Berkeley and Proof in Geometry

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ABSTRACT: Berkeley in his Introduction to the Principles of Human knowledge uses geometrical examples to illustrate a way of generating "universal ideas," which allegedly account for the existence of general terms. In doing proofs we might, for example, selectively attend to the triangular shape of a diagram. Presumably what we prove using just that property applies to all triangles.

I contend, rather, that given Berkeley's view of extension, no Euclidean triangles exist to attend to. Rather proof, as Berkeley would normally assume, requires idealizing diagrams; treating them as if they obeyed Euclidean constraints. This convention solves the problem of representative generalization.

RÉSUMÉ : Dans l'introduction aux Principes de la connaissance humaine, Berkeley emploie des exemples géométriques pour illustrer une façon d'engendrer des «idées universelles» permettant d'expliquer l'existence des termes généraux. En faisant des démonstrations on pourrait, par exemple, porter une attention sélective à la forme triangulaire d'un diagramme. Il est probable que ce que l'on démontrerait en employant cette seule caractéristique s'appliquerait à tous les triangles.

Je soutiens plutôt que, étant donnée la conception berkeleyenne de l'extension, il n'existe aucun triangle euclidien à étudier. La démonstration exige plutôt, comme Berkeley le supposerait normalement, l'idéalisation des diagrammes : leur traitement conforme aux contraintes d'Euclide. Cette convention résoud le problème de la généralisation représentative.

I argue for three claims: (1) For Berkeley, given his view of extension, Euclidean (classical) geometry must be empirically false; a view famously explicit in his early *Notebooks* (NB). (2) The method of selective attention for the purpose of representative generalization, as presented in the *Introduction* to *The Principles of Human Knowledge* (PI),[1] plays no significant role in

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generalizing proof results in classical, i.e., Euclidean, geometry. (3) That, in practice, Berkeley must have considered classical geometry a useful fiction; i.e., that *strictly speaking*, the fundamental terms of classical geometry, "point," "line," "plane," etc., lack reference.[2]

In this regard I make a distinction between abstraction as Berkeley envisioned it in PI, and idealization. I contend as a consequence that idealization, for the purpose of classical proof, automatically serves the goal of representative generalization without need to selectively attend to diagram properties beyond, of course, taking a perceived or constructed figure to be, *qua perceived*, as triangular, square, circular, etc.

Empirical Geometry

In entry A 770 of (NB) (1709) Berkeley writes:

> Qu: whether geometry may not properly reckon'd among the Mixt Mathematics. Arithmetic and Algebra being the only abstracted pure i.e. entirely nominal. Geometry being an application of these to points.[3]

Berkeley's well-known answer in the *Notebooks* is "yes." The object of geometry, unlike arithmetic or algebra, is sensible extension composed of sensible minima. Therefore, many if not all theorems of Euclidean or classical geometry, and therefore the postulates, are false. Some examples: The ratio between circumference and diameter of a circle is the same for all circles (NB B340). Line segments are infinitely divisible. (B26). The diagonal of a square isn't commensurable with its sides[4] (NB B258).

This is well known, but as commentators note, it's risky to take the early *Notebooks* as authoritative about Berkeley's ultimate views. Douglas Jesseph and Zoltan Szabo, for example believe Berkeley's later view of geometry in *The Principles* demonstrates a significant shift in his thinking about geometry.[5] Certainly there's an important change in emphasis. However, textual and formal considerations suggest Berkeley never relinquished his view that sensible extension is composed of sensible minima. In the *New Theory of Vision* (NTV), Berkeley claims that both segments of visible and tangible extension are composed of minima[6] (See also NTV 80-83). In PHK 123, Berkeley questions whether classical geometry requires line segments to be infinitely divisible, suggesting that Euclidean geometry might work with line segments considered to have a finite number of points; a project which, while not pursued, suggests he still believes extensive segments to are composed of minima.

Formal considerations, perhaps more important, also dictate taking sensible finite extension to be non-continuous. I take it as an a priori truth accepted by both Berkeley and later Hume that sensed line segments are composed of sensible atoms. This is a point about phenomenology. The alternative—that as a segment phenomenally diminishes, there will be for every putative minimum one appearing smaller would be unintelligible to Berkeley. Jesseph raises the interesting problem that if Berkeley accepts: (1) that there is nothing hidden in what we immediately see, and (2) line segments appear continuous, then we should accept (3) finite line segments *are* continuous. Jesseph is I think correct to the extent we can't immediately visually or tactually perceive segments as composed of minima. Then, *per impossible*, we would perceive boundaries between minima that are less than a minimum. But this is consistent with there being a last sensible atom as a segment visually diminishes, and a first as a one comes into view.[7] In NTV 80, Berkeley notes since the minimum visible cannot by its nature be distinguished into parts, it must be the same for every "creature" with vision. He believes this is a necessary truth. In NTV 81, Berkeley writes: "Now for any object to contain several distinct visible parts, and at the same time be a *minimum visible*, is a manifest contradiction" (Berkeley's italics). I think this is compatible with a finite visual length being composed of minima though of necessity looking continuous.[8] See also NTV 83, where Berkeley, remarking on the "perfections" of the "visive faculty," mentions two; "*first* that of comprehending in one view a greater number of visible points; *Secondly*, of being able to view them all equally and at once, with the utmost clearness and distinction." This might suggest (problematically for Berkeley), as Jesseph notes, that we should see each minimum *as bounded* by other minima, that is, see the boundaries, which, of course, we can't do. But again Berkeley likely means we see clearly and distinctly all the minima, though not *as joined* minima. There is nothing in the visual content not seen.

