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PROOF AND ONTOLOGY IN EUCLIDEAN MATHEMATICS[1]

1.

Science and common-sense describe what we are: animals with biologically-fixed senses, and a fallible ability to infer facts from sensory revelations. "Epistemic naturalism" requires these capacities to cohere with whatever knowledge we attribute to ourselves; it thus rules out telepathy because we currently have no natural way to explain how creatures like us can read minds. It accepts, however, our purported knowledge of subatomic particles (despite an inability to verify such things via our senses) because we can tell a story about how our instruments transduce events such particles participate in (ones below our sensory threshold) into events we can perceive; and it explains how we infer facts about the former sort of event from facts of the latter sort.

I distinguish my "epistemic naturalism" from two other naturalisms that motivate modern philosophers of mathematics. One sort, "metaphysical naturalism," takes science to issue ontological order to everyone else. When philosophers, for example, claim possible worlds *really* exist, the metaphysical naturalist examines recent physics journals, and if nothing justifying such commitments is there, he refuses the philosopher ontological licence. Metaphysical naturalism constrains philosophers, not in respect to the metaphysical questions they're allowed to raise, but in regard to what tools they can use to answer those questions. "Look only to the metaphysics of science," the metaphysical naturalist counsels.

(Of course, it's not *obvious* what ontological commitments the sciences have. Many heed Quine in such matters: Frame a general criterion of ontological commitment for regimented discourses; regiment science into such a discourse; read off the ontological commitments therefrom. Every step here leads in more than one direction, and all of them are still controversial.[2])

The "Peircean epistemic naturalist," by contrast, admits metaphysical and epistemic questions *only* insofar as scientists do. Since mathematicians, for example, don't worry about whether numbers *really* are identical with one

[1] My thanks to Jonathan Kastin and Arnold Koslow for helpful comments.

[2] One way to introduce controversy: Deny philosophical justification for any such criterion is available. See (Azzouni 1998).

or another set-theoretical construction, this issue is regarded as illicit. Philosophical practice is to be constrained rigidly by the "epistemic framework" scientists use when engaged in "honest inquiry".[3]

I'm opposed to *this* sort of epistemic naturalism as well as to metaphysical naturalism. But I bring these positions up now only to distinguish them from the brand of epistemic naturalism I *am* committed to, and so I leave criticism for another time.

The apparent ontological commitments of mathematics seem to pose formidable obstacles for epistemic naturalism: mathematical posits, for pretty much undeniable reasons, are seen as acausal, and perhaps even as outside of space and time altogether. This makes it hard to understand what epistemic route is available (for creatures like us) to objects like that. But (in any case) it's clear that no epistemic story is ever told about how we learn about such objects. By contrast, we confirm our access to the empirical objects that we claim exist by invoking perception or by describing how instruments augment perception. Confirmation of epistemic access to empirical objects is an essential part of empirical science; *not bothering* to provide epistemic justifications of this sort is routine for mathematics.

This contrast offers a hint for how to solve the problem posed by mathematical posits for epistemic naturalism. Compare Plato with Huck Finn. Plato is independent of our theorizing in a straightforward way: Regardless of what properties we attribute to him, we need an epistemic story to explain how we came to know these properties. Our claims about Plato are *beholden* both to Plato and to our epistemic routes to him. By contrast, Twain *invented* Huck Finn, and he was beholden (epistemically) to nothing at all. Of course, if we make claims about the fictional character *Twain* invented, we're beholden to what Twain wrote. But that's another matter.

Here's terminology: Plato is *ontologically independent* of us; Huck Finn is *not*. Huck's properties (given that a fictional character *has* properties) are stipulated; Plato's are not. A definition: We take an object *O* to be *ontologically independent* of us if, given any property attributed to *O*, we must explain how we confirm that attribution (given our epistemic view of ourselves). Otherwise, we take *O* to be ontologically dependent on us, or ontologically *stipulated*.

Now here's my claim: Mathematical practice doesn't require justifying an epistemic route to mathematical objects, and so mathematical objects are ontologically stipulated. This claim, however satisfying it may be in solving the challenge mathematical ontology poses for epistemic naturalism, seems at odds with the objectivity of mathematics,[4] with how theorem-proving *forces* mathematicians to believe that certain mathematical objects have certain

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[3] This strategy is part of Quine's "naturalism". He uses it, for example, to dismiss philosophical scepticism. See (Fogelin 1997), especially p. 548.

[4] Kreisel once distinguished between the objectivity of mathematics and the existence of mathematical objects. It is objectivity in his sense that is of concern here.

properties. Fictional objects, by contrast, are designable at will. Authors do sometimes claim to be surprised by their characters' actions, but this is widely recognized to be affectation (as opposed to mathematics where such surprises are a way of life).

I can explain this, but it helps to change the analogy: Once the rules for chess are set, the "space" of possible games is fixed. And yet the rules are arbitrary: It's easy to imagine games where pieces act differently than they do in our game. The chess pieces, where "chess pieces" refer to roles in the game and not to actual things on the board, are ontologically dependent on us. They are, however, *algorithmically independent* of us: The implications of a stipulated set of rules can surprise us because they are, to a large extent, epistemically opaque. (This is true of any set of rules even remotely complicated.)

