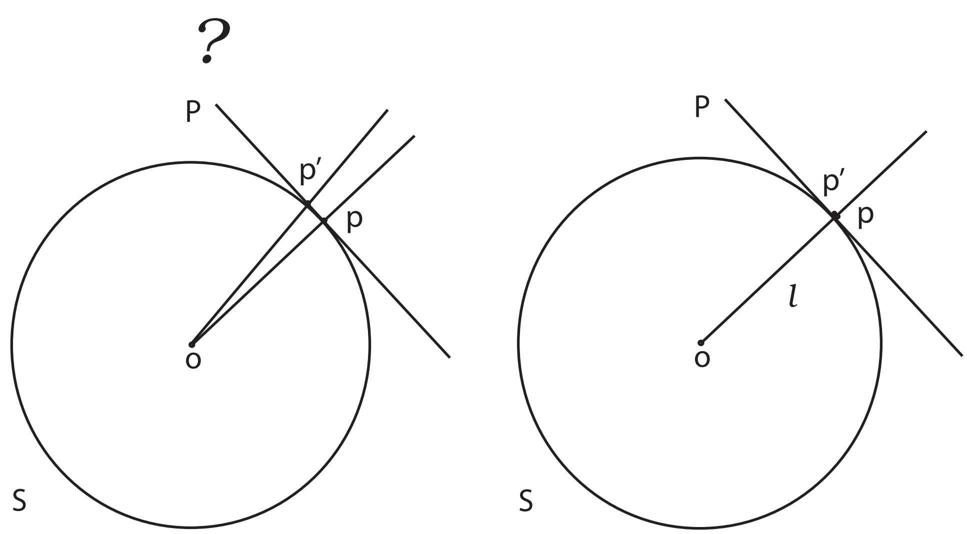

Figure 1.1: Cross Section of the Contact of Mars with a Plane

By now it should be evident that there is a significant problem with the Protagorean Challenge. Someone who raises the challenge grants that there are counterexamples to geometry in nature, *e.g.*, that material spheres such as planets contact planes at more than a point in nature. If the person raising the challenge also grants the validity of geometric demonstrations, we can use the demonstration on the putative counterexample, the material sphere, to show that the material sphere does not contact a plane at more than one point. But one cannot maintain that the material sphere both does and does not share exactly one point with a plane tangent to it. Similar reasoning should show that any sentence with respect to which nature putatively fails to correspond to geometry—any putative member of Γ above—is the source of a contradiction for anyone raising the Protagorean Challenge who does not wish to quarrel with the validity of geometric demonstrations.

How might someone who is inclined to press the Protagorean Challenge avoid being reduced to a contradiction in this way? One salient strategy would be to deny that the conditions for geometric demonstration are ever really met in nature, or, in other words, to deny that geometric objects exist in nature. That way an opponent can't produce a geometric demonstration and thereby obtain a *reductio ad absurdum*. Having argued against a fictionalized Aristotelian pressing the Protagorean Challenge in much the same manner we have just rehearsed, Galileo offers the exit strategy we are now considering:

> [W]hen you want to show me that a material sphere does not touch a material plane in one point, you make use of a sphere that is not a sphere and of a plane that is no plane. By your own statement, spheres and planes are either not to be found in this world, or if found they are spoiled upon being used for this effect. It would therefore have been less bad . . . for you to have said that if there were given a material sphere and a plane which were perfect and remained so, they would touch one another in a single point, but then to have denied that such were to be had (Galileo, 1967, pp. 206-207).

In the example we had been considering a couple of paragraphs ago, Galileo's advice to someone pressing the Protagorean Challenge would be to give up the claim that Mars is really a perfect sphere. To our sensibilities, this seems like a very reasonable thing to do, though a committed Aristotelian might find that concession somewhat harder to swallow.[19]

On my reading of Galileo's text, someone who takes Galileo's advice would give up pressing the Protagorean Challenge and instead move to a different challenge to geometry, namely:

[**No-Shapes Challenge**] There are no geometric objects in nature. That is, there are in nature no points, lines, or surfaces which satisfy the axioms of geometry.

Although Galileo is willing to entertain and respond to the No-Shapes Challenge, he ultimately does not regard the No-Shapes Challenge as a warranted assertion. We will consider Galileo's response in Chapter 3. Leibniz, on the other hand, does take the claim of the No-Shapes Challenge to be warranted. Here is a pair of characteristic statements:

> There is no determinate shape in actual things, for none can be appropriate for an infinite number of impressions. And so neither a circle, nor an ellipse, nor any other line we can define exists except in the intellect. . . [20]
>
> It is true that perfectly uniform change, such as is required by the idea of movement which mathematics gives us, is never found in nature any more than are actual figures which possess in full rigor the nature which geometry teaches us. . . [21]

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[19] Aristotle had argued in *De Caelo* that the celestial bodies are spherical. Cf. *De Caelo*, p. 290a30 - 290b11 (Aristotle, 1984a, pp. 478-479).

[20] Cf. Leibniz's "Primary Truths" (Leibniz, 1989, p. 34). For the Latin original, see (Leibniz, 1923ff, VI.4, p. 1648).

[21] See (Leibniz, 1969, p. 583). The text is entitled "Reply to the Thoughts on the System of Preestablished Harmony Contained in the Second Edition of Mr. Bayle's Critical Dictionary, Article Rorarius". I have deviated from Loemker's translation. Here is the original: "Et quoyque dans la nature il ne se trouve jamais des changement parfaitement uniformes, tels que demande l'idée [sic] que les Mathematiques nous donnent du mouvement, non plus que des figures actuelles à la rigueur de la nature de celles que la Geometrie nous enseigne. . . " (Leibniz, 1875-1890, Vol. 4, p. 568).

Leibniz pushes the No-Shapes Challenge to geometry further than Galileo by developing evidence that the fundamental physics of our universe is incompatible with the existence in nature of geometric shapes or other precise geometric objects. However, like Galileo, part of Leibniz's contribution to science consists in the identification of the shapes that various natural objects and processes have; notably, Leibniz argues in "An Essay on the Causes of Celestial Motions" that the orbit of Mars is an ellipse (Leibniz, 1993). This forces Leibniz to attempt to overcome the No-Shapes Challenge, and he does so in a way that is importantly different from Galileo. It is the fact that both Galileo and Leibniz recognize and make an attempt to overcome the No-Shapes Challenge that makes it natural to discuss them both in the present work despite the fact that on the topic of the applicability of geometry, Leibniz does not engage directly with Galileo's views. We will return to Leibniz both in the next section of this chapter and at length in Chapter 4. For the remainder of this section we will attempt to gain a finer understanding of the Protagorean and No-Shapes Challenges.

The No-Shapes Challenge to geometry in the study of nature is a close cousin to the Protagorean Challenge. Perhaps both challenges can lay equal claim to being an analysis of what it could mean for nature to fail to correspond to (or fail to agree with, *etc.*) geometry. The Protagorean Challenge focuses on the putative failure of the truth values of sentences of geometry to match the truth values of those same sentences interpreted so as to be about nature: "A plane tangent to a sphere intersects the sphere at a point" is claimed to be true as interpreted in geometry, but false as interpreted to be about natural, material objects. The No-Shapes Challenge focuses on a putative nonexistence in nature of the geometer's objects, so that if we grant that geometry does give an account of some realm of objects, then this realm is not the natural realm, nor is it a part of the natural realm. The *prima facie* threat to the applicability of geometry raised by the two challenges is somewhat different, however. As we saw, the Protagorean Challenge threatens to unmask geometry as a source of error in reasoning about nature. The No-Shapes Challenge, on the other hand, threatens to reveal geometry's gross irrelevance in any reasoning about nature. The worry raised by the No-Shapes Challenge is that geometry would be like any other account of a kind of object which turns out not to exist—say, phlogiston—which is rendered useless in the study of nature because one never finds any such thing. Moreover, the non-existence of geometric objects in nature looks to have very wide significance for the proponents of the mechanical philosophy, since they aim to give explanations in terms of the size, shape, position, and motion of bodies. Geometry was to provide the means for saying what the sizes, shapes, positions, and motions of bodies are. The No-Shapes Challenge appears to undermine the suitability of geometry to serve that purpose.

It illuminates both the Protagorean Challenge and the No-Shapes Challenge to ask the question whether it is possible to raise both challenges at the same time. At first blush it would seem that it is not possible, since it appears that in denying various theorems of geometry as interpreted to be about physical, material things, one has at least to grant that there are geometric objects in nature—they just happen to be such as to violate the laws of geometry. In our chief example, in order to deny that in nature all planes touch spheres tangent to them at a point, we had to grant that in nature there are spheres which violate the geometric theorem, and this is incompatible with the No-Shapes Challenge. However, it is possible to take the view that "when it comes to physical and material things, $\neg\phi$" means something rather different from the simple denial of "ϕ", at least when ϕ is a claim of geometry. In other words, one who wanted to raise the Protagorean and No-Shapes Challenges simultaneously might stave off a contradiction by claiming that there is some kind of equivocation happening with the sentences we are substituting for "ϕ".

For example, one might claim that in geometry "sphere" picks out objects, points on the surface of which are all equidistant from some given point; whereas interpreted so as to be about physical, material things, "sphere" picks out objects whose surfaces are more or less equidistant from a given location.[22] Call such objects "material spheres", and suppose that there are similar physical definitions for "point", "plane", *etc.* A proponent of the No-Shapes Challenge might further believe that as a matter of physical fact, material spheres are never actually spheres in the geometric sense. "When it comes to physical and material things, it's not the case that a plane touches a sphere tangent to it at a point" is reckoned to be true because some material spheres have small flat regions which touch material planes over a surface. On the other hand, the geometric theorem also comes out true, since it does assert a truth about spheres in the geometric sense. On this proposal, the phrase "when it comes to physical and material things" both restricts the interpretation to the physical world and changes the standards for what is to count as a sphere.

I suspect that when some philosophers have prefaced sentences of geometry with

[22] I am intentionally leaving it a vague question, just how much difference in the radii of the body is compatible with its being a material sphere. Nothing hinges on this; we could stipulate that a given body is to count as a material sphere if none of its radii is more than 2% greater than any other radius. I am also using "location" to pick out some suitable physical analogue of a point.

such phrases as "when it comes to physical and material things" or "in nature", they have in fact meant to change the standards for what is to count as the type of object described in the sentence.[23] In such circumstances, the meaning of geometric sentence has been altered; "material sphere" as just defined does not mean the same as "sphere". What looks like the denial of a geometric claim—some spheres touch planes tangent to them over a surface—really isn't. Recognition of this fact gives a proponent of the Protagorean Challenge a reason to drop it in favor of the No-Shapes Challenge.

There is another historically important understanding of what phrases such as "when it comes to physical and material things" or "in nature" accomplish when used as a preface to a geometric sentence. It derives from Aristotle and comes into play in discussions of geometry in the Aristotelian tradition.[24] To my own ear, this understanding allows us to come as close as possible to maintaining the Protagorean and No-Shapes Challenges simultaneously without contradiction. By way of explanation I will introduce a fictional example:[25]

Suppose there were discovered a species of pearl-bearing oyster which, unlike the species of pearl-bearing oyster we know, almost always produce nearly perfect spherical pearls. The reason for this is that these oysters only allow nearly spherical irritants to form the "seeds" of their pearls, and whenever the pearl deviates from being per-

[23]For example, I think Galileo uses the phrase with this meaning in some contexts. See Chapter 3.

[24]For Aristotle, see the discussion of the shape of the Earth in *De Caelo*, pp. 297a8-298a22 (Aristotle, 1984a, pp. 488-489). For an example from the Aristotelian tradition, see (Biancani, 1996, pp. 179-184).

[25]This example is meant to illustrate a way of having a shape that parallels, but is somewhat simpler than, Aristotle's understanding of what it is for the Earth to have the shape that it has.

fectly spherical at some stage in its development, there is a natural mechanism in the oyster which corrects the deviation. It is possible to disturb the pearl forming mechanism artificially, perhaps by depriving the oyster of calcium at certain times. But as long as the mechanism is functioning normally and without external constraints, the pearls produced look perfectly spherical to the naked eye.

Someone with Aristotelian inclinations might insist that these oysters' pearls are spheres, where by "sphere" she means exactly what is meant in geometry. However, she may well go on to say that for a physical body to be a sphere is for it to be the product of a natural mechanism which, when operating free from all confounding factors, produces spheres. Because there always are some small confounding factors or perturbations which affect the production of the pearls, there might never be any instant at which all the points on the surface of any given pearl are equidistant from some center. But at every stage of its development, any given pearl tends towards having a surface with that character. Attributing to the pearl a spherical shape registers this fact and helps to explain how the pearl develops over time.

On the suggestion we are now considering, prefixing a geometric sentence with the phrase "when it comes to physical and material things" still changes the meaning of the geometric sentence. However, the claim is that it does *not* change the meanings of the geometric vocabulary (such as "point", "sphere", *etc.*). Rather, the use of that phrase fixes on a certain non-geometric understanding of what it is for a body to have a given shape: for a body b to have a shape s in nature is for b to be the product of a process which in certain idealized circumstances would produce bodies with shape s.[26] This stands in contrast with the geometric understanding of what it is for something to have a shape; to have a shape in geometry requires the surface of that object to have a certain precise character either at a given time or, in the case of abstract objects, timelessly.

For someone who accepts both the natural and the geometric sense of what it is to have a shape, the challenges to geometry tend to arise from conflicts between these two senses. According to what it is to have a shape in nature, a given pearl from our special oysters is a sphere; whereas according to what it is to have a shape in geometry, that same pearl may not have a shape such as to intersect a plane at a point. This forms the grounds for saying that in nature, some spheres touch planes tangent to them in a surface (and not a point). Similarly, if one restricts attention to shape possession in the natural sense, it will follow that the geometer's shapes do exist in nature, since many processes are such as to produce objects with geometric shapes. At the same time, it may hold that in the geometric sense no physical or material object has any of the shapes studied in geometry.

Views of the sort we have just considered which distinguish what it is to have a shape in geometry from what it is to have a shape in nature do have their virtues. In particular, the notion of having a shape in nature seems well adapted for the explanation of the development of various natural processes and objects. On the other hand, the two notions induce two different sets of criteria for the attribution

[26]There is an appearance of circularity here, since the explanation of what it is to have a shape in nature invokes some prior understanding of what it is to have a shape *tout court*. I suspect that the notion of having a shape in nature is in fact derivative from a more basic understanding of what it is to have a shape. That more basic understanding could just be the geometric one, in which case the notion of having a shape in nature requires the geometric notion. But what it is to have a shape *tout court* could also be taken as primitive.

of shapes which are not in harmony with each other in that they assign different shapes to the same object. As we have just seen, this can lead to paradoxes and errors in reasoning, as when one attributes a shape to a body in the natural sense and then proceeds to reason about it as if it had that shape in the geometric sense. Though historically important, I do not believe that such views present us with the most effective framework for challenging the application of geometry in the study of nature.

1.3 Nature lacks geometric structure

In the last section we considered several reasons to give up the Protagorean Challenge to geometry in favor of the No-Shapes Challenge. While the No-Shapes Challenge posed a historically important objection to the applications of geometry in the context of 17^{th} century science, by the end of that period and in fact already in Leibniz one can see that the No-Shapes Challenge is in certain respects too narrow. In the present section I will consider how the No-Shapes Challenge may be generalized. This will lead to a further challenge which, in addition to being more general, is the sort of challenge even a 21^{st} century critic of the applicability of geometry might raise.

Recall that someone pressing the No-Shapes Challenge insists that there are in nature no geometric objects: no geometric points, lines, or surfaces. For someone who holds this view while also holding that geometry does describe some subject matter, it is natural to conceive of geometric objects as abstract in a contemporary sense, *viz.*, as not being located in spacetime or standing in causal relations with anything in spacetime. But if geometric objects are understood from the beginning to be abstract, it conveys no information about the natural world whatsoever to say that there are no geometric points, lines, or surfaces in nature. It does not tell us about what the surfaces of bodies are like, how they move, *etc.*, and so it is compatible with the trajectories of cannon balls being perfectly parabolic or Mars being perfectly spherical. On this conception of geometric objects as abstract, to deny that geometric objects are in nature is just to make a conceptual remark about the kind of objects geometry describes.

The upshot of the last paragraph is that the No-Shapes Challenge to geometry loses its force if one has a conception of geometric objects as abstract, and it would be better to formulate a challenge which holds its force independently of this aspect of one's conception of geometric objects. One can make some progress on this front by thinking about how someone with a conception of geometric objects as abstract would reformulate the No-Shapes Challenge. The guiding thought would not be that nothing in nature *is* a geometric object, but rather that nothing in nature *precisely resembles* geometric objects in the relevant respects. For example, suppose one wanted to claim that no body in nature precisely resembles a sphere. If one conceives of spheres as abstract, say, as collections of ordered triples of real numbers satisfying $x^2 + y^2 + z^2 = r^2$ for some radius r, then one might formulate the claim as follows: there is no natural coördinatization of physical space such that the set of coördinates corresponding to locations on the surface of any body is a sphere.[27] This example suggests that the resemblance to geometric objects may be made sense of in terms

[27]For this formulation to work there must be some constraints on how one can assign coördinates, and that is the point of the word "natural" in the sentence. Not every assignment of triples of real numbers to locations in space counts as a natural coördinatization.

of mappings (in this case coördinatization) between items in nature and geometric objects. I will pursue this suggestion in a moment.

There is another way in which the No-Shapes Challenge is not as general or explicit as one might hope. The proponent of the No-Shapes Challenge assumes some tacit understanding of what it would be for there to be geometric objects in nature. But the existence of geometric objects in nature might be realized in a number of different ways—or, thinking along the lines of the last paragraph, there might be a number of different respects in which some items in nature precisely resemble geometric objects. Historical proponents of the No-Shapes Challenge would at the very least intend to deny that the surfaces of any bodies precisely resemble any geometric object in respect of shape. They would probably also deny that the motion of any body in nature precisely resembles any geometric curve in respect of shape. But there are other salient ways in which items in nature might precisely resemble geometric objects: the totality of locations in physical space itself could have the same structure as (Euclidean) space; or all the moments of time could have the same structure as the points on a line. The more general challenge to the applicability of geometry would be one which rules out such resemblances.

Leibniz is a good example of a thinker who wants to rule out all of the possible resemblances between geometric objects and natural objects we have lately considered. In a letter to the Electress Sophie, Leibniz writes:

> The fact is that matter, the evolution of things, and finally every genuine composite, is a discrete quantity, but that space, time, mathematical motion, intension or the continual increase that is conceived in speed or other qualities. . . is a continuous and undetermined quantity in itself, or one indifferent to the parts that can be taken from it and which are actually taken in nature. The mass of bodies is actually divided in a determi-

> nate manner and there is nothing exactly continuous in it; but space or the perfect continuity which is in the idea marks only an undetermined possibility of dividing it as one will.[28]

In this passage, Leibniz is contrasting space with matter (or the mass of bodies), time with the evolution of things, and mathematical motion with the motion of bodies. All of the former items in these pairs are to be construed as mathematical entities, and all of the latter items as physical ones. On Leibniz's view, these mathematical entities all possess a characteristic kind of continuity which the physical entities lack. This means that the mathematical entities are dissimilar to the physical ones at a fundamental level of structure. For instance, if we pick two points in mathematical space, there is always a point halfway between them. On the other hand, if we pick two physical points in matter, on Leibniz's view there may or may not be any physical point which is halfway between them. If there is a body, the border of which is exactly halfway between the physical points, then the physical point exists. Otherwise the physical point does not exist. This implies that the structure of the mathematical points is different from that of the physical ones. More generally, the assertion that physical entities fail to be continuous in the way that geometric entities are continuous implies that a geometric line, surface, or space is structurally dissimilar to any physical entity or collection of physical entities.[29]

To sum up this section so far, there are two significant shortcomings of the No-Shapes Challenge to geometry: a. it loses its intended force when joined with a

[28] I am using an unpublished translation of the October 31, 1705 letter to the Electress Sophie prepared by Donald Rutherford. I wish to thank Rutherford for permission to use his translation in this work. For the original text see (Leibniz, 1875-1890, Vol. 7 p. 562).

[29] For a much more detailed discussion of this issue, see §4.2.

conception of geometric objects as abstract in a contemporary sense; b. it does not appear to recognize that there are different ways in which objects in nature might precisely resemble geometric objects in certain respects. It would seem that of the two shortcomings, (b.) is the fundamental problem. For if we had a flexible enough notion of resemblance, we could reformulate the challenge to geometry as there not being anything in nature which resembles geometric objects in the relevant respects. Then, even if geometric objects are thought to be abstract, the denial that anything in nature appropriately resembles geometric objects will have the force which the No-Shapes Challenge intends.

One such flexible notion of *precise-resemblance-in-a-respect* is the contemporary notion of sameness of structure, where sameness of structure is witnessed by isomorphic mappings. Recall that S_1 and S_2 are isomorphic just in case there is a one-to-one mapping $f : S_1 \mapsto S_2$ which preserves some privileged collection of operations and relations defined on the elements of S_1 and S_2. These operations and relations give the respects in which S_1 and S_2 have the same structure, and the one-to-one mapping ensures sameness with respect to cardinality. Because there are several different formal ways of characterizing Euclidean space, there are different relations we might demand to be preserved by an isomorphism. For concreteness, we will follow Tarski's method of describing Euclidean space, so that the relations to be preserved are: (1.) point b is between points a and c; (2.) points a and b are exactly as far apart from each other as points c and d. The fact that these geometric relations can be systematically correlated with some other relations makes it possible for the structure of Euclidean space or a Euclidean curve to be realized in multiple ways. For example, if we say that time has the structure of a Euclidean line, then the correlate of spatial betweenness is temporal betweenness, one moment in time occurring after a second moment but before a third. If f is to be an isomorphism between the line and time, we must have that whenever a point b is between a and c on a line, $f(b)$, a moment of time, comes temporally between $f(a)$ and $f(c)$.

I therefore formulate an additional challenge to the applicability of geometry as follows:

[**No-Structure Challenge**] Nothing in nature is isomorphic either to Euclidean space, or to any Euclidean curve, or to any Euclidean surface.

A natural motto for the No-Structure Challenge would be "Nature lacks (Euclidean) geometric structure."

If the No-Structure Challenge can be sustained, it calls into question the use of Euclidean geometry to describe and reason about natural objects and processes. If nothing in nature is isomorphic to Euclidean space, then there are no natural objects and relations which the Euclidean axioms can be interpreted as describing truthfully. At least *prima facie*, we have a problem similar to the one presented by the No-Shapes Challenge: geometry is telling us about a kind of object, the structure of which is absent in nature. So the theory of that structure, Euclidean geometry, looks as if it will turn out to be empty in the context of the natural world. Similarly, we might have held out hope that even if there is nothing isomorphic to Euclidean space in nature, at least at a local level there may be something isomorphic to a Euclidean curve or surface. That would underwrite the use of the Euclidean theory of that curve to describe and reason about that natural object (or those natural objects). But the

No-Structure Challenge denies this as well.

Beyond isomorphism, there are other mappings such as homomorphisms which preserve structure though to a lesser extent. Homomorphisms differ from isomorphisms in that they may fail to be one-to-one correspondences, so that the sets between which there is a homomorphism may differ with respect to cardinality. We could also continue to speak about geometric isomorphisms or homomorphisms but only require that *some* of the geometric relations be preserved by the mapping, *e.g.*, the relation of one point being between two other points. A mapping between some collection of physical entities and Euclidean space which preserved only the betweenness relation of the latter would preserve the affine structure of the space but not the metric structure. These other mappings could form the basis for other variants of the No-Structure Challenge, such as that nothing in nature is homomorphic to Euclidean space or to a Euclidean curve. If these variants could be sustained, they would show a greater and greater structural discrepancy between nature and Euclidean space.

Similarly, it would be easy to formulate variants of the No-Structure Challenge aimed at other geometric theories. If one wanted to call into question the applicability of the theory of 4-dimensional Minkowski space to spacetime physics, at least *prima facie* one way to do so would be to say that nothing in physical spacetime is isomorphic to 4-dimensional Minkowski space.

1.4 A Final Challenge

To overcome the challenges to the applicability of geometry which they recognize, both Galileo and Leibniz advocate some important role for approximations. We will consider in what way Galileo and Leibniz advocate approximations in Chapters 3 and 4. For Leibniz, whenever we attribute a geometric shape to a body or a geometric curve to the motion of a body, we must be engaging in some kind of approximation, since as we have seen Leibniz does not think the geometric shape or curve could correspond exactly to the body or its motion. Galileo, on the other hand, is unwilling to assert that there is never a body which corresponds exactly in shape to any geometric surface. But he is willing to grant that for scientific purposes, we may well need to treat a body as though its shape corresponds perfectly to a given geometric shape even when we know it does not. The difficult task is to sort out just when one is justified in making such an idealizing assumption. The fact that both Galileo and Leibniz advocate some use of approximations in order to apply geometry suggests a final strategy for challenging the use of geometry in the study of nature. The guiding thought behind the strategy is to consider what the prerequisites are for the legitimacy of approximating some aspect of nature by a geometric object, and then to deny that the prerequisites are met.

Let us begin with the easier case of approximation of one geometric curve by another and then turn to the thornier issue of a geometric curve approximating some aspect of nature. In geometry, we routinely use one curve as an approximation to another, as for instance when we use the tangent to a curve or an osculating circle as an approximation of the curve itself. Sometimes two curves of independent interest agree to high approximation over some interval where they are defined, as is the case with $\sin x$ and $\tan x$ for small values of x (see Figure 1.2). Slide rule makers generally take advantage of this fact and put only a single scale giving values for both $\sin x$ and

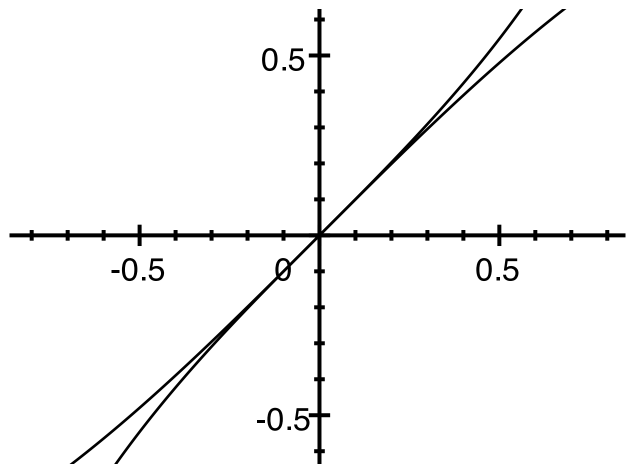

Figure 1.2: Graph of $\sin x$ vs. $\tan x$ for small x

$\tan x$ for small x. In effect, this allows one to calculate the value for just one of the two trigonometric functions and then, if x is small enough, use the same value for the other.

A key feature of these ordinary cases of mathematical approximation is that there is some fixed or determinate discrepancy between the two curves. If the discrepancy is small enough for certain purposes, we can use the one curve as an approximation to the other. On the other hand, if there is no determinate discrepancy, or if for some reason we are not in a position to assess the discrepancy, we are not ordinarily justified in making the approximation.

Now consider a case in which we want to approximate the shape of a body with a geometric object. For the sake of concreteness, let us suppose we want to approximate the shape of the Earth by a sphere, knowing full well that the shape of the Earth does not correspond exactly to a sphere. This at least appears to presuppose two things: first, that the Earth does possess some shape or other; second, that (as in the mathematical cases above) there is some fact of the matter as to the quantity or kind of discrepancy between the Earth's shape and the sphere. One might also add a third prerequisite: that one must be able to assess, *viz.*, estimate or calculate, the quantity or kind of the discrepancy in order to determine whether the existence of such a discrepancy is compatible with the original scientific purpose. In what follows I will tend to assume that if the first two prerequisites can be met, then the third can as well.[30] The guiding thought in formulating the last challenge will be that these first two prerequisites cannot be met, or rather that generalized versions of them cannot.

With the preceding as background I am in a position to state the last challenge to the applicability of geometry I will discuss in this thesis, namely:

[**No-Discrepancies Challenge**] Given any natural item N and any geometric item G, there is no determinate or well-defined discrepancy between N and G.

An intended consequence of the No-Discrepancies Challenge is that geometric objects do not even approximate things in nature. This already implies the truth of the No-Structure Challenge, but it implies a good deal more. An apt motto for the No-Discrepancies Challenge would be: "Nature is blurry."[31]

We can think about the possible discrepancies between the natural items and the geometric objects in several different ways, partly depending on what we take nature

[30]My main reason for ignoring the third prerequisite is that it involves the way in which the geometric and natural objects are given to us, as well as our powers of reasoning with respect to them. It is therefore subjective in a way that the three prior challenges to geometry were not. Those challenges concern the putative failure of nature to match up with or correspond to geometry independently of our capacities to assess such correspondences. However, there is no reason one couldn't press the issue and argue that the third prerequisite can't be met even if the first two can. To the best of my awareness neither Aristotle, nor Galileo, nor Leibniz presses this issue.

[31]This motto is inspired by William Tait's recent discussions of geometry. See especially §5.1.

to be like. In Leibniz's case, the aim of an approximation appears to consist in forcing the quantitative difference between the aspect of nature and the approximating geometric object to be so small as to be entirely negligible. Here we can take "discrepancy" in a quantitative sense similar to that of "distance". On the other hand, Galileo's concern in approximating the shape of the Earth with a sphere is whether the shape of the Earth differs from the sphere in terms of tangency properties. Galileo takes himself to be justified in approximating the shape of the Earth by a sphere if and only the shape of the Earth shares the property with the sphere of touching a plane tangent to it at exactly one point. The shape of the Earth might in fact differ by a very large amount in the sense of "distance" while resembling the sphere in kind as far as the tangency facts go. Therefore one must keep in mind that "discrepancy" might mean different things in different contexts.

One important notion of discrepancy makes use of the idea of structure preserving mappings from the last section. For one might hold that there are facts of the matter concerning the question, "By how much does the structure of nature differ from the structure of some given geometric objects?" For instance, even if there is not an isomorphism between some locations or other point-like elements on the surface of a body and a given Euclidean curve, one might still think there is a fact of the matter as to how far away the shape of the body is from being isomorphically mappable into the given curve. One could also think there is some fact of the matter as to how far away the representation of physical space is from being isomorphically mappable into Euclidean space or some other geometric space.

I will return to the No-Discrepancies Challenge in Chapter 5, where I will examine

William Tait's recent attempt to account for the applicability of geometry even while granting the challenge. I will argue *contra* Tait that the No-Discrepancies Challenge is incompatible with the deductive applicability of geometry. Therefore the fact of the applicability of a given geometric theory imposes a substantial constraint on nature: there must be determinate discrepancies between the geometric structures characterized by the theory and the aspect of nature to which the theory is applied.

1.5 Outlook

Looking forward to the historical content of the next three chapters, a common theme will be the extent to which Aristotle, Galileo and Leibniz are realists about geometry in the sense that they believe nature either to have significant geometric structure or to be approximated by geometric structures. Having formulated the No-Discrepancies Challenge, I can now state one sense in which I will argue all are realists about geometry: Aristotle, Galileo and Leibniz are all committed to the denial of the No-Discrepancies Challenge. In particular, all three believe that there are entities, taken in a broad sense of the term, corresponding to the shapes of bodies such that there are facts of the matter about the quality or kind of discrepancy between these entities and given geometric curves.

For Aristotle, the natural world is replete with geometric structure: the universe itself is a sphere, as are all the planets; the planets move in circular orbits; a stone dropped above the surface of the Earth falls in a straight line connecting its position with the Earth's center.[32] Galileo, too, is willing to countenance geometric structure

[32]For Aristotle's arguments for these claims, see *De Caelo*, *passim* (Aristotle, 1984a, pp. 447-512).

in nature, as when he endeavors to show that accelerations are continuous in the way the line is continuous;[33] or as when he suggests in a general way that if we think there are no geometrical objects in nature we should broaden our notion of what counts as a geometric object.

Of the three main historical figures I will discuss, Leibniz is the least realist about geometric structure in nature. Nonetheless, Leibniz is willing to countenance that there are items in nature, the structure of which imitates geometric structure to arbitrarily small margins of error. Leibniz's intent seems to be to come as close as one can to conceding that nature has geometric structure while still in the end denying it.

[33]For evidence of this claim, see §2 of (Palmerino, 2001, pp. 385-405).