Selective Attention and Representative Generalization

Berkeley comments at least once in the *Notebooks* about selective attention, writing, "Considering length without breadth is considering any length be the Breadth what it will" (NB A722). In PI, Berkeley considers geometrical proof in the context of discussing how, in a world of particulars, language can possess general terms. Commenting on the bisection of a line, he writes: "I believe we shall acknowledge that an idea when considered in itself is particular, becomes general by being made to represent or stand for *all other particular ideas of the same sort*" (PI 12 my italics).

Similarly in PI 15:

> Thus, when I demonstrate any proposition concerning triangles it is to be supposed that I have in view the universal idea of a triangle, which ought not to be understood as if I can frame an idea which was neither equilateral nor scaleon, nor equicrural, but only that the particular triangle I consider, whether of this or that sort it matters not, does equally stand for and represent *all rectilinear triangles whatsoever*, and is in that sense universal. All of which seems very plain and not to include any difficulty in it (my italics).[9]

In PI 16 Berkeley writes:

> To which I answer that, although the *idea* I have in view whilst I make the demonstration be, for instance, that of an isosceles rectangular triangle whose sides are of a determinate length, I may nevertheless be certain it extends to *all* other rectilinear triangles of *what sort of bigness soever.* And that because neither the right angle, nor the equality, nor determinate length of the sides are at all concerned in the demonstration. . . .And here it must be acknowledged that a man may consider a figure merely as triangular, without attending to the particular qualities of the angles, or the relations of the sides. *So far he may abstract: but this will never prove, that he can frame an abstract general inconsistent idea* of a triangle. (my italics, [also NB A723, PHK 126]

Ideas (excluding ideas of reflection and imagination) are, for Berkeley, sensible objects. Thus, opposing Locke, Berkeley takes the abstract general idea of a triangle to be inconsistent, having to be simultaneously equilateral and scalene.[10] The method of selective attention allegedly avoids this. We focus on a property of a constructed triangle, say, its triangular character, needed to prove a theorem; e.g., that the sum of its angles equals two right angles. Since just that property of the diagram is involved the theorem allegedly applies to all triangles.

However, though we can pay attention to the linearity of the drawn sides of a triangle, *ignoring* its width and depth, we can't observe that those lines conform to the postulates, axioms, and definitions of classical geometry. We can't perceive of constructed triangles that their boundaries are infinitely divisible though that's arguably implied by the postulates.[11] Although in *The Elements* it's not part of the definition of a straight line—"a line which lies evenly with the points on itself" (Heath 153)—that line segments are continuous, that appears implied by postulate 2, a construction postulate, which states that [one can] "produce a finite straight line *continuously* in a straight line"[12] (Heath 196). If continuousness implies denseness—that between any two segment points there exists a point between them, then Euclidean straights are not composed of minima. Moreover, as Heath notes, later commentators on Euclid's "proposition (theorem) 10—"to bisect a given finite straight line"—thought infinite divisibility was either a presupposition of classical geometry, or a consequence of the ability to construct incommensurable lengths (Heath 268). For Berkeley all sensible finite lengths, being composed of minima, would be commensurable. In NB B262 he reminds himself to consider whether the "incommensurability of diagonal and side" [of a square] assumes a unit be "divisible ad infinitum."

We might say that Euclidean theorems could be true of some but not all figures. For example, the Pythagorean theorem applies to right triangles with certain sets of triples, for example, sides of 3, 4, and 5 minima. However, the deeper question is why someone believes the Pythagorean theorem true in *any* particular case. Presumably she would refer to proposition 47 in a text of Euclid (Heath, 349). [or a translation of the time.][13] But of course there would be no way of knowing whether the construction used in proving proposition 47 had the requisite number of minima to be a Pythagorean triple. We would rightly say that's irrelevant to the proof, but again that returns us to the question of what if anything the demonstration is about.