We should beware of taking the psychological impact of algorithmic independence as a symptom of ontological independence: a set of axioms (and now I've switched from the chess analogy to speaking explicitly of mathematics itself) yields surprising implications. It doesn't follow from this surprise that the axiom system is about an ontologically independent collection of objects.

In contrast to games, which, if they evolve at all, do so *nonconservatively*, mathematics often advances by introducing new devices into old settings, and supplementing the rules for manipulating mathematical terms with new ones, while at the same time respecting the old results. It's rare that this can be done successfully, and this goes much of the way to explain why mathematics is hard.

I've just described how the mathematical stipulationist solves the problem for epistemic naturalism posed by mathematical posits (and how he responds to certain objections[5]). It may seem, however, that in his frenzied rush to avoid this problem, the stipulationist has underrated the role of ontological commitment in mathematics. Here is how to put the objection: Your view is largely a formalist one. After all, if mathematical objects are ontologically stipulated, we must execute such stipulations by postulating certain sentences "about" them to be true (indeed, on this view, *actual* mathematical objects are functionally idle). This leads to postulating a set of axioms to prove theorems "about" the stipulated objects. Ontological commitment in mathematics then becomes the mere result of the subject/predicate form of mathematical language plus the presence of quantifiers such as "There is".

But (and here the criticism begins) object-centered thought is a powerful component of mathematics because mathematics advances by the successful invention of *new* mathematical objects. Think of how the number system was successively enriched, and how new types of functions proved so fruitful.

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[5] For responses to other objections, see my 1994. I owe the label "mathematical stipulationism," to Jonathan Kastin.

Progress by augmentation is (usually) not a matter of fresh axioms (alone), but the recognition of new objects which bring fresh axioms with them. In contemporary set theory it's known that new, more general and convincing, axioms are needed to decide the Continuum Hypothesis. But the issue is (usually) couched as a search for previously unrecognized cardinals which will bring desired axioms with them. Perhaps (nearly enough) no successful mathematical advance occurs by merely introducing "new axioms"; always, progress is via positing objects which shed light on already known ones. The stipulationist must explain why this methodological obsession with objects yields such mathematical fruit as it has; the worry is that *he* can't because on *his* view object-centered thought is just heuristic.[6]

2.

Contrary to the previous sentiments, the stipulationist *can* explain the importance of ontological positing for mathematics. To remain compatible with epistemic naturalism, however, he needs to show that the ontologizing drive in that subject is never motivated by sensitivity to the presence of anything ontologically independent of us that mathematical terms refer to.[7] Rather (apart from sheer interest and curiosity), ontological positing occurs because of a need to prove things, prove things in greater generality, or make applications easier.

I want to illustrate how the invention of objects is useful to mathematicians with the case of Euclidean geometry, specifically, book I of the *Elements*. One caveat: Mathematical positing is a supple tool, and there is no one simple way its results are useful: what I say here only makes a stab at describing its general benefits.[8]

Let's start by reviewing the contemporary notion of an axiomatic system. An assumed logical framework supplies the admissible logical idioms (connectives, quantifiers, …), as well as a model theory in which the sentences of the language are to be interpreted. An axiomatic system is characterized by axioms governing a set of primitive (nonlogical) terms, which are not given definitions, and whose references are fixed (to the extent possible) by

[6] The concerns raised in this paragraph echo those in (George 1996, p. 91).

[7] A discipline can *start* life empirically—and at this stage its terms are understood to refer to items ontologically independent of us. But this ceases should it evolve into mathematics, properly speaking.

[8] Indeed, what follows doesn't even exhaust what can be said about the case of Euclidean geometry! My hope is to sketch out a type of study of interest to historians of mathematics. I should add this: a common (nominalist) strategy among philosophers is reworking ordinary mathematical discourse, to yield a set of (formalized) sentences missing the apparent ontological commitments of the original discourse, but still equal to it in scope and applicability. If my diagnosis of the value of mathematical positing is correct, such programs are likely to result in inferior products (in theorem-proving capacity and/or ease of applicability). Indeed, if my diagnosis is right, such programs are pointless in any case.

the axioms. Other terminology is defined from the primitive terms and logical idioms.

One point: Evaluation of how successfully a mathematical discourse targets its intended subject matter is relative to the logical framework, whether, for example, the "intended" models of a pre-formalized mathematical discourse are picked out by a set of axioms to the exclusion of other "unintended" models. Thus, notoriously, second-order number theory doesn't have the unintended models of first-order number theory. This reveals a gap between standard mathematical practice, which takes its terminology to successfully refer (only) to certain entities, regardless of whether the axioms implicitly in use do so, and logical reconstructions of that practice, which allow reinterpretations in unintended models.[9]

In any case, modern axiomatics ill-fits classical Euclidean geometry, and the result is a tendency to criticize Greek practice from the contemporary perspective. To illustrate, recall that Euclid's work starts with definitions of primitive terms. Some examples:

1. A **point** is that which has no part.
2. A **line** is breadthless length.
3. The extremities of a line are points.
4. A **straight line** is a line which lies evenly with the points on itself.
5. A **surface** is that which has length and breath only.[10]

It's hard to view these items charitably. Kline (Kline 1972, p. 87-88) writes: "The initial definitions of point, line, and surface have no precise mathematical meanings and, as we now recognize, could not have been given any because any independent mathematical development must have undefined terms."