Chapter 2

Aristotle

Natural philosophers in the 17^{th} century commonly criticized Aristotle and his followers for shunning mathematical methods in their scientific research. One of the most vocal critics in this regard was Galileo; Galileo took every opportunity to emphasize the importance of mathematics in science and to deride Aristotelians whose work was not properly informed by mathematics. In a famous passage from *The Assayer*, Galileo makes a direct attack on an Aristotelian—the Jesuit Father Horatio Grassi—whose work Galileo did not find properly mathematical. This becomes clear when one cites the passage at slightly fuller length than is often done:

> In [Grassi] I seem to discern the firm belief that in philosophizing one must support oneself upon the opinion of some celebrated author... Possibly he thinks that philosophy is a book of fiction... Well, [Grassi], this is not how matters stand. Philosophy is written in this grand book, the universe, which stands continually open to our gaze. But the book cannot be understood unless one first learns to comprehend the language and read the letters in which it is composed. It is written in the language of mathematics, and its characters are triangles, circles, and other geometrical figures without which it is humanly impossible to understand a single

word of it...[1]

One can imagine how these words must have insulted Father Grassi when he read them. Galileo strongly suggests that Grassi does not know mathematics and that consequently he is unfit to do natural philosophy.[2]

At times Galileo makes a deeper criticism of the Aristotelians than that they pursue natural philosophy badly in virtue of their ignorance of mathematics: he suggests that they deliberately (and stupidly) ignore mathematics because they believe there to be severe difficulties in principle involved in the application of mathematics to anything of a sensible, material nature. In Galileo's *Dialogue Concerning the Two Chief World Systems*, Galileo has Simplicio, the interlocutor representing the Aristotelian school of natural philosophy, argue that the theorems of geometry simply fail to hold when we consider things that have a material nature.[3] Specifically, Simplicio maintains that when we consider material bodies in space, it is false that spheres meet planes tangent to them in a single point.[4] By putting such statements in Simplicio's mouth, Galileo ties a certain skepticism towards the applicability of mathematics to a long tradition of Aristotelian thought. As we saw in Chapter 1, this tradition extends

[1]See (Galileo, 1957, pp. 237-238). Galileo is using the word "philosophy" here in the sense of "natural philosophy".

[2]As I mentioned, Galileo was not the only natural philosopher who objected to the Aristotelians' eschewal of mathematics. Descartes, for example, agreed with Galileo on this point. In a letter to Mersenne about Galileo's work, Descartes writes that "I find [Galileo] philosophizes much more ably than is usual, in that, so far as he can, he abandons the errors of the Schools and tries to use mathematical methods in the investigation of physical questions". Cf. (Descartes, 1991, p. 124).

[3]I discuss Simplicio's statement of this challenge to the applicability of geometry in §1.1; it is one of the clearer statements of what I there call the "Protagorean Challenge". I will discuss Galileo's reaction to the challenge at length in Chapter 3.

[4]See (Galileo, 1967, p. 203). The discussion between Simplicio and Salviati on this matter continues until page 210.

back through Benedict Pereira and Alessandro Piccolomini to Averroës and (in a way) all the way back to Aristotle.[5] Galileo's text thereby raises a difficult historical question which I hope to address in this chapter: Did Aristotle himself hold there to be severe difficulties in principle standing in the way of applying mathematics either to physics or to empirical inquiries generally?

The question I have just posed must be carefully distinguished from another historical question which will not be my concern in this chapter: Did Aristotelian philosophers (other than Aristotle) use Aristotle's doctrines in order to raise severe difficulties in principle standing in the way of applying mathematics to physics or to empirical inquiries? I believe the answer to this latter questions is unequivocally "yes".[6] Moreover, it is a worthwhile historical project for scholars of the 16^{th} and 17^{th} centuries to investigate the ways Aristotle's thought was sometimes—though not always—used to oppose mathematics during the period.[7] This historical project remains valuable in virtue of the light it sheds on the development of early modern science, whatever Aristotle's views may have been.

Turning now to Aristotle's own views of the applicability of mathematics, one finds recent interpreters divided on the issue. Some recent authors, especially those whose work centers on 17^{th} century developments in science, interpret Aristotle to have held

[5]See especially §§1.1–1.2. Even though Aristotle is a textual source of the worry that a hoop does not touch a straight edge at a point, that does not mean Aristotle himself endorsed the worry. The arguments in this chapter support the view that in fact he did not.

[6]The discussion of Pereira and Piccolomini in Chapter 3 provides an example.

[7]A considerable amount of work has been done on figures such as Piccolomini and their involvement in the so-called debate "*de certitudine mathematicarum*". See Chapter 3 for some discussion and for pointers to the historical literature. Further work will surely be required in order to illuminate the relationship between mathematics and the many Aristotelianisms of the Renaissance and the early modern period.

that mathematics is indeed inapplicable either to physics or to empirical inquiries quite generally. To cite just one example, Stephen Gaukroger gives an extended treatment of Aristotle's philosophy of science in his book *Explanatory Structures*, and one of the main conclusions of his discussion is that for Aristotle, "mathematical proofs are simply inappropriate in physics" (Gaukroger, 1978, p. 202). Similar statements about Aristotle are scattered throughout the literature on the 17^{th} century.[8]

Partly in response to the authors just mentioned, other recent interpreters of Aristotle have argued that, far from rejecting the use of mathematics in physics or in other empirical inquiries, Aristotle offers a general theoretical account of how a branch of mathematics is properly related to an empirical inquiry when the former is applied in the latter. Two particularly helpful papers offering arguments in support of this interpretation are James Lennox's "Aristotle, Galileo, and the 'Mixed Sciences' " and Jonathan Lear's "Aristotle's Philosophy of Mathematics" (Lennox, 1986; Lear, 1982). In this chapter I will be developing an interpretation of Aristotle that is closely aligned with Lennox's and Lear's. In particular, I will argue that Aristotle is more concerned to describe how mathematics is applied in empirical contexts than to argue that it cannot be done.

The first group of interpreters just described most commonly cite Aristotle's *Posterior Analytics* in support of the view that Aristotle rejected mathematics either in physics or in empirical inquiries generally. The key argument is held to come in Chapter 7 of Book Alpha, the chapter in which Aristotle lays down his infamous

[8]For two further examples, see (Dutton, 1999, p. 51n) and (Maull, 1980, p. 25).

prohibition of "kind crossing".[9] In this chapter, I will to go back to the *Posterior Analytics* to see whether the latter work supports the claim that Aristotle took there to be some difficulty in principle involved in applying mathematics to empirical inquiries. While I acknowledge that Aristotle's arguments raise a difficult puzzle about how any one science can be applied to any distinct science—namely, the kind crossing puzzle—I will argue that this puzzle should not lead us to saddle Aristotle with the unsavory view that mathematics is inappropriate in empirical inquiry or in the study of nature. On the contrary, Aristotle counts on there being ways of resolving the puzzle which allow for branches of mathematics to apply to natural domains, and he gives indications in the *Posterior Analytics* of how he thinks the puzzle is properly resolved. The main contribution of this chapter over and above the excellent work of Lennox and Lear is to offer a precise account of what a solution to the kind crossing puzzle should look like.

2.1 Mathematical and Empirical Science in the Posterior Analytics

The main subject of the Posterior Analytics is *episteme*; the word "*episteme*" has been variously translated as "science", "knowledge", "scientific knowledge" and "un-

[9] Along with Lear, I do not read Aristotle as himself pressing the Protagorean worry that the theorems of geometry should somehow turn out false when taken as claims about material objects. In the part of *Metaphysics* B in which Aristotle discusses the issue, Aristotle is not presenting his own view but rather presenting difficulties for the views of his opponents. See (Lear, 1982, §2). Nonetheless, the prohibition on kind crossing does seem to raise a real difficulty for the applicability of mathematics in empirical inquiry, and this prohibition is evidently in Aristotle's own voice.

derstanding".[10] The breadth of meaning in the translations of "*episteme*" testifies to the fact that translators have misgivings about the straightforward identification of the meaning of "*episteme*" with that of any English term. It is clear, however, that Aristotle considers such disciplines as geometry, arithmetic, harmonics, and astronomy to fall under the concept *episteme*; for this reason it seems natural to translate *episteme* as "science" or "scientific knowledge", even if to do so involves us in anachronism.[11]

Aristotle's analysis of science employs the theory of syllogistic developed in the *Prior Analytics*. A scientific argument (or demonstration) must be a valid deduction in the sense of the *Prior Analytics*, but it must also meet a number of further conditions which Aristotle lays down in the *Posterior Analytics* (henceforth *AnPst*). As Aristotle says in A 2 of *AnPst*, the premises of a scientific demonstration must be "true and primitive and immediate and more familiar than and prior to and explanatory of the conclusions" (71b23).[12] Aristotle later proves that they must also be necessary (74b28). The aim of these conditions is to ensure that when we possess a scientific demonstration, we will thereby possess knowledge of its conclusion in a very strict sense—that is, in the sense of "*episteme*". A slightly modernized version of the

[10]For translations of *episteme* as "science" and "scientific knowledge", see (McKirahan, 1992, p. 3). Barnes opts to translate *episteme* as "understanding" in his translation of the *Posterior Analytics*; see (Aristotle, 1993, p. 2). Unless I indicate otherwise, in the remainder of this chapter I will use Barnes's translation of the *Posterior Analytics* and give citations using the familiar Bekker numbers. I will also sometimes refer to Chapters and Books of the *Posterior Analytics* in the following way: "X y" refers to Chapter y in Book X. For example, "A 7" refers to Book Alpha, Chapter 7. For other works by Aristotle I will use the translations found in (Aristotle, 1984a).

[11]For a couple of instances of Aristotle's use of the word "*episteme*" in the *Posterior Analytics*, see 71a3 and 75b12. The Greeks had no concept which corresponds closely to our modern concept of science, and for this reason some commentators object to "science" as the translation of "*episteme*".

[12]For an explanation of what Aristotle means by "primitive", "immediate", *etc.*, see Barnes's commentary in his translation of the *Posterior Analytics* at (Aristotle, 1993, p. 94).

definition of "*episteme*" Aristotle offers at A 2 might run as follows: an agent S has *episteme* of a proposition p if and only if (i) S knows that p cannot be otherwise, *i.e.*, that p is necessary, and (ii) S knows of some explanation E that it is the correct explanation of the fact that p (71b10).[13] Because a deductively valid syllogism from necessary premises yields a necessary conclusion, it is clear that when we possess a scientific demonstration of a proposition p we will meet condition (i) with respect to p; because a scientific demonstration must be explanatory of its conclusion, we will also meet condition (ii).

The very high standards Aristotle places on *episteme* give rise to the suspicion that *episteme* is extremely difficult to obtain. A contemporary reader might conclude from Aristotle's definition that we can only have *episteme* of those truths we nowadays take to be necessary. This ordinarily encompasses the truths of logic and mathematics but excludes almost everything else, and in particular it excludes many empirical truths we would expect to form a part of scientific knowledge. Aristotle's view on the scope of necessary truth is quite different from ours, however; Aristotle takes more truths to be necessary than we do, so that we may have *episteme* of many propositions which to our eyes appear contingent. For example, Aristotle writes in the *Movement of Animals*:

> [W]hen we say it is impossible to see a sound, and when we say it is impossible to see the men in the moon, we use the word in different ways: the former is of necessity, the latter, though their nature is to be seen, will not actually be seen by us. Now we suppose that the heavens are of

[13] Here are Aristotle's own words: "We think we understand (*epistasthai*) something simpliciter... when we think we know of the explanation because of which the object holds that it is its explanation, and also that it is not possible for it to be otherwise" (71b10). For more discussion of Aristotle's definition, see *ibid.*, p. 89. My modernized formulation of Aristotle's definition follows Barnes's at *loc. cit.*

> necessity impossible to destroy and to dissolve, whereas the result of the present argument would be to do away with this necessity.[14]

In the passage just cited, Aristotle emphasizes the strict necessity which accompanies the astronomical proposition that the heavens are indestructible. Elsewhere Aristotle argues that "[t]he shape of the heaven is of necessity spherical."[15] Hence on Aristotle's conception of necessity, the definition of "*episteme*" given above does not imply that such propositions from astronomy cannot be known in the strict sense carved out by the definition. What we would call "scientific knowledge" and what Aristotle would call "*episteme*" coincides more than may at first appear.

It is not my aim here to give a general overview of *AnPst*; rather, I want to focus on what Aristotle says in *AnPst* about the relationship between mathematical and empirical sciences. I claim that there is ample textual evidence to show that a primary goal of *AnPst* is to give an account of the relationship between branches of pure mathematics and the natural sciences which apply them. Consequently, I regard it as misguided to cite *AnPst* as evidence for the view that Aristotle rejected the application of mathematics to natural science. In the remainder of this section, I will consider particular stretches of *AnPst* which take up the question of how branches of mathematics—in particular, geometry and arithmetic—are applied.

Aristotle first takes up the question of the application of mathematics in A 7, where he claims that geometry and arithmetic prove results in optics and harmonics, respectively (75b14).[16] As we will see, Aristotle does not think it is ordinarily the

[14] See 699b18-699b23; I am using the translation which appears in (Aristotle, 1984a, p. 1089).

[15] See *De Caelo* 286b10 at (Aristotle, 1984a, p. 473).

[16] As Barnes points out, Aristotle is committed to optics and harmonics having their own

case that one science can yield results in any distinct science, yet he explicitly makes an exception for such pairs as geometry/optics because—as he says at 75b16—optics "falls under" geometry (*thateron hypo thateron*). Aristotle also explicitly allows that geometry be applied in mechanics; mechanics is another example of a natural science "falling under" a mathematical one (76a23).[17] Since some translators of *AnPst* have preferred the rendering "one science is subordinated to another" to "one science falls under another", natural sciences which apply branches of mathematics have come to be called "subordinate sciences" in scholarly literature on Aristotle. On either rendering, "subordination" or "falling under" designates the relationship which holds between a mathematical theory and a natural science when the natural science applies the mathematical theory.[18]

At various points in *AnPst*—but especially in the stretch from A 7 to A 13—Aristotle discusses the "falling under" relation and thereby the question of how mathematics is applied. One recurring theme in these discussions is that demonstrations in a subordinate science make use of the theorems of the corresponding "superordinate" science. Aristotle puts this point in different ways at different times: for example, at 76a23 Aristotle says that "geometrical demonstrations attach to mechanical or optical demonstrations"; he also says that optics is "proved from the same items as

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principles—*i.e.*, principles distinct from those of mathematics—so what Aristotle presumably means here is that (for example) geometry helps us to prove results in optics, not that optical results are literally results of pure geometry. See p. 160 of Barnes's commentary following his translation of *AnPst*.

[17]For a complete listing of all such pairs mentioned in *AnPst*, see *ibid.* pp. 158-159.

[18]Aristotle also indicates that whenever one science falls under another, the former science is the domain of the more empirical scientist, whereas the latter science is the domain of the more mathematical scientist (79a3). This provides evidence for the stronger claim that whenever science x is subordinated to science y, x is more empirical and y more mathematical.

geometry" and that the student of optics "should indeed supply arguments from the principles and conclusions of geometry" (77b2). Although Aristotle's statements on this matter are somewhat vague, commentators seem to agree that what Aristotle intends can be clarified by considering those subordinate sciences in Aristotle's ken.[19] Euclid's *Optics* is particularly helpful in that regard, since it is very likely that the latter work gives a representative sample of the optical demonstrations with which Aristotle would have been familiar.[20] To fix our ideas about how optics made use of geometry in Aristotle's time, let us give some brief consideration to the kind of proof which appears in the *Optics*.

The subject matter of the *Optics* is visual perception, and it is largely exhausted by the theory of perspective. The characteristic situation treated by the *Optics* is one in which a viewer is taking in some scene; to make this situation amenable to geometrical investigation, the viewer's eye is assumed to occupy a single point, and "visual rays" are assumed to emanate from the eye outward toward objects in the viewer's visual field. For purposes of giving proofs, visual rays are just geometrical lines containing the eye at one point and some distinct point in the eye's visual field. Optical arguments prove such conclusions as that certain objects in the visual field of the eye appear larger than others, that they appear to the left or to the right of others, and so on. Richard McKirahan gives a detailed discussion of a proof from the *Optics* in his article "Aristotle's Subordinate Sciences"; McKirahan chooses the proof of the theorem that "[o]f equal segments on the same straight line, those seen from

[19]See *ibid.*, p. 159. See also (McKirahan, 1978, pp. 197-220). McKirahan gives a detailed treatment of an argument from Euclid's *Optics* in section III. I will be making use of McKirahan's treatment of the *Optics* in what follows.

[20]See *ibid.*, p. 199.

a greater distance appear smaller."[21] In this case we assume that the "object" seen by the eye is a line with several line segments of equal length marked off on it; we connect the eye up with the ends of the line segments by means of visual rays. We now have a planar figure which is open to geometrical argument. Most of the optical proof is pure geometry, with many of the propositions cited as theorems of Euclid's *Elements*. The geometrical part of the argument concludes with the statement that the angle formed by the visual rays connecting the eye to the closest line segment is larger than the angle formed by the visual rays connecting the eye to line segments farther away. According to the fourth principle of the *Optics*, "things seen under greater angles appear greater."[22] Hence we get the conclusion that the closest line segment appears larger.

The kind of argument just described fits closely with Aristotle's comments about the role of mathematics in the proof structure of subordinate sciences. Geometrical demonstrations attach to optical demonstrations in the quite literal sense that much of an optical demonstration consists of a geometrical subproof. The *Optics* interprets the geometrical subproof as having implications for visual perception by means of the first principles of optics. However, the first principles of optics often play a relatively minor role in the course of the main proof, and this perhaps accounts for Aristotle's

[21] *Ibid.*, p. 200.

[22] Cf. *ibid.*, p. 207. The word I have translated as principle ("*horos*") is most commonly translated as "definition", as this is how Euclid uses the word (*loc. cit.*). However, not all of the *horoi* of the *Optics* can be plausibly called "definitions", and consequently some translators have preferred to render "*horos*" as "determination". I have used "principle" because it is a familiar term not far in meaning from "primitive sentence", *i.e.*, something which one can appeal to in a proof and which one does not prove. Other *horoi* of the *Optics* include that "those things are seen on which the visual rays fall, and those things on which they do not fall are not seen" and that "things seen under more angles are seen more precisely". For a full list of the *horoi*, see *loc. cit.*

tendency to ignore them in such statements as that optics is "proved from the same items as geometry" (77b2). This is not strictly correct even on Aristotle's view, since as Barnes points out Aristotle is committed to optics having principles of its own, distinct from those of geometry.[23] Nonetheless one can see that the geometry is, so to speak, the driving force of many of the proofs, so that Aristotle's statements may not be overly misleading about the character of optics in his time.

Aristotle emphasizes another characteristic feature of the relationship between subordinate and superordinate sciences at several points in *AnPst*: whereas practitioners of a subordinate science such as optics know the facts of their discipline, the practitioners of the superordinate science possess the explanations of those facts.[24] In the example just considered, Aristotle would presumably argue that it is because line segments closer to the eye subtend a larger angle at the eye that they also appear larger. Aristotle does not explicitly say so, but his statements suggest that since a subordinate science is more empirical than a corresponding superordinate science, practitioners of the subordinate science might come to know of the facts through observation (79a3, 79a15). We might know from quite ordinary experiences that when we mark off equal segments along the edge of a box in front of us, we see those segments which are closer to our eyes as longer than the others. However, it is plausible to suppose that when we first acquire this knowledge, we do not as yet have an explanation of that fact—our position is like that of a practitioner of a very empirical

[23] Again, see Barnes's commentary at (Aristotle, 1993, p. 160).

[24] Cf. 76a11, 78b34, 79a2, and 79a15. Aristotle presumably means the word "know" here in a weaker sense than that picked out by "*episteme*", since the practitioner of optics would have to know the explanation of the fact that p in order to have *episteme* of p. At 79a2 and 79a15, the word translated as "know" is "*eidenai*".

optics. We only possess a proper explanation when we go through a (largely geometrical) proof like the one described two paragraphs ago. Hence it is only insofar as we are geometers in addition to students of optics that we possess proper explanations for the optical facts.

Although we can provide some plausible cases in which a superordinate science contains the explanations for facts known to practitioners of a corresponding subordinate science, there are also some cases where this view of the matter is quite implausible. Aristotle infamously provides the following example: "it is for doctors to know the fact that curved wounds heal more slowly, and for geometers to know the reason why" (79a15). Aristotle presumably has in mind the geometrical fact that the more a closed planar figure resembles a circle (the more "curved" it is), the higher the ratio of its area to its perimeter will be.[25] Here Aristotle overlooks the fact that the explanation for curved wounds healing more slowly—supposing that they do heal more slowly—ought also to make mention of the way the human body produces skin. Aristotle seems to be imagining that the skin grows from the edges of the wound towards the center, and everywhere with equal speed. Without any account of how skin grows, the bare geometrical explanation is incomplete: we can imagine *contra* Aristotle that skin is produced at an equal rate at all areas inside wounds, so that wounds heal at the same speed regardless of size or shape. If Aristotle is right about curved wounds healing more slowly, he must give some explanation of skin growth that rules out the contrary possibility just considered. But in that case the explanation for curved wounds healing more slowly is not contained within geometry—it also

[25]Cf. (Aristotle, 1993, p. 160).

essentially involves some facts of medicine.[26]

Let us turn away from the difficulties just rehearsed and consider one final aspect of the relationship between subordinate and superordinate sciences. At A 13, Aristotle make the following somewhat puzzling statement:

> The items in question [viz., the subordinate sciences] are things that, being something different in their essence, make use of forms. For mathematics is concerned with forms: its objects are not said of any underlying subject—for even if geometrical objects are said of some underlying subject, it is still not as being said of an underlying subject that they are studied (79a8-79a13).[27]

We get a better idea of what Aristotle has in mind here by considering Book II of the *Physics*, where Aristotle writes:

> Now the mathematician, though he too treats of these things [i.e. shapes], nevertheless does not treat of them as the limits of a natural body; nor does he consider the attributes indicated as the attributes of such bodies. That is why he separates them; for in thought they are separable from motion, and it makes no difference, nor does any falsity result, if they are separated... [Optics, harmonics and astronomy] are in a way the converse of geometry. While geometry investigates natural lines but not qua natural, optics investigates mathematical lines, but qua natural, not qua mathematical.[28]

[26]The wound case makes clear that Aristotle must in some way qualify his general view that superordinate sciences contain the explanation of facts belonging to subordinate sciences. Here is one suggestion (couched in vocabulary which will be explained in the next section): a superordinate branch of mathematics explains a conclusion belonging to a subordinate science just in case (i) the predicate term of the conclusion belongs to the mathematical theory, (ii) the subject term picks out an instance of a species dealt with by the mathematical theory, and (iii) the predicate holds *per se* of all instances of the species. Consider the demonstration: "all stars are spherical; spheres have volume equal to πr^3, where r is the radius of the star; hence stars have volume equal to πr^3." The reason why stars have volume equal to πr^3 is because stars are instances of the species sphere and spheres *per se* have volume equal to πr^3.

[27]It is not obvious what "the items in question" refers to here; with Barnes, I am reading the scope of that phrase to indicate subordinate sciences. See *ibid.*, p. 159.

[28]See 193b32-194a11 in the *Physics* at (Aristotle, 1984a, p. 331). The italics here are Aristotle's.

In the two paragraphs just cited, Aristotle gives the following sort of picture of the relationship between a mathematical science like geometry and the subordinate sciences which apply it. In doing geometry, we study the shapes which material bodies can instantiate without considering the material bodies which actually do instantiate those shapes. In fact, for the purposes of a geometrical investigation we do not even consider whether there are any material bodies which instantiate precisely the shapes we are studying. As Aristotle says in the *Physics*, we accomplish this by separating out in thought the shapes of material objects from their material instantiations and ignoring their other attributes, *e.g.*, the materials out of which they are made.[29] Or, as Aristotle puts it in *AnPst*, we study shapes not as predicated of some underlying material object, even if in fact material objects constitute all the instantiations of shapes there really are.[30] Students of astronomy, in contrast, study geometrical objects or attributes only insofar as these items are instantiated in physical space and are relevant to their science. Spherical celestial bodies are instances of the universal studied by geometry—we might call this universal "spherehood"—and that is how theorems of geometry can apply also to the objects in the subject matter of astronomy. Unlike the geometer, however, the astronomer may well investigate the material nature of the spheres she studies. The material nature of stars is relevant to astronomers, since among other things the material nature of the stars may explain

[29] Lear makes a detailed proposal about what this kind of separation amounts to in (Lear, 1982, §1).

[30] Aristotle rejects the view that the shapes studied by mathematicians exist independently from their material instantiations. This surfaces somewhat at A 11, where Aristotle writes that "[t]here need not be any forms, or some one item apart from the many, in order for there to be demonstrations. It must, however, be true to say that one thing holds of many" (77a5). Thus it is true to say of many things that they are spheres, though there need not be some form of spherehood existing apart from those many spheres in order for there to be geometrical demonstrations.

the way they move, the changes to which they are susceptible, or even the shape they possess.[31]

So far we have seen in broad outline how Aristotle conceives of the way natural sciences apply branches of mathematics. On pages 158–162 of his commentary on *AnPst*, Barnes provides a detailed list and discussion of every place Aristotle mentions the topic of subordination. By Barnes's count, Aristotle describes fully ten different aspects of the relationship between subordinate and superordinate sciences in *AnPst*.[32] By contrast, I am not aware of a single place in *AnPst* where Aristotle explicitly states that branches of mathematics are inapplicable in the setting of natural or empirical science. As we will consider in some detail in the next section, Aristotle does argue that distinct sciences are generally inapplicable to one another; in light of the textual evidence we reviewed in this section, however, it would be out of place to conclude from these arguments that Aristotle took mathematics to be inapplicable in natural science. Rather, the only judicious reading of Aristotle must interpret him as allowing for exceptions in the case of the subordinate sciences. In fact, Aristotle himself explicitly marks out the subordinate sciences as exceptions to the general rule (75b15).

[31]In Book II of *De Caelo*, Aristotle writes that "[i]t would be most reasonable and consequent upon what has been said that each of the stars should be composed of that substance in which their path lies, since, as we said, there is an element whose natural movement is circular". Cf. *De Caelo*, 289a14 at (Aristotle, 1984a, pp.476-477).

[32]Cf. Barnes's commentary at (Aristotle, 1993, pp. 158-159). If we consolidate items on the list so closely related to one another as to virtually identical, we are plausibly left with eight items; three of the items amount to Aristotle's recognition that demonstrations in subordinate sciences make use of theorems of the corresponding superordinate science.

2.2 The Kind Crossing Puzzle

The conclusion of Aristotle's argument that distinct sciences are inapplicable to one another has come to be known as the prohibition on "kind crossing" or *metabasis* (cf. 75b10). The heart of the argument appears in A 7 of *AnPst*, although there Aristotle draws upon results from earlier chapters, especially A 6. Aristotle's argument involves several technical notions which we have not yet discussed; I will first introduce those notions and then turn to A 7.

To appreciate the kind crossing argument, we must first observe that in addition to the requirements on the premises of demonstrative syllogisms listed above in the introduction, Aristotle requires that the subject and predicate of each premise hold of each other *per se* (*kath' hauto*).[33] "*Per se*" is a bit of Aristotelian jargon defined in A 4 as follows: "Something holds of an item *per se* both if it holds of it in what it is. . . and also if what it holds of itself inheres in the account that shows what it is."[34] Barnes offers a clearer, more modern formulation of what this amounts to in his commentary on *AnPst*:

(1) A holds of B *per se* $=_{df}$ A holds of B and A inheres in the definition of B.

(2) A holds of B *per se* $=_{df}$ A holds of B and B inheres in the definition of A.[35]

It is relatively easy to give examples of the use of "*per se*" indicated in (1): three

[33] This is one of the topics of A 4. Cf. 73a35-73b24. This way of putting Aristotle's condition on premises highlights the fact that Aristotle is assuming all statements in a scientific demonstration to be put in the subject-predicate form required by his theory of syllogistic.

[34] 73a35-73a39. Here I deviate slightly from Barnes's translation: whereas Barnes translates "kath' hauto" as "in itself", I am following a somewhat older tradition of translating it as "*per se*".

[35] See (Aristotle, 1993, p.112). Again, I am replacing Barnes's "in itself" with "*per se*".

sided holds of triangle *per se* since all triangles are three-sided and the definition of "triangle" states that a triangle is a three-sided polygon. It is rather more difficult to find uses "*per se*" indicated by (2). According to Barnes, one of the best examples from *AnPst* comes at B 16 where Aristotle predicates "eclipse" of "the earth's being in the middle", since whenever we have the earth in the middle what we have is an eclipse, and moreover the definition of (lunar) "eclipse" will mention the fact that the earth is in the middle.[36]

On the uses of "*per se*" indicated by (1) and (2), what we have is a predicate's holding of a subject as a matter of definition. This implies that the terms contained in the premises of a demonstration must stand in a very tight conceptual relationship with one another. Moreover, as Aristotle points out, it implies that that the premises of a demonstrative syllogism are necessary (73b24).

The next technical notion we need in order to understand the argument of A 7 is that of the "kind" or "subject genus" (*genos*) of a science. Aristotle indicates at A 28 that sciences are to be identified according to the range of objects or "subject genus" which practitioners of the science investigate (87a39). Geometry is the study of spatial magnitudes such as lines, points, and solids; arithmetic is the study of units. If units were (*per impossibile*) the very same things as spatial magnitudes, then arithmetic and geometry would constitute a single science.[37]

Somewhat more formally, we can consider the subject genus of a science as con-

[36]Cf. 98b22. As I indicate in the main text, Aristotle presumably has a lunar eclipse in mind; the point is that during a lunar eclipse the earth is standing between the sun and the moon.

[37]Of course, in light of the development of analytic geometry it may seem less odd to suggest that numbers and spatial magnitudes might be identical. It is clear, however, that Aristotle thought they were different. See 75b5 and p. 131 of Barnes's commentary.

sisting of two things: (a) the most general class of objects studied by a science (*e.g.*, spatial magnitude), along with the various subsets of that class (*e.g.*, polygon, triangle, right triangle, or prism, rectangular prism, right rectangular prism); (b) the attributes which hold *per se* of any subset of elements indicated in (a).[38] The aim of the scientist is to discover the character of the elements in (a) by providing demonstrations that those elements have certain properties *per se* (75a42). Hence the vocabulary of a single science contains just those terms which designate subclasses of (a) or their *per se* properties, since these are what are needed for the demonstrations of that science.

An important feature of Aristotle's view of subject genera is that they are natural and non-arbitrary. Aristotle hints at this view in *AnPst* when he writes that "existent things belong to different kinds" (*gene*).[39] In his discussion of the subject genus, McKirahan mentions a number of places in the *Metaphysics* where Aristotle makes such claims as that "being falls immediately into genera; and therefore the sciences too will correspond to these genera."[40] Hence we might say that it is not up to the whim of the scientist to study this or that set of objects; rather, the scientist studies kinds of things marked off as such by their very nature. Although I do not know any place where Aristotle says so explicitly, an implicit part of this view seems to

[38] Aristotle gives slightly different characterizations of the subject genus at different points in *AnPst.* For a list of these characterizations along with discussion, see (McKirahan, 1992, pp. 57-60). My description of the genus is most in line with Aristotle's statement at 87a39: "A science is one if it is concerned with one kind—with whatever items come from the primitives and are parts or attributes of them in themselves" (*kath' hauta*). Among the *per se* attributes mentioned here are presumably the *per se* incidentals, *i.e.*, attributes of objects which hold as deductive consequences of the object's definitions yet are not themselves part of the definition. For more on *per se* incidentals, see pp. 113-114 of Barnes's commentary.

[39] *AnPst* A 32, 88b2.

[40] See *Metaphysics* Γ, 1004a4, (Aristotle, 1984b, p. 1585).

be that the subject genera studied by different sciences do not typically overlap. In other words, if X's are a part of the subject genus of some science S, then they will not also be a part of the subject genus of some distinct science S'; or if A is a *per se* attribute of some object O in science S, then A will not be a *per se* attribute of any object considered by a distinct science S'. As we will see, this premise plays a crucial role in the argument in A 7.

Let us turn to that argument now. What Aristotle aims to show is that all of the terms appearing in the premises of a demonstration must be names for items in the same subject genus, so that a demonstration of a conclusion stated in the terms of one science could not include premises containing terms from a distinct science. If a demonstration did contain terms from more than one genus, then that demonstration would "cross" (*metabainein*) subject genera. Aristotle wants to prove that this is impossible. If he is successful, then this will have the further consequence that the theorems of one science S can play no role in the demonstrations of a distinct science S', since of course for there to be any theorems of S occurring in demonstrations from S' there must at least be one term from S occurring in the demonstration. This a particularly stark and precise way of showing that S cannot be in any way applied by S'.

The key premise of Aristotle's argument comes at 75b11-14, where Aristotle says that "the extremes and the middle terms must come from the same kind, since if they do not hold in themselves [*kath' hauta*], they will be incidentals." This statement is couched in Aristotle's theory of syllogistic and so requires some additional explanation. According to Aristotle, deductively valid arguments generally have two premises and a single conclusion; the premises are statements in subject-predicate form, and the subject or predicate term from one premise reappears in the other premise. The best known example goes by the name "Barbara": "All A's are B's, all B's are C's, hence all A's are C's." Here the term "B" is common to both premises, which is Aristotle's criterion for being a "middle term" (*meson*). The terms "A" and "B" are called the "extremes" (*akra*). Now suppose, as Aristotle does, that the middle term "B" and the extremes "A" and "C" of a deduction do not come from the same subject genus.[41] Aristotle is arguing in the passage just cited that in such a case, "B" will not stand in any *per se* relationship to the other terms: that is, it will be false that A holds of B *per se*, that that C holds of B *per se*, *etc.*[42,43] But as we recently observed, in order for a deductively valid deduction to constitute a scientific demonstration, the premises of a demonstration must state *per se* relationships holding among the predicate and subject terms. Therefore there can be no demonstration from the terms "A," "B," and "C".