Jesseph and Szabo do recognize that proof results can't strictly apply to diagrams used in a demonstration.[14] Indeed Principles PHK 126, as Szabo, points out, illustrates the difficulty Berkeley would have in thinking proof results apply to actual constructions. Berkeley first reminds the reader that he has explained in PI 15 what he means by "universal ideas" with respect to "theorems and demonstrations:" "that the particular triangle I consider, whether of this or that sort it matters not, does equally stand for and represent *all rectilinear triangles whatsoever*, and it is in that sense universal" (my italics). Presumably the quantifier's scope in the italicized phrase includes the diagram (a specific rectilinear triangle) in the proof. PI 15 and 16 (above) support this presumption.[15]

In PHK 126, however, Berkeley gives "universal" a limited extension; a demonstration refers only to those figures where a needed construction, e.g., bisecting a line segment, is empirically possible. Berkeley claims that the actual size of a segment in a diagram—that it is an inch long—though said to contain ten thousand parts," is "indifferent to the demonstration." He writes rather that the inch line is "*universal in its signification* in the sense that it 'represents innumerable *lines greater than itself*' in which may be distinguished ten thousand parts or more, though there may be not an inch in it" [my emphasis]. The difficulty is that whereas the discussion in PI asks us to ignore the actual dimensions of figures used for proofs, PHK 126 makes the size of drawn segments relevant to what figures theorems refer to. Szabo notes this issue about quantification; on the one hand, Berkeley seems to claim that a line segment used in demonstrations can represent all segments, while on the other hand it apparently represents only segments where a division is practically possible. Szabo correctly writes that if we have to check "whether the proof of the theorem can be applied to a particular idea," we have in fact no standard of generalization.[16]

The following perhaps exemplifies Szabo's point. Suppose a geometer proves the sum of the angle theorem for a constructed obtuse triangle. How could she know the theorem applies to a constructed acute triangle? The ordinary (and Berkeleian) reply is that in the proof angle size plays no role. Angle size is indeed irrelevant in Euclid's proof but not simply because it plays no role, though that's true, but because the conclusion isn't strictly true of any sensible triangle.[17]

Idealization vs. Abstraction

Idealizations, to borrow a phrase from Michael Weisberg, are "intentional fictions."[18] My claim here is that Berkeley would as a matter of course take all of classical (pure) geometry to be an intentional fiction; the points, lines, planes, etc., related by the postulates are, *strictly speaking*, referentially empty.[19]

As mentioned, Szabo correctly notes the difficulties Berkeley has in thinking both that geometry was about sensible extension and that there could still be a standard of generality for geometrical proof. He suggests one solution would be to deny that Berkeleian ideas are in fact the subjects of proof. Szabo writes: "first of all the possibility that [classical] geometry does not have objects has not been discussed at all."[20] But although Berkeley doesn't explicitly discuss whether classical geometry literally has objects, some admittedly brief comments from *De Motu* show he found referentially empty *general* terms useful in mechanics and geometry.

> And just as geometers for the sake of their art make use of many devices which they themselves cannot describe nor find in the nature of things, even so the mechanician makes use of certain abstract and general terms, imagining in bodies force, action, attraction, solicitation, etc. which are of first utility for theories and formulations, as also for computations about motion, even if in the truth of things, and in bodies actually existing, they would be looked for in vain, just like geometers' *fictions made by mathematical abstraction* (DM 39) (my italics).

The phrase "made by mathematical abstraction" is interesting, for although Berkeley evidently thinks such abstraction legitimate, it isn't a process of selectively attending to a real property, say, the color, of a perceived object, which then can represent that color on the surface of other objects. In DM 17, referring to "impressed forces," Berkeley writes:

> *Force, gravity, attraction*, and terms of this sort are useful for reasonings and reckonings about motion and bodies in motion, but not for understanding the simple nature of motion itself or for indicating so many distinct qualities. As for attraction, it was certainly introduced by Newton, not as a true, physical quality, but only as a mathematical hypothesis (Berkeley's emphasis).[21]

Similarly, although we can't selectively attend to Euclidean properties of perceived figures if there are no such properties, or, perhaps equally sufficient, if we can't in principle discern whether a figure is Euclidean, we take it as a matter of useful convention that the figure satisfies the postulates. The convention isn't arbitrary but rather idealizes appearances. Idealizing appearances here means that we consider various real "straights," for example, plumb lines, or lines constructed with a straight edge, to have Euclidean properties, that is, to conform to the Euclidean postulates; for example, no intersecting straight lines enclose a space. In fact Berkeley *must have* viewed in this way—as idealizations or geometrical fictions—Newton's figures in the *Principia,* Euclid's (or a translator's) diagrams in *The Elements,* when Berkeley studied geometry, and his own diagrams in *The Analyst*. Idealizations are neither sensible objects nor Platonic Forms, (something Berkeley certainly rejected), but ways we decide to treat sensible objects, say, geometrical diagrams, either for theoretical reasons (e.g., doing proofs) or for practical concerns (e.g., carpentry, architecture).

Jesseph suggests that in *The Analyst*, where Berkeley uses classical geometry to criticize Newton and Leibniz's calculus, he rejected his earlier critique of Euclidean geometry in the *Notebooks*. Jesseph quotes the following:

> It hath been an old remark that Geometry is an excellent Logic. And It must be owned that when the Definitions are clear; when *the Postulata cannot be refused*, nor the Axioms denied; when the distinct Contemplation and Comparison of Figures, their Properties are derived, by a perpetual well-connected chain of consequences, the Objects being still kept in view, and the attention ever fixed on them; there is acquired a habit of Reasoning, close and exact and methodical: which habit strengthens the Mind, and being transferred to other Subjects if of general use in the inquiry after Truth. But how far this is the case of our Geometrical Analysts, it may be worth while to consider[22] (my italics).