Complicating the issue of what Euclid and others wanted such definitions to do is the clear intrusion of sensory or physical details and/or the use of the idea of motion in definitions preceding Euclid's, and in those after his. There is, for example, Plato's definition of a straight line as "that line the middle of which covers the ends," Proclus' "A line stretched to the utmost," and Heron's gloss of another definition of Proclus': "that line which, when its ends remain fixed, itself remains fixed when it is, as it were, turned round in the same plane."[11]

What's the point of such definitions? It's natural, perhaps, if thinking of lines and points as notions arising (or abstracted) from what we see, or (more Platonically put) as *resembling* what we see, to try to define them via the visual, or in terms of actions on the visual. This would be, however, to confuse the genetic question of how we come upon certain notions with the

[9] The mathematical stipulationist can explain this datum cleanly. See (Azzouni 1994).
[10] (Heath 1956, p. 153). I've included 3 for future reference.
[11] These are from (Heath 1956, p. 168). Heath hypothesizes that Euclid's definition is a (failed) attempt to modify one of Plato's by removing references to sight.

issue of how we are to define them: Some story must be told, we can all agree, about how humans think up mathematical ideas, and it must start with the sensory details of physical objects. But it's a mistake to think that definitions of mathematical objects should be connected to such a story. Perhaps this attributed motive for the geometric tradition's definitions of primitive terms is the right one, and the criticism just given on target, but I'll eventually offer another explanation which treats these definitions as part of an autonomous development in geometry, rather than as due to an intrusion of epistemic concerns.

Somewhat related to this question about Euclidean definitions is a similar one about the role of diagrams. Diagrams are found in most contemporary mathematical work, and can also be found in Hilbert's "Foundations of Geometry". Of course Hilbert, like other mathematicians of our day, uses them for "intuitive purposes" only. An illustration occurs on page 5 of (Hilbert 1971): A diagram accompanies axiom II, 4, and the following remark is made: "Expressed intuitively, if a line enters the interior of a triangle, it also leaves it." This is "intuitively expressed" because the notion used ("interior") is not defined in the mathematical discourse itself, and is depicted pictorially (a triangle is drawn with a line passing in and out of what is obviously its "interior").

Contemporary axiomatics does not allow any step in a rigorous proof to rely on assumptions embodied in diagrams. But then what does "intuitive illumination" do for us (on this view)? Two possibilities come to mind: First, there are notions which are easy to grasp but not easily amenable to the particular mathematical analysis under study (assumptions, theorems and their proofs) and an illustration of the point of a proof by means of pictorial depictions of such notions helps orient the reader. Hilbert's "interior" and "exterior" are examples.[12]

Second, diagrams often depict items with specific properties although the proofs they accompany are about more general classes of objects; for example, Hilbert's drawing is of an acute-angled triangle, though the point illustrated applies to all triangles. Nevertheless, specific examples help readers by giving them something concrete to focus on while going through proofs.

If these uses for diagrams are backtracked to ancient Greek mathematics, an analogous role can perhaps be found for their definitions of primitive terms as well: They are heuristic guides that give the student an intuitive grasp of otherwise difficult mathematical notions. Support for this view can be found in the fact that such definitions play no role in subsequent proofs.

Unfortunately, Greek mathematicians took these definitions more seriously than the above interpretation can allow: Their constant tinkering with

[12] Interiors (and exteriors) of triangles are recognized at a glance; thus this rather general pair of notions is appealing for intuitive explanations. But they cannot be easily added to Euclidean geometry (quite a bit else is needed) if we are to treat them mathematically.

the definitions for straight lines, points, and so on, suggests they did not see them as heuristic. Indeed, the same point applies to their diagrams: The Greek focus on whether mechanical methods for constructing curves were admissible suggests to some that (some) Greek mathematicians had constructivist scruples about *mathematical objects*; indeed, a general constructivist view of Greek mathematics has appeal, one which understands their practice not as taking mathematical objects as antecedently given, but rather as "isolated entities about which one reasons by bringing other entities into existence and into relation with the original objects and one another".[13] It's possible, however, that constructivist scruples (at least as they appear in the tradition leading to book I) were concerned with what sorts of *diagrams* are admissible in proofs, and not with mathematical *objects* at all. It's this view I go on to explore.

3.

My conjecture (which only historians can decisively confirm or not) is that we see in the first book of Euclid's *Elements* a blend of a "pictorial" proof-system (in which diagrams are an essential part of the proofs), and a language-based proof-system (in which diagrams are merely heuristic). I'll first lay out the pictorial proof-system for two-dimensional Euclidean geometry, and then indicate why it's reasonable to thinks elements of book I are still dedicated to its methods.