We just argued for the following conclusion: if the extremes and the middle terms come from different subject genera, then there can be no demonstration. We might

[41] When Aristotle speaks of terms coming from the same subject genus, what he presumably means is that the terms name some group of elements or attributes in the genus. Aristotle often fails to distinguish use and mention, and he appears to be guilty of that here. In what follows, when I speak of terms coming from a subject genus, I will mean by that the term names some group of elements or attributes included in the genus.

[42] What Aristotle says, in a somewhat elliptical fashion, is that if the extremes and middle term do not come from the same genus, then the terms of the premises of the deduction will hold only incidentally ("*symbebekota*"; translators sometimes render this as "accidentally"). At this point in *AnPst* Aristotle appears to use "incidental" as incompatible with "*per se*"; however, only several lines above Aristotle seems to mention the *per se* incidentals: at 75b1 Aristotle describes the demonstrations of a science as making plain the "items incidental to [the subject genus] *per se*". For more on the *per se* incidentals, see footnote 35 above.

[43] In what follows I will sometimes talk of two terms A and B failing stand in any *per se* relationship to each other. This just means it is false that A holds of B *per se* and vice-versa.

be able to make the result more intuitive by arguing for the contrapositive: if there is to be a demonstration, the extremes and the middle term must all come from the same subject genus. Suppose, then, that we start with two terms "A" and "B", and we would like to form a demonstration from them. This is only possible if they stand in a *per se* relationship to one another, in which case they must come from the same genus G.[44] If we want to form so much as a deductively valid argument from "A" and "B", we must generate a further premise which contains either "A" or "B". Without loss of generality, suppose we generate a premise containing "A" and an additional term "X". If A and X are to relate to one another *per se*, then once again "A" and "X" must come from the same subject genus, *viz.*, G. Hence all of the terms of the deduction must come from the same genus, G, if there is to be a demonstration.

One corollary of this argument is that if we have a mathematical theory M and some non-mathematical scientific theory S, no terms from M can appear in demonstrations from S, so that *a fortiori* no demonstrations from S make use of theorems from M. Of course, there is no special reason to choose a mathematical and a (non-mathematical) scientific theory. We could just as well choose two mathematical theories, which is what Aristotle himself actually does at A 7 when he says that you cannot prove something geometrical by arithmetic (75a39). Some of the authors mentioned in the introduction to this chapter are motivated to emphasize the putative inapplicability of mathematics to natural science because in the period they write

[44]In case this is unclear: suppose, without loss of generality, that A holds of B *per se*. "B" is a name for items in some unique subject genus; call this genus "G". "A" then designates a *per se* attribute of the items designated by "B". But a genus includes the *per se* attributes of items contained in the genus, so that "A" also designates something in the genus G. From this it follows that "A" and "B" both belong to the same genus.

about—the 17^{th} century—applications of mathematics to natural science exploded in number and importance. One way of accounting for this change is to say that 17^{th} century natural philosophers came to reject the eschewal of mathematical methods which dominated natural philosophy in the Aristotelian tradition. We can in turn account for that eschewal by reference to such texts as A 7 of *AnPst*, where Aristotle himself supposedly banishes the use of mathematics in natural philosophy. As I emphasized in §2.1, I regard this use of *AnPst* as illegitimate.[45] Immediately after the general argument we just rehearsed, Aristotle explicitly mentions exceptions to the general rule: optical proofs employ geometrical reasoning, and proofs in harmonics employ arithmetical reasoning (75b14). The kind crossing puzzle is to figure out how these exceptions are possible. That is, the argument rehearsed above did not seem to be merely general: it seemed to be exceptionless. So how can Aristotle hold that nevertheless in some cases, one scientific theory manages to apply to another? This is the question I will try to answer in the next section.

2.3 Defusing the Puzzle

Aristotle gives a hint of an answer at A 7 when he says that the subject genera of two sciences "must be the same, either simpliciter or in some respect" if a demonstration is to cross genera (75b9). What this suggests is that at least in some cases,

[45] A further unhappy consequence of this view is that it forces Aristotle to hold that material objects can only have their shape, or other properties described by a mathematical theory, accidentally: since terms for shapes are mathematical terms, whereas terms designating material bodies are natural terms (from some distinct genus), there can be no *per se* relationship among such terms. Cf. Maull *op. cit.*, *loc. cit.* We have already seen Aristotle contradict this view explicitly by maintaining that the shape of the heavens is of necessity spherical. I hold this to be further evidence that the reading of Aristotle considered here is a misinterpretation.

distinct sciences can overlap with one another in the sense that they treat some of the same items or treat items as having some of the very same properties *per se* (cf. 76a14). We saw just this sort of thing in §2.1: whereas geometry treats of lines in abstraction from all material conditions (for example, location in physical space), optics treats of certain lines—chiefly visual rays—and it treats them as being located in the space in front of a viewing subject. Despite the differences in the way optics and geometry consider lines, lines are part of the subject genera of both sciences. To return to an example from §2.1, spheres form part of the subject matter of both geometry and astronomy; this is because many celestial bodies, including the stars and the heaven as a whole, are spherical in shape.

Aristotle's notion that the subject genera of different sciences might overlap or "be the same" in a qualified sense is meant to allow there to be demonstrations which contain terms and premises from different sciences. Consider, for example, the following argument:

(α) Visual rays are lines.

(β) A line is length without breadth.[46]

$\therefore$ (γ) Visual rays are lengths without breadth.

I claim that nothing prevents (α) - (γ) from being a demonstration according to the strictures of *AnPst*, and I consider it likely that Aristotle would have regarded it as such. Premise (α) is grounded by the definition of "visual ray", which runs roughly as

[46]This is definition of "line" given in Euclid's *Elements*. For the original Greek with translation, see (Thomas, 1939, p. 437).

follows: "a visual ray $=_{df}$ a line containing one point at the viewer's eye and a distinct point somewhere in the viewer's visual field." Thus line would hold of visual ray *per se*, and "All visual rays are lines" would be a suitable premise for a demonstration. Premise (β) is cribbed from Euclid's *Elements* and is the definition of "line" that appears there; hence (β) is an acceptable premise for a demonstration if there ever was one. Moreover, the premises seem to be explanatory of the conclusion: visual rays are lengths without breadth because they are lines. Here we have another case congenial to Aristotle in which the explanation is contained within the superordinate science. Having convinced ourselves that (α) - (γ) is in fact a demonstration, we observe that nonetheless "visual ray" does not come from the same subject genus as "line" and "length without breadth."

One might object to the analysis of (α) - (γ) just given by pointing out that if line holds of visual ray *per se* and if a subject genus includes the *per se* attributes of the items it researches, then the subject genus of optics also includes lines. Moreover, if having length without breadth is a *per se* attribute of lines, then this attribute is part of the subject genus of optics as well. But then all of the terms in the demonstration do, after all, come from the same subject genus, so that (α) - (γ) is not an instance of kind crossing.

There are several ways we might respond to this objection while trying to make sense of Aristotle's statement that kind crossing does occur in some cases. First, we might grant that although visual rays, lines, and lengths without breadth are all part of the subject genus of optics—so that "visual ray," "line," etc. are all properly speaking optical terms—this does not prevent "line" or "length without breadth" from being mathematical terms as well. Earlier we encouraged the assumption that each type of thing falls naturally into one genus and therefore into the domain of exactly one science. However, it seems clear that in certain cases, the subject genera of sciences do not relate to one another in that way: the very sort of item studied by one science may also fall within the domain of investigation of another science. Because visual rays are lines, lines fall under the subject genus of optics. However, we all associate the term "line" more with geometry than optics, so the argument given in (α) - (γ) does look as if we switch topics from optics to geometry. On this way of responding to the argument of the last paragraph, it turns out that genuine cases of kind crossing never happen, though they do sometimes appear to happen. Aristotle's statement that kind crossing sometimes occurs is just meant to point out this latter fact.

The reaction just considered has the defect that it turns kind crossing into a merely apparent phenomenon, and some of Aristotle's remarks in A 7 give the impression that the phenomenon is more than just apparent. One way of ensuring that genuine kind crossing occurs is to interpret Aristotle's use of "subject genus" slightly differently. Aristotle often speaks as if the subject genus only contains what I labeled as "(α)" above, *viz.* the most general class of objects studied by a science (*e.g.*, spatial magnitude), along with the various subsets of that class (*e.g.*, polygon, triangle, right triangle, or prism, rectangular prism, right rectangular prism).[47] The aim of the science remains the demonstration of *per se* attributes of subsets of elements from (α), but these attributes are not themselves part of the subject genus. On this reading,

[47]For a guide to the textual evidence on the meaning of "*genos*", see (McKirahan, 1992, pp. 57-60).

the subject genus of geometry will be spatial magnitude whereas the subject genus of optics will be certain aspects of visual perception. Some of the demonstrations of optics will show that items studied in geometry hold *per se* of items in optics, as for instance when the student of optics demonstrates that all visual rays are lines. But this does not make lines part of the subject genus of optics, as is shown by the fact that the student of optics is not interested in the attributes of lines *per se* but only of certain lines (especially visual rays). More generally, the terms of optics and geometry will be rigidly separate in the sense that any one term belongs at most to one of the two sciences; nonetheless, items designated by terms coming from different genera can relate to one another *per se*.

I am not aware of any stretch of *AnPst* which allows us to establish Aristotle's opinion on the matter: that is, whether Aristotle allowed distinct sciences to share terms but did not allow genuine kind crossing, or whether he allowed genuine kind crossing (hence terms from different genera relating to one another *per se*) but barred the sharing of terms between sciences. That he adopted some such view is guaranteed by his explicit assertions that one science can at times apply another science in its demonstrations.[48] I will not attempt here to give further evidence for one view over the other, especially in light of the fact that the two views seem to be mere verbal variants on one another: one view defines "subject genus" so as to include all of the items which hold *per se* of any other items in the genus, the other defines "subject genus"

[48]I suppose it is possible that Aristotle condoned both the sharing of terms between sciences and genuine kind crossing; I do not mention this possibility because it lacks theoretical simplicity. The only position which Aristotle really cannot adopt is that sciences do not share terms and there is no genuine kind crossing, since in this case the application of, *e.g.*, arithmetic in harmonics is a logical impossibility.

more narrowly, but on either view (α) - (γ) counts as an acceptable demonstration. Instead, I want to consider a difficulty raised by either of the views just considered: Given that sciences can share terms, or given that terms coming from distinct subject genera can relate *per se* to one another, what is the point of Aristotle's general prohibition on kind crossing? For if terms from distinct sciences can relate *per se* to each other—as I will henceforth suppose for convenience of expression—then it seems there is no special difficulty with any one science applying any other, making use of the other in its demonstrations, *etc.* In that case, it seems that Aristotle's so-called "kind crossing" prohibition doesn't really bar us from doing anything whatsoever.

To see Aristotle's intent in laying down the kind crossing prohibition, we must first recall Aristotle's general viewpoint, which is that being divides into different natural kinds which constitute the domains of investigation of the natural scientist.[49] Even though we do in some instances get terms from distinct sciences relating *per se* to one another, Aristotle seems to think this is an exception to the general rule. Hence it is true for the most part that terms coming from different subject genera do not relate to one another *per se*. We should also observe that Aristotle only seems to allow terms from distinct subject genera to have *per se* relations when one subject genus is in a sense a proper part of the other. Terms of optics relate *per se* to terms in geometry because the objects studied by optics—visual rays *etc.*—are instances of the more general type studied by geometry, namely spatial magnitude. Hence Aristotle's admission that kind crossing occurs in cases of subordination does not go against the

[49]Much of the evidence for this reading of Aristotle comes from *Metaphysics* Γ. On the topic of the kinds or genera which form the subject matter of the individual sciences, see the relevant paragraphs of the preceding section of this chapter.

metaphysical view that, at least at a very general level, the kinds into which reality divides really are a partition of reality; terms belonging to different kinds taken at a high enough level of generality presumably do not stand in *per se* relations to one another.

It is also helpful in understanding the point of Aristotle's general prohibition on kind crossing to consider the uses Aristotle makes of that prohibition in *AnPst*. The first use comes already in A 7 when Aristotle writes that in geometry one cannot prove "anything that holds of lines not as lines and as depending on the principles proper to them—*e.g.* whether straight lines are the most beautiful of lines" (75b18). Aristotle is pointing out here that the question of whether lines are beautiful or not is not demonstrable in the setting of geometry, since geometry has only to do with those attributes of lines contained in their definitions, along with any attributes we can demonstrate geometrical items to have starting from those definitions. As we saw above, a line is length without breadth, and this definition has no geometrical consequences whatsoever for whether lines are beautiful or not. It follows that "pretty", "ugly", and other terms of aesthetic appraisal will not stand in any *per se* relationship to geometrical terms and are thus outside the domain of geometry entirely. What the kind crossing prohibition points up in this context is that the practitioner of a science only investigates the attributes of elements of the subject genus which he can show to be *per se* and necessary. Any other attributes are irrelevant (cf. 77a40).

A second use Aristotle makes of the prohibition on kind crossing is in arguments that there is no master science from whose first principles one can prove the first principles (and hence the conclusions) of all the other sciences (76a17). The idea of

Aristotle's argument is that in order for the master science to prove the conclusions of the other sciences it must generate demonstrations which contain terms borrowed from those sciences. But in that case the demonstration would have crossed kinds, which Aristotle has already characterized as illegitimate. If Aristotle does allow for kind crossing in some instances, as he clearly appears to do, then this flat footed rejection of a master science is unwarranted. One could reasonably object to Aristotle that all the other sciences are subordinate to the master science, and this is what allows the master science to prove their first principles.

It is important to recognize that the present objection holds regardless of whether (as I have been supposing) Aristotle took the mechanism of kind crossing to be terms in different sciences standing in *per se* relations with each other. Nonetheless, Aristotle's explicit requirement that premises of a demonstration contain terms which stand in *per se* relations to each other might give Aristotle ammunition against the present objection. Aristotle might argue as follows: a master science capable of proving the first principles of everything would have to have the universal subject genus, *i.e.*, it would have to be an investigation into the *per se* attributes of everything that exists. But existents qua existents have very few *per se* attributes; for instance, none of the items of geometry hold of existents *per se*, since there are many things which aren't geometrical objects at all (*e.g.*, the color red). But in this case, the master science will not be able to prove the first principles of geometry; *a fortiori*, it will not be able to prove the first principles of everything else.

The last few paragraphs indicate that the force of Aristotle's prohibition on kind crossing is to emphasize the parochial nature of individual sciences. Anything which does not stand in a necessary and definitional relationship with the items in a science's subject genus falls completely outside that science's domain. This is compatible with the view that in some limited number of cases—such as the case of geometry and optics—the subject genera of two sciences may overlap or "be the same" in a qualified sense.

2.4 Conclusion

Although *AnPst* has encouraged many readers to regard Aristotle as rejecting mathematical methods in empirical inquiry, I have argued that this is a misinterpretation. On Aristotle's overall picture of the workings of science, it is true that the practitioners of a science consider a limited range of objects and a correspondingly limited range of attributes. Moreover, in most cases the subject genera of distinct sciences do not bear a close enough relationship to each other to support one science's making an application of the other. Nonetheless, in light of the stunning successes achieved by mathematical pursuits in optics, harmonics, astronomy, *etc.*, Aristotle repeatedly makes explicit exceptions for areas of natural science to apply branches of pure mathematics. Good interpretations of *AnPst* must not render these applications unintelligible. In this chapter, I have suggested how Aristotle accounts for these applications given both the explanatory resources and the technical constraints of *AnPst*.

Chapter 3

Galileo

Regardless of Aristotle's own view of the relationship between mathematics and the natural sciences, it must be acknowledged that 16^{th} century Aristotelians made use of Aristotle's doctrines and texts to challenge the applicability of mathematics. In his *Dialogue Concerning the Two Chief World Systems*, Galileo signals that he intends to engage one of those challenges when he puts the following words in the mouth of Simplicio, the representative of the Aristotelians in the *Dialogue*:

> After all, Salviati, these mathematical subtleties are true in the abstract, but applied to sensible and physical matter they fail to correspond. For instance, mathematicians may prove well enough from their principles that a sphere touches a plane at a point... but when it comes to matter, things happen otherwise. What I mean about these angles of contact and ratios is that they all go by the board for material and sensible things.[1]

As I discussed in §1.1, a number of philosophers in the Aristotelian tradition had expressed views like Simplicio's,[2] and even today Simplicio's objection can sound

[1] See (Galileo, 1967, p. 203) and (Galileo, 1998a, p. 239). I have made some modifications to Drake's translation.

[2] In the present chapter we will primarily consider Alessandro Piccolomini and Benedict Pereira, two Jesuit Aristotelians closer to Galileo's own time.

plausible. After all, many philosophers do accept that geometric proofs suffice to establish the truth about geometric objects. On the other hand, if we put a spherical body on top of a flat surface, or if we draw a circle and a line tangent to it on the blackboard, we are apt to see that these things do not touch each other at a single point.[3] Such occurrences may convince us, as Simplicio is urging, to take a skeptical attitude towards geometry as a guide to nature.

Through Salviati, his mouthpiece in the *Dialogue*, Galileo gives a lengthy and elaborate reply to Simplicio's objection. In §§3.2 – 3.3 of this chapter I provide an analysis of Galileo's reply. Although other authors have discussed the same passage in Galileo, I believe that my analysis is novel in key respects. In particular, I offer a detailed account of how Galileo's responses to Simplicio's philosophical criticisms bear on our evaluation of the physical argument which prompts them.[4] In §3.4, the final section of the chapter, I argue that reflection on Galileo's reply can provide us with several lasting insights into the question of the applicability of mathematics:

First, Galileo gives powerful reasons for holding that despite their initial plausibility, views according to which a sphere touches a plane at a point in geometry but not in nature are untenable. More importantly, Galileo suggests a new criticism on behalf of opponents of the applicability of geometry which saves what is valuable in Simplicio's original objection, *viz.*, the criticism that geometric points, lines, and surfaces do not exist in nature.

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[3]When I try to produce a line tangent to a circle on paper or on a computer screen, I tend to find that either they do not touch or that they touch over a very small line.

[4]I have been especially helped by the discussions in (Feldhay, 1998), (McMullin, 1985), and (Palmerino, 2001). Although my focus in this chapter is not in how my interpretation of Galileo differs from theirs, I will indicate some points of difference in the footnotes.

Second, Galileo's reply to Simplicio illustrates the point emphasized in Mark Steiner's work on the applicability of mathematics that the phrase "application of mathematics" does not pick out a unique concept; there are several notions of an application of mathematics one ought to distinguish.[5] In particular, one should distinguish a *deductive* notion and a *descriptive* notion.[6] Drawing this distinction helps us to interpret the text of the *Dialogue*, since parts of Galileo's response play the role of defending the deductive, and others the descriptive applicability of geometry. Moreover, consideration of how the two notions relate in Galileo's discussion sheds light on both.

Finally, Galileo offers several strategies for overcoming the objection that there are no geometric points, lines, or surfaces in nature. One strategy is to argue that the objection assumes either an overly narrow conception of geometric objects, or an overly narrow conception of nature, or both. Another is to defend the legitimacy of idealizations which employ geometric approximations. In what follows we will examine how Galileo employs both these strategies in order to surmount Simplicio's objections.

3.1 The Extrusion Argument

To appreciate Simplicio's objection and Galileo's response to it, one must understand something about the physical argument to which it is an objection. Following Palmieri (2008) and others, I will refer to the physical argument as the "extrusion"

[5]See (Steiner, 1998, Chs. 1-2) and (Steiner, 2005).

[6]I explain these notions in §3.4.

argument. Out of considerations of space I will provide a somewhat simplified version of that argument here.[7] The aim of the argument is to show that even if it is granted to the Copernicans that the Earth spins daily around its own axis, nonetheless heavy bodies resting on the surface of the Earth would remain at rest (so long as they are not otherwise acted on). If successful, the extrusion argument would defend Copernicanism against the objection that if the Earth were rotating, any bodies resting on the Earth's surface would be thrown into space.

Galileo puts the extrusion argument into the mouth of his representative, Salviati, who reasons along the following lines: Suppose that the Earth is rotating and consider some body at rest on its surface at an initial time. Because the Earth is rotating, the body does possess an impetus to be thrown in the direction of the line tangent to the point of contact of the body with the Earth. The body also has a natural impetus to fall towards the center of the Earth. In order for the body to remain at rest on the Earth's surface, the tendency of the body to fall towards the center of the Earth must dominate its tendency to be thrown off. In other words, let us suppose that some interval of time has elapsed, and let us further suppose that the body's motion towards the Earth's center would have sufficed to bring it back to the surface if it had been projected some distance along the tangent. In that case, the body will simply have remained on the surface over the interval.

[7] In particular, I will leave the weight of the body out of consideration. The full text of the extrusion argument occurs at (Galileo, 1967, pp. 188-203) with Drake's commentary at *ibid.*, pp. 478-479; for the original Italian see (Galileo, 1998a, pp. 203-219). The extrusion argument has been discussed at length by prior authors. A fuller sketch of Galileo's argument appears in (Gaukroger, 1978, pp. 189-198). For more detailed analyses of the argument and diagnoses of its flaws, see (Hill, 1984) and (Finocchiaro, 2003). Palmieri (2008) defends the view that Galileo's extrusion argument is in fact plausible given Galileo's understanding of the angle of contact between a circle and a line tangent to it, so that the defect in Galileo's argument only becomes apparent when contrasted with a more recent, Newtonian analysis of the situation. For further sources, see *ibid.*, pp. 446-447 n. 4.

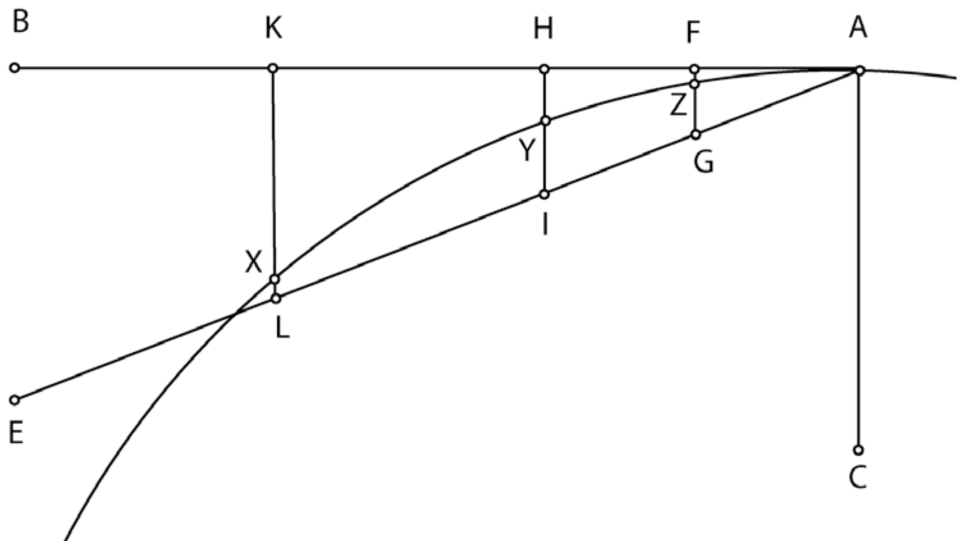

Figure 3.1: Horn angle BAX formed by the surface of the Earth AX and the tangent AB. As we approach A from B, the distance to the Earth vanishes.

Salviati moves on to argue that so long as the body has any tendency to fall towards the center of the Earth, no matter how small, this would in fact suffice to return it to the Earth if it were projected some distance along the tangent. For suppose some body is at rest at point A on the Earth's surface AX (see Figure 3.1). If the body were projected along the tangent AB, it would move with constant speed in the direction of the tangent. Therefore equal times may be represented by equal distances along AB; choosing point K twice as far from A as point H is, we may say that one unit of time has elapsed in the body's moving from A to H and another unit in moving from H to K. We will also choose F so that AH = 2AF.

Galileo represents the downward speed which the body would have acquired in free fall by parallel line segments perpendicular to AB and included in angle BAE: in particular, FG, HI, and KL. That is, at the time the body has arrived at H, the body would have reached a speed corresponding to segment HI in free fall, whereas it would have reached a speed corresponding to KL by the time it had reached K. This method of representation respects the key idea that in cases of constantly accelerated motion such as free fall, equal increments of speed are added in equal times. For we see that since AK = 2AH and since triangles AHI and AKL are similar, KL = 2HI. Or, in other words, in one unit of time the body has traveled to H and would have acquired downward speed HI in free fall, and in two units of time the body has traveled to K and would have acquired downward speed 2HI in free fall.

So far we have only compared how far the body moves along the tangent AB versus how much speed the body would have acquired in free fall over the same period of time. Now we must compare how much speed the body would have acquired in free fall with the distance of the body from the surface of the Earth at the corresponding time. As we approach the initial time, we consider the body at K, at H, and at F. Taking the speed KL as a unit, the corresponding speeds are 1, $\frac{1}{2}$, and $\frac{1}{4}$. In fact, each time we decrease the distance from A by half, the speed again decreases by half. However, by inspection of the diagram we see that the distances to the Earth's surface KX, HY, and FZ are decreasing much faster; that is, HY is much less than half of KX, and FZ is much less than half of HY.[8] We may continue to decrease the distance from A by one half, and each time the distance from the surface of the Earth will decrease by much more than one half. Therefore at some time when the body is sufficiently close to A, the speed acquired in free fall will suffice to return the body

[8]Strictly speaking, Galileo seems to be getting the distances to the surface of the Earth wrong here. The distance to the Earth at K is not given by KX but by the line segment whose first point is K and whose other point is the place of intersection between the circle and the line connecting K with the center of the circle. However, for very small quantities AK, AH, AF, *etc.*, the difference between the two representations of the distance to the Earth vanishes. For now we will ignore that difference.

to the Earth's surface. It follows that the body in fact stays at rest upon the surface. The gist of the argument is well summarized by Salviati: "For the distance traveled being so extremely small at the beginning of its separation (because of the infinite acuteness of the angle of contact), any tendency that would draw it back to the center of the [Earth], however small, would suffice to hold it on the circumference" (Galileo, 1967, pp. 194–195).

I have tried to formulate the extrusion argument in a way that renders its defect as apparent as possible (at least from our post-Newtonian point of view).[9] Salviati does manage to relate the position of the body along the tangential axis to the amount of speed the body would have acquired in free fall at the corresponding time. However, Salviati never shows us any time at which the actual distance fallen equals or exceeds the distance to the Earth's surface; his argument simply never takes into account how far the body falls. Whether the body actually does far fall enough depends on the strength of surface gravity. We must take care not to get fooled by the diagram into thinking otherwise: it may look as if the body does fall far enough, since, for example, HI is longer than HY and contains HY as a proper part. But one must recall that HI represents a speed, not a distance.

The fact that Galileo's extrusion argument is flawed makes my enterprise here somewhat delicate, since I cannot appeal to the argument as a witness to a successful application of geometry to physics. Nonetheless, I believe that in defense of his argument Galileo makes a number of general observations and suggestions which give insight into the applications of geometry. Part of my task will be to reveal these

[9]See (Palmieri, 2008) for an analysis which attempts to render the extrusion argument plausible from within Galileo's framework.

observations and suggestions.

In contrast to our contemporary assessment of the extrusion argument, the interlocutors of the *Dialogue* seem to regard the argument as at least valid if not sound. This has as a consequence that over the course of the philosophical argument which contains Galileo's response to Simplicio and forms the target of my analysis, everyone assumes the following conditional: if a line tangent to the surface of the Earth touches the Earth at a point—thereby forming a horn angle with it—then bodies resting on the Earth's surface will not be thrown off by its daily rotation. The debate in effect concerns whether one can establish the antecedent of this conditional, not whether the consequent follows from it.[10] For purposes of analyzing Galileo's philosophical argument—I will henceforth refer to it as "the target argument"—I will also accept this conditional.[11]

3.2 Simplicio's Attack and Salviati's First Line of Defense

As we saw in the opening lines of the chapter, Simplicio objects to the extrusion argument with the criticism that "mathematicians may prove well enough from their principles that a sphere touches a plane at a point... but when it comes to matter, things happen otherwise." Simplicio's claim is that Salviati has made an illegitimate

[10] I do not mean to suggest that Galileo's interlocutors are ignorant of the various further premises on which the conclusion depends, only that they tacitly accept them. The only premise under contention concerns whether a tangent to the Earth's surface touches it at a point, so that if that premise were granted, the conclusion would also be granted.

[11] The philosophical argument I am analyzing takes place at (Galileo, 1967, pp. 203-210) and (Galileo, 1998a, pp. 219-227).

appeal to the premise that a sphere touches a line tangent to it at a single point. Salviati needs this premise to establish that a line tangent to the Earth touches the Earth at a single point, thus forming a horn angle with it. Of course, one also needs the premise that the Earth is a sphere. One might have expected Simplicio to question the sphericity of the Earth, but he does not—at least not at first. Instead, he focuses his attack on the mathematical premise, claiming that when it comes to sensible matter, things happen otherwise than that a sphere touches a plane tangent to it at a point.

Simplicio's objection is not without precedent. As we saw in §§1.1 – 1.2, the Jesuit Aristotelians Alessandro Piccolomini and Benedict Pereira had expressed similar views in their works. Recall that Piccolomini had written in his commentary on the pseudo-Aristotelian *Mechanics* that "Even if celestial bodies are free of every imperfection and are perfectly round, nonetheless they cannot be touched in this way by a straight line without the contact comprehending some interval."[12] Pereira in his *De Communibus Omnium Rerum Naturalium Principiis* had similarly denied that a spherical substance touches a line at a point, even if a sphere *per se* does touch a line at a point.[13] Educated 17^{th} century readers would presumably take the fact that Simplicio's objection echoes the views of Piccolomini and Pereira as a signal that Galileo intends to engage with these thinkers in this part of the *Dialogue*.[14]

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[12]For the original Latin and brief discussion, see Chapter 1 n. 18. See also Biringucci's Italian translation at (Piccolomini, 1582, p. 38).

[13]See §1.1 for a discussion of the relevant passage in Pereira's work. More accurately, Pereira asserts that while spheres *per se* touch a plane at a point, a circle which is in some substance does not touch a straight line at a point. I think it is uncontroversial that Pereira means also to imply that a sphere inhering in a substance does not touch a plane at a point. For further discussion of Pereira's view see also (Feldhay, 1998, pp. 92-94) and (De Pace, 1993, pp. 75-120).

[14]Galileo does not mention Piccolomini or Pereira by name, so the evidence that he is replying

Piccolomini, Pereira, and like-minded Jesuit Aristotelians were among the chief opponents of the mathematization of physics and therefore a natural focus of Galileo's polemical attacks. Piccolomini and Pereira had both endorsed the view that the mathematical disciplines (*i.e.*, geometry and arithmetic) are not proper sciences, since they fail to meet the criteria Aristotle laid down for sciences in the *Posterior Analytics*.[15] Specifically, they argued that while proper sciences only contain demonstrations which give the causes of conclusions demonstrated, the mathematical disciplines do not. A plausible further conclusion—and one which Feldhay believes is evident in Piccolomini's work—is that disciplines such as astronomy which apply geometry cannot be sciences either (Feldhay[1998], pp. 84-5). From a particular Jesuit Aristotelian viewpoint, to mathematize physics would be to make it unscientific.[16]

Galileo does not engage explicitly in the *Dialogue* with Piccolomini's or Pereira's arguments that mathematical disciplines are themselves not proper sciences. To a great extent, the way Galileo engages with these thinkers is shown over the course of our target argument. Part of my aim will be to reconstruct Galileo's response to the sort of position held by Piccolomini and Pereira.

The argument proceeds as Salviati provides a geometric proof that a sphere

to them in this part of the *Dialogue* remains somewhat circumstantial. However, it seems to be the consensus view; see (Galileo, 1998b, p. 510) and (Feldhay, 1998, p. 129).

[15]For Pereira's own statements, see (Pereira, 1586, p. 26); for discussion, see (Mancosu, 1996, p. 13) and (Feldhay, 1998, p. 92). For quotations of Piccolomini's statements and a discussion of them, see (Feldhay, 1998, pp. 83–84, 136–137). See Chapter 2 of the present work for further discussion of the conception of science described in the *Posterior Analytics*.