In my view, the best way to think of the phrase "*when the Postulata cannot be refused*" is that within a certain perceptual range some postulates—e.g., that in a plane two oblique straight lines intersect at only one point—appear self-evident.[23] Hume later claimed this postulate is perceived as true only within a limited visual expanse; a point Berkeley perhaps, as a point about observation, would have agreed with.[24]

However, Berkeley, for demonstrative purposes, would idealize the sensible construction as Euclidean, as he would have needed auxiliary lines or circles permitted by the construction postulates. Idealization—conceived here—involves no special act of imagination, and certainly not abstraction as articulated in PI; rather the boundaries of constructed polygons, for example, are simply *treated* as Euclidean straights; satisfying the classical postulates.[25] Berkeley accepted Euclidean straights (representing light rays) as useful fictions in geometrical optics[26] (NTV 13, 14). In dynamics he accepted the parallelogram of forces as a fictional but useful device for computing resultant forces (DM 18). Moreover, though not mentioning the case, he likely would have accepted as idealizations the frictionless surfaces and perfect spheres Galileo assumes in formulating the law of free fall.[27]

Of course unlike the parallelogram of forces, diagrams in proofs are real figures; for Berkeley, bits of sensible extension. But as idealized—i.e., treated as satisfying the postulates—they are no more real than the perfect spheres and frictionless planes of Galileo. And as Galileo, to make use of classical geometry, introduced idealizing assumptions, Berkeley must have taken all of Euclidean geometry *itself* to be a useful fiction. Apropos here is a section from a "Dialogue" of Leibniz (1677) between **A** (presumably Leibniz), and an interlocutor **B,** about geometric constructions. **B** notes the importance of "contemplating constructed figures accurately."

> **A**: True, but we must recognize that these figures must also be regarded as characters, [symbols] for the circle described on paper is not a true circle and need not be; *it is enough that we take it for a circle*.
> **B**: Nevertheless it has a certain similarity to the circle, and this is surely not arbitrary.
> **A**: Granted; therefore figures are the most useful of characters[28] (my italics).

The significant point is that taking Euclidean geometry to idealize appearances (within a certain range) solves the problem of representative generalization without need for acts of selective attention.[29] The "standard of generalization," (Szabo) in doing a proof is built into one's conception of the diagram. In treating, *as opposed to recognizing*, a construction with straight edge and compass as a Euclidean isosceles triangle, theorems deduced (its base angles are equal) *ipso facto* apply to other observed or constructed figures *taken* to be Euclidean isosceles triangles.

Selective attention of course plays some role here. We might want to know the criteria used for selecting a particular or constructed figure as Euclidean. However we solve that problem, it's one distinct from the alleged role of selective attention in doing proofs. For example, with straight edge and compass I describe a triangle on paper, in order to prove the sum of the angle theorem. I've already decided to consider both the drawn boundaries as Euclidean straights, as well as the additional required line constructed through the vertex parallel to the base. I've assumed, via the fifth postulate, that one and only one such parallel exists. The problem of representational generalization is solved. That is, the "sort" or "kind" I'm dealing with—a Euclidean triangle—is established by making these assumptions, which is in fact to idealize the figure. The theorem proven then is true of any other Euclidean triangle. If I simply attend to the sensible "triangular character," of the figure, the Berkeleian idea, then the proof doesn't get started. Again idealizations are neither Berkeleian ideas nor Platonic forms. Conceived operationally they are not sensible objects at all, but rather involve conventions about how to treat certain sensible objects. And if we get very odd results in applying this geometry to the world we refine our measurement procedures, or—as with Einstein—ultimately change the geometry we apply to the world; a possibility likely not considered in the early 18h century.[30]

Brief (Speculative) Post-script: Berkeley, Formalism and Geometry.

I think Berkeley might have found congenial, had they been around, the ideas of David Hilbert (1899). For Hilbert, basic Euclidean terms, "point," "line," "plane," etc. lack extra-systemic reference, but are implicitly defined by their relations in the postulates.[31] In particular they don't necessarily refer to space.[32] Berkeley's query in NB A770 (quoted above) whether geometry is applying the formal ("nominal") systems of arithmetic and algebra to "points," is suggestive here. But it seems unlikely he would *identify* Euclidean geometry simply with its axiomatic structure. If we take the *Analyst* 2 passage above seriously, then the "postulata" of geometry must at least seem self-evident about what we see (or perhaps touch).[33]

There have been good discussions of Berkeley's "formalist" approach with respect to arithmetic and algebra.[34] Robert Baum notes the ambiguity in discussions of formalism, between (1) when the non-grammatical terms in a system lack any denotation, and (2) when such terms denote, but can be manipulated according to rules with no attention paid to their referents. Berkeley, as Baum observes, generally accepts the latter interpretation for arithmetic. (PHK 120-122, *Alciphron* VII 12,13), but does mention what he calls "*the algebraic mark which, denotes the root of a negative square, hath its use in logistic operations, although it be impossible to form an idea of any such quantity*" (*Alciphron*, VII, 14, my italics).