Pictorial proof-systems are two-tiered. There is, on the one hand, the construction of *admissible diagrams*, and on the other, the meta-diagrammatic reasoning accompanying each diagram to establish that it has certain desired properties.[14] Let's first consider admissible diagrams.

A diagram is constructed from certain accepted conventional analogue symbols, in particular: **points**, **lines**, and **circles**.[15] Recall the five postulates:

1. To draw a straight line from any point to any point.
2. To produce a finite straight line continuously in a straight line.
3. To describe a circle with any centre and distance.
4. That all right angles are equal to one another.
5. That, if a straight line falling on two straight lines make the interior angles on the same side less than two right angles, the two straight

[13] (Mueller 1981, p. 14-15). He does *not*, however, think Greek mathematicians had scruples about mechanical methods, as his remarks on p. 16 show.

[14] Historical evidence for this two-tier view is found in the stylized way Euclid presents his proofs. See (Mueller 1981, p. 11). *Kataskeuē* corresponds to the construction of the admissible diagram, and *Apodeixis* to meta-diagrammatic reasoning about it.

[15] Hereon, boldface indicates reference to the vocabulary items of the pictorial language, and *not* to abstract mathematical objects (which will be indicated by italics). When quoting from the literature, I'll not impose these conventions.

lines, if produced indefinitely, meet on that side on which are the angles less than the two right angles.[16]

The first three postulates tell us, respectively, that we can connect any two **points** with a **straight line**, that we can extend any **straight line** further, and that we can draw a **circle**, and stipulate its **center** and **radius**.

Proofs must be recursive: we must be able to recognize whether an item is a proof or not. For example, in first-order number theory we can determine (mechanically) of each line in a proof whether it's an axiom (or an instance of an axiom schema) or not, or whether it follows from previous lines by one of (finitely many) rules of inference (or not). Of course application of such a mechanical procedure to purported proofs presupposes them clear enough to begin with so that there is no ambiguity whatsoever about what symbol of the first-order language a particular inscription is a token of. A method of recognizing proofs is mechanical *relative* to this presupposition; putting it another way: the recursiveness of the set of proofs requires that certain relevant properties of inscriptions are entirely accessible to the proof-checker. (In the case of an axiom system, that *this* physical inscription is a token of the same symbol that *that* physical inscription is of.)

A similar requirement holds of admissible diagrams, although, since the set of relevant properties the proof-checker must recognize is larger than the set of relevant properties of letters in language-based proof-systems, we must proceed with care. Indeed, this question arises immediately: Not every property of diagrammatic figures is proof-relevant. Which ones are, and which not?

Well, unsurprisingly, admissible diagrams must be clearly drawn and labelled. Second, even though diagrams use pictorial elements (e.g., **lines** and **circles**) which are analogue (indeed, postulates 1-3 require this property of them), to preserve recursiveness we cannot include, as proof-relevant, properties of such figures which, practically speaking, are not recognizable. Since assignments of lengths to analogue **curves** allows arbitrarily small differences in lengths between them (which, therefore, can be impossible for us to detect[17]) this precludes length as proof-relevant. Consequently, *measuring* **lines** and **circles**—in particular, the use of a ruler in the construction of admissible diagrams—is excluded.

For similar reasons, certain "mechanical" constructions of **curves** cannot be admitted into pictorial proof-systems either; constructions of **circles** by compass and **lines** by straightedge are acceptable because the presence of such things, and their stipulated properties, can be recognized by eye: for example, that they intersect in **points** and never in **curves**, and that all **points** on **circles** are equi-distant from their **centers**. **Quadratices**, however, which are constructed as the locus of the intersections of uniformly

[16] (Heath 1956, p. 154-155). I've included 4 and 5 for future reference.

[17] Arguably, length is not even well-defined on **curves**.

moving **radius** and **line**, are not obviously recognizable, and don't have diagrammatically "obvious" properties.[18]

Let's give more details about pictorial proof-systems. Consider again contemporary axiomatic systems: As we've seen, axioms govern nonlogical vocabulary, and they correspond to the assumptions that appear in proofs of theorems: there are also axioms or inference rules that govern (in a similar way) the logical vocabulary given by the framework. But, in addition, there are other presuppositions which arise when adopting a particular axiomatic system (and logical framework). For example, there are rules for how the vocabulary items of a formal language *L* can be concatenated into "well-formed formulas". These "presuppositions" are not expressed by axioms in *L* itself, and consequently do not appear as premises in proofs of *L*; they are (usually) given in another axiomatic system couched in a metalanguage *M* which has *L* as its subject matter. Something analogous goes on with pictorial proof-systems. There are *framework facts* which are not presuppositions of the pictorial proof-system itself, but rather part of the "set-up" apparatus of the pictorial *language*. Rules for them must be found, if anywhere, in another system describing this proof-system.

The most significant framework fact governing diagrams is that they are to be constructed on (potentially infinite) *flat* surfaces. Of course, there are no *flat* surfaces (really), but the constraint here is a conventional one: We treat the surface we draw on as flat, and this means we disavow "distortions" in the diagrams which are due to "distortions" in the surface we're working on.