[16]The debates surrounding the question whether the mathematical disciplines are proper sciences is known as the debate "*de certitudine mathematicarum*" after the title of Piccolomini's work *De Certitudine Mathematicarum Disciplinarum*, an appendix to (Piccolomini, 1547). There is by now a large literature in English about this debate; cf. (Mancosu, 1996, Ch. 1) and (Feldhay, 1998, §1). For extensive further literature in English and other languages, see (Feldhay, 1998, p. 135 n. 9).

touches a plane at a point. From the point of view of geometric rigor, Salviati's proof seems unobjectionable and not worthy of detailed interpretation. However, several of Salviati's remarks suggest that he means his proof to count as a scientific demonstration in the sense of the *Posterior Analytics*. In defense of this reading, we may note that Simplicio offers a proof of the lemma that a straight line is the shortest curve between two points, but Salviati rejects Simplicio's proof on the grounds that its middle term is less well established than the conclusion. Such an argument would run afoul of a necessary condition for a scientific demonstration.[17] Moreover, Salviati takes some pains to start his argument from the "definition" and "essence" of a sphere (Galileo, 1967, p. 204); this would appear to be another move towards persuading an orthodox Aristotelian that the criteria for scientific demonstration were being met.[18]

If Galileo were successful in giving a scientific demonstration that a sphere touches a plane (or a line) at a single point, this should suffice to convince an orthodox Aristotelian that our judgment that the Earth touches a plane at a single point counts as scientific knowledge—for an orthodox Aristotelian would hold that there is a scientific demonstration that the Earth is a sphere.[19] This, together with the geometrical demonstration just given, would yield a scientific demonstration that the Earth touches a plane (or a line) at a single point. Insofar as Simplicio's objection is inspired

[17]This is the requirement that demonstrative understanding proceed from items that are "more familiar than" the conclusions. This requirement ensures that one does not accept a conclusion as known demonstratively on the basis of a demonstration whose premises are less well established than the conclusion itself is. Cf. (Aristotle, 1993, pp. 2-3, 71b10-72a6).

[18]The demand is that the conclusions of a scientific demonstration proceed from premises in which properties are predicated of their subjects *per se*. See (Aristotle, 1993, pp. 6-8, 73a21-74a4). For Barnes's commentary on this requirement, see *ibid.*, pp. 112-114. For my own discussion of the requirement see Chapter 2, especially §2.1.

[19]Aristotle argues that the Earth is a sphere at 297a8-298a20 in his *De Caelo*; cf. (Aristotle, 1984a, pp. 488-489).

by Piccolomini's worries that we cannot know scientifically that a sphere touches a plane at a point, Salviati's mathematical demonstration would serve to defuse those worries. This suggests a strategy to use against opponents like Piccolomini: to show on a case-by-case basis that the geometric theorems actually needed in physics have scientific demonstrations in the sense of the *Posterior Analytics*.[20] Galileo does not pursue that general strategy here; for his immediate purposes it suffices to give the one demonstration.

Simplicio does not make any statements concerning the scientific status of Salviati's geometrical demonstration that a sphere touches a plane at a point. Simplicio seems to grant that the demonstration is fine by geometric standards, but he insists that it proves the theorem "for abstract spheres, but not material ones" (Galileo, 1967, p. 206). He illustrates this shortcoming of the proof by raising apparent counterexamples to the theorem taken from the material world: for instance, a metallic sphere resting on a plate would either become deformed or mash the plate, causing the two objects to touch over some surface with positive area. This is reminiscent of Piccolomini's statement about celestial bodies given some paragraphs ago. Simplicio also points out that material spheres and planes are "hard to come by" (*loc. cit.*), and this suggests a different kind of criticism of the premise that a sphere touches a plane at a point which we will consider in a moment.

Salviati's geometric demonstration now comes into play, for Salviati has shown

[20]It is unlikely that Galileo's opponents would agree that Salviati's mathematical demonstration is scientific. They would likely object that since Salviati's demonstration is a *reductio ad absurdum*, it therefore cannot give the cause of its being the case that a sphere touches a plane at a point. Nevertheless, I claim that Salviati's Aristotelian rhetoric has the purpose of trying to persuade an Aristotelian opponent that his proof is in fact causal. For more on the status of *reductio* proofs in this debate, see (Mancosu, 1996, pp. 24-28).

that any surface which touches a plane at more than a single point is *a fortiori* not a sphere. If Simplicio's supposed "metallic sphere" or Piccolomini's celestial body touches a plane at more than one point, then it is really no sphere at all. In this way, Salviati has the resources to deny any putative counterexample to the geometric theorem taken from the material world or otherwise. To put the point another way, Salviati is now in a position to deny the alleged failure of what the geometers prove to be the case about a given type of curve to hold of that type of curve in nature. Any real evidence that a geometric theorem about (for example) parabolas doesn't hold of a given trajectory is *ipso facto* evidence that the trajectory in question is not a parabola. It is emphatically not evidence that parabolas in nature fail to have the properties which geometers prove them to have. The moral of Salviati's lesson is that Simplicio should not call such "metallic spheres" spheres in the first place: "[W]hen you want to show me that a material sphere does not touch a material plane in one point, you make use of a sphere that is not a sphere and of a plane that is no plane" (*loc. cit.*).

Salviati points out that there is still a consistent position open to Simplicio which was suggested by Simplicio's remarks about material spheres and planes being hard to come by. Simplicio can admit that if there were a material sphere and a material plane, they would touch each other at a single point. This is shown by the geometric proof. However, Simplicio can simultaneously deny that there are any material spheres or planes whatsoever. Simplicio accepts this redescription of his position, saying that this must be "the philosopher's proposition" (*ibid.*, p. 207).

Although Simplicio's view is seen to be consistent by this point in the argument, it raises deep worries for anyone defending Galileo's position in the *Dialogue*. First, if Simplicio is correct then it follows that the Earth is not a sphere and the extrusion argument does not go through. We will discuss this problem in the next section. Moreover, we could presumably state Simplicio's view more fully by saying that there are no geometric points, lines, or surfaces in nature: no body has a geometric shape; no body has a trajectory which is a geometric curve; *etc.* Informally speaking, this is a view according to which the subject matters of geometry and physics are *mismatched*. That Simplicio holds this view in general is suggested by the way he originally formulates his objection: "After all, Salviati, these mathematical subtleties are true in the abstract, but applied to sensible and physical matter they fail to correspond". It is further confirmed when Simplicio denies that the sphere, the pyramid, the shape of a horse, or the shape of a grasshopper can ever be perfectly obtained in the material world (*ibid.*, p. 209). If one holds this position, one might plausibly conclude that it is never appropriate to build arguments in physics which appeal to geometric theorems—for there are no objects in nature for the geometer to reason about. All that natural philosophers actually do is to mistakenly consider given material objects to have geometric shapes and then draw conclusions about the objects which may or may not be true. From this point of view, the proposal that physics be mathematized looks disastrous.

Salviati attacks the view that the subject matters of geometry and physics are mismatched head on by trying to show that Simplicio has an overly narrow conception of those subjects. Salviati's strategy at this point in the argument is *inflationary*: it is to show that more belongs to the provinces of geometry and physics than Simplicio recognizes. Salviati begins by explaining the mismatch in a manner intended to be congenial to Simplicio: it is "because of the imperfection of matter [that] a body which ought to be perfectly spherical and a plane which ought to be perfectly flat do not achieve concretely what one imagines of them in the abstract" (*ibid.*, p. 207). Spelled out a bit further, Simplicio seems to think that the geometer studies only a handful of what we will call the "traditional shapes"—lines, conics, *etc.* The geometer's traditional shapes do exist in the abstract. But because material bodies are concrete they are subject to many accidents, and therefore they never strictly have a traditional geometric shape. But then it follows that geometry never studies any of the shapes actually obtained in nature.

In response, Salviati tries to force Simplicio to recognize two new categories of objects: abstract shapes above and beyond the geometer's traditional shapes, and concrete objects which either do have or at least may have the geometer's traditional shapes. To find examples of the former, Salviati has Simplicio consider the abstract correlates of the so-called "imperfect metallic spheres" and "imperfect metallic planes" which Simplicio allows to exist in nature: "I tell you that even in the abstract, an immaterial sphere which is not a perfect sphere can touch an immaterial plane which is not perfectly flat in not one point, but over a part of its surface" (*loc. cit.*). These abstract shapes Simplicio is being pressured to admit may resemble spheres and planes quite closely, though it is important to distinguish them from spheres and planes. Simplicio is also forced to admit that highly irregular shapes such as that of a rock broken by a hammer exist, namely by pointing out any nearby rock broken by a hammer (*ibid.*, p. 210). Presumably these highly irregular shapes also exist in the abstract. The upshot Salviati seems to be pressing is that these non-traditional shapes exist in the abstract as well as in the concrete, and moreover that there are facts about these shapes such as what the locus of contact would be between such shapes. The final suggestion, though Salviati is never explicit about it, is that it is in principle open to the geometer to investigate these latter facts.

Salviati also wants to give Simplicio some probable reasons for thinking that some bodies in nature actually do have geometric shapes, or at the very least that they *may* have them. Part of this work is done when Simplicio admits that highly irregularly shaped rocks broken by a hammer have whatever shape they have perfectly. This shows that it is possible for some highly irregular shapes to exist in nature, since after all the bodies with such irregular shapes are actual. Moreover, Salviati argues (*ibid.*, p. 209), of all shapes it is easiest to obtain simple shapes like spheres, since one may obtain a sphere to as high an approximation as one likes by, *e.g.*, spinning a spheroid in a circular hole which is slightly smaller than it. Sagredo, the third interlocutor in the *Dialogue*, draws the conclusion: "[I]f of shapes which are irregular, and hence hard to obtain, there is an infinity which are nevertheless perfectly obtained, how can it be right to say that the simplest and therefore the easiest of all is impossible to obtain?" (*ibid.*, p. 210). The suggestion is that Simplicio's assertion that there are no material spheres or planes whatever oversteps his evidence.

In summary, Simplicio originally objected that there was a failure of geometric theorems to hold of material, sensible objects. This is shown to be a confusion; the apparent counterexamples to the geometric theorems rest on attributing some shape to the material objects which they do not in fact possess. But there is a consistent and deeply troubling position lurking in the background: it is the view on which no geometric points, lines, or surfaces (in general, *geometric objects*) exist in nature. Salviati argues that this view is false, since (a) for any body there is an abstract individual which corresponds to the material object's shape,[21] and (b) it is possible, perhaps even probable, that some bodies do have some (simple) traditional geometric shapes. We may therefore conclude that there are some shapes which exist both materially, as the shapes of bodies, and in the abstract. Once we have arrived this far, Salviati wants to drive home the point that whatever facts hold of a given individual in virtue of its shape hold whether or not that individual is abstract or concrete:

> [W]henever you apply a material sphere to a material plane in the concrete, you apply a sphere which is not perfect to a plane which is not perfect, and you say that these do not touch each other in one point. But I tell you that even in the abstract, an immaterial sphere which is not a perfect sphere can touch an immaterial plane which is not perfectly flat in not one point, but over a part of its surface, so that what happens in the concrete up to this point happens the same way in the abstract: and it would be novel indeed if computations and ratios made in abstract numbers should not thereafter correspond to concrete gold and silver coins and merchandise.[22]

By this point in the argument, the lack of correspondence which Simplicio alleged is supposed to have vanished. If two material objects have shapes which make for

[21]To reiterate, by pointing out the existence of abstract individuals corresponding to the shapes had by material bodies, I believe Galileo is trying to persuade us that the shapes in question fall at least in principle into the subject matter of geometry. (Finocchiaro, 2003, pp. 241) seems to argue for a related but slightly different interpretation; namely, that Galileo is exhorting us to adopt a regulative principle either to find or (if necessary) to invent a geometric representation corresponding to physical situations. In any case, I believe Finocchiaro and I agree that Galileo is urging an expansive conception of the scope of geometry. However, I think Finocchiaro does not account for the role that idealization plays when finding precise geometric representations is not feasible. See §3.3.

[22]See *ibid.*, p. 207. I have modified Drake's translation so that it better matches Galileo's punctuation. In particular, the colon before the last sentence of the passage indicates that Galileo took it to be tightly connected to what came before.

such-and-such surfaces of contact, then two abstract objects with the same shapes have the very same surfaces of contact, and vice-versa.[23] Simplicio's alleged lack of correspondence arose because he equivocated in his use of the word "sphere".

3.3 Aggravated Problems for Salviati

Though Salviati's response seems to handle Simplicio's blanket rejection of the application of geometry to physics rather well, it opens the door to serious objections in the context of the extrusion argument discussed in §3.1. That argument took as a premise that the Earth is a sphere, but someone might now object on Simplicio's behalf that the Earth isn't really a sphere, as a glance towards the Alps should confirm for all involved.[24] "After all," she might say, "we can't go calling bodies which aren't perfectly spherical 'spheres', else geometric conclusions we might draw on the assumption that these bodies are spheres might well lead us astray."

Given the tenor of Salviati's response to Simplicio described in §3.2, one can perhaps imagine Salviati granting this objection and reformulating the extrusion ar-

[23]That the statement holds also in the "vice-versa" formulation is shown by corresponding geometric demonstrations. McMullin construes the sentence after the colon in the last quotation as showing that Galileo assumes a further correspondence between geometry and the physical world, namely "that the concepts needed to geometrize space and time are, in fact, the simple ones drawn from everyday sense-experience, the ones for which Euclid had long ago provided a definitive grammar" (McMullin, 1985, p. 252). Although I agree that Galileo takes physical space to be Euclidean, I do not see how the quotation indicates that assumption. Moreover, McMullin writes that the quotation shows that for Galileo "the primary qualities which characterize body are assumed to correspond exactly to our everyday notions of space and time" (*loc. cit.*). On my reading, this misses an alleged lack of correspondence which Galileo is taking very seriously: the lack of correspondence claimed by one who believes that geometric objects do not exist in nature.

[24]Simplicio does not seem to want to reject the exact sphericity of the Earth at this point in the *Dialogue*, perhaps because this would go against orthodox Aristotelianism. Earlier in the First Day of the *Dialogue*, however, Simplicio does seem to reject the view that the Earth is perfectly spherical. Cf. (Galileo, 1967, p. 69).

gument accordingly. Salviati would point out that although the Earth is not spherical, it does have whatever shape it has perfectly, so that in principle there is some fact of the matter for every point on the surface of the Earth what its contact with tangent lines would be—supposing, that is, that there is a unique tangent defined for each point on the Earth's surface. To proceed in this manner, one would first need a fully precise geometrical description of the Earth's shape. One would then need some geometric tools for investigating the tangents to all the points on the surface.

The problem with this way of proceeding is that it is wholly intractable—both for the natural philosophers of the 1640's and for us today. In the first instance, there is the problem of generating a precise representation of the shape of the Earth. The interlocutors in the 1640's did not have any such representation, and arguably we don't either.[25] Moreover, the question again arises whether the surface of the Earth can really be considered a geometric curve: whether it can depends partly on the actual shape of the Earth and partly on one's notion of which curves count as geometrical. As Carla Rita Palmerino helpfully reminds us (Palmerino, 2001, p. 402), in his earlier work *The Assayer* Galileo had drawn a distinction between regular curves and irregular curves on which irregular curves seem to fall outside the scope of geometry:

> [R]egular lines are called those that, having a single, firm, and determinate description, can be defined and whose accidents and properties can be demonstrated. But the irregular lines are those that, not having any determination whatsoever, are infinite and casual, and thus indefinable,

[25] For one thing, the surface of the oceans changes shape too quickly; by the time you had one fully precise representation, the Earth's shape would already be different. Even more static landscapes might pose some difficulties: does anyone know what the surface of the Earth is at a suitably random spot in the Gobi Desert or the Himalayas?

> and of which therefore no property can be demonstrated and nothing, in sum, can be known.[26]

If the shape of the Earth's surface is irregular in this sense—and in fact it seems to be—then at any rate no human being could demonstrate or otherwise come to know any of its properties. Since geometry is a demonstrative science, it follows that in this case the shape of the Earth would not comprise any part of geometric investigation. But this effectively reopens the question of the applicability of geometry to physics, since it raises the possibility that the highly irregular shapes possessed by actual bodies have no geometric definitions.

Although Salviati does not explicitly consider this line of objection, he seems aware of it and offers what looks to be a response to it:

> Do you know what does happen, Simplicio? Just as the computer who wants his calculations to deal with sugar, silk, and wool must subtract the weight of the boxes, bales, and other packings, so the geometrical philosopher [*filosofo geometra*], when he wants to recognize in the concrete the effects which he has demonstrated in the abstract, must deduct the impediments of matter [*impedimenti della materia*], and if he knows how to do so, I assure you that things are in no less agreement than arithmetical computations. The errors, then, lie not in the abstractness or concreteness, not in geometry or physics, but in a calculator who does not know how to make a true accounting.[27]

In this passage, Salviati makes a dramatic switch of tactics in his response to Simplicio's objections. He is no longer trying to get Simplicio to grant the general point that some bodies do have precise geometric shapes, nor is he trying to get

[26]Cf. *loc. cit.*; I am here using Palmerino's translation. For Drake's translation, see (Galileo, 1957, p. 241).

[27]See (Galileo, 1967, p. 207). This longer passage follows immediately upon the long passage quoted at the end of the last section. I have made some changes to Drake's translation.

Simplicio to grant that the Earth's shape is a geometric curve. Rather, he argues that the proper way to assess whether the extrusion argument succeeds is to determine whether the impediments of matter have been deducted, where this latter operation is to be understood on analogy with the way a merchant has to make sure that he has subtracted the weight of the packaging when he wants to determine how much sugar he has (*i.e.*, when he calculates the tare weight). Salviati's new approach to the problem is *idealizing* rather than *inflationary*, though it takes some work to see what the idealizing approach amounts to here.

Koertge (1977) argues that "impediment" and "accident" are important technical terms for Galileo and gives an analysis of Galileo's use of such terms over the course of his career. On Koertge's analysis, one class of impediments to which Galileo repeatedly refers consists in the "discrepancies between the mathematical approximation and the real situation" (*ibid.*, p. 392). The discrepancy between the actual shape of the Earth and that of a true sphere is one of Koertge's examples. Another important example is the discrepancy between so-called Galilean gravity, which is assumed to pull along parallel lines towards the surface of the Earth, and real gravity, which is assumed to pull along lines which converge at the center of the Earth. In keeping with Koertge's analysis, I take it that when Salviati speaks of the impediments of matter in the passage cited above, he is referring to and thereby acknowledging the discrepancy between the shape of the Earth and the shape of a true sphere. The problem is then to see what we are being enjoined to do when we are urged to deduct that discrepancy.

If we take Salviati's analogy with the merchant seriously, then it would seem we need first to quantify the discrepancy, then to show that once the resulting quantity is taken into account in our calculations the effects demonstrated remain the same (*i.e.*, bodies are not thrown off the surface of the Earth even supposing its daily rotation). However, in the case at hand this looks to be a non-starter: if we cannot obtain a precise geometrical or otherwise quantitative representation of the shape of the Earth, then we also cannot obtain a precise geometrical or otherwise quantitative representation of the difference between the Earth's real shape and that of a sphere. In the case of the merchant this is not a problem, since the merchant can easily weigh the package and subtract the weight to find the quantity of sugar. Similarly, suppose we are reasoning about a balance in Venice assuming Galilean gravity. If we know the distance of Venice from the center of the Earth, we should be able to quantify the difference in the force on the arms of the balance due to Galilean as opposed to real gravity. It shows what a master polemicist Galileo is that he likens the very difficult case at hand concerning the difference of the Earth's shape from the sphere's shape to simple applications of arithmetic carried out by merchants every day.

Since Salviati cannot quantify the discrepancy between the Earth's shape and the sphere's, he instead resorts to a non-geometric argument meant to show that if the Earth does in fact have some highly irregular shape, it is nonetheless extremely likely that any plane tangent to the Earth touches it in a single point, since "anyone who got to the bottom of this matter would find that it is a great deal harder to discover two bodies which touch with parts of their surfaces than with a point alone" (Galileo, 1967, p. 208). By having Salviati adopt this strategy, Galileo suggests a certain account of why it is legitimate to take on the assumption that the Earth is a sphere in the extrusion argument: it is legitimate because if it is objected that the Earth is not really a sphere, it can be shown *with high probability* that the conclusions drawn from that assumption which are actually used in the extrusion argument continue to hold even allowing for deviations of the real world from that assumption.[28]

Astute readers of this part of the *Dialogue* will also recall that immediately after Simplicio objects to the assumption that a sphere touches a plane tangent to it at a point, he tells Salviati that he thinks a plane would contact the surface of the Earth for "tens and hundreds of yards touching the surface of water, let alone the ground, before separating from it" (*ibid.*, p. 203). Salviati argues that in this case it follows *a fortiori* that bodies resting on the Earth's surface would not be thrown from it, "for if even assuming that the tangent lies removed from the earth except at one point, it has been proved that the projectile would not be separated, because of the extreme acuteness of the angle of contact... how much less cause will it have for becoming separated if that angle is completely closed and the surface united with the tangent?" (*loc. cit.*). Thus the conclusion of the extrusion argument is taken to hold whether or not a plane tangent to the Earth touches it at a single point—the suggestion is that it holds whatever the shape of the Earth happens to be. This points to a different account of why it is legitimate to assume the Earth is a sphere

[28]Palmerino argues for a different interpretation of Galileo's argument, writing that on Galileo's view "[A] mathematical truth... amounts to a physical possibility. This is why Salviati's answer to Simplicio begins with a geometrical proof and ends with a probabilistic argument. Galileo's spokesman demonstrates, first, that the mathematical proposition '*sphaera tangit planum in puncto*' [a sphere touches a plane at a point] is true. Then he moves on to show that the same proposition holds true for physical objects also." Cf. (Palmerino, 2001, p. 404). As should be clear from the last section, I disagree with Palmerino's analysis: The mathematical proof that a sphere touches a plane at a point shows that the claim holds for all spheres, physical or otherwise. The probabilistic argument described in this paragraph does not aim to show that it is *likely* that spheres in nature touch a plane at a point, but rather that it is likely that whatever shape the Earth in fact has, the contact of that shape with a plane (or a line) is a point.

in the extrusion argument: because if it is objected that the Earth is not a sphere, it can be *demonstrated* that the desired conclusion of the extrusion argument holds even allowing for deviations of the real world from that assumption.

We can now formulate Salviati's idealizing strategy in more general terms. Recall that the general problem for the application of geometry in physics was raised by the specter of a mismatch between the subject matters of geometry and physics. The specific problem for the extrusion argument is raised by the worry that the shape of the Earth is not a sphere and not even a geometric curve. The idealizing strategy is compatible with there being a mismatch and hence no geometric curve which corresponds to the shape of the Earth. However, the proponent of idealization argues that we can in a physical argument assume away an alleged mismatch—for example, by assuming the Earth to be perfectly spherical—provided that deviations of the real world from our assumptions can be shown not to affect the outcome of the physical argument. If by "shown" in the last sentence we really just mean "shown to high probability", we obtain a more relaxed position in the spirit of Salviati's probabilistic argument that planes tangent to irregular shapes touch the latter at a point. If by "shown" we mean "deductively proved", we obtain a much stricter standard.

Before ending the analysis of the target argument I would like to consider two final questions. The first question is whether Salviati has given convincing reasons to think that the extrusion argument goes through even supposing that the Earth may be an irregular spheroid. I contend that it does not. The key to the extrusion argument is that the angle formed by the tangent of the projectile sitting on the Earth's surface and the surface itself is either infinitely small (in the case of a horn angle) or zero (in the case the two curves coincide over some interval).[29] However, it is not difficult to imagine that at some region on the Earth's surface there is a sharp triangular edge. We might imagine a perfectly sharp cliff. If we consider a ball sitting at the edge of the cliff, the angle formed by the tangent to the projectile's trajectory and the Earth's surface is positive, even 180 degrees. Under that supposition the extrusion argument breaks down. This kind of counterexample to Salviati's argument suggests that the real issue is not whether the tangent touches the Earth's surface at a single point.

The second question is: How well does Salviati's argument respond to the worries Pereira and other Aristotelians are raising about the application of geometry to physics? Salviati's argument is the most powerful against an orthodox Aristotelian opponent who is convinced we possess scientific knowledge that the Earth is a sphere and becomes convinced by Salviati's demonstration that we have scientific knowledge that a sphere touches a plane tangent to it at a point (*i.e.*, "scientific" in the sense of the *Posterior Analytics*). Because Salviati's proof proceeds by *reductio* it is unlikely that many of Salviati's opponents fall into that category. However, let us suppose this worry were overcome and a scientific demonstration of the geometric theorem were found. There remains the pressing worry of the unorthodox Aristotelians who endorse "the philosopher's proposition" that spheres are never to be found in nature, even that no geometric curves exist in nature. Salviati first urges the Aristotelian to recog-

[29]Galileo usually talks of the angle formed by the tangent to the surface of the Earth and that very surface. However, if the Earth has some sharp triangular region, strictly speaking it would have no tangent at that point. However, that seems to be besides the point, since the desired tangent is really the tangent to the trajectory of the projectile and it is reasonable to assume that *this* curve does have a tangent.

nize more geometric curves and more natural substances whose shapes correspond to geometric curves. What Salviati does not supply the Aristotelian, however, are any definitions either of the geometric curves or of the natural substances which could serve as the premises of scientific demonstrations. The worry would then be just the one Galileo raised in the *Assayer* that there could be no science of such curves, nor any demonstrative knowledge that natural substances correspond to those geometric curves. For instance, even if it is granted that the rock broken at random with a hammer has some geometric shape, how will we ever be able to demonstrate that the rock has that shape? That the rock has the shape it has seems to be accidental in a way which guarantees for the Aristotelian that there can be no science of its shape. On the other hand, Salviati's idealizing gambit is also unlikely to satisfy Aristotelian worries, since all premises of scientific demonstrations are required to be not merely true but necessary.[30] Salviati seems to be recommending to us that under certain circumstances we may employ false premises, but this is thoroughly contrary to Aristotle and even to unorthodox Aristotelianism. Aristotelians of Pereira's ilk would consequently not be much persuaded by Salviati's defense of the application of geometry to physics.

3.4 Conclusions

Having discussed Galileo's defense of the extrusion argument, I now want to reflect philosophically on that defense, treating it as a kind of case study. While Galileo's

[30] See (Aristotle, 1993, pp. 2-3 & 6). A 2 lays down the criterion that demonstrations proceed from truths, and A 4 that they proceed from necessities.

defense fails to shore up the argument for which it was developed, I believe it succeeds in other respects. Galileo makes a number of helpful suggestions about how to respond to an opponent challenging the applicability of geometry. Moreover, reflection on what Galileo *does* (sometimes without *saying* he is doing it) yields insight into some general features of the problems of the applicability of mathematics.

First, recall what Galileo's opponents had initially maintained: (i) geometric demonstrations suffice to establish truths about geometric objects, *e.g.*, suffice to show that a sphere touches a plane at a point; (ii) geometric theorems are false if understood as claims about things in nature, since for example natural spheres such as stars or planets touch a plane in a surface. Galileo shows that even if such views are granted to be consistent, they are nonetheless unstable. There is at least an apparent problem with consistency, since those who accept geometric demonstrations should also accept the demonstration that an object in nature which is granted to be a sphere touches a plane at a point.[31] But in that case they would appear to accept that spheres in nature both do and do not contact a plane tangent to them at a point. The natural way to avoid the inconsistency is to claim some kind of equivocation, so that "being a sphere" or "having a shape" mean different things in geometry and physics. Someone who avoids inconsistency in this way already gives up on the idea that nature contains counterexamples to geometric results. Then the problem with geometry is not that nature contains counterexamples to it, but rather that objects in nature do not meet the requirements the geometer lays down on his objects. By this point, we have arrived at a new criticism of the applicability of geometry: ge-

[31] Cf. §1.2.

ometric objects—geometric points, lines, and surfaces—don't exist in nature. This latter criticism is more suitable as a challenge to the applicability of geometry than the one with which we started, since it is internally coherent while conserving the kernel of the original complaint.

Second, some readers of the *Dialogue* may be surprised to see that after he entertains a general challenge to the applicability of geometry to physics, Galileo goes on to a lengthy discussion of the proof of the theorem that a sphere touches a plane at a point, a theorem he believes he needs in order to make the extrusion argument work. Part of what motivates Galileo to do this, as we saw in the preceding paragraph, is that Galileo can use the proof to reformulate his opponents' objection. However, I believe there is also a more ordinary justification for turning to the geometric proof and its epistemic status. Galileo wants to use the claim that a sphere touches a plane at a point as a premise in an argument, and so he is doing what is necessary to exhibit his evidence for that claim. In other words, Galileo is defending what I (following Mark Steiner) refer to as a "deductive application" of geometry: *i.e.*, an application witnessed by a deductive argument taking a theorem of geometry as a premise and leading to a physical conclusion.[32] Since issues of mathematical proof and evidence are topics in their own right in the philosophy of mathematics, they tend not to be considered also problems of the applicability of mathematics. Galileo's discussion shows in a natural way how issues of mathematical proof and evidence are

[32] I am unsure whether my usage of "deductive application" precisely matches Steiner's. Steiner regards geometry as empirical, and so it may be that he does not view an application of geometry as a true application of mathematics. Cf. (Steiner, 1998, p. 29 n. 16). For present purposes I will rely on my own explanations of "deductive application" and "descriptive application" with apologies to Steiner if my usage departs from his.

also problems of the applicability of mathematics.

In order to build a deductive argument which witnesses a deductive application of geometry, Galileo must relate the subject matter of geometry to the physical situation he is addressing. The physical situation involves the Earth, bodies on or near the surface of the Earth, and their motions. Galileo wants to use geometrical means to say what these physical things are like, for example that the Earth is a *sphere* and that the trajectory of a body thrown from the surface of the Earth is a *line tangent to* the Earth. In doing so, Galileo is making a descriptive application of geometry: *i.e.*, he is relating the subject matter of geometry to the subject matter of physics for purposes of describing (whether accurately or inaccurately) the physical world.

I have just said that in order for Galileo to make a deductive application of geometry, he must make a corresponding descriptive application of it. I think it is plausible that the word "must" here indicates at least a practical necessity, even on the informal conceptions of deductive and descriptive applications we have so far considered. One might wonder whether the notions of deductive and descriptive applications could be formalized, so that a corresponding formal result relating the two notions would be possible. It is noteworthy that on a conception of logic as Aristotelian syllogistic—the dominant conception of logic in Galileo's time—there is a natural formalization and a corresponding result. Take a deductive application of geometry to be witnessed by a syllogism with one geometric premise and a conclusion containing at least one physical term. Then take a descriptive application of geometry to be witnessed by any predication relating a geometrical and a physical term. For there to be a syllogism meeting the requirements of a deductive application of geometry, the premise of the syllogism which isn't a theorem of geometry will have to relate one of the theorem's terms to a physical term in the conclusion. For the syllogism must have a term common to both premises (a middle term), meaning that one of the geometric terms in the theorem appealed to must appear in the syllogism's other premise. Also, if the syllogism is to prove something about a physical term, that term must appear somewhere in the premises. But the only way for it to appear among the premises is as a term related to the geometric middle term. Q.E.D.[33] In the case at hand, Galileo wants to prove something about the Earth using the premise that a sphere touches a line at a point. For this he needs a premise relating the term "Earth" to "being a sphere" or "touching a line at a point"; in actual fact, the premise he uses is "the Earth is a sphere".[34]

To claim that geometric objects do not exist in nature is at least *prima facie* to challenge the descriptive applicability of geometry. As I interpret Galileo's response to Simplicio, the bulk of what follows the discussion of the mathematical proof that a sphere touches a plane tangent to it at a point is an attempt to meet that challenge. We saw in §§3.2 – 3.3 that Galileo offers two strategies for defending the descriptive applicability of geometry against such a challenge. There is the inflationary strategy of getting his opponent to grant that there is more to geometry and to the physical

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[33]This argument is similar to Aristotle's argument against kind-crossing at A 7 in the *Posterior Analytics*. For Barnes's translation and analysis of the argument, see (Aristotle, 1993, pp. 12-13, 130-132). For my own analysis and discussion, see Chapter 2.

[34]I believe it is an open question whether there is an adequate way of formalizing the notions of deductive and descriptive applications of geometry in logical systems with greater expressive means than Aristotelian syllogistic. It is furthermore an open question whether there are interesting theorems relating the two. One difficulty is that in more expressive logical systems such as first-order logic, it is easy to build arguments using premises whose subjects have nothing in common. From the premise "$0 \neq 1$" and the premise "snow is white", we can conclude for example "$0 \neq 1$ & snow is white".

world than the opponent has so far recognized. There is also the idealizing strategy which tries to identify conditions under which one may legitimately use geometry to approximate some natural phenomena even when the phenomena do not correspond precisely to the geometric approximation. A common theme of the two strategies is that they involve coördinating our conceptions of geometry and physics.