However, neither Baum, nor Jesseph discuss how Berkeley might find "formalism," in the sense of an uninterpreted calculus, like algebra (a set of marks manipulable through rules), as a way of envisioning classical geometry. The historical problem was developing geometry's axiomatic structure in words or symbols but dispensing with diagrams except, as with Hilbert, as aids in grasping the formalism.

Notes

1 George Berkeley, *A Treatise Concerning the Principles of Human Knowledge* (PHK), [1734 edition]. *Three Dialogues Between Hylas and Philonous*, [1734 edition], ed., Colin Murray Turbayne, (Kansas City: Bobbs Merill, 1965).

2 By "strictly speaking," I mean that nothing in the sensible world is a Euclidean point. line, plane, or circle, etc.

3 NB A 770

4 1709 edition, reprinted in *Berkeley, Philosophical Works*, Introduction, M. R. Ayers, (London, Dents and Sons, 1975). At the end of the paper I consider briefly, in the context of discussing the work of David Hilbert (1899), whether Berkeley's formalist approach to algebra and arithmetic has application to his views about geometry.

5 Douglas M. Jesseph, *Berkeley's Philosophy of Mathematics*, (Chicago, University of Chicago Press, 1993), 75. Zoltan Szabo agrees that there is a significant change from the *Notebooks* to the *Principles*. Zoltan Szabo, "Berkeley's Triangle,"*History of Philosophy Quarterly,* Vol. 12, No. 1, (January 1995), 78.

6 Berkeley writes: "Each of these magnitudes (visible and tangible) are greater or lesser, according as they contain in them more or fewer points, they being made up of points or minimums. For what ever may be said of extension in abstract, it is certain sensible extension is not infinitely divisible. There is a *minimum tangible*, and a *minimum visible*, beyond which sense cannot perceive" (Berkeley's emphasis). *Essay Towards a New Theory of Vision* 54, (1732 edition), in *Works on Vision*, ed. Colin Murray Turbayne (Indianapolis, Bobbs Merill, 1963).

7 Jesseph, 68-69. Jesseph thinks the concept of the "minimum sensible" is incoherent. For example, he suggests minima would have to have the same shape in all

directions, thus circular, therefore not able "to cover the plane." More likely I think, as David Raynor suggests, Berkeley, as Hume did later, takes minima visibilia to be extensionless points possessing colour. See David Raynor, "Minima Sensibilia in Berkeley and Hume," *Dialogue*, 19, 2, (1980), 196-200. Raynor appeals to Berkeley's (as Euphranor) inference in *Alciphron* from the one point argument "[that] the appearance of a long and of a short distance is of the same magnitude, or rather no magnitude at all." Again, it doesn't follow that a perceived extensive segment can't be composed of minima. See also Robert Fogelein "Hume and Berkeley on the Proofs of Infinite Divisibility," *Philosophical Review*, Vol. 97, No. 1 (Jan., 1988), 47-69, and Emil Badici, "On the Compatibility between Euclidean Geometry and Hume's Denial of Infinite Divisibility," *Hume Studie*s, Volume 34, Number 2, (2008). 231-244. Harry Bracken writes: "Berkeley takes a *minimum visible* to be that "point which marks the threshold of visual acuity." See Harry Bracken, "On Some Points in Bayle, Berkeley, and Hume," *History of Philosophy Quarterly* 4.4 (1987), 437. Berkeley does claim that it's illusory to think geometrical "demonstrations" are about the diagrams described on paper—the latter being mistakenly held, he claims, as an "unquestionable truth" by "mathematicians" and "students of logic" (NTV 150). Rather geometry he believes is about tangible extension signified by the diagrams. This I think is mistaken. For example, (1) subtle changes I see in the boundaries of curved objects, from circular to somewhat more elliptical, should be signs of differences I feel when I trace my finger around the boundaries. But they often feel the same. Sight is the touchstone for correctness here. (2) What Berkeley calls the "extraordinary clearness and evidence of geometry," the intuitive power of the postulates (*Analyst* 2) would likely *not* be recognized simply by touch. There is some evidence that the non-sighted (e.g. Nicolas Saunderson (1682-1739, third appointee to the Lucasian chair of Mathematics at Cambridge in 1711), though being able to learn and teach Euclidean geometry would likely not take the postulates as intuitively self-evident to touch. I have dealt with this issue elsewhere.

8 A reviewer mentioned a computer display composed of pixels as an example of a surface that looked continuous though composed of discrete elements.