This raises an important point. I've previously spoken quite blithely of the proof-relevant properties of diagrammatic figures: It must be noticed that these properties are not the actual (physical) properties of diagrammatic figures, but conventionally stipulated ones *the recognition* of which is *mechanically executable*. Again, the situation is exactly the same with games: pieces are conventionally endowed with powers (e.g., to "take" other pieces under certain circumstances). Of course, our recognition of when we can utilize a piece a certain way is based, to some extent, on its physical properties (its location on the board, say)—but the crucial point for our purposes is that all powers a game piece is stipulated to have are mechanically recognizable.

So, in the case at hand, the "cash value" of the idea that we are to ignore the distortions in diagrams due to surface irregularities means no more than that the proof methods we use to construct admissible diagrams, and then to read properties off of them, are not affected by those distortions.

[18] Anyway, it's worth pointing out that augmenting a pictorial system with additional **curves** may infirm the system altogether. We can conventionally stipulate the properties of **circles** and take them as mechanically recognizable because there are no **ellipses** (for example) in the system. Introduce (arbitrary) **ellipses** and it becomes impossible to tell whether what we have drawn in front of us is a **circle** or an **ellipse**.

Notice again, how powerfully this motivates the disallowal of measurement techniques and mechanical methods for constructing curves.

It's easy to understand why some framework facts would be seen as too obvious to bother explicitly stipulating—after all, (relative) surface flatness is needed in many contexts other than geometric ones. But this is not true of all framework facts, and it's here we find a purpose for Euclidean definitions: they're directed not towards mathematical posits, *points*, *lines*, …, but towards diagrammatic vocabulary, **points**, **lines**, …, that we can imagine a novice as actually faced with for the first time.

Since diagrammatic items are visually given to us, Euclidean definitions for them are, properly speaking, not definitions at all, but conventional stipulations (corresponding to the syntactic rules of language-based proof-systems) meant to guarantee that these items don't introduce "proof-theoretic" ambiguity when used. "Definitions" 1 and 2 don't tell us that **points** have no parts or that **lines** have no breadth; they tell us that in constructing diagrams, drawing **points** and **lines** are well-defined actions in certain respects. If, for example, we apply postulate 1, and connect two **points** with a **line**, we take the location of the **line** at those **points** to be well-defined (which it could not be if the dimensions of **points** and the breadth of **lines** were relevant in constructing a diagram).[19]

Next, consider the definition of a **straight line**. Here an "optical stipulation" is acceptable: either one which tells us that the **line** is "even" or that, if we view a **line** from one end from within the surface it's on, the ends will cover the middle).[20] Again, what's required is not that a **straight line** be straight, but that it be treated as such when we construct diagrams, and that the respects in which it is not straight not interfere with our capacity to read (stipulated) properties off of the figures in the diagram.

We have distinguished between framework facts *presupposed* by a pictorial system and stipulated conventions *within* that pictorial system. Apart from the framework fact that figures are to be drawn on flat surfaces (which presumably is too obvious to bother mentioning), I've suggested that some of the framework facts are characterized in book I by Euclid's definitions for primitive terms.[21] Stipulated conventions of the Euclidean pictorial proof-system itself are labeled "postulates". Some of these are rules about how

[19] Consider placing chess pieces on the squares of a board: the location of a piece is always well-defined, even if the physical item is inadvertently placed on more than one square. The location of the piece is stipulated by the rules of play—similarly, the location of a **point** (and **lines** through that **point**) are stipulated even though **points** have dimension, and **lines** drawn through a **point** are not exactly at that **point**.

[20] I'm unhappy with this second idea because I don't see how we're supposed to execute it. But we can certainly imagine someone looking at a **line** from side to side (from above it) and seeing that it's not even.

[21] This interpretation explains why these definitions aren't cited in subsequent proofs. The syntactic rules used to construct proofs in language-based proof-systems aren't cited when constructing proofs either, and for the same reason.

lines and **circles** may be drawn (1, 2 and 5), and one is about how we are allowed to label diagrams (4). Notice that what 5 stipulates is that we can extend two **straight lines**, under certain circumstances, so they meet.

4.

I've described pictorial proof-systems as two-tiered. In so doing, I *wasn't* alluding to the distinction between the rules for drawing the items of a pictorial proof-system, and descriptions of their properties from outside (the framework facts). Rather, I was pointing out something different: Once a diagram has been constructed, we then need to indicate, *using the diagram itself*, that its figures have certain properties. This is what I mean by "meta-diagrammatic reasoning," and I need to stress that it's not reasoning that *utilizes* framework facts as premises in arguments, but rather, reasoning that takes such facts (and their obvious implications) as given (because it takes the diagrams's conventional properties as given). Nevertheless, in a pure pictorial proof-system, constructions, and meta-diagrammatic reasoning about them, should be kept separate.[22]

Meta-diagrammatic reasoning often involves (just about literally) *pointing* or *marking out* pictorial facts in a diagram: these pictorial facts, therefore, are not *proven* from assumptions or *assumed* as premises in reasoning (as contemporary proofs recast them); rather, they are simply *seen* in the diagram and marked as such. *Some* of the pictorial facts are, *given* the conventions governing diagram-construction, "obvious". By this I mean that they follow (intuitively) from the framework facts.