Finally, in trying to meet the challenge to the descriptive applicability of geometry, Galileo encounters and transmits a pressure to admit more curves into scientific consideration than would traditionally count as part of the subject matter of geometry. This is a familiar theme from the 17^{th} century and beyond. The cycloid is a particularly well known example of a curve that was seen to be physically important by the latter half of the 17^{th} century but which some mathematicians following Descartes did not regard as properly geometric.[35] Over the course of the century, as mathematical investigations of the cycloid became more sophisticated, there was pressure to include the cycloid as a legitimate geometric object. Similarly, by pressuring us to admit more curves into scientific consideration than we might hitherto have recognized, Galileo aids a process by which geometry is made more descriptively applicable over time: geometry is made descriptively applicable to physics by means of an expansion of geometry itself.[36]

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[35] See (Bos, 2001, Ch. 29) and (Descartes, 2001, Bk. II).

[36] Similarly, in some cases geometry can be made applicable to the physical world by means of a manipulation of the physical world, as when Galileo suggests a mechanical process by which we could form a body into a sphere.

Chapter 4

Leibniz

In the preceding chapter, I examined Galileo's response to the Scholastic Aristotelians on the issue of the applicability of geometry to the study of nature. Taken as a group, Aristotle, Scholastic Aristotelians such as Benedict Pereira, and Galileo form a natural object of study for historians of philosophy and science on account of the great extent to which (chronologically) later members of the group engage with the arguments of those who came before. Crucially for my purposes here, the later members of the group respond to the earlier ones on the issue of the status of geometry and its proper uses in empirical inquiry. A particularly important bit of shared background is the theory of demonstrative knowledge developed in Aristotle's *Posterior Analytics*.

Despite the fact that Leibniz responds to Aristotle and Galileo in his philosophical and scientific work, Leibniz does not engage as directly with some of the arguments which occupied the earlier thinkers regarding geometry and its relationship to the study of nature. In particular, the theory of demonstrative knowledge developed in the *Posterior Analytics* does not appear to play much of a role in Leibniz's own understanding of the applicability of geometry to physics, and so it will largely drop out of my discussion in this chapter.[1,2] Nonetheless, I believe it is natural to turn to Leibniz at this point, for he deepens the worries about the applicability of geometry which Galileo confronts while at the same time attempting to justify the legitimacy of his own mathematical physics. It will be helpful to explain Leibniz's views by setting them in contrast to Galileo's, so I would like to begin with a brief reminder about the latter.

On the interpretation I offered in the preceding chapter, Galileo confronts the challenge that geometry and physics are mismatched subjects in the sense that geometric objects—points, lines, curves, and solids—do not exist in nature. This is understood to imply that no body has a shape corresponding strictly to any geometric surface, no motion corresponds strictly to any geometric line, *etc.* Galileo *rejects* this challenge as overstepping all available evidence and offers some reasons for thinking that it is probably false. However, Galileo recognizes that in many cases, the mathematical physicist will have to resort to approximations and take natural

[1] In this chapter I will cite the works of Leibniz using the following abbreviations: A = (Leibniz, 1923ff), cited by series, volume, and page number, so that A6.4.159 refers to series 6, volume 4, p. 159; AG = (Leibniz, 1989); C = (Leibniz, 1903); G = (Leibniz, 1875-1890), cited by volume number and page, so that G7.563 refers to volume 7 p. 563; GM = (Leibniz, 1849-1863), cited by volume and page number so that GM.4.91 refers to volume 4 p. 91; L = (Leibniz, 1969); RA = (Leibniz, 2001); Tentamen = (Leibniz, 1993). Whenever possible, I will give citations in A, the Akademie edition. When this is not possible, I will give citations from Gerhardt (either G or GM), the formerly standard edition of Leibniz's works. When translations into English are also available, I will cite an English translation as well.

[2] Leibniz does offer a breezy argument, in an early letter to Thomasius, that geometry is a true science in the sense of the *Posterior Analytics*. The argument is that since geometers give their proofs by way of constructions, and since constructions are motions, geometers give their proofs from motions ("*ex motu*") and therefore from causes ("*ex causa*"). See (G1.21-22). For further discussion, albeit brief, see (Beeley, 1999, pp. 138-139). In any case, the issue does not seem to arise in Leibniz's later works.

objects to correspond strictly to geometric ones even when they do not. These approximations are legitimate when the discrepancy between the real situation and the geometric approximation can be assessed and shown not to affect the outcomes of the demonstrations in which the geometric approximation is being employed.

Unlike Galileo, Leibniz accepts the view that nothing in nature corresponds strictly to any geometric object. Writing in 1686, Leibniz claims that "no determinate shape can be assigned to any body, nor is a precisely straight line, or circle or any other assignable shape of any body, found in the nature of things" (RA, p. 315). Later, in 1702, Leibniz reaffirms the view: "It is true that perfectly uniform change, such as the mathematical idea of motion, is never found in nature any more than are actual figures which possess in full rigour the properties which we learn in geometry" (G4.568, L, p. 583).[3] Despite his frequent changes of mind on other topics, Leibniz's rejection of what he calls "precise" or "definite" shapes in nature seems to be a stable part of his view from the 1680's until the end of his life.

On the other hand, Leibniz (like Galileo) is a mathematical physicist. Part of Leibniz's scientific practice involves representing and reasoning about bodies and their motions as though they do have precise shapes. To pick a prominent example, in "An Essay on the Causes of Celestial Motions", Leibniz offers a demonstration that the trajectories of the planets of our solar system, such as Mars, are elliptical. Leibniz shows no concern in that text for his independent arguments that, strictly speaking, the orbit of Mars could not be an ellipse. Those arguments are simply ignored.

The principal goal of this chapter is to explain Leibniz's justification for hold-

[3] I have made a small change to Loemker's translation, rendering "à la rigueur" as "in full rigour" instead of "in full force".

ing that in certain ordinary mathematical and scientific contexts, one is justified in representing and reasoning about nature as though it contains "precise shapes"—for instance, as though it contains bodies and motions strictly corresponding to continuous geometric curves and surfaces—despite his own arguments that there can be no such "precise shapes" in nature. When Leibniz is confronted with this difficulty, the justification he tends to offer is that even if nothing in nature corresponds exactly to any geometric shape, there can be things in nature which approximate geometric shapes to within any specified margin of error. For example, Leibniz writes in 1679 that "[E]ven if straight lines and circles do not and cannot possibly exist in nature, it suffices nonetheless that there can exist figures which differ so little from straight lines and circles that the error be less than any given" (A6.4.159).[4] Part of my task will be to explain how and to what extent this kind of justification solves the original difficulty. I will argue that if he is correct, Leibniz shows that in spite of all his arguments against precise shapes in nature, the *phenomena of nature* may still be just as if there were precise shapes. On the other hand, I will argue that this justification fails to provide an account of how to justify specific instances of scientific reasoning. It does not tell us, for example, how one may rigorously reason about the orbit of Mars while representing that orbit as an ellipse. To understand Leibniz's position on this latter issue, I will examine and to a certain extent reconstruct Leibniz's account of geometric approximations in physics.

Some interpreters of Leibniz, recently Timothy Crockett, contend that Leibniz's arguments against precise shapes in nature are at the same time arguments against

[4]This is my translation. I will give the fuller context of this statement along with the original Latin text in §4.2.

the reality of extended physical objects *tout court* (Crockett, 2005, 2009). Such interpretations regard Leibniz's rejection of precise shapes in nature as a key step on the road to Leibniz's mature idealism. The secondary goal of this chapter is to make a case against idealist interpretations of Leibniz's arguments concerning precise shapes in nature. In particular, I will argue that it is a corollary of Leibniz's solution to the difficulty just considered that one can make sense of precise quantitative discrepancies between geometric objects and the shapes of natural objects. Thus even if physical objects do not have "precise shapes", they do have some shapes—or if one prefers one may speak here of some extensional properties—with which precise shapes can be compared. Samuel Levey's suggestion is that the shapes actual things have according to Leibniz closely resemble what we would today recognize as fractal curves (Levey, 2003, 2005). I will provide evidence for thinking that Levey's suggestion is compatible with Leibniz's view of the ways geometry and nature approximate one another.

The plan of the chapter is as follows. In §4.1 I will provide an account of why on Leibniz's view nothing in nature corresponds precisely to any geometric curve or shape. Because other interpreters such as Crockett and Levey have devoted considerable attention to explicating Leibniz's arguments on this topic, I will focus mainly on giving an account of what features of the physical world are incompatible with "precise shapes" (rather than analyzing Leibniz's arguments that the world has those features). In §4.2 I will analyze Leibniz's justification for treating natural objects as though they have precise shapes, and in general for using mathematical techniques in the empirical sciences, even when nothing in nature strictly corresponds to any geometric object. I will pay considerable attention to the question of what Leibniz means by his claims that the difference between geometry and nature is "less than any given". Finally, in §4.3 I will use the preceding discussion to argue against those idealist readings of Leibniz according to which Leibniz's arguments against precise shapes are ultimately arguments against the reality of shape and extension *tout court*.

4.1 A World Without Precise Shapes

Leibniz offers a number of different arguments against the existence of precise shapes in nature; without individuating too finely, one may recognize at least four lines of argument: (1) an argument from sense perception that any hypothesis about an object's shape would be ruled out by a better, more complex hypothesis if the object were seen under more powerful magnification;[5] (2) an argument that precise shapes are ruled out by the actually infinite division of matter directly and without consideration of time (the so-called "synchronic" argument);[6] (3) an argument that precise shapes are ruled out by the infinite division of matter together with facts about how bodies and their motions are in perpetual flux over time (the so-called "diachronic" argument);[7] (4) an argument that perfect shapes in nature would amount

[5]Cf. Leibniz's letter to the Electress Sophie of October 31, 1705 (G7.562-563). Leibniz's letter to Arnauld in October of 1687 is also suggestive of this line of argument. For the letter, see (G2.119); for Robert Adams's discussion of it, see (Adams, 1994, pp. 229-230).

[6]Leibniz sometimes writes as if the actually infinite division of matter directly implies that there are no precise shapes in nature, as when he says to Arnauld: "...because of the actual subdivision of parts, there is no definite and precise shape in bodies" (G2.97-98, AG p. 80). There has been considerable interpretive effort spent to understand the inference from infinite division to the exclusion of perfect shapes in nature. For recent commentary, see (Levey, 2005, Forthcoming) and (Crockett, 2005, 2009). Adams discusses this argument in (Adams, 1994, pp. 229-232) and employs the distinction between the synchronic and diachronic arguments from infinite division.

[7]This argument occurs in a text entitled "There is no Perfect Shape in Bodies"; cf. (A6.4.1613-1614, RA pp. 297-299). For an extensive discussion of the argument, see (Levey, Forthcoming).

to barren or uncultivated parts of the universe and are therefore incompatible with God's wisdom.[8] My focus will be to explain how the features of the world revealed by the first two arguments rule out the existence of precise shapes in nature.[9]

The argument from sense perception is most clearly given in correspondence with the Electress Sophie of Hanover. Leibniz writes that in order to understand the exclusion of all "exact and indeterminate continuity" from matter—this presumably includes any exact, continuous shape—one should consider what "experience confirms by our senses":

> There is no drop of water so pure that one cannot recognize some variation on examining it closely. A bit of stone is composed of certain grains and through the microscope these grains appear as boulders in which there are a thousand freaks of nature. If the force of our vision were always increased it would always find somewhere further to go. There are everywhere actual variations and never a perfect uniformity, nor are two pieces of matter entirely similar to one another, in the large just as in the small (G7.562-563).[10]

Leibniz's argument invites us to take a closer look at the bodies around us and their apparent shapes. With the naked eye, the book before me may look to be a rectangular prism. That visual perception has the status of an initial representation of its shape. If I look more closely, I can see irregularities in the cover, warping of the

[8]See the letter to the Electress Sophie already cited (G7.561-565).

[9]Why focus on these two arguments? The (synchronic) argument from infinite division merits special attention because it directly concerns the relationship between geometric curves and the physical world, and also because it appears to have played a central role in Leibniz's thought about the issue (see n. 6). The argument from sense perception is, as I will argue later in this chapter, a much weaker argument against the existence of precise shapes in nature. But I focus on the argument because it will help me to clarify the relationship between three things: the physical world; geometric objects; the phenomena (*i.e.*, how the physical world appears to beings like us).

[10]This text is taken from an unpublished translation by Donald Rutherford of Leibniz's letter to Sophie of October 31, 1705. I wish to thank Rutherford for permission to use his translation in this dissertation.

pages, and so on. As I take in this information, I frame a more complex representation of its shape. But in principle, Leibniz argues, each representation of the book's shape would be shown inadequate under further magnification, so no representation that I or any finite mind can produce will survive the magnification procedure. The argument seems to show that any particular, finite representation of the shape of a body can be refuted as inadequate. This leaves open the possibility that the object does have some definite but infinitary shape which finite minds cannot frame to themselves; it also seems compatible with a more thoroughgoing rejection of the physical reality of bodies with shapes.

An odd feature of the argument from sense perception is that it ignores the limits of the acuity of the senses. There are limits to what can be detected by sensory means even allowing that the senses may be augmented by microscopes or other devices.[11] Even if one grants that a perfectly spherical body could not exist in the natural world, nonetheless there might be some bodies so nearly spherical that we could never perceive the difference. This consideration shows that for Leibniz's style of argument to be effective, there must be some gross and readily apparent differences between the shapes of actual bodies and continuous, geometric shapes. It also suggests that for Leibniz to sustain the position that in principle there is some level of magnification sufficient to show that any body is not, say, a sphere, Leibniz needs some argument against perfect shapes in nature independent of facts about sense perception. The argument from infinite division is just such an argument; let us turn to it now.

Leibniz presents an account of the fundamental physics of the world which he

[11] Moreover, Leibniz recognizes on many occasions that there are such limits; see the discussion in §4.2.

understands to preclude the existence of precise shapes in nature. He repeatedly asserts that the absence of precise shapes in nature is due to the fact that bodies, or the parts of matter which make up bodies, are actually infinitely divided.[12] On Leibniz's view, the fact that there are no precise shapes in nature is a consequence of fundamental physics. To understand Leibniz's position it will be helpful to consider first the account of the division of matter, then the way in which the division of matter precludes precise shapes in nature.

The actually infinite division of matter comes about as a consequence of the way bodies move through a plenum, *i.e.*, through spaces which are entirely filled by other bodies. For motion to occur in a plenum, bodies must move in closed circuits or loops. When a body B moves forward, it pushes some bodies ahead of it, and these bodies push still further bodies, until ultimately we loop back around to some bodies pushing B. Of course, any bodies moving in a circuit are completely surrounded by still more bodies. Therefore a good model for motion in a plenum would be a hose filled with water which is circulating through the hose. In Book II Propositions 33 and 34 of his *Principles of Philosophy*, Descartes had famously argued that if the hose or container has different diameters along its length, the bodies moving through the container will have to have speeds in inverse proportion to the diameters (Descartes, 1964-1976, Vol. VIIIa, pp. 57-60). But in that case, if the diameter is always changing, the speed at any two locations, no matter how close they are to each other, will have to be slightly different. To accommodate the different diameters and fill the container, the bodies will have to break up into ever smaller pieces. Descartes refrains from

[12]See for example Leibniz to Arnauld at G2.119, also "A Specimen of Discoveries" at (A6.4.1622, RA, p. 315).

describing this fracturing as actually infinite, preferring to describe it as "indefinite" and incomprehensible to us (Descartes, 1964-1976, Vol. VIIIa, pp. 59-60). Leibniz argues that the fracturing should be understood as actually infinite (A6.3.553-556, RA, pp. 181-187). Thus Leibniz endorses the core of Descartes' argument, though he gives a slightly different interpretation of its results.

If there were such a thing as a perfect fluid, Leibniz argues that the the bodies in the container would be broken all the way into points. If there were perfectly hard bodies with diameters smaller than the dimensions of the container, they would resist being broken. However, Leibniz does not believe perfect fluids or perfectly hard bodies to be physically possible. Rather, all bodies lie on a scale between perfect hardness and perfect fluidity. So we should think of the division of a physically continuous body, such as the fluid in the container, as follows:

> ... [E]ven a body that is everywhere flexible, but not without a certain and everywhere unequal resistance, still has cohering parts, although these are opened up and folded together in various ways. Accordingly the division of the continuum must not be considered to be like the division of sand into grains, but like that of a sheet of paper or tunic into folds. And so although there occur some folds smaller than others infinite in number, a body is never thereby dissolved into points or minima... It is just as if we suppose a tunic to be scored with folds multiplied to infinity in such a way that there is no fold so small that it is not subdivided by a new fold: and yet in this way no point in the tunic will be assignable without its being moved in different directions by its neighbors, although it will not be torn apart by them... although some folds are smaller than others to infinity, bodies are always extended and points never become parts, but always remain mere extrema. (A6.3.555, RA, pp. 186-187)

As the body moves around the container, it is actually infinitely divided into parts determined by their own distinct motion or endeavor with respect to their neighbors. Over any interval we choose, no matter how small, the fluid body is broken into smaller and smaller parts, each of which consists of more bodies, to infinity. Leibniz insists that the bodies into which the whole fluid are broken are not points, and that properly speaking points only exist as the boundaries (or "extrema") of bodies. Thus there are not points at every possible location even though every interval is densely packed with points. We will return to the question of the structure of the parts in a few paragraphs.

We now want to see how it follows from the fact that bodies are actually infinitely divided that bodies do not have any precise or definite shape. This is a contentious issue among Leibniz interpreters. Leibniz tends not to fill out the details of the argument, and there are several different proposals for how to do so. In this section I will follow the presentation given by Levey in his paper "Leibniz on Precise Shapes and the Corporeal World" (Levey, 2005). In §4.3 I will consider another suggestion made by Timothy Crockett.

Suppose for the sake of contradiction that there is a body whose surface is a precise, geometrically definable curve. Such a surface must be continuous in the mathematical sense. However, by the argument of the last few paragraphs, in any interval of the supposed surface of the body, there will be infinitely many distinct parts marked off as such by their slightly different motions in comparison with their neighbors. The surface of the original larger body is in fact composed of the surfaces of the smaller parts. The surfaces of these parts are contiguous—they touch each other with no space in between—but not continuous. It follows that there are actually infinitely many discontinuities over any interval we consider. Since all precise, geometrically definable surfaces were assumed to be continuous, it follows that the body does not have a precise, geometrically definable surface over any interval. Of course, it would be no use to move down a level and consider only the surface of a part of the original body, since we can start with any part of the body and run the *reductio* once again.

Leibniz draws similar conclusions also about times and motions:

> [I]t will be worthwhile to consider the harmony of matter, time and motion. Accordingly I am of the following opinion: there is no portion of matter that is not actually divided up into further parts... Similarly there is no part of time in which some change or motion does not happen to any part or point of a body. And so no motion stays the same through any space or time however small... (A6.3.565-566, RA, p. 209)

It follows that uniform motion or uniform acceleration are not to be found in nature, either. Rather, over any stretch of time a body's motion is subject to infinitely many variations as it is being battered by the bodies surrounding it. The trajectories of bodies through space, then, will not be precise geometric curves either. To return to our example from the introduction, the orbit of Mars cannot precisely speaking be an ellipse.

Even on Leibniz's conception of them, physical bodies and their motions do have what we can recognize as a mathematical structure, taking "mathematical structure" in a fairly broad sense. Bodies have extrema—*viz.*, surfaces and points—they have parts, and the parts stand in part-whole relations of arbitrarily high complexity. However, it is important to see that the structure of bodies, motions, and other physically "continuous" aspects of nature is not the same as the structure of geometric objects such as curves or surfaces. A theoretician in Leibniz's situation might have taken his discoveries about bodies to reveal that all continuous quantities, even in mathematics, have the same structure, albeit a different structure than mathematicians had hitherto assumed. Leibniz does exactly the opposite, insisting that what it is to be mathematically continuous and what it is to be physically continuous (better, "contiguous") are two very different things which should not be confused with one another on pain of paradox. As Leibniz explains in the letter to the Electress Sophie of Hanover I discussed earlier:

> The fact is that matter, the evolution of things, and finally every genuine composite, is a discrete quantity, but that space, time, mathematical motion, intension or the continual increase that is conceived in speed or other qualities. . . is a continuous and undetermined quantity in itself, or one indifferent to the parts that can be taken from it and which are actually taken in nature. The mass of bodies is actually divided in a determinate manner and there is nothing exactly continuous in it; but space or the perfect continuity which is in the idea marks only an undetermined possibility of dividing it as one will. In matter and in actual realities the whole is a result of the parts, but in ideas or possibles. . . the whole is prior to the divisions (G7.562).[13]

Leibniz is in effect proposing new definitions for the terms "mathematically continuous" and "physically continuous". A mathematically continuous quantity is a whole prior to any possible division of it into parts. All consistent ways of partitioning a continuous quantity are equally possible; we may choose to divide it into parts in whatever way we like. A physically continuous quantity is actually divided into parts which are prior to the whole quantity and together constitute it. How the quantity is divided into parts is determined by the physical facts, in particular by facts about motion. Points or surfaces only exist in the physically continuous quantity when there are parts which have those points or surfaces as boundaries. Moreover, the parts of the physically continuous quantity have their own separate boundaries which merely

[13]This text is taken from an unpublished translation by Donald Rutherford of Leibniz's letter to Sophie of October 31, 1705.

touch one another. This is the sense in which they are not really continuous, for if they were really continuous, two parts which were next to each other would not have separate boundaries. The following shows in a particularly clear way how the structure of the mathematically and physically continuous are different from each other: In a physically continuous quantity such as a body, there can be distinct points p and p' whose distance from one another is zero. For instance, these points may lie on the surfaces of two parts which are touching each other. In a mathematically continuous quantity, if p and p' are at zero distance from one another, then $p = p'$.[14]

In some contexts, and especially when issues in the foundations of geometry and physics are concerned, Leibniz stresses the difference between the mathematically continuous and the physically continuous. In other contexts, especially if his remarks are aimed at practicing mathematicians or physicists, Leibniz plays down the importance of the distinction. The main reason the practicing physicist may ignore the distinction is that on Leibniz's view, physically continuous things or processes can approximate mathematical continuity to any given margin of error. In the next section I will consider how such approximations are possible and how they form a part of Leibniz's justification of geometric methods in physics.

[14]Beeley writes that from his early works through his later career, Leibniz "more or less consistently employs a model at the core of nature so to speak which has its origins in an essentially mathematical concept" (Beeley, 1999, p. 138). I believe Beeley's statement is partly correct, but also partly misleading. The physical world has a great deal of structure according to Leibniz, but it is important to see that this structure is not the same as any geometric or mathematical structure. Nonetheless, the fact that the physical world has such a rich structure will help to explain how the physical world can be well approximated by geometric structures. This in turn will help explain the applicability of geometry to physics. See §4.2.

4.2 An Error Less than Any Given

Leibniz's rejection of precise shapes in nature raises a worry about his justification for applying geometry to physics. Such applications involve representing and reasoning about bodies and their motions as though they correspond to geometric curves or surfaces. But Leibniz argues on many occasions that such correspondences never hold in full strictness. Leibniz therefore needs a justification for the applicability of geometry to physics which is compatible with there never being any strict correspondence between nature and the objects of geometry.

Leibniz is aware of this worry, and his response to it is multifaceted. In the following discussion it will be helpful to distinguish between two explanatory goals Leibniz might be aiming at in justifying the applicability of geometry to physics. One goal would be to explain how geometric truths either can or do count as laws of *the phenomena of nature*, or how nature appears to us, despite the fact that there are no precise geometric shapes in nature. This would help to explain why we are justified in holding it to be true of the phenomena of nature that the area of any ellipse (say) is equal to the product of π, its semi-major axis a, and its semi-minor axis b, and therefore why we are entitled to appeal to geometric truths in physics. For purposes of abbreviation, I will call this the goal of explaining how geometric truths govern the phenomena. A different goal would be to explain how we are justified in taking any particular phenomenon, for instance, the trajectory of Mars as it appears to us, to be approximated by some geometric curve, or how we are justified in reasoning about the trajectory of Mars by means of the approximation. I will call this the goal of explaining the existence and legitimacy of geometric approximations. I will turn to this latter explanatory goal only once the explanation of how geometric truths govern the phenomena is in place.

4.2.1 How Geometric Truths Govern the Phenomena

It is clear enough from a number of texts that according to Leibniz, geometric truths do govern the phenomena. In 1695 Leibniz writes that "Number and line are not chimerical things...for they are relations that contain eternal truths, by which the phenomena of nature are ruled" (G4.491-492, AG, pp. 146-147); and in a letter to De Volder written on January 19, 1706, Leibniz claims:

> The science of continua, *i.e.* of possibles, contains eternal truths that are never violated by actual phenomena, since the difference [between real and ideal] is always less than any assignable given difference (G2.282-283, AG, pp. 185-186).[15]

In this respect mathematical truths are like metaphysical truths, both of which Leibniz refers to as "eternal laws" to which the appearances conform (G2.275, AG, p. 181). Because on several occasions Leibniz insists that geometric truths govern the phenomena only a few lines after arguing against the existence of precise shapes in nature, there is a burden on Leibniz to explain how both of these could hold at once.

Leibniz confronts the worry about how geometric truths *can be* laws of the phenomena of nature as well as the related but distinct worry about whether they *are* laws of the phenomena. Regarding the former, it is clear that geometric truths could not be laws of the phenomena if those phenomena were to "violate" geometry, a possibility Leibniz considers in his reply to Bayle's encyclopedia entry on Rorarius

[15] The remark in brackets was added by the translators. From the context, it is reasonably apparent that Leibniz is talking about the difference between the real and the ideal.

(cf. G4.568, L, p. 583). I am unaware of any text, including the reply to Bayle, in which Leibniz spells out what it would be for the phenomena to violate geometry. But I assume that, like Galileo, Leibniz is considering the objection that geometric theorems are false when they are understood as claims about natural phenomena. Recalling the example from Scholastic Aristotelians such as Pereira, the phenomena would violate geometry if material spheres do not touch material planes tangent to them at a point, *etc.*[16] Leibniz at least suggests that this is the relevant objection when he writes in the same reply to Bayle that "It is true that perfectly uniform change, such as the mathematical idea of motion, is never found in nature any more than are actual figures which possess in full rigour the properties which we learn in geometry" (G4.568, L, p. 583). Leibniz's way of speaking is suggestive of figures in nature which do not possess the properties geometry proves such figures to have; sometimes we do speak loosely about "spheres" in nature which clearly do not possess the properties of spheres proved in geometry. I believe that ultimately, however, Leibniz's remark is most charitably interpreted as claiming that geometric figures do not exist in nature at all.

Leibniz's response to the worry that the phenomena might violate geometric truths is to insist that conformity with geometric truths is a criterion of reality in phenomena. In the same reply to Bayle, Leibniz writes:

> Yet the actual phenomena of nature are arranged, and must be, in such a way that nothing ever happens which violates the law of continuity. . . or any of the other most exact rules of mathematics. . . Actual things cannot escape [mathematics'] rules. In fact, we can say that the reality of phenomena, which distinguishes them from dreams, consists in this fact (G4.568-569, L, p. 583).

[16] For further discussion of this objection to the applicability of geometry, see Chapters 1 and 3.

Also, in the letter to De Volder discussed above, the fuller context is as follows:

> The science of continua, *i.e.* of possibles, contains eternal truths that are never violated by actual phenomena, since the difference [between real and ideal] is always less than any assignable given difference. And we don't have, nor should we hope for, any mark of reality in phenomena, but the fact that they agree with one another and with eternal truths (G2.282-283, AG, pp. 185-186).

These considerations foreclose any possibility that natural phenomena should violate or contain counterexamples to geometric truths. Any course of experience we might have which appeared to violate geometric truths should be rejected as unreal, as analogous to a dream, precisely because it does not cohere with geometric truth. Moreover, coherence with the eternal truths of mathematics and metaphysics is what makes an experience an experience of something real, as opposed to a dream or a hallucination.

The next element in Leibniz's explanation of how mathematical truths can govern the phenomena is an insistence that it is at least possible for there to be natural objects which approximate geometric objects arbitrarily closely. That Leibniz believes such approximations are possible comes out clearly in a text entitled "De Organo sive de Arte Magna Cogitandi", where Leibniz writes the following:

> For even if straight lines and circles do not and cannot possibly exist in nature, it suffices nonetheless that there can exist figures which differ so little from straight lines and circles that the error be less than any given. That is sufficient for the certainty of demonstration as well as practice. That figures of this kind can exist, however, is easily demonstrated, if only this one thing is admitted, namely that some lines are given.[17]

[17] This is my translation of the passage. Here is the original: "Nam etiamsi non darentur in natura nec dari possent rectae ac circuli, sufficiet tamen dari posse figuras, quae a rectis et circularibus tam parum absint, ut error sit minor quolibet dato. Quod satis est ad certitudinem demonstrationis pariter et usus. Posse autem dari hujusmodi figuras non difficulter demonstratur, modo admittatur hoc unum, aliquas dari lineas" (A6.4.159).

I interpret the syntax of Leibniz's claim to be the following: For any margin of error ϵ and any circle C, it is possible that there is a natural object N such that the difference between C and N is less than ϵ. A similar claim holds at least for lines. However, the context of the quotation is the question of how one can construct the various geometric curves given some particular curves, such as circles and lines, as primitives. If one can use natural approximations to circles and lines in place of true circles and lines to construct the remaining curves, I presume one can also obtain arbitrarily good natural approximations of those remaining curves (by means of compass and straightedge constructions). In that case, we would have it that in general, for any margin of error ϵ and any geometric curve Γ constructible with compass and straightedge, it is possible that there is a natural object N such that the difference between N and Γ is less than ϵ.[18] Leibniz is not explicit about what is meant by the difference or error in this claim. I assume he means there is some way of superimposing the natural object onto the curve so that the distance from the curve to the natural object is always less than ϵ. This is how I will understand the difference or error between two curves in the remainder of this chapter.[19]

[18]Geometry in Leibniz's time (and well before it) investigated curves which are not constructible using a compass and straightedge. I know of no reason why Leibniz would not countenance the physical possibility of arbitrarily good natural approximations of the then known nonconstructible curves as well as the constructible ones—at least in the case of continuous curves such as the cycloid. Inasmuch as doing so helps Leibniz secure the applicability of the theories of nonconstructible curves, it would seem to strengthen his position to admit the physical possibility of natural approximations of nonconstructible curves. But this issue is not addressed in the quotation discussed here, and I am unaware of any text in which Leibniz explicitly addresses the issue.

[19]It should be noted that the process of comparing a geometric curve to a natural object is not trivial in the Leibnizian context where geometric objects and the physical world are structurally dissimilar. It appears to be a consequence of Leibniz's view that the geometric and the physical do not even have the same topological properties: as I discussed in the preceding section, Leibniz countenances the existence of distinct physical points lying at zero distance from one another. Nonetheless, it is clear that Leibniz takes such comparisons to be possible and takes there to be

If the difference between a geometric curve and the shape of a physical object is small enough, a human observer will never be able to distinguish the one from the other. That there are such limitations on human observers is something Leibniz emphasizes at various points in his career. For example, in an early work entitled "The Theory of Abstract Motion", Leibniz writes:

> [S]ensation cannot discriminate whether some body is a continuous or contiguous unit, or a heap of many discontiguous ones separated by gaps; whether parts are wholly at rest, or redound on themselves by an insensible motion; whether an angle of intersection is very slightly oblique, or exactly a right angle; whether the angle of contact is made at a point, or a line or surface... (A6.2.273, RA, p. 343).[20]

In this text, Leibniz stresses that if from the physical theory he is proposing "no sensible error disturbs our reasons", then it "suffices for the phenomena" (*ibid.*). Leibniz makes similar remarks in defending the technique of approximating curves by large collections of polygons as one does in integral calculus. For when doing calculus one also argues that the difference between two quantities, namely the given curve and some corresponding approximation of it by polygons, can be made less than any given quantity. In such cases any error between the original curve and the approximation can be rendered completely "insensible".[21]

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quantitatively precise facts about the extent of the difference between a geometric and a physical object. I suspect that such comparisons require one first to identify all physical points at zero distance from one another, so that the space in which the objects are compared is geometric space. See the discussion of continuity later in this section.

[20]When Leibniz speaks of the "angle of contact" here, he is presumably speaking of the contact between a sphere and a plane tangent to it. The issue of the contact between a sphere and a plane (or line) has arisen repeatedly in this work; cf. the discussion of Galileo's extrusion argument in §3.1.

[21]See for example (A2.1.53), cited by Beeley (1999, p. 140). Beeley's article contains numerous references to passages in which Leibniz argues that very small differences are either insensible or make no difference to the phenomena.

In summary, Leibniz's solution to the problem of how geometric truths can be laws of the phenomena of nature is as follows. Even though there are no precise shapes in nature, it is possible for things in nature to differ so little from precise shapes that this difference is beyond the limits of the acuity of human sensation. As far as the phenomena are concerned, things can therefore appear to have perfect shapes. If the difference between the shape of the natural object and the perfect geometric shape is sufficiently small, no sensible error can arise from conflating the two.