9 Euclid does define a line as "*breathless length*" (Book I, Definition 2, Heath, 158). And, as Proclus observes, we can actually perceive breathless length. He writes: "*And we can get a visual perception of the line if we look at the middle division separating lighted from shaded areas, whether on the moon or on the earth. For the part that lies between them is unextended in breath, but it has length, since it is stretched out all along the light and the shadow.*" Proclus, *A Commentary on the First Book of Euclid's Elements*, trans. Glenn R. Morrow, (Princeton, Princeton University Press, 1970), 82. I would add that we perceive breathless length when we focus on the boundary between the wall and ceiling of a room. We do then, as a referee pointed out, observe the conformity of a line with at least one Euclidean principle. It remains true, however, that we can't simply perceive a Euclidean *straight* line.

10 I don't consider the contentious question of what Locke in fact meant by abstract general ideas.

11 Euclid, *The Thirteen Books of the Elements,* Vol. 1, (books I and II) translated by Sir Thomas Heath, (New York, Dover: 1956); page references are in the text.

12 Proclus, (mid-5th century AD) discussing proposition 10, states "it is an axiom [for some geometers] that every continuum is divisible, hence a finite line being continuous is divisible." Proclus does appear to claim however that the continuousness of any line segment which follows from postulate 2 doesn't imply infinite divisibility. 216-218.

13 For an account of translations of Euclid in the period see Stefan Storrie "What is it the unbodied spirit cannot do? Berkeley and Barrow on the nature of geometrical construction," *British Journal for the History of Philosophy*, 20. 2, (2012), 249-268. (I thank him for email discussion). Storrie speculates that Berkeley might have been influenced by Barrow's view that Euclidean objects, right lines, circles, etc. can be constructed by "generative motion" (24) But, as Storrie notes, Barrow claims that no sensible line or circle is guaranteed to be Euclidean. Barrow in fact writes: "But for the line to be 'perfectly right' we must conceive of the sensible right line as having no 'roughness' or 'exorbitances' by an act of reason rather than sense. In this way geometrical objects are not sensible but objects of reason." Isaac Barrow, *The Usefulness of Mathematical Learning Explained and Demonstrated*, (1683) tr. Kirkby (London: 1734), 75. Barrow does contend, however, that all conceivable lines, presumably including Euclidean straights, exist in nature. (76)

14 The question of course is whether Berkeley thought this. David Sherry recently writes: Berkeley can't seriously maintain that geometric demonstrations mostly fail for want of an accurate drawing, yet he is committed to this position by his thesis that geometrical diagrams are the very ideas with which geometrical theorems are concerned." Yet, given the imprecision of tools and surfaces, it's unlikely (and at any rate how would one know) that a construction satisfied the postulates. My view [see text] is that in practice, Berkeley would take all of classical geometry as a useful fiction. But that again makes problematic Berkeley's discussion of representative generalization in PI. Ultimately I don't think Berkeley—in his own reading and doing geometry—would make the mistake Sherry thinks he does of confusing "seeing" with "seeing as." See David Sherry, "Don't Take Me Half the Way, On Berkeley On Mathematical Reasoning," *Studies in the History and Philosophy of Science*, 24, 2, (1993), 214-215.

15 In *The Analyst*, his later critique of the calculus, Berkeley writes: "Whether the diagrams in a geometrical demonstration are not to be considered as signs, of all possible finite figures, of all sensible and imaginable extensions or magnitudes of the same *kind*." Berkeley, *The Analyst,* [1734] edited A. A. Luce, in *Works*, vol. 4, op. cit., query 6, 96. (my italics) But what constraints are there on instances of the "kind?" For example, are they meant to be Euclidean figures?

16 Szabo, 58. We might think PHK 126 means that in applied geometry the bisection theorem only applies to sensible segments that can be halved. But the bisection proof itself doesn't refer to what segments can actually be bisected with the tools at hand. If it did then, as Szabo observes, (if I understand him) we have no "standard of generalization."

17 The size of sides and angles is of course important for applying theorems. Jesseph (74) suggests we think of the diagonal of a square of side N approaching $N*2^{1/2}$ as N increases, and that this "application of the Pythagorean theorem . . . can [illustrate] Berkeley's theory of representative generalization" while denying the theorem applies to a particular construction. However the issue for me isn't about the application of the theorem, but rather its proof. What role does the diagram play in the proof? If none then what is selectively attended to in a proof?

18 Michael Weisberg, "Three kinds of Idealization,"*The Journal of Philosophy* Vol. CIV, number 12, (December, 2007), 639. By the phrase "made by abstraction" Berkeley might mean, among other things, derivatives of various degrees in Newton or Leibniz's calculus. Newton's notion of a point center of mass might be another example for Berkeley, In either case it's not clear in what way these are made by abstraction, as Berkeley thinks of legitimate abstraction in PI.

19 We can illustrate points, say, by a chalk mark on a blackboard or (*pace* Hume) an ink dot. But it's not simply that, aside from position (location), we can (as we do) ignore the mark's other dimensions. That's selective attention. However, a Euclidean point must satisfy the relations specified in the postulates, (e.g., two straight lines intersect at only one point.) and that's not observable for all pairs of lines visually taken as straights. As Hume noted, for very long line segments it's arguably not even correct. See fn. 24.