What are examples of such obvious pictorial facts? I single out three, all of which have been the subject of intense scrutiny by commentators, both ancient and modern.

First, there is the uniqueness of the **straight line** of postulate 1, and the uniqueness of the extension of a **straight line** in postulate 2. Euclid clearly relies on the uniqueness of these constructions, even though they are nowhere asserted.[23] In this pictorial proof-system, however, the uniqueness of the **straight line** of postulate 1 is obvious, given Euclid's definitions, and the framework facts I've already mentioned (i.e., that the surface the **line** is on is flat, that **points** have no dimension, and **lines** no breadth). Indeed, on this interpretation, the point of such framework facts is to guarantee well-definedness of **straight lines** drawn between **points** (among other things),

[22] Greek practice, however, eventually collapses *all* three of the distinctions I've made here. This leads to methodological tensions which will be indicated later.

[23] (Kline 1972, p. 60). (Heath 1956, p. 195.6 notes that Proclus, and later Saccheri, attempt to *prove* the uniqueness of the *line* of postulate 1. Both Kline and Heath recommend explicitly postulating uniqueness (as in (Hilbert 1971, p. 3)). In line with my interpretation I take the issue to concern **points** and **lines**, although, of course, ancient and modern commentators are concerned with *points* and *lines*. Similar remarks apply to the later examples.

and so two **straight lines** cannot be drawn between two **points** simply because one (or both) will fail to be even; that is, to treat them *both* as even is to treat them as identical. Notice: This fact, because it's pictorially obvious, *on the basis of framework facts*, cannot be *proven* pictorially, because to do so requires transforming the framework facts into a (set of) axiomatic assumptions. To add uniqueness as an additional condition on postulate 1 is easy, as many have noted: my only point is that since it's unnecessary to do this in the pictorial proof-system described here, this may explain why Euclid (and the tradition he was relying on) left it out. Similar remarks apply to postulate 2.[24]

My second example is the assumption, involved in the proof of proposition 1, among others, that two **straight lines**, two **circles**, or a **circle** and a **line** always intersect at **points**.[25] Two distinguishable issues arise. The first is that *at most* one **point** occurs at such intersections (and never curves), and the second is that *at least* one **point** occurs at such intersections (that there are never gaps). I claim both these assumptions are framework-factually obvious. My first claim pretty clearly follows from the definitions of **lines** and **circles**; but what about the second? It's notable that definition 3 stipulates the extremities of a **line** to be **points**; why isn't a similar stipulation about intersections equally unobvious?

(Mueller 1981, p. 28-29) observes that Euclid generally takes the existence and location of **points** for granted:

> Perhaps the most enlightening example of Euclid's treatment of points are provided by cases like I, 5 and 12 in which Euclid begins a construction by simply "taking" a point "at random" on a line or in a region of space …. In general Euclid attempts to prove the existence of a point only if the point satisfies a condition uniquely, or, as in the case of the point *C* in I,1, almost uniquely. In other cases Euclid feels free to invoke points not set out in the *ekthesis* [givens of the diagram] as he needs them. In this respect points are anomalous among the objects of Greek elementary plane geometry.

But can one really claim that this free-wheeling existential practice with **points** is framework-justified (and so, "obvious")? It might seem not since the mere fact that **points** have no parts doesn't indicate, one way or the other, anything about where they can be located. But this is a problem *only if* we see **points** as mathematical *posits*. Another view to take of them is as conventionalized marks which, like marks in general, can be made at will *anywhere*. What we know of Greek philosophy shows that *points* are,

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[24] Anyway, on this view of book I, supplementing *postulates* 1 and 2 with uniqueness is the wrong way to go—uniqueness of such constructions should be guaranteed by the *framework facts*.

[25] (Kline 1972, p. 87), (Heath 1956, p. 165).

by Euclid's time, taken to be quite substantial, and even as physically real. But a pictorial proof-system does not have to understand them that way. On this view, definition 3 is a transitional signpost from a pure pictorial proof-system where nothing need be said about the location of **points**, to a language-based system where one must be told under what circumstances *points* can be found.[26]

The third example arises in the proof of proposition 6: Euclid clearly relies on the obviousness of one **triangle** DBC being interior to another ABC as indicating that the **triangles** cannot be equal to each other. (Mueller 1981, p. 35) suggests that common notion 9, "the whole is greater than the part," was subsequently added to book I when someone thought to justify this judgement.

Contrast these diagrammatically obvious cases with the content of postulate 5: here, we need to be told that two **lines**, under the circumstances described in the proposition, will meet; this is *not* obvious.