A consequence of the considerations in the last few paragraphs is that one must reassess Leibniz's argument from sense perception against precise shapes in nature.[22] At best that argument shows that in many ordinary cases, we can convince ourselves that bodies do not have the simple geometric shapes we might at first perceive them to have. This might make it plausible to us (and it might have made it plausible to the Electress Sophie) that no body has any precise geometric shape. But ultimately, sense perception is entirely unable to distinguish whether natural objects have precise geometric shapes. While this limits the effectiveness of the argument from sense perception, I believe it makes Leibniz's position as a whole a stronger one. For one might have worried that Leibniz's other arguments against precise shapes, such as the argument from infinite division, prejudge various empirical questions in objectionable ways. At first glance, it would seem to follow from Leibniz's arguments that if we pursue astronomical observations far enough, we shall discover that the orbit of Mars is not really an ellipse. But this is not the case: the argument against precise shapes reveals nothing about how astronomical observations of Mars will turn out.

[22] Cf. §4.1.

So much for Leibniz's explanation of how geometric truths *can* govern the phenomena in the absence of precise shapes in nature. It does not follow from the fact that they *can* govern the phenomena that they *do in fact* govern them. Or in other words, it does not follow from the fact that there can be natural objects which approximate geometric objects to within any given margin of error that there are in fact such natural objects.[23] If no natural objects do suitably approximate the geometric ones, then the truths of geometry would at best amount to empty or vacuous laws. Leibniz seems to reject this possibility by insisting that eternal truths such as those of geometry *do* govern the phenomena, and that the difference between real and ideal *is* less than any given (as in the 1706 letter to De Volder cited above). However, he is far less explicit about which geometric objects are approximated by natural ones, and I am not aware of any place Leibniz takes this question on in a systematic way.

The one case Leibniz singles out for separate discussion is that of mathematical continuity itself. Leibniz writes in his reply to Varignon that "[O]ne can say in general that though continuity is something ideal and there is never anything in nature with perfectly uniform parts, the real, in turn, never ceases to be governed by the ideal and the abstract" (GM4.93, L, p. 544). There is both in this text and other texts the suggestion that the laws of the ideal, or of the mathematically continuous, hold also for the real, though I do not discern any explicit argument about which laws hold for both and which do not.[24] Earlier I gave as an example a law over which the two kinds of continua differ, namely the law that if the distance from point p to point p'

[23]In other words still, we want to know for which geometric curves Γ we have the following claim: For any margin of error ϵ and any geometric curve Γ, there exists some natural object N such that the difference between N and Γ is less than ϵ.

[24]See also (G4.568-569, L, p. 583).

is 0, then $p = p'$. Leibniz is aware of this discrepancy between the mathematically and the physically continuous, though it is not entirely clear what his response to the discrepancy is.[25]

A reasonably straightforward argument can be made, however, which I think does give some content to the idea that mathematical continuity and physical continuity differ from each other by less than any given amount. First, recall earlier that Leibniz said sense perception cannot distinguish "whether some body is a continuous or a contiguous unit" (A6.2.273, RA, p. 343). This provides reason to think that on Leibniz's view, sense perception cannot distinguish whether there is only one point at a single location, or perhaps two or more. So one may identify contiguous points with each other without causing any error a human being can sense.[26] Now, for the time being taking physically contiguous points to be one and the same point, the second task is to see how physical continuity approximates mathematical continuity. The task is made difficult by the fact that Leibniz's usual definitions of mathematical and physical continuity are complicated, especially so because they involve notions of possibility and determinacy. But on at least on occasion, Leibniz is willing to use a simpler criterion of mathematical continuity, as when he writes to Des Bosses that "when points are situated in such a way that there are no two points between which

[25] Cf. (A6.3.563-564, RA, p. 205).

[26] There is one difficulty with this suggestion. Leibniz does at least once give a sensible criterion for whether bodies are continuous or merely contiguous, namely whether there is sensible cohesion between them. For example, given a spherical body on a flat body, one may tell whether these are continuous or merely contiguous as according to whether one can push the spherical body around without resistance. See (A6.3.537, RA, p. 149). This contradicts Leibniz's earlier claim that sense perception cannot discern the difference between continuity and contiguity. I am unsure whether this is genuinely a change of mind or an inconsistency. I am inclined to think the strongest position for Leibniz is to insist that sense perception cannot distinguish contiguous points and therefore to abandon any sensible criterion for distinguishing continuity from contiguity.

there is no midpoint, then, by that very fact, we have a continuous extension" (G2.515, AG, pp. 201-202). Now in the case of physically continuous quantities, points only exist as boundaries of bodies (or motions), so a midpoint between two points will only exist in the case that there happens to be a body (or motion) containing a boundary at that location. There might in fact not be any such body (or motion), and therefore no such midpoint. Nonetheless, given any small interval $\pm\epsilon$ around any location, we are guaranteed that there are points inside the interval since "there is no portion of matter that is not actually divided up into further parts" (A6.3.565, RA, p. 209).[27] Therefore even if the physically continuous quantity does not always contain the midpoint for any pair of points in it, the physically continuous quantity does contain points within ϵ of the midpoints. I believe this is a reasonably straightforward sense in which the difference between physical and mathematical continuity is less than any given. Choosing ϵ to be far finer than the level of acuity of human sensation, we can show that as far as the appearances are concerned, the physically continuous quantity is indiscernible from the mathematically continuous.

While Leibniz stresses that mathematical and physical continuity differ by less than any given amount, he does not explicitly tell us which geometric objects are arbitrarily well approximated by natural objects. The claim Leibniz makes in the 1706 letter to De Volder suggests that such approximations are common. Recall Leibniz's statement: "The science of continua... contains eternal truths that are never violated by phenomena, since the difference [between real and ideal] is less than any assignable difference". "The science of continua" refers to geometry, and the difference between

[27]Or, one might also say, since the parts are divided into further parts *ad infinitum*.

real and ideal presumably includes the difference between continuous mathematical objects and actual physical ones. Leibniz's statement therefore encourages a liberal view on the extent to which nature approximates geometry. In a similar spirit, Leibniz makes the remark about nature that "the more one knows her, the more geometric one finds her" (G3.54, L pp. 352-353). Strictly speaking, however, none of this provides us with an argument or a precise description of which geometric curves or surfaces have arbitrarily good approximations in the physical world. This may not be a bad consequence, however; Leibniz can leave the matter open to observation and scientific investigation.[28]

4.2.2 The Existence and Legitimacy of Geometric Approximations

For now I will take it that Leibniz has explained how geometric truths both can and do govern the phenomena. The core of this explanation consisted of an argument that nature can approximate geometry arbitrarily well and that, in the very important case of mathematical continuity, it actually does so. I want to turn now to the question of the existence and legitimacy of geometric approximations. This is the converse of the preceding issue: we first want to know, given any natural object, are there arbitrarily good geometric approximations of it?[29] And second, assuming there are

[28]The exception here is continuity: it is *a priori* in roughly our sense of the word that physical processes are continuous. See Leibniz's response to Malebranche for the sense of continuity in question at (G3.51-55, L, pp. 351-354). Earlier I gave some of the texts which explain why the continuity of physical processes is *a priori*: anything in conflict with it would be rejected as unreal.

[29]Spelled out more fully, the syntax of the question is: Given any margin of error ϵ and any natural object N, is there some geometric object Γ such that the difference between N and Γ is less than ϵ?

some good geometric approximations for natural objects, how are we justified in using those approximations to reason about the natural objects?

The issue of the existence of arbitrarily good geometric approximations to natural objects corresponds more closely to what philosophers and historians of science usually mean by the "mathematizability" of nature. For one does not mathematize nature by choosing a geometric curve and an error margin and then hunting around for natural objects differing by less than the error margin. Rather, one starts with some natural objects or systems (and perhaps some margin of error determined by the context) and hunts for sufficiently good geometric approximations of those objects or systems. The first question from the last paragraph asks whether, in principle, this hunt can ever be in vain because for small enough margins of error the geometric approximations give out. The second question asks after the kind of justification we can offer, once we have chosen some suitable geometric approximation, for the soundness of our reasoning based on the approximation.

Because the notion of approximation we are using is symmetric, some of the arguments given in §4.2.1 are relevant to the first question. Just as mathematical continuity is arbitrarily well approximated by physical continuity, physical continuity is arbitrarily well approximated by mathematical continuity. Leibniz's comments in the 1706 letter to De Volder and his claim that the more one examines nature, the more geometric one finds her also support some general optimism about the existence of arbitrarily good geometric approximations. However, this again falls short of any precise characterization of which natural systems are arbitrarily well approximated by geometric ones. This appears to leave the question of how well geometry approximates nature at least partly to observation and scientific investigation, and partly to the breadth of one's conception of geometry.

To get some further purchase on his view of the existence and legitimacy of geometric approximations in particular instances, I will examine a case from Leibniz's own scientific practice. In "An Essay on the Causes of Celestial Motions", published in the *Acta Eruditorum* in 1689, Leibniz presents an argument which is meant to explain why the planets in our solar system move in ellipses with the sun at one focus. Leibniz's chief assumption is the existence of a fluid vortex circulating around the sun; the key property of the vortex is that it circulates harmonically, which is to say that "the velocities of circulation round the centre decrease proportionally as the distances from the centre increase" (GM6.149-150, Tentamen, pp. 129-130). Leibniz gives a demonstration of the elliptical trajectories of the planets from the assumption of the fluid vortex.[30] The demonstration is offered as an explanation, assuming the vortex theory, of why the planets move as they do: they are being pushed by the fluid around them.

The principal explanandum is the fact that the planets move in elliptical orbits around the sun such that equal areas are swept out in equal times by a radius drawn from the sun to the planet. Leibniz writes that this fact is a law which Kepler had discovered on the basis of Tycho's "more accurate than usual" observations (GM6.147, Tentamen, pp. 127-128). Later in the text, Leibniz again cites observations as the basis for the claim that radii drawn from the sun to the planet sweep out equal

[30]The assumption of the harmonically circulating vortex is not the only assumption in Leibniz's argument. In particular, Leibniz must make some assumptions about the planets' motions towards and away from the sun (which he calls the "paracentric motion"). See (GM6.152, Tentamen, p. 132).

areas in equal times, which is the ultimate ground for claiming that the fluid vortex circulates harmonically (GM6.151, Tentamen, p. 131). Now as I understand them, Leibniz's arguments against precise shapes in nature imply that precisely elliptical trajectories and harmonic circulation are physically impossible. But these arguments are completely ignored in the "Essay". The "Essay" is written as if these conditions on the motions of the planets and the surrounding vortex held exactly.[31]

I take Leibniz's arguments against precise shapes in nature to be decisive here, and therefore it follows that the trajectory, for example, of Mars, is not precisely elliptical. Any attribution of a geometric shape to a body's trajectory must be an approximation. This raises two important questions about Leibniz's view of the situation: (A) Is the true trajectory of Mars arbitrarily well approximated by geometric curves? Or, more precisely, for any margin of error ϵ does there exist some geometric curve Γ such that the difference between Mars's trajectory and Γ is less than ϵ? (B) Is the true trajectory of Mars arbitrarily well approximated by some particular ellipse? More precisely, for any margin of error ϵ is the difference between the given ellipse and Mars's trajectory less than ϵ?

With regard to (A), I believe it is difficult to determine Leibniz's view. I do not know of any explicit statement by Leibniz which would tell us just how close geometric approximations can come to true orbital trajectories. He seems to offer the ellipse as supported by all observations, and that may mean he holds that the approximation suffices as far as the appearances or phenomena are concerned. In this case, the

[31] Leibniz even writes that Descartes may have been hindered in his own astronomical investigations either because "he could not reconcile [Kepler's laws] sufficiently with his own opinions, or because he remained ignorant of the fruitfulness of the discovery and did not consider it to be so accurately followed by nature" (GM6.148, Tentamen, p. 128).

margin of error would be smaller than our ability to detect the approximation by sense perception. Such a small margin of error presumably suffices for all scientific purposes. However, the existence of one very good approximation does not guarantee the existence of approximations for all ϵ, however small.

There is more relevant evidence with regard to (B). For if the difference between a particular trajectory and a particular geometric curve is less than ϵ for all ϵ, and if the trajectory is nonetheless not identical with the curve, then the difference between the trajectory and the curve is infinitesimal. This makes Leibniz's views on infinitesimal quantities relevant to deciding whether a particular real shape or real motion can be arbitrarily well approximated by a particular geometric object. Unfortunately, there is no scholarly consensus on what Leibniz's view on infinitesimals is, and it is beyond the scope of this chapter to make any detailed contribution to that debate. Instead I will describe two ways of proceeding, one which interprets Leibniz as accepting infinitesimal quantities and one which interprets Leibniz as rejecting them. I will conclude with some reasons for preferring the latter interpretation. My hope is that connecting the issues of infinitesimal quantities and geometric approximations will ultimately provide resources for fixing on an interpretation of Leibniz which settles both issues.

The first interpretive strategy takes Leibniz as granting that the difference between the trajectory of Mars and some particular given ellipse is less than ϵ for all ϵ. It nonetheless maintains that the trajectory and the ellipse are not identical. It is clear that at least during certain parts of his career, and especially in his early years, Leibniz is willing to entertain the possibility of non-identical magnitudes differing by less than any amount. For example, in comparing the circle to an infinitangular polygon Leibniz writes the following:

> But that being so, you will say, an infinitangular polygon will not be equal to a circle: I reply, it is not of an equal magnitude, even if it be of an equal extension: for the difference is smaller than can be expressed by any number (A6.2.267, RA, p. 342).

Leibniz could use the same distinction regarding the ellipse which approximates Mars's trajectory, namely that it is not the same in magnitude as Mars's trajectory even though it has an equal extension. This in turn would justify the use of some mathematical techniques in reasoning about Mars's trajectory; importantly, it would justify using Leibniz's new technique for finding areas of ellipses as also giving the area swept out by Mars along its trajectory. Even if Leibniz does not draw a distinction between magnitude and extension, I take the principal virtue of admitting that the given ellipse and Mars's trajectory differ by less than any amount is that it justifies reasoning about Mars's trajectory as if it were an ellipse without fear of producing errors.[32] In terms of the main explanatory goals mentioned in the beginning of this section, it gives Leibniz the basis for explaining the legitimacy of (at least some) geometric approximations. Moreover, on the interpretation we are now considering the existence and legitimacy of geometric approximations is explained by the general claim that the real and the ideal differ by less than any given quantity.

[32]It is unclear exactly which mathematical results transfer from one magnitude to another which differs from it, though by less than any amount. It would seem as if areas and volumes would be one case in which the results do transfer, so that if an ellipse has area equal to πab, then so does a trajectory differing from the ellipse by less than any quantity. But certain other properties might not transfer. In any case, the position attributed to Leibniz on this interpretive strategy is made stronger inasmuch as there is some wide range of properties shared by quantities differing only infinitesimally, because this justifies conflating the quantities quite generally.

The second interpretive strategy begins with Leibniz's later rejection of infinitesimal quantities as fictions and his corresponding willingness to infer from the fact that two objects differ by less than any quantity that the two objects are identical.[33] With these assumptions it follows that if the trajectory of Mars differed from some given ellipse by less than ϵ for all ϵ, then the trajectory of Mars would simply *be* that ellipse. Since this result directly contradicts Leibniz's arguments against precise shapes in nature as well as his rejection of infinitesimal quantities, this interpretive strategy must hold that Mars's trajectory and the ellipse differ by some positive, finite quantity.[34] But this removes the justification we considered in the last few paragraphs for reasoning about the trajectory of Mars using the ellipse as an approximation. If the trajectory of Mars and the ellipse differ by some finite amount, it becomes difficult to say which if any of the properties of the one hold also of the other. Also, on this interpretation the existence and legitimacy of geometric approximations is clearly not to be explained by the claim that the real and the ideal differ by less than any given quantity.

The interpretive strategy just described needs a different explanation for the legitimacy of geometric approximations. I believe the best such explanation is given by Leibniz in the letter to the Electress Sophie of October 31, 1705. Leibniz is discussing the difference between the complex real motions and shapes of bodies, on the one hand, and the simpler geometric shapes those bodies and motions appear to us to

[33] For an article which collects together textual evidence that Leibniz is willing to draw the inference just described, see (Levey, 2008).

[34] On the interpretation I am offering, this means that every way of superimposing the approximating ellipse onto Mars's trajectory leaves some positive, finite distances between the ellipse and the trajectory.

have, on the other. I take this to be analogous to the difference between Mars's true but very complicated motion and the Keplerian ellipse. Leibniz writes the following:

> Eternal truths founded on limited mathematical ideas do not fail to serve us in practice, insofar as it is permissible to abstract from inequalities too small to be able to cause significant errors in relation to the aim proposed; just as an engineer who traces a regular polygon on a plot of land is unconcerned if one side is longer than the other by some inches.[35]

On the account Leibniz is here proposing, whether it is legitimate to use a given geometric curve as an approximation to a real motion or real body depends on the aims of the scientific context and what counts as a significant error in that context. There is no foundational or context independent theory of which properties the approximating curves and real shapes share. Rather, the context determines how close the approximating curve and real shape need to be and what errors may be caused if the discrepancy is too large.

One consideration against the first interpretation, and in favor of the second, is that the first interpretation requires a commitment to infinitesimal quantities that appears to be very difficult if not impossible to paraphrase away. On Leibniz's mature view of infinitesimals as fictions, it is important that arguments which appeal to infinitesimals can be transformed into arguments for the same conclusions that do not appeal to infinitesimals. This makes the legitimacy of infinitesimalist mathematics, and Leibniz's calculus in particular, independent of whether one accepts infinitesimal

[35]The translation is my own, though I have been helped by the unpublished translation of the same text by Donald Rutherford. The original text is as follows: "Cependant les verités eternelles fondées sur les idées mathematiques bornées ne laissent pas de nous servir dans la practique, autant qu'il est permis de faire abstraction des inegalités trop petites pour pouvoir causer des erreurs considerables par rapport au but qu'on se propose; comme un ingenieur qui trace sur le terrain un polygone regulier, ne se met pas en peine si un costé est plus long que l'autre de quelques pouces" (G7.563-564).

quantities as real entities. But the justification for geometric approximations envisioned in the first interpretation does not seem to admit of paraphrase solely in terms of finite quantities. This is in effect because two quite particular finite quantities, *e.g.*, a particular ellipse and Mars's trajectory, are being claimed to differ by less than ϵ for all ϵ. This is to be contrasted with cases such as infinite series, where a claim that an infinite series has a sum r can be cast as the claim that for any ϵ, the difference between partial sums and r is less than ϵ so long as we take enough terms in the series. Here talk of the infinite series really amounts to talk of partial sums.[36] Talk of Mars's trajectory or a particular ellipse, on the other hand, seems to be talk about particular finite quantities.[37]

Further support for the second interpretation can be derived from Leibniz's own discussion of infinitesimals in the "Essay". In what later became known as the "Lemma on Incomparables", Leibniz remarks:

> In the demonstrations I have employed incomparably small quantities, such as the difference between two finite quantities, incomparable to the quantities themselves. Such matters, if I am not mistaken, can be exposed most lucidly as follows. Thus if someone does not want to employ infinitely small quantities, one can take them to be as small as one judges sufficient as to be incomparable, so that they produce an error of no importance and even smaller than allowed. In the same way as the Earth is like a

[36]Leibniz takes this view of infinite series in "Infinite Numbers", a text written in 1676: "Whenever it is said that a certain infinite series of numbers has a sum, I am of the opinion that all that is being said is that any finite series with the same rule has a sum, and that the error always diminishes as the series increases, so that it becomes as small as we would like" (A6.3.503, RA, p. 99).

[37]The example of Mars's trajectory and the ellipse may be infelicitous. At times Leibniz does consider rejecting circles as fictional limit entities and paraphrasing talk of circles as talk about sequences of polygons with a great enough number of sides to make the error as small as one wishes. Perhaps he could take the same line to paraphrase away talk of ellipses as well as talk of infinitesimal differences between ellipses and other figures. See Leibniz's discussion in "Infinite Numbers" at (A6.3.498, RA, p. 89). In any case, one could avoid using an ellipse for purposes of the example and consider instead a case in which a shape was being approximated by a polygon (perhaps like the engineer trying to trace a polygon on the plot of land).

> point, or the diameter of the Earth as an infinitely small line with respect to the sky, so it can be demonstrated that if the sides of an angle have a basis incomparably smaller than them, the angle they enclose will be incomparably smaller than the right angle, and the difference between the sides will be incomparable with the sides themselves..." (GM6.150-151, Tentamen, pp. 130-131)

The "Lemma on Incomparables" gives the scientist or mathematician who rejects infinitesimal quantities a different way of understanding arguments that are, when taken at face value, about infinitesimals. The claim that Mars's trajectory and some given ellipse differ by less than any given quantity can be understood as the claim that Mars's trajectory and the ellipse do differ by some finite quantity, only one which is small enough as not to produce significant errors. On this way of construing the argument, the soundness of the argument is uncontroversial. But this way of reconstruing talk of infinitesimal quantities, when applied to the first interpretation outlined above, in effect yields the second interpretation I am now arguing for. Thus another reason to prefer the second interpretation is that it gives Leibniz a position which is by Leibniz's own lights both sound and uncontroversial.

4.2.3 Summary

In this section I have given an account of Leibniz's explanations of two distinct but related issues concerning the applicability of geometry to physics. I first gave an account of Leibniz's explanation of how geometric truths can and do govern the phenomena of nature even when nothing in nature corresponds precisely to any geometric object. This explanation crucially relies on the claim that natural objects can approximate geometric ones to within any margin of error, and in particular that physical continuity does in fact approximate geometrical continuity arbitrarily well. This makes it plausible that in particular cases, real shapes may differ so little from geometric curves that the difference is insensible, so that the shape of a body is a geometric curve as far as the appearances of nature are concerned. I then argued that Leibniz's best explanation of the existence and legitimacy of geometric approximations is to point out that in many cases, natural objects are well enough approximated by geometric curves for the purposes of doing science. All that science requires is for the error between the real shape and the approximating geometric curve to be small enough so as not to cause errors significant in light of the aim at hand. To return to the example I discussed at some length, Leibniz's soundest justification for approximating the trajectory of Mars with a particular ellipse would be to say that the error between the trajectory and the ellipse is small enough so as not to cause significant errors given the aims of astronomy. This justification stands in contrast with another possible justification which I argued provides us with a poorer interpretation of Leibniz: namely, that the error between the trajectory and the ellipse is less than any given quantity.

4.3 Phenomenal and Worldly Aspects of Shape

Timothy Crockett has recently developed an interpretation of Leibniz according to which the arguments against precise shapes in nature ultimately support the view that "the world, as it is in itself, does not contain genuinely extended things" (Crockett, 2009, p. 736). This means, in the first instance, that the world as it is in itself does not contain extended bodies with metaphysically determinate boundaries. But on Crockett's interpretation, the arguments against precise shapes also ultimately provide evidence against the reality of extended matter as well (Crockett, 2009, pp. 750-751). Crockett's interpretation is therefore in line with earlier interpretations of Leibniz which see the arguments against precise shapes in nature as providing Leibniz with support for idealism and an idealist analysis of body (cf. Adams, 1994, esp. Ch. 9). In the present section, I will argue that Leibniz's explanation of how geometric truths can be laws of the phenomena of nature poses a difficult challenge for Crockett's proposal about how to understand the arguments against precise shapes. In particular, Leibniz's explanation presupposes that there is a fact of the matter concerning the error or discrepancy between a mathematical curve and the shape of a body no matter how small the margin of error. It similarly presupposes a fact of the matter, no matter how small the error margin, about the discrepancy between physical as opposed to mathematical continuity. I will argue that the existence of determinate discrepancies of the kinds just described requires that bodies have determinate boundaries and that matter be extended. My arguments will therefore provide support for the view Crockett dubs "surface realism" and attributes to Levey (cf. Levey, 2005).

Let us return to the argument against precise shapes discussed in §4.1 which begins with the premise that matter is actually infinitely divided. As I presented it (following Levey, 2005), the argument also assumes for purposes of *reductio* the existence of a body whose surface is a precise, geometrically definable curve. I believe all the parties to the current debate on Leibniz on shape, including Crockett, Levey and myself, agree that so far as Leibniz's account of the phenomena is concerned, it does often seem to human observers that the bodies around them have shapes which are definite and geometrically definable—at the least from a distance and without the benefit of microscopes.[38] However, Crockett argues that the appearance of bodies with definite boundaries is ultimately shown to be "wholly phenomenal", *i.e.*, that determinate surfaces "only exist in perception or imagination" (Crockett, 2009, p. 750). Because he finds difficulties for Leibniz in the very idea of a body with a metaphysically determinate surface, Crockett does not analyze the argument against precise shapes as taking metaphysically determinate surfaces for granted (even for *reductio*).[39] Rather, on Crockett's interpretation the main assumptions of Leibniz's argument are the infinite division of matter together with a particular account, grounded in Leibniz's texts, of the individuation of bodies. Briefly summarized, the argument goes as follows: If extended matter is infinitely divided, then everything in the universe is a fluid.[40] Some parts of the fluid evidently have more cohesiveness than others, although as I discussed above, no part of the universe is perfectly hard or perfectly fluid, either (see §4.1). What makes something *one body* rather than many is the cohesiveness of its parts, where this is understood to mean their common motion or endeavor relative to other parts of the universe (Crockett, 2009, p. 752-754). However, cohesiveness or common motion is a matter of degree. If we consider any given surface as a candidate for the boundary for a body, we will find fluids outside the surface moving together

[38]In the case of Crockett, see (Crockett, 2009, p. 756) for the textual evidence. It is also worth pointing out that Leibniz himself does not hesitate to describe ordinary cases in which an observer sees something as circular or as having some other geometric shape. See, for example, the letter to Foucher at (G1.370, L, p. 152), and the 1705 letter to the Electress Sophie at (G7.563). I will discuss the latter text in more detail in a few paragraphs.

[39]I assume that in talking of metaphysically determinate surfaces, Crockett is talking about surfaces, the existence and character of which is mind-independent.

[40]Since Leibniz is a plenist mechanist, it is also the case (on his view) that the entire universe is filled with fluid.

with parts in the interior; we will also find fluids inside the surface that do not have a motion in common with other parts in the interior. But then the surface fails to pick out a unique body, and since the candidate boundary was suitably arbitrary it follows in general that no surface could pick out a body. As Crockett puts it, "there is no fact about the world in virtue of which determinate boundaries among bodies exist" (Crockett, 2009, p. 755). Hence the apparent boundaries between bodies familiar from our everyday experience must exist only in our perception, not in the world. As a corollary to the main argument, it follows that there cannot be extended matter, either. This is because for extended matter to exist, matter must in some way be divided into parts which bear spatial relations to each other (Crockett, 2009, p. 736 n. 6). But the main argument has shown that the division of matter into parts cannot be made sense of, in effect because the boundaries of these parts cannot be made sense of. Hence Leibniz's entire argument can be taken to as a refutation of the claim that extended matter exists (Crockett, 2009, p. 759).

I wish to argue against Crockett's reconstruction of Leibniz's argument on the grounds that it renders the conclusions of the argument inconsistent with Leibniz's explanation of how the truths of geometry can be laws of the phenomena of nature. I do not dispute that the premises of the argument, as Crockett presents them, are claims Leibniz would have endorsed at some time in his career. Nonetheless, I do not believe Crockett's reconstruction of the argument represents Leibniz's own settled view of what the argument accomplishes. This is chiefly because when one looks at the texts from the later parts of Leibniz's career in which Leibniz announces the absence of precise shapes in nature as a well established conclusion, nearby one often finds the explanation of how geometric truths can nonetheless be laws of the phenomena of nature—as if Leibniz is trying to forestall a misunderstanding.[41] Hence I take it to be important to interpret Leibniz's thesis that there are no precise shapes in nature compatibly with his explanation of how geometric truths can be laws of the phenomena of nature.

To see the inconsistency between Crockett's proposal and Leibniz's explanation of how geometric truths can be laws of the phenomena of nature, note first that if Crockett is correct, it follows not just that no body in nature actually has a determinate boundary, but also that it is physically impossible for a body to have a determinate boundary.[42] As against this, Leibniz's explanation relies on the possibility of the existence of figures in nature which differ so little from geometric curves that the difference between the figure in nature and the geometric curve is less than any given quantity.[43] But for those bodies which Leibniz is now claiming to be physically possible, there must be determinate discrepancies between their shapes and geometric curves, no matter how small the margin of error between the two is allowed to be. Such discrepancies in turn presuppose that the bodies in question have determinate boundaries. For instance, suppose the boundaries of a body were allowed to be

[41]Here I have especially in mind the 1702 reply to Bayle, the 1702 letter to Varignon, and the 1705 letter to the Electress Sophie. These appear at (G4.554-571, L, pp. 574-585), (GM4.91-95, L, pp. 542-544), and (G7.558-565), respectively.

[42]I believe both Crockett and I can grant that on Leibniz's view it is metaphysically possible for there to be bodies with determinate boundaries, since God might have created atoms which are perfectly hard, and these atoms would have had determinate boundaries. See the letter to Arnauld at (G2.119). For God to do so would contradict God's wisdom, however (cf. GM3.565, AG, p. 171). Nonetheless, given facts about motion in the actual world, and given Crockett's preferred account of what makes something one body, Crockett's argument, if successful, shows that bodies with determinate boundaries are physically impossible.

[43]See the text of "De Organo" (A6.4.159) and the discussion in §4.2.

vague, leaving a collection of equally good candidate boundaries which never depart from each other by more than a nanometer. Then there will be no facts about the discrepancy between the shape of such a body and geometric shapes that hold with the margin of error of a picometer (a thousandth of a nanometer). Therefore it is physically possible for bodies to have non-vague, determinate boundaries or surfaces, even if they cannot have surfaces which correspond precisely to any geometric curve.

Recall also Leibniz's explanation of how geometric truths manage to be non-vacuous with respect to the phenomena of nature. If my interpretation is correct, the non-vacuity of geometric truths requires that there actually be shapes in nature such that the difference between those shapes and some corresponding geometric curves is smaller than the acuity of our sense perception would allow us to detect. It would therefore follow from the non-vacuity of geometric truths that bodies have boundaries determinate enough that the difference of those bodies and some corresponding geometric curves is less than a margin of error determined by the acuity of our sensation. This, too, is incompatible with there being no fact of the matter about the boundaries of any body, though it is perhaps compatible with some degree of vagueness.

It appears to follow from the argument two paragraphs ago that extended matter is at least possible, since the bodies Leibniz is asserting to be possible are surely extended and presumably also made of matter. From the fact that Leibniz also seems to countenance natural bodies with shapes that are phenomenally indistinguishable from geometric shapes, or even natural bodies that closely approximate geometric shapes, it would seem to follow that extended matter is actual. Nonetheless, I think one gets a stronger argument for the actual existence of extended matter by considering

Leibniz's statements comparing physical and mathematical continuity directly rather than by focussing on his statements about shape. The passages in the letters to De Volder and Varignon discussed in §4.2.1 indicated that physical continuity, while being importantly and strictly different from mathematical continuity, is nonetheless approximated by mathematical continuity to within any margin of error. This is part of what makes it legitimate to reason about matter as though it were spatially continuous in the mathematical sense even though it is only continuous in the physical sense. But being spatially mathematically continuous is paradigmatically what it is to be an extended thing, so that if something can be approximated to within any margin of error by spatial mathematical continuity, that thing is surely extended.

Crockett is skeptical that there could be extended matter for Leibniz largely because he doubts that sense can be made of divisions or boundaries in matter. Crockett assumes that for Leibniz, what it is for matter to be extended is for it to consist of parts that are spatially related to each other. If one cannot make sense of boundaries between parts, one cannot make sense of the parts themselves or relations between parts, either. But if the account I proposed in §4.2.1 of the way physical continuity approximates mathematical continuity is correct, then Leibniz believes that for any location in a body and any margin of error ϵ, there is a division or boundary which is closer to the location than ϵ. So not only are there some facts about where divisions in matter exist, there are facts that hold to within any margin of error, no matter how small.

At a higher level of generality, the argument I am making amounts to the following: Leibniz's explanation of the applicability of geometry to physics requires that physical objects approximate geometric objects to within very small margins of error. It also requires physical objects to have a continuity which is arbitrarily well approximated by mathematical continuity. Such approximations require various facts of the matter, to quite high precision, about the boundaries of bodies and as well as their physical continuity. Indeed, such approximations require that bodies have boundaries that are either not at all vague or the vagueness of which only comes into play for very small margins of error; they surely also require that bodies be spatially extended entities. Since these approximations are compatible with Leibniz's thesis that there are no precise shapes in nature, the latter thesis should not be taken to imply that there are no such facts.