20 Szabo, 59. I take Sazbo to be referring not to his own comments, but to "solutions" to the issue he raises about proof that might have been but weren't discussed by Berkeley. However, the partial phrase from *De Motu* 39 quoted above, "*fictions made by mathematical abstraction*," perhaps refers to the idealizations of Euclidean geometry. G. J. Warnock apparently takes this view. Discussing puzzles engendered by Berkeley's view of geometry in the *Notebooks*, he believes DM 39 (above), particularly the phrase "fictions made by mathematical abstraction," gives evidence Berkeley radically changed his earlier views that proofs were about actual diagrams, [but now holds] that "geometry itself is an abstract calculus *applicable* (more or less roughly) to the physical world but not descriptive of its properties." G. J. Warnock, *Berkeley*, (Baltimore, Penguin, 1953), 220. Discussed by Helena M. Pycior, "Mathematics and Philosophy: Wallis, Hobbes, Barrow, and Berkeley," *Journal of the History of Ideas,* Vol. 48, No. 2 (Apr. - Jun., 1987), 265-286. It's not clear however what Warnock means by an "abstract calculus." Idealizations of drawings or constructions are considered, for theoretical or applied purposes, to be constrained by the Euclidean postulates. And as Warnock notes, this is more or less successful. For contemporary physics—e.g., the General Theory of Relativity—a non-Euclidean (Riemannian) geometry is adopted. A clear distinction between geometry as a formal system as opposed to being essentially about space I believe comes much later. See conclusion.

21 I don't discuss whether Berkeley is correct about Newton. See A. Rupert Hall and Marie Boas Hall, "Newton and the Theory of Matter," in *1666*, *The Annus Mirabilis of Sir Isaac Newton*, edited Robert Palter, (Cambridge: MIT Press, 1970) 54-67.

22 *The Analyst*, 65 Jesseph, 84-85. Jesseph notes that classical geometry as a model for clear thinking is found as well in Malebranche. Also of course famously in Descartes, *Discourse on Method* Part Two, in *The Philosophical Writings of Descartes*, trans. John Cottingham, Robert Stoothof, Dugald Muerdoch, Vol. 1, (Cambridge, Cambridge University Press, 1985), 120. This pedagogic point I believe is ultimately consistent with Berkeley's empirical critique of Euclidean geometry in the *Notebooks*.

23 In my experience of teaching geometry, students vigorously resist the idea that two lines on the board can be straight, intersect, and share more than one point.

24 Hume writes "I do not deny, where two right lines incline upon each other with a sensible angle, but 'tis absurd to imagine them to have a common segment. But supposing these two lines to approach at the rate of an inch in twenty leagues, [60 miles] I perceive no absurdity in asserting, that upon their contact they become one." David Hume, *A Treatise of Human Nature*, (1739-40), ed., L, A. Selby Bigge, (Oxford, Clarendon Press, 1888), 51. Proclus, commenting on Proposition I of the *Elements*, to construct an equilateral triangle, writes: "For the fact that the interval between two points is equal to the straight line between them makes the line which joins them one and the shortest.; so if any line coincides with it in part, it also coincides with the remainder," Op. cit., 169. Denying this implies that in a plane two sided polygons are possible. That such a polygon is impossible is one way Euclid's first postulate has been expressed.

25 There are debates in the early modern period about the role of constructions—say with straight edge and compass—in creating geometric figures. See David Sepkoski, "Nominalism and Constructivism in Seventeenth-Century Mathematical Philosophy," *Historia Mathematica* 32, (2005), 33–59.

26 Berkeley writes: "these lines and angles have no real existence in nature being only a hypothesis framed by the mathematicians, and by them introduced into optics that they might treat of that science in a geometrical way" (NTV 13,14). For a discussion of Berkeley's instrumentalism about mechanics see, Lisa Downing, "Siris and the scope of Berkeley's instrumentalism," *British Journal for the History of Philosophy* Volume 3, Issue 2, (1995), 279-300. Jesseph considers Berkeley in PHK an instrumentalist about geometry in a "weak" sense; that "geometry should be regarded as true at least for the most part, but holding that it is not fully accurate as a description of what we actually perceive" (Jesseph 77). I agree in general though I'm not clear what's meant by "true at least for the most part." To idealize geometrical constructions, or iron balls and inclined planes (Galileo), or gravitational forces, (Newton's mass points) is, I agree, to treat geometry or mechanics instrumentally. In all cases constraints are imposed on sensible objects, not just to simplify calculations, but to permit mathematical treatment in the first place. In geometry taking the boundaries of polygons to be Euclidean straights permits the deduction of theorems. But then the principles of geometry [I would add mechanics] are strictly false, rather than true for the most part, but, as Jesseph notes, could be construed as limiting cases (e.g., a perfect vacuum).

27 See, for example, Galileo Galilei, *Dialogues Concerning Two New Sciences*, (1638), trans. Stillman Drake, (University of Wisconsin Press, 1974), 162. Sagredo, an interlocutor,—remarking on the "postulate" that whatever a plane's inclination, the moving ball's degree of speed [velocity] depends only on vertical distance from the ground, notes the assumption that "the planes are quite solid and smooth, and that the movable is of a perfectly round shape." See also Ernest McMullin, "Galilean Idealization," *Studies in the History and Philosophy of Science*, 16, 3, (1985), 247-273.