Is it fair to use the term "obvious" the way I do? I think so: Given the understanding of "**straight line**" and "**circle**"—that the **points** on a **circle** are all equi-distant from its **center**, that the **points** on a **straight line** are all "even" with each other, and that **points** have no dimension—we can understand that these items will never have **curves** as intersections. Similarly, the suggestion that perhaps there is no **point** where two **lines** intersect can be treated as bizarre: "But this is what we do when we mark positions". On the other hand, the idea that if two **straight lines** are not parallel, they must therefore meet, requires worrying about whether their approach to each other is asymptotic or not—something not easy to see on the mere basis of the stipulated properties of **lines**.

Let me stress that I am merely speculating as to the cause of these *lacuna*: I'm recommending we see all three cases, not as mere failures of rigor (an inference from a diagram which should be treated as purely heuristic, or worse, a simple matter of leaving out unrecognized assumptions) but as indicating that the methods of a pictorial proof-system are still present and motivating Euclid. In any case, to require of a pictorial proof-system that these additional framework stipulations be made explicit,[27] does not affect the cogency of this interpretation of book I, nor points made later in the paper.

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[26] It's interesting that Aristotle generally uses "*στιγμη*" (*puncture*) for "point", whereas this is replaced in Euclid, Archimedes, and later writers, by "*σημειον*" (=*nota*, a conventional mark). See (Heath 1956, p. 156).

[27] Unfortunately, the logical apparatus needed to do this within the confines of a pictorial proof-system is not available to the Greeks, for such framework assumptions are being handled as definitions of primitive terms, and not as stipulations in a *metalanguage* for a pictorial proof-system. Any Greek mathematician who sensed the need to make the mathematical content of the framework facts explicit was forced away from a pictorial-proof interpretation of the material in book I, and towards a language-based system with stronger ontological commitments. See the discussion to immediately follow in 5.

5.

Let's turn now to the question of interpreting diagrammatic symbols. One might think that, just as the nonlogical terms of a formal language are interpreted, so too items in a diagram must be interpreted: **points** as *points*, **lines** as *lines*, and so on. However, ontological positing of this sort is not necessary (as I've already hinted in 4): We can take the tools of book I to be actual physical diagrams endowed with *conventional properties*, and we can take them to refer to nothing at all. Meta-diagrammatic reasoning, on the other hand, refers to these conventionalized diagrams; so the ontology of Euclidean geometry is just physical items, **lines**, **points**, and so on, with additional conventional properties. Such properties (by the way) contradict the physical properties of Euclidean figures no more than the conventional properties of chess pieces contradict *their* physical properties (e.g., that a bishop can only move along a diagonal contradicts the laws of physics in which no such restrictions on bishops can be found).

Clearly this conventionalized interpretation of geometry, despite its cogency, was *not* an option for ancient geometers: geometric posits appear quite early in the mathematical tradition. But why? I sense two sets of reasons, which we may distinguish as philosophical and mathematical.

First, the philosophical reasons. It seems obvious that geometric theorems are applicable to the physical world, and because of this, that these theorems must be true. In turn, what we might call "substantial" theories of truth motivate the idea that geometric theorems, to be true, must be about "real" objects and "real" properties that these objects have. That this is an important issue for the Greeks is clear from Aristotle:

> Nor are the geometer's hypotheses false, as some have said: I mean those who say that 'you should not make use of what is false, and yet the geometer falsely calls the line which he has drawn a foot long when it is not, or straight when it is not straight.' The geometer bases no conclusion on the particular line which he has drawn being that which he has described, but (he refers to) what is *illustrated* by the figures.[28]

I speculate that the Greeks sensed an ontological dilemma here: First, if geometric statements are true, they have to be true of something. Either they are true of actual physical objects (such a diagrams) in which case they are (palpably) false, or they are true of something else (abstractions of a certain sort). What drives the dilemma is that the conventionalist property story I offer is *not* a live option for them: Since the falsity of geometry is not an option either, one must posit mathematical objects with properties that cannot be attributed to physical drawings, and describe Euclidean geometry as about such posits. The tier-structure I've attributed to book I

[28] Aristotle, *Anal. post*. I. 10, 76 a 31-77 a 4. Cited by (Heath 1956, p. 119).

then partially collapses; diagrams illustrate (refer to) mathematical objects; meta-diagrammatic reasoning is about these objects as well.

This philosophical construal of geometric methodology need not all by itself drive out pictorial proof-procedures, *if* it is recognized that inferences from conventionalized drawings are not mathematically misleading. The quote from Aristotle shows that "those who say" are not willing to be particularly conciliatory on this matter, and besides it's clear that one would have to explicitly evaluate pictorial proof-procedures, and show that they are mathematically sound. Such a study is quite natural for post-Gödelian mathematicians, but not, I surmise, for ancient Greeks. In any case, natural philosophy, as we find it in Zeno, Aristotle, and their successors, shows that it was widely thought that since geometry is successfully applicable, its posits must be somehow physically real: this too militates against seeing geometry as a subject matter which proceeds by endowing physical items with conventional properties.[29]

Once the subject matter of geometry is taken to be that of mathematical posits, two things happen. First, the previously pictorially-obvious properties of diagrams become properties of mathematical objects assumed without proof or postulation; also, certain pictorial proof-methods become inferences from properties of drawings to properties of the posits these drawings illustrate: neither result is acceptable, and what was part of an earlier pictorial proof-system appears now to be mere weakness of rigor. Second, the framework facts about **points** and **lines** must be recast as definitions of mathematical posits (and thus become pointless).