In light of the argument so far, I believe it should be granted that for Leibniz, extended matter and extended bodies with determinate boundaries are physically real. If that is correct, then the argument against precise shapes in nature induces a distinction between two classes of surfaces, the "ideal", "mathematical" surfaces which form the subject matter of geometry, and the "real", "physical" surfaces that correspond to the boundaries of bodies. Leibniz's argument attempts to show that the two classes of surfaces contain no surface in common. It does not attempt to show that extended bodies aren't physically real. Even granting this much, I believe there is *prima facie* a promising interpretive strategy left open to one who wishes to interpret Leibniz as an idealist. Such an interpreter may argue that being physically real is compatible with being wholly phenomenal and mind-dependent. Thus even though there is an important distinction between mathematical and physical surfaces from the point of view of the foundations of physics, from the point of view of philosophy the physical surfaces are every bit as phenomenal as the mathematical ones. In the remainder of this section, I wish to argue against this way of interpreting Leibniz as an idealist. Briefly put, my argument is that according to Leibniz, so far are physical surfaces from being wholly phenomenal that they are in fact not a part of human experience at all. This lends support to the view that the reality of the shapes of bodies is mind-independent and hence real not just in a physical sense, but real *tout court*.

Consider the following passage from Leibniz's 1705 letter to the Electress Sophie:

> There are... divisions and actual variations in the masses of existing bodies, to whatever limits one should go. It is our imperfection and the defect of our senses that makes us conceive physical things as mathematical beings, in which there is some undetermined thing. And one can demonstrate that there is no line or figure in nature which gives exactly and keeps uniformly through the least space and time the properties of a straight line or circle or something else whose definition can be comprehended by a finite mind... [N]ature cannot, and the divine wisdom does not wish to, trace exactly these figures of limited essence which presuppose something determined and consequently imperfect in the works of God. However, they are found in the phenomena, or in the objects of our limited minds: our senses do not recognize and our understanding conceals an infinity of little inequalities which nevertheless do not prevent the perfect regularity of the work of God, although a finite creature could not comprehend it (G7.563).[44]

Leibniz here draws the distinction between mathematical and physical surfaces along the lines just described. Mathematical surfaces are simpler, having "limited essences" and definitions finite minds such as ours can comprehend. When bodies and their motions appear to us, their shapes appear to us as mathematical surfaces and their trajectories appear as mathematical curves. But the appearances are misleading:

[44]This is an unpublished translation by Donald Rutherford.

nothing in nature or in the works of God corresponds precisely to mathematical curves and surfaces. The boundaries and trajectories of bodies are infinitely complex, they are beyond our comprehension, and they are not part of our sensory experience of the world. So described, it is hard to see how these boundaries could be "phenomenal" or the contributions of our sensory faculties. Rather, the physical boundaries exist in nature and are part of what God does, though they are hidden from us by the workings of our minds.

Here is another way of taking Leibniz's argument: Leibniz is claiming that when physical bodies and their motions are in our perceptual ken, we perceive those bodies and their motions to correspond to mathematical curves and surfaces from which their shapes and trajectories are perceptually indistinguishable. So long as there are mathematical curves and surfaces perceptually indistinguishable from the physical boundaries and motions, we will always perceive the former and not the latter. We do not see the physical curves and surfaces for what they truly are because they are different from the corresponding mathematical objects, yet we do not see them as different. For example, Leibniz can agree to the truisms that the orbit of Mars appears to us, and that we sense its motion. Yet we sense that motion as tracing out an ellipse, which we know independently not to be full truth of the matter. The true, infinitely complex motion is not as such a part of our experience of Mars's orbit.[45]

[45] Another text by Leibniz which supports the interpretation I am offering appears in "Infinite Numbers" of 1676 (A6.3.498-499, RA, pp. 89-91). The interpretation of "Infinite Numbers" is somewhat more difficult, however, owing to the fact that Leibniz goes back and forth over the question whether we have sensory consciousness of the complexities of physical shapes or not. I believe that even as a matter of interpreting "Infinite Numbers", the view can be sustained that for Leibniz, the infinitely complex physical shapes are not part of human experience. See Levey's discussion of this passage at (Levey, 2005, pp. 78-83).

The overall thrust of this section has been to argue against idealist readings of Leibniz's argument against precise shapes in nature on the grounds that such readings are not faithful to the ways in which physical surfaces are approximated by mathematical ones. One might worry, however, that the surface realist is no better off; and the surface realist *would be* no better off if he could give no account of physical surfaces which are distinct from yet arbitrarily well approximated by traditional mathematical surfaces. In previous work, Levey has argued that Leibniz's conception of physical surfaces in many respects anticipates the modern notion of a fractal; Levey has offered the Koch curve as a particularly apt model for the kind of surfaces Leibniz takes real bodies to have (Levey, 2003, esp. §6). In a technical appendix to this work, I offer a proof that Levey's suggestion is compatible with the facts about approximation I have been stressing. In particular, it can be shown that for all ϵ, there is a continuous and everywhere differentiable curve which approximates the Koch curve to within ϵ.[46] This provides further evidence in favor of surface realist interpretations of Leibniz.

4.4 Conclusion

In this chapter, I have offered an account of Leibniz's defense of the methods of mathematical physics in light of his rejection of anything in nature which corresponds precisely to a mathematical curve or surface. If I am correct, the account explains why Leibniz would take himself to be justified in treating bodies and their motions as if they correspond precisely to mathematical surfaces and curves, which is just what he does in "An Essay on the Causes of Celestial Motions". I argued that to understand

[46] See Appendix A.

Leibniz's defense, we must distinguish between two *explananda*: the lawlike character of geometric truths with respect to the phenomena of nature, and our justification in using geometric objects as approximations to physical ones in particular cases of scientific reasoning. I also argued that Leibniz's explanations, while related to each other, are importantly different. The common element in the two explanations concerns the great extent to which the physical and the mathematical approximate one another. A consequence is that Leibniz's argument against precise shapes in nature should not be taken to be incompatible with either the physical existence or the ultimate reality of extended bodies with determinate boundaries.

Chapter 5

Geometry and Nature

In the two preceding chapters I provided an analysis of Galileo's and Leibniz's defenses of the use of geometry in studying nature. Both thinkers confronted the challenge that geometry fails to correspond to anything in nature, and in particular the challenge that nothing in nature has a precise geometric shape. Neither thinker took the challenge to geometry to be insurmountable; a common theme is that the actual shapes of bodies and their motions may be approximated by geometric objects provided certain conditions are met. Galileo's and Leibniz's acceptance of methods of approximation in the sciences, which we contemporary thinkers undoubtedly share, suggests that it is consistent to regard the use of geometry in empirical inquiry as a success while also holding that nothing in nature corresponds precisely to any geometric object or structure. Indeed, from the point of view of contemporary science, physical space is non-Euclidean, yet the 17^{th} century practice of using Euclidean geometry to study nature was a tremendous success. Looking back on 17^{th} century scientific practice, it is natural to view the earlier scientists as successfully applying geometry to various empirical matters even though nothing in nature corresponded precisely to what 17^{th} century scientists could recognize as a geometric object. We can partly explain this success by pointing out that in the relevant contexts, Euclidean space provides a reasonably good approximation to the structure of physical space.

If it is compatible with the applicability of geometry or a given geometric structure that nothing in nature correspond precisely to that geometric structure, one may wonder whether the applicability of a given geometry puts any constraints whatsoever on the relationship between nature and the structure described by the geometry. In this final chapter, I will argue that the applicability of a given geometry to nature does impose a non-trivial constraint on the relationship between nature and the corresponding geometric structure: in particular, when one represents some natural objects or processes by a geometric structure, there must be determinate discrepancies between features of the natural objects or processes and the geometric structure.[1] If we suppose we can find a pair consisting of a geometry and an aspect of nature such that there are no determinate discrepancies between the elements of the two, then there is a specific sense (to be articulated later in this chapter) in which one cannot justify the application of the geometry to that aspect of nature. This gives the sense in which determinate discrepancies between the geometry and nature "must" exist.

The thought that the applicability of a geometry does impose constraints on the geometry-nature relationship arises naturally from the discussion of approximations in the preceding chapters. Galileo gives a particularly vivid description of how the geometric natural philosopher is like the merchant who, in order to calculate how

[1]To use the language introduced in Chapter 1, I will offer an argument that the applicability of geometry is incompatible with truth of the No-Discrepancies Challenge.

much sugar is on the scale, must subtract the weight of the packaging.[2] The natural philosopher's aim, by comparison, is to demonstrate that some effect holds of real physical things and not just some fanciful abstractions. In order to do this, he must take some account of the discrepancy between the geometric representation of natural things and their real properties. For instance, he must take account of the difference between the precise geometric shape attributed to something in the context of a physical demonstration and its real (perhaps horribly complex) shape. This presupposes that there is a fact of the matter about how the geometric shape and the object's real shape do or do not differ. Otherwise the natural philosopher's method of approximation cannot even get started. Returning to Galileo's analogy, it is as if the merchant needs to calculate how much sugar he has, yet there is no fact of the matter about how much of the weight on his scale is due to the packaging.

To put my aim in this chapter in slightly different terms: In this dissertation as a whole, I have been investigating a family of challenges to the applicability of geometry, each of which attempts to articulate some way in which geometry fails to fit or to correspond to nature. An insuperable challenge in the family would be one which articulates a sense in which the failure of correspondence between geometry and nature is so severe that the applicability of geometry can no longer be accounted for. I will argue in this chapter that there is such an insuperable challenge: namely, the challenge that there are no determinate discrepancies between geometric structures and aspects of nature. Part of my concern in the previous interpretive chapters has been to argue that the historical figures I discuss deny the soundness of the challenge.

[2]See especially §3.3.

I now wish to provide my own arguments against its soundness.

I will proceed by giving a critical examination of a view which William Tait attributes to Plato and which Tait appears to endorse himself. The view Tait develops attempts to account for the role of geometry in everyday life and in the exact sciences while denying that geometric representations of natural phenomena are approximations of them. They are not approximations precisely because there are no determinate discrepancies between properties of natural phenomena and geometric objects or structures. Rather, the relationship between some natural item and a geometric object is the relationship between an individual and the Platonic Form it participates in. My strategy will be to expose difficulties with Tait's conception of the relationship between geometry and nature which would generalize to other views which attempt to deny the existence of determinate discrepancies between geometric structures and aspects of nature.

The structure of the chapter is as follows. In §5.1, I will give an outline of Tait's positive account of the relationship between natural phenomena and geometric objects. In §5.2, I will develop several arguments against the adequacy of Tait's account as a description of the relationship between geometry and nature in scientific practice. Finally, in §5.3 I will assess the import of a key motivation for Tait's account, namely the thought that bodies do not have determinate boundaries.

5.1 Forms and Individuals

Tait develops a conception of the relationship between natural phenomena and geometric structures in two articles aimed primarily at interpreting Plato's understanding of the exact sciences (Tait, 1986, 2002). On Tait's reading, Plato is giving a description of exact science which present-day thinkers "can agree with" (Tait, 2002, p. 178). I take this to mean that the conception of exact science Tait attributes to Plato is one that Tait endorses at least in its chief aspects. Since my present aim is to examine the view Tait describes on its own merits, I will simply refer to the view as Tait's and set aside any question of faithfulness to Plato.

Tait seems to grant that, at some stage in our individual or cultural history, we might conceive of geometry as a theory of the shapes or other broadly quantitative properties had by bodies or other natural phenomena (Tait, 1986, p. 170). As we become more sophisticated in our understanding of geometry and natural phenomena, however, we learn that "the marks on a chalkboard and the surveyor's line of sight are not really geometric objects" (*loc. cit.*). Not only is a cardboard box not "perfectly" a right rectangular prism, it is not even the sort of thing which has edges or vertices in the precise geometric sense (Tait, 2002, p. 182). Despite the fact that the marks on a chalkboard are never perfectly, *e.g.*, a right triangle, part of what it is to learn geometry is to learn to see some chalk marks as right triangles in the appropriate circumstances. The typical circumstance Tait seems to have in mind is one where there are some marks on a chalkboard which are at least roughly triangular in an ordinary sense and where the instructor tells us that the triangle is a right triangle. This provides us with a partial explanation of the relationship between an individual and the Form it participates in: it is that relationship which holds between a collection of marks on the chalkboard and a geometric right triangle in the situations just described. It is important to note that on the basis of this explanation, the Form Right Triangle does not pick out any precise extension and therefore should not be thought of as a universal (Tait, 2002, p. 184). The many marks on the chalkboard which participate in the Form right triangle may have very different shapes in an ordinary sense, and there is no clear cutoff beyond which some marks are too deformed to count as a right triangle.

Tait presents a similar account of the relationship between geometric structures and natural phenomena found outside of the lecture hall. No aspect of natural phenomena strictly corresponds to any geometric object or structure: "[W]e should not think of the phenomena as providing a well-defined model of the language of geometry in which the theorems fail to hold. Rather, they simply fail to provide a well-defined model" (Tait, 1986, p. 167). Thus the trajectory of a planet could not "literally" or "absolutely" be an ellipse. Nonetheless, in mathematical astronomy it may be reasonable to idealize the phenomena in such a way as to treat the trajectories of the planets as ellipses. It is the phenomenon of an orbital motion *as idealized* which is (or at least might be) an ellipse in a literal or absolute sense. The actual motion can be an ellipse at most "roughly". A significant difference between the setting of the lecture hall and the setting of natural science is that in the case of the latter, the sensible objects do not come to us with verbal descriptions telling us that they are, say, ellipses rather than ovals. Tait does not offer his own theory of how a scientist knows which geometric idealization is the appropriate one—in Plato's language, which Forms the individuals participate in. In effect, this issue appears to be left as a matter of the competence of the scientist.[3]

[3] Tait does give what he takes to be Plato's account: "[T]he phenomena are created in the image of the Forms, and these are implanted in the soul so that experience recalls them to us as the

Tait does provide some considerations which argue for, or which at least explain, his contention that no natural phenomena correspond strictly to any geometric object or structure. The crux of these considerations is that the notion of distance, which is fundamental to (at least Euclidean) geometry, is not a well defined notion when taken over the phenomena. In my view, the considerations Tait raises about distance are best analyzed as two separate lines of argument. The first takes "phenomena" in the old sense of "appearance" and argues that the notion of distance is not well defined given the limits of human perception. It therefore concerns bodies and other aspects of nature *as they are sensed*. The important limit on human perception is that a human observer could never tell by sense perception that two lengths are incommensurable with each other (Tait, 2002, p. 181). This implies that the geometric concept of distance outstrips anything we could establish by perceptual experience, so that nothing in perceptual experience corresponds precisely to the geometric notion (Tait, 1986, p. 159). A related perceptual fact which seems relevant here, though Tait does not mention it, is that a human observer can be shown three line segments, l_1, l_2, and l_3, such that by perceptual standards $|l_1| = |l_2|$, and $|l_2| = |l_3|$, yet $|l_1| \neq |l_3|$.[4] This drives home the thought which Tait does emphasize, namely that the phenomena do not correspond precisely to the geometric notion of equality of length.

The second line of argument ignores the limits of human sense perception and attempts to show that bodies themselves do not have well defined lengths or bound-

right form of structure in terms of which to understand our sense experience. Why *these* forms of structure? Because they are 'best' " (Tait, 1986, p. 157). I do not assume that Tait means to endorse Plato on this point.

[4]This follows from the fact that there are thresholds beneath which human observers cannot discern differences in length. See (Krantz, Luce, Suppes, & Tversky, 1971, pp. 26-28).

aries. Tait insists that when we regard a body as having a well defined length or boundary, we are already idealizing it, in effect taking it to occupy some particular region of space at an instant. But "[i]n the physical theories in which we employ this idealization, there is nothing that corresponds precisely to the notion of a sensible object. For example, the log [mentioned earlier] will correspond to a region of high mass/energy, but there is no way to pick out precise boundaries for this region" (Tait, 1986, p. 160). One might think that if our physical theories countenance such entities as spatial or spatiotemporal regions, then the relationship between the natural world and the geometry which gives the abstract structural description of physical spacetime would be precise correspondence of the sort Tait wishes to deny. Tait would presumably have to view the representation of the natural world in terms of a geometric spacetime as yet another idealization. This is a thorny issue to which we will return in §5.3.

To keep Tait's two lines of argument straight, I will use terms such as "natural phenomena" and "sensible object" when the issue concerns the world as it is sensorily perceived. I will use terms such as "nature" and "body" when the issue concerns the world independent of considerations of human sensory faculties. With the exception of the passage cited in the last paragraph, Tait appears to be concerned in the first instance with the relationship between geometry and sensible objects. However, Tait appears to take the relationship of geometry to sensible objects as being analogous to its relationship to nature. I am less convinced that the relationship between geometry and the phenomena is a good guide to the relationship between geometry and nature; I will strive to keep the two relationships separate in my discussion.

From Tait's contention that natural phenomena do not provide any well defined model of the language of geometry it follows fairly straightforwardly that on Tait's view, there is no isomorphism between the structure of Euclidean space and the phenomena. This is because an isomorphism would provide the means to define the language of geometry in terms of natural phenomena. Moreover, I presume Tait's view does not uniquely apply to Euclidean geometry but would generalize to a broad variety of geometries with suitably precise notions of distance and angle measure. Put briefly, Tait's view implies that the world lacks geometric structure even giving "geometric" a fairly broad reading, *i.e.*, as picking out a fairly wide class of geometric structures. Now even if the world did not have a structure which strictly corresponds to any of the geometric structures just alluded to, it might nonetheless have a structure which suitably *approximates* one of them. It is important to see that Tait also rejects this suggestion. In a discussion of the way in which the Pythagorean theorem applies to the phenomena, Tait writes the following:

> The surveyor does indeed apply the Pythagorean theorem and gets good results. But the results, expressed in terms of empirical measurements and constructions, are only "rough." And one should not take "rough" here to mean "approximate." For example, the circle can be approximated to any degree of accuracy by an inscribed regular polygon. But here the difference between the two figures is itself a precise magnitude, an area. But the sense in which the sensible figure S [where S is some particular sensible triangle – D.M.] is roughly right triangular or in which the result of the empirical construction roughly corresponds to [the Pythagorean theorem] is different from this. It is not a case of one geometric object (in our sense) differing from another by some precise amount: one of the terms of the correspondence is such that the geometric ideas do not perfectly apply to it. (Tait, 2002, pp. 183-184)

This passage provides the strongest textual evidence that on Tait's view, geometric representations of natural phenomena are not approximations. This is because an approximation requires a determinate discrepancy between two objects, and determinate discrepancies are missing when one considers a pair consisting of a geometric and a sensible object. The relation "x approximates y" can hold only between geometric or otherwise suitably idealized objects. In describing his view, Tait sometimes refers to Whitehead's claim that nature as perceived has ragged edges.[5] He also describes the exemplifications in the phenomena of mathematical concepts such as magnitude and quantity as "blurred" (Tait, 1986, p. 161). Both of the latter informal descriptions of his view seem aimed at capturing the idea that determinate discrepancies between geometric and sensible objects do not exist.

Although Tait often emphasizes the lack of correspondence between geometry and natural phenomena, his aim is not to deny the applicability of geometry to the phenomena. Rather, his aim (following Plato) is to clear the ground of simple-minded accounts of the applicability of geometry and prepare the way for a more sophisticated account. In the next section I will give a critical analysis of Tait's more sophisticated account.

5.2 Applicability and its Presuppositions

A mathematical theory such as geometry can be applied in a natural scientific theory in a variety of ways. Here I will be concerned primarily with the applicability of geometry in a deductive sense: a geometric theory is applied deductively in a given scientific theory when the scientific theory takes geometric theorems as assumptions

[5]For an example of Tait's references to Whitehead, see (Tait, 1986, p. 181). For the original passage in Whitehead, see (Whitehead, 1920, p. 50).

in deductive arguments. So, for example, Euclidean geometry is applied deductively in the theory developed in Euclid's *Optics* because the latter contains arguments which appeal to geometric theorems in order to derive conclusions about visual rays and perspective (cf. Euclid, 1972). Deductive applications of mathematics, and in particular of geometry, are pervasive in the sciences. An adequate account of the applicability of geometry should not be at odds with the legitimacy of its deductive applications. The main charge I wish to make against views which deny the existence of determinate discrepancies between aspects of nature and geometric structures is that they are at odds with the legitimacy of geometry's deductive applications.

I will proceed by examining Tait's analysis of deductive arguments in the exact sciences which appeal to geometric theorems. I will ultimately argue that the difficulties with Tait's analysis can be suitably generalized to cover similar views.

In the discussion of the surveyor I cited in the preceding section, Tait considers the following argument concerning a sensible right triangle S:

(T1) S is right triangular.

(T2) The squares on the sides of a right triangle are equal to the square on the hypotenuse.

∴ (T3) The squares on the sides of S equal the square on the hypothenuse.[6]

Tait provides an analysis of this argument in terms of the theory of Forms. Using

[6]See (Tait, 2002, p. 183). Tait is primarily concerned with the way in which (T1) and (T2) amount to an explanation of (T3) rather than a deductive argument for it. However, he clearly recognizes that (T1) - (T3) is a deductive argument (Tait, 2002, p. 184). I should also note that while I am examining the same argument as Tait, I am altering Tait's labeling and order.

"S" as a name, letters like "f" and "g" for predicates, and Greek letters such as "Φ" to denote forms, Tait analyzes (T1) - (T3) schematically as:

($T1_f$) S is f (*i.e.*, S participates in Φ)

($T2_f$) Φ is g

$\therefore$ ($T3_f$) S is g.[7]

That is, the first premise of the argument relates S to a Form, Φ, Right Triangle. In §5.1 I reviewed some of Tait's explanatory remarks about what that relation amounts to. The second premise is a proposition about the Form Right Triangle, namely the Pythagorean theorem. On Tait's analysis the propositions of geometry are just claims about Forms (Tait, 2002, p. 185). Finally, though Tait does not say just which Form is involved in the conclusion, the conclusion surely relates S again to a Form.

The difficulty is that there is no interpretation of this argument open to Tait on which the argument is sound. If we read (T1) - (T3) in the straightforward way, so that the premise according to which S is right triangular is interpreted in the literal or absolute sense, then the argument is valid. The difficulty is that S, being a sensible object, is not literally or absolutely a right triangle. S is at best "roughly" a right triangle. So on this first reading, the argument fails to be sound because its first premise is not true. Suppose, then, that we insist on reading (T1) as ($T1_f$), assuming along with Tait that S is at best a rough exemplification of the Form Right Triangle. In that case, the difficulty is that (T2)—which on both Tait's analysis ($T2_f$) and

[7](Tait, 2002, p. 183)

by my own lights is the Pythagorean theorem—does not concern itself with objects which are only roughly right triangular. It tells us what is the case with objects which are exactly right triangular. Hence on Tait's preferred analysis the argument is invalid, and the appearance of validity rests on an equivocation. We might be able to make a valid argument by replacing (T2) with a different statement, namely that if something is *roughly* a right triangle then the squares on its sides are *roughly* equal to the square on its hypotenuse.[8] But the Pythagorean theorem does not assert this; replacing (T2) with a claim about rough right triangles amounts to a change of subject. Moreover, I think a strong case could be made that Euclidean geometry does not deal with conditions which hold only roughly in Tait's sense of the word "roughly".

In order to shed light on the defect of the argument, I would like to compare ($T1_f$) - ($T3_f$) with a well known deduction from the lore of Newtonian physics. In his article "From the Phenomenon of the Ellipse to an Inverse-Square Force: Why Not?", George Smith considers one way Newton *might have* argued that the planets in our solar system are subject to an inverse-square force directed at the Sun, but in fact *did not* so argue (Smith, 2002a). The argument appears to have become part of the lore of Newtonian mechanics, and even attributed to Newton himself, because Laplace used it in 1798 in his *Celestial Mechanics* (Smith, 2002a, p. 32). The argument runs as follows:

[8]This is the obvious replacement, though strictly speaking one could replace the premise with many other premises so long as they contained information about objects which are roughly right triangles.

(E1) The orbital trajectories of the planets are ellipses with the Sun at one focus.[9]

(E2) If the orbital trajectories of the planets are ellipses with the Sun at one focus, then the planets are subject to an inverse-square force directed at the Sun.

$\therefore$ (E3) The planets are subject to an inverse-square force directed at the Sun.

Assuming Smith's account, the problem with this argument, and the reason Newton chose not to use it, is parallel to the problem with the previous argument. Interpreted in the straightforward and literal way, the argument is valid. However, Newton knew at a fairly early stage, very likely before the *Principia* of 1687, that the orbital trajectories of the planets were not exactly elliptical (Smith, 2002b, p. 153). Therefore (E1) is not true; at best (E1) holds to high approximation. What would be needed for a sound argument, then, is not (E2) but rather (E2a):

(E2a) If the orbital trajectories of the planets are ellipses with the Sun at one focus to high approximation, then the planets are subject to an inverse-square force directed at the Sun (perhaps: to high approximation).

The defect of (E2a) is that any number of force laws are compatible with the planetary orbits being Keplerian ellipses to high approximation, including the law according to which the planets are subject to a force directly proportional to the planet's distance from the center of the ellipse. Put in numerical terms, if f is the force on the planet and r is its distance from the center of force, then f may be proportional to r^n for

[9]That is to say, the planetary orbits are Keplerian ellipses.

$-2 \leq n \leq 1$ even though the planet's orbit is a Keplerian ellipse to high approximation. For the technical details I refer the reader to Smith (2002a). The basic moral should be a familiar one: behavior that holds at a limit, or when a condition is met precisely, is often very different from behavior that holds near the limit, or when the same condition is met only approximately. (E2) is a claim about Keplerian ellipses which is *fragile* in just this way: it attributes a property to Keplerian ellipses which need not hold even approximately for trajectories which are only approximately Keplerian ellipses. If Smith's account is correct, Newton recognized this fact and instead developed arguments which did not rest on fragile connections. Rather, Newton took the trouble to prove that if the planetary orbits are very nearly circular and also stationary to high approximation, then the corresponding bodies are subject to an inverse-square force to high approximation (Smith, 2002a, pp. 32-33).

I can now state in more general terms, *i.e.*, in terms not specific to Tait's preferred account, the difficulty with maintaining that there are no determinate discrepancies between geometric structures and aspects of nature. To make a deductive application of geometry, one usually begins by claiming some correspondence between a geometric structure and some aspect of nature. One then takes the theorems of the relevant geometric theory as holding also for that aspect of nature. But if there are really no facts of the matter about the extent to which the aspect of nature differs from the geometric structure—or perhaps just the one fact that it *does* differ to some extent or other—then there is no sufficient reason to maintain that the geometric theorems still hold. Even if one insists that the difference between the geometric structure and nature is very small though otherwise not determinate, the difficulty remains for the familiar reason that behavior which is very close to a limit may be quite different from behavior at the limit. In other words, on the account of geometry offered by my opponents, typical deductive applications of geometry are defective for the same reason Newton held the derivation of the inverse-square from the Keplerian ellipse to be defective. Such views are therefore generally at odds with geometry's deductive applicability.

In the remainder of this section, I would like to consider two responses to the objection I have just raised. The first response is to point out that in the example of the application of the Pythagorean theorem discussed above, things are not as bad as I have just argued. After all, we have for any triangles with sides a, b, and c that $c^2 = a^2 + b^2 - 2ab\cos\theta$, where θ is the angle opposite side c. But this means as long as θ is approximately 90 degrees, $2ab\cos\theta$ will be approximately equal to 0, and we will have approximately the relationship between the squares on the sides of the triangle claimed by the Pythagorean theorem. My opponent could maintain that at least in the case of the Pythagorean theorem, so long as S is roughly a right triangle, the Pythagorean theorem will hold roughly for S. Although such facts are not explicitly part of arguments which apply the Pythagorean theorem, the arguments' strength may be taken to rely on those implicit facts.

I reply that the application of the Pythagorean theorem above is not the general case. In the application of the Pythagorean theorem, we are lucky that behavior close to the limit—concerning a triangle which is nearly right—is relevantly close to the behavior at the limit—concerning a precise right triangle. As the example of the argument from the Keplerian ellipse to the inverse-square force shows, this is not in general true. In geometry one finds many examples in which behavior near a limiting case is unlike behavior at the limit: to choose the most basic example, parallel lines have no point in common, but lines which are very nearly parallel do.[10] One way of reformulating my main objection to Tait would be to say that Tait's account as it stands appears to treat all cases of the deductive application of geometry on the model of the special, lucky case of the application of the Pythagorean theorem. But by failing to distinguish between arguments which are fragile in the way the ellipse argument is fragile from arguments which are robust (*i.e.*, not fragile), the attitude Tait ought to take towards deductive applications of geometry in general is the attitude one has towards the case in which behavior at the limit is unlike behavior near the limit.[11]

[10] Another only slightly less basic example: If one chooses two points on the circumference of a circle, all of the points on the line segment connecting the two points lie inside the circle. But for points which lie only approximately on the circumference of the circle this is no longer the case.

[11] The distinction between conditional claims being fragile or robust rests on another important distinction, namely the distinction between a condition's being met *exactly* and being met *approximately*. A conditional claim is fragile when it is true only if the condition described in the antecedent is met exactly. If it remains true when the condition described in the antecedent is met to high approximation, then it is robust. (One may or may not need to add the qualification "to high approximation" to the consequent of the conditional.) George Smith and others have emphasized that the distinction between the exact and the approximate is an important distinction made within physics; for example, a major open question of 17^{th} century astronomy is whether the planetary orbits are ellipses exactly or only to high approximation (Smith, 2002a, p. 35). Just as views such as Tait's seem to ignore the distinction between fragile and robust conditionals, they also seem hard pressed to recognize the exact/approximate distinction within physics. For once we have acquired a suitably sophisticated understanding of geometry (cf. §5.1), it would seem we should conclude immediately that the planetary orbits could not be exactly elliptical, nor could any aspect of nature correspond exactly to any precise geometric condition we might lay down. Thus at least at first glance, the view ignores the exact/approximate distinction in physics by denying that any precise geometric condition is met exactly. One salient way of recognizing the exact/approximate distinction within physics using resources available to Tait would be to say it only concerns the phenomena once they have been suitably idealized. The question of whether the planetary orbits are exactly as opposed to approximately elliptical is really the question of whether the orbits as idealized are exactly or merely approximately elliptical. An obvious worry about this way of drawing the exact/approximate distinction is that the question whether the planetary orbits are elliptical seems to be a question about the planetary orbits themselves, not about the planetary orbits as idealized. In any case, if this way of drawing the distinction were congenial to Tait, it is evident that a more thoroughgoing account of idealization in the sciences would be needed.

The overall thrust of my objections to views which deny the existence of determinate discrepancies between geometric structures and aspects of nature is that if such views were correct, then deductive applications of geometry would come with significant risk. This risk must be addressed in some fashion on pain of being unable to recognize the deductive applicability of geometry. I would like to examine whether Tait's way of addressing the risk is effective as a response to my objections. Tait registers the risk by insisting that geometric concepts only apply to the phenomena *to the extent that the phenomena participate in a corresponding geometric structure or Form.* For Tait, the application of the Pythagorean theorem constitutes an explanation of why the squares on S's legs equal the square on S's hypotenuse only *to the extent that S participates in the Form Right Triangle* (cf. Tait, 1986, p. 170). This suggests that Tait might prefer to shore up the application of the Pythagorean theorem we have been discussing by reading it as having the following form:

($T1'_f$) S participates in Φ

($T2'_f$) Φ is g

$\therefore$ ($T3'_f$) To the extent that S participates in Φ, S is g.

Alternatively, Tait might stick to the original rendering of the argument's conclusion ("S is g"), but he might insist that the argument be read with the disclaimer that it only holds good to the extent that S participates in the Form Right Triangle. For my purposes here, both of these ways of hedging the risk amount to the same thing.

If I am correct about the way in which Tait suggests we hedge the risk involved in deductive applications of geometry, I believe this reveals something important about the conception of exact science Tait intends to develop. It is a conception on which the primary aim of an exact science is to deepen our knowledge of some theoretical models (or Forms) and to engage with the phenomena only insofar as they correspond to those theoretical models or otherwise shed light on the theoretical models. In the case of geometry, the chief cognitive aim according to Tait is to acquire knowledge of the Forms and how they relate to each other. It is not an accident that geometry, on such a conception, ignores the precise features of sensible individuals and subsumes a number of different shapes, in the ordinary sense, under one Form. Part of the benefit of pursuing an exact science (on Tait's conception of an exact science) is precisely that one may achieve generality by ignoring the variability in the phenomena.