28 Leibniz, "Dialogue on the Connection between Things and Words" (August, 1677), in *Philosophic Papers and Letters*, Vol. 2, Ed. Leroy E. Loemker, (Chicago, Chicago University Press, 1969), 184.

29 This needs some modification. Unless doing applied geometry, we do ignore the size of angles and line segments in the diagram, for example, proving the sum of the angle theorem. Selective inattention.

30 Robert Fogelin, (57) criticizing Hume's empirical conception of geometry, puts the point this (more limited) way. "To begin with, in geometrical proofs, equalities are stipulated rather than discovered by observation. In geometry, lines are set equal to each other." I note that we often do by this using hash marks to set lines equal. Also see Kenneth Manders, "The Euclidean Diagram" (1995), in Paolo Mancosu, *The Philosophy of Mathematical Practice*, (Oxford, Oxford University Press, 2007), 80. Again, I think Berkeley, working through proofs in a work on optics or astronomy must have taken this view.

31 David Hilbert, (1899) *Foundations of Geometry*, translated from the 10th edition by Leo Unger, (Open Court: La Salle, Illinois, 1971), See 3-6, for the axioms. Here is the first axiom: "*For every two points A, B there exists a* [straight] *line*] *a that contains each of the points A,B*." No extra systematic meaning is given to ’A’”,’B‘ or ‘a.’ other than perhaps, that they are considered members of "sets of objects." Ian Mueller, following Hilbert expresses the first postulate, as follows: $\forall A \forall B\ [A \neq B \rightarrow \exists a\ [L\ (A,a)\ \&\ L\ (B,a)]$. *Philosophy of Mathematics and Deductive Structure in Euclid's Elements*, (Massachusetts, MIT Press, 1981), 2. A nice illustration of the axiomatic view is in Morris R. Cohen and Ernest Nagel, *Logic and Scientific Method*, (New York, Harcourt, Brace and Co., 1934), 135-141. They take a small axiom set for projective geometry, but let straight lines refer to committees, and points to committee members, etc.

32 In a well-known passage from *Geometry and Experience*, Einstein writes ". . . *as far as the propositions of mathematics refer to reality, they are not certain; and as far as they are certain, they do not refer to reality. It seems to me that complete clarity as to this state of things became common property only through that trend in mathematics, which is known by the name of* "*axiomatics.*" *Lecture before the Prussian Academy of Sciences,* January 27, (1921). Expanded and reprinted in *Sidelights on Relativity*, (Whitefish, Montana, Kessinger 2010, lecture 2). Einstein refers to Moritz Schlick as coining the phrase "implicit definitions." Frege, interestingly, thought implicit definitions couldn't exhaust the meaning of geometrical terms like "point," or "line." Axioms he believed need a subject matter prior to

axiomatization. For geometry it was "spatial intuition." Gottlob Frege, *Philosophical and Mathematical Correspondence.* (Chicago, University of Chicago Press. 1980), 43. Referred to by Susan G. Sterrett, "Frege and Hilbert on the Foundations of Geometry"; talk given October 1994, Univeristy of Pittsburg Graduate Student Colloquium. On the correspondence between Frege and Hilbert see as well Alberto Coffa, "From Geometry to Tolerance," In *From Quarks to Quasars*, University of Pittsburg Series in the Philosophy of Physics, Vol. 7, ed. Robert G. Colodny, 1986.

33 Also possibly suggestive is a passage in NTV 152 where Berkeley writes: "[that] *it is therefore plain that visible figures are of the same use in geometry that words are. And the one may as well accounted the object of the science as the other*" (my italics). But Berkeley's basic claim in NTV 150-153 is that whether one uses visual diagrams or words the object of geometry is tangible extension. Given the ambiguity of ordinary language, and what he thinks to be a stable correlation between visible and tangible figures, it's more useful he believes to use the former (as opposed to words) to represent the latter. I find no evidence in this section of NTV that Berkeley thought diagrams are dispensable in proofs.

34 For example, Douglas Jesseph, op. cit., 89-118. Robert Baum, "The Instrumentalist and Formalist Elements in Berkeley's Philosophy of Mathematics," *History and Philosophy of Science*, part A, 3, 2 (1972), 119-134. George Berkeley, *Alciphron or the Minute Philosopher*, (1732), in *The Works of George Berkeley*, ed. A.C. Fraser, Vol II, (Clarendon Press, Oxford, 1871), 344. Also Michael Detlefsen, "Formalism," in Stewart Shapiro ed. *The Oxford Handbook of Philosophy of Mathematics and Logic*, (Oxford, Oxford University Press, 2005), See 263-268 for a discussion of Berkeley. David Hilbert, (1899) *Foundations of Geometry*, translated from the 10th edition by Leo Unger, (Open Court: La Salle, Illinois, 1971).

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AUTHOR QUERIES

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