I did say there are strong mathematical reasons for moving beyond a pictorial proof-system. First, the pictorial proof-system I've described does not generalize well at all: For example, based as it is on methods of *drawing*, it can't be generalized to three dimensions. Even if we restrict ourselves to two dimensions, as we've already seen, pictorial proof-systems are too restrictive: they can't handle curves lacking stipulationally-obvious properties. These drawbacks would have been quite obvious to ancient mathematicians, who were intent on generalizing mathematical studies in both these ways.[30]

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[29] I can't argue here, with the detail needed, that this conventionalized view of geometry *can* explain its applicability. Here's a quick run-through of the idea: We can measure the deviations of the actual physical properties of objects from their "conventionalized" properties. When, in a physical domain, these deviations are below certain thresholds (e.g., on relatively flat surfaces), Euclidean geometry can be applied.

[30] Mueller (Mueller 1981, p. 29) notes that "the constructional postulates of book I … are unique in Greek mathematics." He seems to suggest that the reason for this is "Euclid's concern for and consciousness of the methods employed in proofs in book I". It's odd that this concern for rigor didn't spill over to the other books of the *Elements*. I speculate that, just as the other books rely on distinct mathematical traditions that Euclid included in the *Elements* without much concern for their overall unity with the rest of the material, so too, the constructional postulates and other unique items of book I, are there because

Second, pictorial proof-procedures are proof-theoretically awkward: they multiply the cases needed to be shown in a proof. Although **lines** can be taken to be of indeterminate length (except as qualitatively comparable to other **lines** in the same diagram, i.e., as shorter or longer than other **lines**), and similarly for **angles**, we cannot take, for example, an **acute-angled triangle** to conventionally be any **triangle** whatsoever. Language-based proof-procedures are far more convenient in this respect: we can simply take a term to refer to a class of items, and then make sure we presuppose nothing in our proof that does not hold of every member of that class. In a pictorial proof-system, one must either be pedantically rigorous (consider *every* possible construction), or exhibit only select constructions, leaving the rest of the cases for readers to work out. One sees the result of this in the ancient commentary on Euclid, e.g., Proclus, where the alternative constructions omitted by Euclid are detailed.[31]

It's worth noting that (nearly enough) there are great mathematical advantages in transforming conventions which are *used* in a proof-system (such as framework facts) into ones which are *mentioned*. This is acutely obvious when we consider non-Euclidean geometries. Pictorial proof-systems that embody one or another non-Euclidean geometry must be drawn (literally) on other sorts of surfaces (spheres, for example). But, apart from the impracticality of pictorial proof-systems, the framework facts can embody rich mathematical content which is only assumed in a pictorial proof-system, but which can be studied in its own right if the framework facts are transformed into conditions holding of a class of *objects*.

Furthermore, alternatives (to a given set of framework facts) arise that can be explored as a subject matter of a branch of mathematics, but which are not amenable (at all) to pictorial proof-systems.[32] All this, however, requires positing positing objects to be the subject matter of the mathematical areas in question.

Let me stress one last point. It's clear that another way to explore the mathematical content of the framework principles of book I without introducing additional ontological commitments, is to offer *postulates* for both the diagrammatic proof-system *and* for its metalanguage. I've suggested a number of reasons in the foregoing for why (historically speaking) this option was not available to ancient Greeks geometers. Notable are implicit philosophical views that militate against such an idea, an absence of the appropriate logical apparatus, and, not least in impact, the natural appeal of naïve object-centered thought (the appeal, that is, of positing objects that simply *have* the properties we need them to have).

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of the historical tradition *they* come from: a pictorial proof-system for **points**, **lines** and **circles**.

[31] See (Heath 1956, pp. 245-6).

[32] One sees this process in the explicit study of formalized language: formal language can be studied which *cannot* be used (e.g., language with infinitely long sentences).

6. CONCLUDING REMARKS

I don't want to leave the impression that I think Euclidean geometry has a (shrouded) prehistory as a pure pictorial proof-system. That would be nice if true, but my purposes are served equally well if pictorial proof-methods are only confusedly mixed in with genuine ontological positing right from the start of the geometrical tradition leading to book I. My primary point is to illustrate the mathematical limitations of a pictorial proof-system (one which does not posit mathematical objects—although it does rely on endowing physical objects with conventionalized properties), and the mathematical advantages of moving beyond it. My historical aspirations are thus satisfied if attributing methodological cross-purposes, due to competing methods of proof in book I, explains puzzling aspects of ancient geometry.

On the philosophical side, I wanted to illustrate how certain views about truth and mathematical application (still at large in the philosophical community) blind us to the available options for interpreting mathematical practices, and also, by the way, how the stipulationist view illuminates the use of ontological posits in mathematics.

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