The cost of the added generality gained by pursuing an exact science in the way Tait describes is that one has to take a very modest view of the applicability of the science. This is because behavior which is close to the conditions laid down by an exact science is not always relevantly similar to the behavior which obtains when the conditions are met exactly. I do not wish to claim that it is impossible, or even always irrational, to pursue exact science in the way Tait is suggesting. Sometimes our aim in the sciences is primarily to deepen our knowledge of geometric structures or other abstract models. However, I do wish to claim that some branches of knowledge which we would ordinarily consider to be exact sciences, for example physics as Newton and many others have pursued it, cannot be exact sciences in Tait's sense. The scientific practices simply do not conform to Tait's description. Newton does not derive the inverse-square from the ellipse and then hedge his bets by adding the disclaimer: the planets are subject to an inverse-square force directed at the Sun *to the extent that their orbits are Keplerian ellipses*. Rather, Newton gives an argument that the planets are subject to an inverse-square force at least to very high approximation given that their orbits are circular and stationary to high approximation. Registering these approximations requires taking a view of the planetary orbits as having some determinate discrepancy (perhaps equal to zero) with respect to various precise geometric conditions. Hence in some sciences since at least the time of Newton, notably in much of physics, geometry and the world do not relate in the way Tait supposes they must in an exact science.[12]

5.3 Nature, Shapes, and Geometric Structure

In the preceding section I argued that views (such as Tait's) according to which there are no determinate discrepancies between geometric structures and nature cannot make sense of the legitimacy of geometry's deductive applications in physics. If successful, my arguments show that the family of views of which Tait's is a particularly well developed example have an unsavory consequence. However, I have not dealt with the original motivations for denying the existence of determinate discrepancies between geometry and nature. Many of these motivations sound plausible, especially the thought that tables and chairs are not the *kinds* of things that have

[12]Similarly, if one assumes Tait's account of exact science to hold for a particular science, I think one must take the explanations in that science to be fairly weak. I presume that on Tait's view, even today we can use (E1) - (E3) to explain the fact that the planets are subject to an inverse-square force, albeit with the disclaimer that the explanation holds only insofar as the orbits are Keplerian ellipses. This remains the case even though (i) the orbits are only roughly Keplerian and (ii) orbits which are only roughly Keplerian need not be governed by an inverse-square force but could instead be governed by a force directly proportional to distance.

vertices or edges, or the thought that there is no non-arbitrary way to ascribe precise boundaries to a body. In this section I would like to address these motivations to assess their import for our conception of the relationship between geometry and nature.

Some of the motivations I discussed concern primarily the relationship between geometry and visual phenomenology. For example, there was the observation that the geometric concept of distance seriously outstrips anything we could establish by perceptual means (see §5.1). For much of the history of our thinking about the relationship between geometry and nature, facts about how nature *appears* to us sensorily were taken to be particularly important and informative of how nature *is*. In those circumstances, arguments from visual phenomenology were particularly relevant. I would argue that this is no longer the case. Whether locations in physical space have precise, well defined distances from one another does not hinge on our ability to discern those distances perceptually. If anything, our inability to detect the facts about precise distances in nature tells us more about our perceptual apparatus than it does about nature.

Setting aside issues of human sense perception, the main motivation for denying the existence of determinate discrepancies between geometric structures and nature was the thought that bodies really don't have determinate boundaries. When we ask about a very round ball whether it is a sphere, it is at least plausible to answer that not only is the ball not a sphere ("that's not the kind of thing it is"), but since it doesn't have precise boundaries there's no determinate answer about the extent to which it differs from being a sphere. Following Tait's way of setting up the issue, when we consider the ball as having some precise shape, we are really just picking out a particular region of space at a particular time which we imagine the body to occupy. At a more fundamental physical level, that spacetime region will contain higher mass than some regions nearby. But there will be no sharp joints in spacetime which stand out as the boundaries of the ball. We would be making an arbitrary choice in fixing on any particular boundary.

I believe Tait's way of setting up the issue of the boundaries of bodies is sensible. The problem amounts to figuring out what in physical spacetime corresponds to bodies. Note, however, that this way of setting up the problem assumes a prior relationship between geometry and nature. For the problem assumes that physical spacetime has a geometric structure containing at least spatiotemporal regions. The geometric structure in question is presumably a model of relativistic spacetime theory, *i.e.*, the spacetime theory contained in general relativity. In that case, one might hope that the relationship between geometry and nature could be given by simple structure preserving mappings between relativistic geometric structures and physical spacetime. It may be granted that the relationship between bodies' shapes and physical spacetime is much more complicated. Nonetheless, at the fundamental level one would not need complicated notions (such as Platonic participation) to account for relationship between geometry and nature.

The difficulty with this last suggestion, of course, is that a view which denies the existence of determinate discrepancies between geometry and nature will go on to deny that there are determinate discrepancies between physical spacetime and the geometric structures described by general relativity. For instance, on Tait's view the relationship nature has to relativistic spacetime is presumably the very relationship an ordinary round ball has to the geometric sphere: Platonic participation. When we move to the level of the geometric structure of physical spacetime, however, I do not see what the motivation is for denying that physical spacetime has the same structure as some of the geometric models described by general relativity. Nor do I see the motivation for denying that physical spacetime at the very least does approximate those geometric models in a way that presupposes the existence of determinate discrepancies between the models and the real structure of spacetime. Indeed, if my arguments in §5.2 are successful, they show that the view according to which there are no determinate discrepancies between geometric structures and physical spacetime cannot make sense of the deductive applicability of geometry in the theory of relativity. My arguments could be taken as a motivation for thinking that the geometric structures described by general relativity do at least approximate the real structure of physical spacetime even if they don't quite capture it. The arguments could also be taken in the spirit of a methodological guideline: to ensure the deductive applicability of a geometric theory, make sure the theory picks out a geometric structure which at least approximates the structure of physical space or spacetime.

In short, I believe there remains a difficult problem concerning what account to give of our practices of ascribing shapes to bodies in light of our more fundamental theory of the geometric structure of spacetime. It is beyond the scope of this chapter to flesh out the details of such an account. But I think it is plausible that on a good account of shape attribution, whether a body has a particular shape may involve sensitivity to the context of ascription. In particular, it may depend on what conclusion one is trying to demonstrate. This may in turn substantiate the view that bodies do not have determinate boundaries independent of a context of attribution. Such an account of attributions of shapes to bodies would not, however, be particularly revealing as to the fundamental relationship between geometry and nature. That relationship concerns rather the relationship between certain geometric models and physical spacetime. I would propose that this latter relationship be understood in terms either of sameness of structure or in terms of approximation of structure. Otherwise one is hard pressed to recognize the deductive applicability of the geometry.

5.4 Conclusion

In this dissertation I have considered a family of challenges to the applicability of geometry, each of which articulates a sense in which geometry has been held not to correspond to nature. I believe these challenges play two important roles for our understanding of science. First, in their historical context, the challenges represent practical hurdles to the development of successful mathematical natural science. This is especially the case with the challenge according to which nothing in nature corresponds precisely to any geometric curve or surface. Surmounting that challenge requires the development of a variety of tools, chief among them the vast expansion of geometric structures for which we have an adequate theory, on the one hand, and techniques of approximation, on the other. In discussing Galileo and Leibniz, I gave special emphasis to their contributions towards an understanding of geometric approximations.

Techniques of approximation allow a given geometric theory to be useful for describing and reasoning about various aspects of nature despite significant failures of correspondence between nature and the geometric structures described by the theory. This raises the question whether the applicability of a given geometry imposes any constraints whatever on the relationship between the structure it describes and the aspect of nature to which the theory is being applied. In this final chapter, I argued that there is such a constraint: there must at least be determinate discrepancies between the geometric structure and the aspect of nature to which it is being applied. The reason why there is such a constraint is simply that without it, the techniques of geometric approximation cannot get started. A concrete manifestation of the problem is that in the absence of determinate discrepancies between geometry and nature, the deductive applicability of geometry can no longer be accounted for.

Hence the second role challenging the applicability of geometry has for our understanding of science is as a means for investigating the relationship between geometry and nature in those branches of science which apply geometry, notably physics. So far I have spoken as if there are two such relationships, structural similarity and approximation. The notion of approximation especially deserves further examination. Like most intuitive notions, it surely breaks apart into a number of distinct concepts. The concept of one object's being a good approximation of another depends on some relevant measure of distance or resemblance, and these measures differ according to context. Even within mathematics, approximations are extremely heterogeneous, so that one object is a good approximation of another if, for example, they never stray very far from one another by Euclidean measures of distance, or if their first derivatives agree at a particular point, or if they are the same except over a set of measure zero, *etc.* I believe that it is by paying closer attention to the notions of approximation operative in empirical science that we can shed light on the relationships between geometric structures and nature when geometry is applied.

Appendix A

An Everywhere Differentiable Approximation of the Koch Curve

It is a consequence of Leibniz's account of the physical world that no body ever has a shape which corresponds precisely to any geometric surface, nor does the motion of any thing correspond precisely to any geometric curve.[1] This fact raises at least two important questions for Leibniz: (i) Which (if any) mathematical objects do provide accurate representations of the shapes and trajectories of bodies?; (ii) How can mathematical physicists employ the ordinary geometric curves in their work despite the failure of correspondence between those curves and the physical objects? In §4.2 I provide my interpretation of Leibniz's answer to the latter question. The main idea is that the shapes and trajectories of bodies may be suitably well approximated by traditional geometric curves; in particular, they may be approximated to a margin of error small enough that the discrepancy is of no practical import for the physicist or

[1] See Chapter 4 for extensive discussion of this issue.

even imperceptible. This leaves open the question whether *arbitrarily* close geometric approximations for the shapes and trajectories of physical objects always exist. In other words, given any physical object's shape or motion and any margin of error ϵ, does there exist a geometric curve which approximates that shape or motion to within ϵ? To the extent I can discern an answer to this question in Leibniz's writings, Leibniz appears to take the optimistic attitude that sufficiently close geometric approximations do generally exist.[2]

Leibniz's contention that real shapes and motions can be so well approximated by geometric curves places some constraints on Leibniz's conception of physically real shapes and motions. However, it does not provide us with a rich enough conception of those shapes and motions in order to know how to represent them accurately with mathematical objects and thereby answer question (i). I take Samuel Levey to have provided the most promising suggestion for an answer: for Leibniz, the boundaries and motions of bodies are fractally complex and may be thought of on the model of fractals such as the Koch curve (Levey, 2003). Hence although a body whose trajectory traces out a perfect circle is physically impossible for Leibniz, a body whose trajectory traces out a Koch curve is at least physically possible. Moreover, we can think of the actual trajectories of bodies as closely resembling fractals such as the Koch curve even if such trajectories are not detectable by our eyes or other instruments.

The aim of this appendix is to show that it is consistent to view the shapes and motions of bodies both as being fractally complex and as being approximated to

[2]On this topic see especially §4.2.2.

within any margin of error by traditional geometric curves, indeed even by geometric curves which are differentiable everywhere. This is demonstrated by defining an approximation to the Koch curve which is differentiable everywhere and which never departs from the Koch curve by more than any given margin of error ϵ.[3] I take this to show that our best accounts of Leibniz's answers to (i) and (ii) are compatible with each other and represent a consistent Leibnizian view of the relationship between geometry and physical reality.

A.1 The Approximation

A.1.1 Overview of the Proof

The Koch curve is the limit towards which a certain sequence of curves converges uniformly. Informally speaking, the members of this sequence are generated in the following way: one begins with a line segment of length $= 1$ which represents the 0^{th} curve in the sequence; given the k^{th} curve of the sequence, one generates the $k + 1^{st}$ curve by dividing each line segment of the k^{th} curve into three line segments, erecting an equilateral triangle on the middle segment (in what one defines to be the "positive" direction), and then erasing the base of the triangle. Figure A.1 provides an image of the first three stages of this process; Figure A.2 gives the seventh stage.[4]

[3]Talking of one curve never departing from another by more than some error margin ϵ is a helpful though somewhat loose manner of speech. What it really amounts to is that for any value of the input parameter $x \in [0, 1]$, the distance between the two points on the curves corresponding to that value for x is less than ϵ. It should also be borne in mind in what follows that a point on a curve $k(x)$ is a point on a plane, so that an expression of the form $|k_1(x) - k_2(x)| < \epsilon$ is a claim about the two-dimensional Euclidean distance from point $k_2(x)$ to point $k_1(x)$.

[4]I have drawn all figures with the exception of A.2 using software called "The Geometer's Sketchpad". Figure A.2 was produced by an Internet user named "Fibonacci" and released under the GNU

Figure A.1: The first three stages in the iterative construction of the Koch curve.

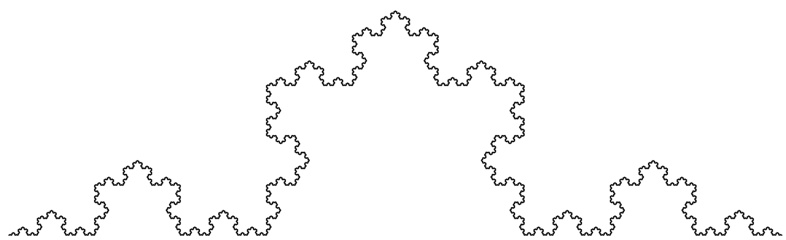

Figure A.2: The seventh stage in the iterative construction of the Koch curve.

Because this sequence of curves converges uniformly to the Koch curve, the members of the sequence themselves provide arbitrarily good approximations of the Koch curve. In other words, if one is provided a margin of error ϵ, one can always find a large enough k such that for any l greater than or equal to k, the l^{th} member of the sequence never departs from the Koch curve by more than ϵ. Thus if one merely wanted to approximate the Koch curve to within any given margin of error ϵ using a finite number of line segments connected end to end, a sufficiently advanced member of the sequence of curves just described provides the required approximation.

Despite the fact that the members of the sequence of curves under discussion are continuous and are built out of elementary geometric figures (*viz.*, lines), they are not smooth and indeed their first derivatives do not always exist. Nonetheless, for

Free Documentation License. It was downloaded from the following URL:
`http://commons.wikimedia.org/wiki/File:Koch_curve.svg`.

any given member of the sequence one can in turn define an approximation of it which never departs from it by more than any given margin of error ϵ and which is differentiable everywhere (except at its endpoints). This is accomplished in the following way: given some margin of error ϵ, one chooses a radius r ($\leq \frac{\epsilon}{2}$) of a circle which one inscribes in each of the various 60 and 120 degree angles in the curve one is approximating. One then maps those points which are sufficiently close to the vertex of an angle onto the arc of the corresponding circle, leaving the remaining points fixed. Thinking of the curves as trajectories of two particles, whereas in the original curve a particle makes various sharp turns of 60 and 120 degrees, in the differentiable approximation the particle avoids the sharp turns by taking circular paths to move from line segment to line segment. Figure A.3 gives a diagram of how to approximate the 1^{st} member of the sequence of curves which converges to the Koch curve.

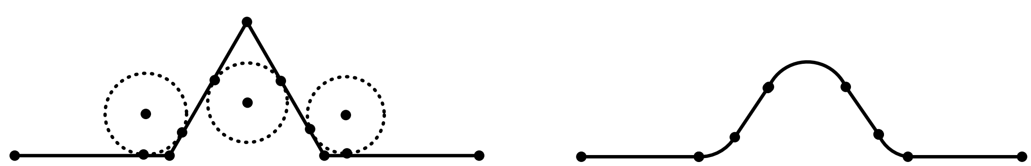

Figure A.3: To the left is the first curve in the sequence pictured together with inscribed circles which will be of use in approximating it. In the approximation to the right, one has connected the line segments with circular arcs, thereby smoothing out the sharp corners.

It follows that for any margin of error ϵ, one may approximate the Koch curve $K(x)$ to within $\frac{\epsilon}{2}$ by choosing any sufficiently advanced member $k_n(x)$ of the sequence of curves which converges to $K(x)$. One furthermore has the means to approximate $k_n(x)$ to within $\frac{\epsilon}{2}$ by a curve which is everywhere differentiable using the technique just described. This last curve will then be an approximation of the Koch curve that is differentiable everywhere and never strays from the Koch curve by more than ϵ.

A.1.2 The Koch Curve and Sequence

The Koch curve is a certain function $K : [0,1] \mapsto \mathbb{R} \times \mathbb{R}$ described by Helge von Koch in his 1904 paper "Sur une courbe continue sans tangente obtenue par une construction géométrique élémentaire".[5] $K(x)$ is the limit to which a sequence of functions $k_n : [0,1] \mapsto \mathbb{R} \times \mathbb{R}$ converges. In this subsection I will review various facts about the Koch curve which follow from Koch's original paper. The principal fact about the Koch curve needed for the present purpose of approximation is the following: Given any $\epsilon > 0$, for sufficiently large $n \in \mathbb{N}$ one has that for all $x \in [0,1]$, $|K(x) - k_n(x)| < \epsilon$.

I begin with a description of the sequence of functions $k_n(x)$. $k_0(x)$ is simply a line segment one unit long. One may define $k_0(x)$ as mapping a given parameter x with $0 \leq x \leq 1$ to the corresponding point $(x, 0)$. One may define $k_{n+1}(x)$ in terms of $k_n(x)$: one maps a given point on the base of a triangle built at the $n+1^{st}$ stage to the point of intersection of the line perpendicular to the base at the given point and the corresponding line on the top of the triangle; one maps the remaining points to themselves. For instance, to define $k_1(x)$ in this way, one proceeds as follows:

For $x \in [0, \frac{1}{3}], k_{n+1}(x) = k_n(x)$

For $x \in (\frac{1}{3}, \frac{1}{2}], k_{n+1}(x) = (x, \sqrt{3}(x - \frac{1}{3}))$

For $x \in (\frac{1}{2}, \frac{2}{3}), k_{n+1}(x) = (x, \sqrt{3}(\frac{2}{3} - x))$

For $x \in [\frac{2}{3}, 1], k_{n+1}(x) = k_n(x)$

[5] For the original paper see (Koch, 1904). For a translation into English, see (Koch, 2004).

Since $k_0(x)$ lies on the x-axis, it suffices to map a given point on the base of the triangle constructed at the 1^{st} stage to the point on the top of the triangle with the same x coördinate as the given point. The y coördinate is easily found using facts about right triangles. At $k_2(x)$ and beyond, a particular $r \in [0,1]$ does not in general map to a corresponding point (r, y); in other words, the value of the parameter is not generally equal to the value of the abscissa on the corresponding point of the curve. But this does not preclude definitions of $k_2(x)$ and beyond following the instructions given in the last paragraph. The definitions become cumbersome, however, and so I omit them.

It is straightforward to see that the $k_n(x)$ just described do converge uniformly to a limit, so I will briefly rehearse the argument. The main insight is that if one defines the $k_n(x)$ in the way just outlined, then the distance between $k_{n-1}(x)$ and $k_n(x)$ for any x is at most $\frac{\sqrt{3}}{2} \times \frac{1}{3^n}$—*i.e.*, the height of the triangles built at the n^{th} stage. Therefore for any $m \geq n$, the distance between $k_n(x)$ and $k_m(x)$ is less than
$(\frac{\sqrt{3}}{2} \times \frac{1}{3^{n+1}}) + (\frac{\sqrt{3}}{2} \times \frac{1}{3^{n+2}}) + (\frac{\sqrt{3}}{2} \times \frac{1}{3^{n+3}}) \ldots$
$= \sum_{k=n+1}^{\infty} \frac{\sqrt{3}}{2} \times \frac{1}{3^k} = \frac{\sqrt{3}}{2} \times \frac{1}{3^{n+1}} \sum_{k=0}^{\infty} \frac{1}{3^k} = \frac{\sqrt{3}}{4 \times 3^n}$.

It follows that the sequence $k_n(x)$ is uniformly Cauchy and therefore uniformly convergent. For given an $\epsilon > 0$, one can calculate a natural number N such that for all natural numbers $m, n > N$ and $x \in [0,1]$, $|k_n(x) - k_m(x)| < \epsilon$: one merely needs an N that satisfies $\frac{\sqrt{3}}{4 \times 3^N} < \epsilon$. Given that the sequence $k_n(x)$ converges uniformly to a limit $K(x)$, the principal fact I need arises as an immediate consequence of the definition of "uniform convergence". For by definition the sequence $k_n(x)$ is convergent to $K(x)$ just in case for every $\epsilon > 0$ there exists $n \in \mathbb{N}$ such that for all $x \in [0,1]$ and all natural numbers $m \geq n, |K(x) - k_m(x)| < \epsilon$. One is therefore assured that given any $\epsilon > 0$, there is an n large enough so that for all $m \geq n$ and $x \in [0,1]$, $|K(x) - k_m(x)| < \epsilon$.

A.1.3 A Differentiable Approximation of $k_n(x)$

The immediate goal is to show that given any $\epsilon > 0$ and function $k_n(x)$, one can define a corresponding everywhere differentiable function $f_n : [0,1] \mapsto \mathbb{R} \times \mathbb{R}$ such that for all $x \in [0,1], |k_n(x) - f_n(x)| < \epsilon$. The strategy is as follows. Given an $\epsilon > 0$ and some $k_n(x)$, one first notes that $k_n(x)$ is simply a finite collection of line segments connected end-to-end at either 60 or 120 degree angles. Choosing a radius $r \leq \frac{\epsilon}{2}$, one inscribes circles in the interiors of each of the angles of $k_n(x)$. Then one maps any point P on $k_n(x)$ lying between the two points of tangency of any given circle to the point P' on the circumference of the circle which is on the line connecting the center of the circle and P. Such P and P' will be shown never to be more than ϵ apart from each other. For the rest of the points of $k_n(x)$, one simply lets $f_n(x) = k_n(x)$, in which case there is no distance at all between them.

Given a particular $n \in \mathbb{N}$ and $\epsilon > 0$, one may define in this way an approximation $f_n(x)$ of $k_n(x)$ (see Figure A.4). One first chooses $r = \min(\frac{\epsilon}{2}, \frac{1}{3^{n+1}})$.[6] Using this r, one inscribes a circle in the angle ABD by first bisecting the angle and then putting the center of the circle with radius r at $\frac{2r}{\sqrt{3}}$ away from B on the angle bisector in the interior of the angle. The x coördinates of the points of tangency E and G are then $\frac{1}{3^n} - \frac{r}{\sqrt{3}}$ and $\frac{1}{3^n} + \frac{r}{2\sqrt{3}}$, respectively. Thus for $x \in [0, \frac{1}{3^n} - \frac{r}{\sqrt{3}}]$, define $f_n(x) = k_n(x)$.

[6] One chooses r no larger than $\frac{1}{3^{n+1}}$ to make sure that the circles are at the right scale. Otherwise if someone chose a very large ϵ, there would be no points of tangency of the circles with the line segments.

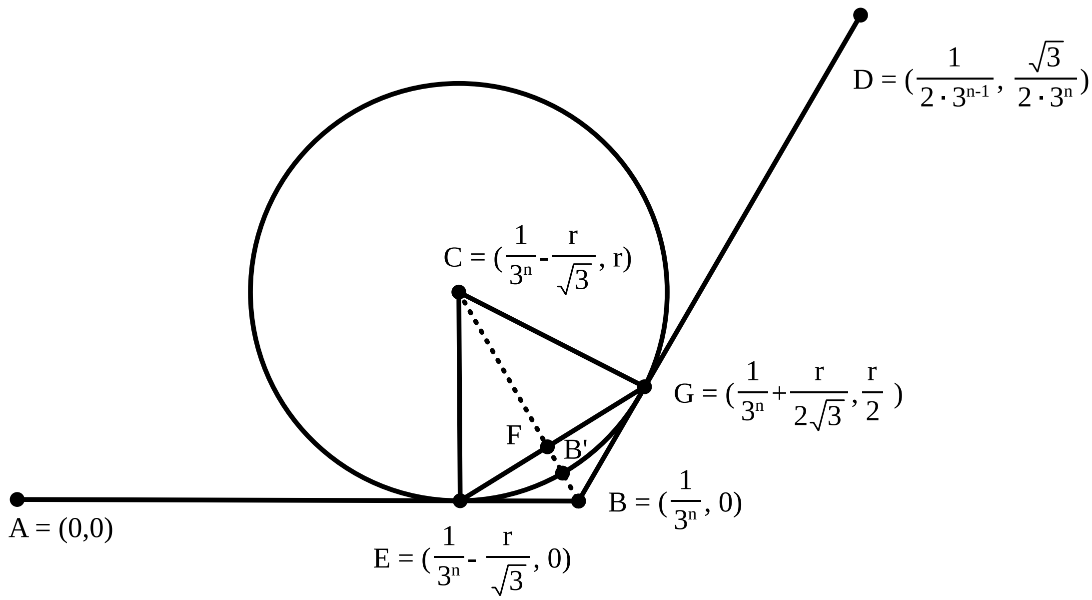

Figure A.4: The first two segments of $k_n(x)$ and the circle used in the approximation. Point B is mapped to B' and the points between E and G are similarly mapped to corresponding points on the circle.

For $x \in (\frac{1}{3^n} - \frac{r}{\sqrt{3}}, \frac{1}{3^n} + \frac{r}{2\sqrt{3}})$, if $k_n(x) = (a, b)$ then define

$f_n(x) = [\frac{r}{\sqrt{(a-(\frac{1}{3^n}-\frac{r}{\sqrt{3}}))^2+(b-r)^2}} \times (a - (\frac{1}{3^n} - \frac{r}{\sqrt{3}}), b - r)] + (\frac{1}{3^n} - \frac{r}{\sqrt{3}}, r)$.[7]

To continue the definition, one needs to construct another circle at D. But before I continue, I should note that over the first interval just defined, the distance between $k_n(x)$ and $f_n(x)$ is 0, and over the second interval the distance never exceeds $|FB|$, the height of the triangle GBE. This is because the arc of the circle $GB'E$ lies entirely within the triangle GBE. But $|FB| = \frac{r}{2\sqrt{3}}$, so that $|FB| \leq \frac{\epsilon}{4\sqrt{3}} < \epsilon$. Moreover, so far the function is evidently differentiable over the first interval, which is a simple line segment, and over the second interval, which is the arc of a circle.[8] The only

[7]This daunting expression is just a vector calculus translation of the following simple instructions: Start from C, then construct a vector in the direction from C to (a, b) of length $= r$.

[8]Of course, $f_n(x)$ is not differentiable at the endpoints $(0, 0)$ and $(1, 0)$. When I say that $f_n(x)$ is everywhere differentiable, I will always mean to exclude the endpoints.

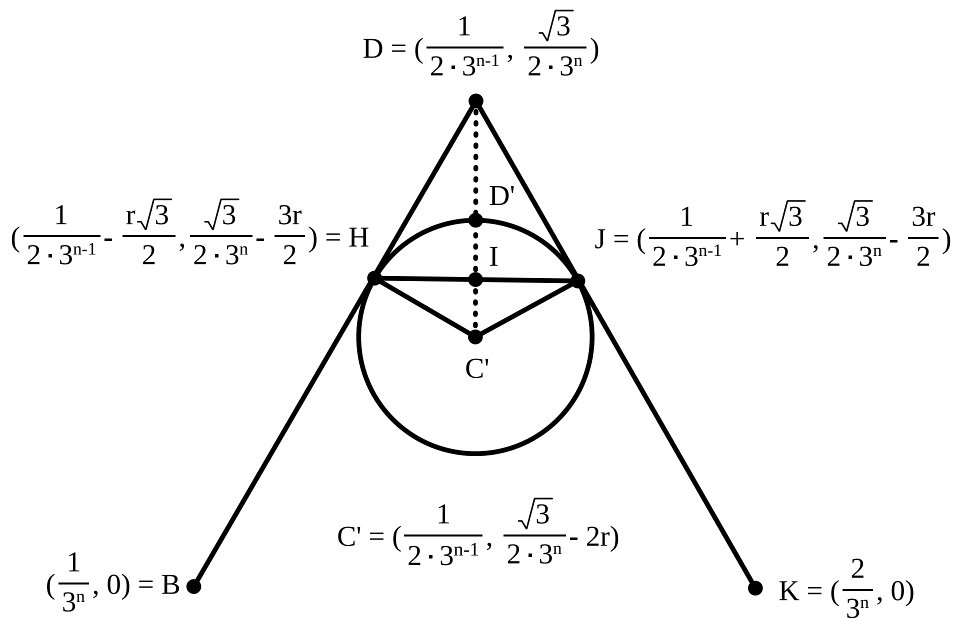

Figure A.5: The second and third segments of $k_n(x)$ and the circle used in the approximation. Point D is mapped to D' and the points between H and J are similarly mapped to corresponding points on the circle.

suspicious point is where the intervals meet, namely the point of tangency of the circle with the line segment. But this may be checked, and it will be found that the left-hand and right-hand derivatives at $x = \frac{1}{3^n} - \frac{r}{\sqrt{3}}$ are both equal to $(1, 0)$, so that the $f_n(x)$ is differentiable at the point of tangency as well.

Continuing the definition (see Figure A.5), one constructs another circle using the same radius r, inscribing it into the angle BDK by first bisecting the angle and then putting the center of the circle at $2r$ away from D on the angle bisector in the interior of the angle. The x coördinates of the points of tangency H and J are then $\frac{1}{2\times 3^{n-1}} - \frac{r\sqrt{3}}{2}$ and $\frac{1}{2\times 3^{n-1}} + \frac{r\sqrt{3}}{2}$, respectively. Therefore over the interval $x \in [\frac{1}{3^n} + \frac{r}{2\sqrt{3}}, \frac{1}{2\times 3^{n-1}} - \frac{r\sqrt{3}}{2}]$, define $f_n(x) = k_n(x)$. Over the interval $x \in (\frac{1}{2\times 3^{n-1}} - \frac{r\sqrt{3}}{2}, \frac{1}{2\times 3^{n-1}} + \frac{r\sqrt{3}}{2})$, if $k_n(x) = (a, b)$ then define

$f_n(x) = [\frac{r}{\sqrt{(a-\frac{1}{2\times 3^{n-1}})^2+(b-(\frac{\sqrt{3}}{2\times 3^n}-2r))^2}}(a-\frac{1}{2\times 3^{n-1}}, b-(\frac{\sqrt{3}}{2\times 3^n}-2r)] + (\frac{1}{2\times 3^{n-1}}, \frac{\sqrt{3}}{2\times 3^n}-2r).$

Note that over the first of the intervals just defined, the distance between $k_n(x)$ and $f_n(x)$ is 0, and over the second interval the distance never exceeds $|DI|$, the height of the triangle HDJ. This is because the arc of the circle $HD'J$ lies entirely within the triangle HDJ. But $|DI| = \frac{3r}{2}$, so that $|DI| \leq \frac{3\epsilon}{4} < \epsilon$. Moreover, since these intervals are again a line segment and an arc of a circle, $f_n(x)$ remains differentiable over the intervals and also at their point of tangency $x = \frac{1}{2\times 3^{n-1}} - \frac{r\sqrt{3}}{2}$.

Since I have described how to define $f_n(x)$ at both the 60 and 120 degree angles in $k_n(x)$, I have in effect handled all cases. Continuing in this way, therefore, one will eventually define an everywhere continuous function $f_n(x)$ which never departs from $k_n(x)$ by more than ϵ. Therefore for every $n \in \mathbb{N}$ and every $\epsilon > 0$, there exists a function $f_n(x)$ such that for all $x \in [0,1]$, $|k_n(x) - f_n(x)| < 0$.

A.1.4 Getting to Within ϵ of $K(x)$

Let $\epsilon > 0$ be given. By §A.1.2, there exists an $n \in \mathbb{N}$ such that for all $x \in [0,1]$, $|K(x)-k_n(x)| < \frac{\epsilon}{2}$. By §A.1.3, there exists a function $f_n(x)$ such that for all $x \in [0,1]$, $|k_n(x) - f_n(x)| < \frac{\epsilon}{2}$. Therefore for all $x \in [0,1]$,

$$|K(x) - f_n(x)| = |K(x) - k_n(x) + k_n(x) - f_n(x)| \leq |K(x) - k_n(x)| + |k_n(x) - f_n(x)|$$
$$< \frac{\epsilon}{2} + \frac{\epsilon}{2} = \epsilon$$

Since $f_n(x)$ is continuous everywhere except at its endpoints and never departs from $K(x)$ by more than ϵ, $f_n(x)$ is the desired approximation.

A.2 Discussion

Von Koch's original intention was to produce a curve which is everywhere continuous but nowhere differentiable. From a purely mathematical point of view, what the present result shows is that if one is prepared to settle for an approximation of Koch curve that never departs from it by more than any given ϵ, one can regain differentiability everywhere. Despite the fact that this goes against von Koch's intention, it may be of interest that continuous curves which are nowhere differentiable can be so closely approximated by continuous curves which are differentiable everywhere.

From the point of view of Leibniz's philosophy, it is important that a fractally complex curve such as the Koch curve can be approximated to within ϵ by simple geometric curves, differentiable or otherwise. Von Koch's original paper already shows that if one is willing to use an approximation which is continuous but fails to be differentiable at some finite number of points, one may simply choose $k_n(x)$ for a high enough natural number n. Thinking of $k_n(x)$ as a trajectory, bodies following the line segments making up some $k_n(x)$ move in straight paths except when they undergo sudden changes of direction of 60 and 120 degrees. Such a trajectory is unusual in the Leibnizian or even 17^{th} century physics of moving bodies, which generally represents velocities as continuous and therefore represents motions using differentiable curves. The present result suggests that everywhere differentiable curves are at least capable of approximating what is, from the standpoint of Leibniz's fundamental physics, a fractally complex trajectory or shape.

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