REID'S ANSWER TO MOLYNEUX'S QUESTION

(A shorter version of this paper, omitting sections 7, 9, and the appendix, is scheduled to appear in *The Monist*.)

Although Reid never addresses Molyneux's question by name,[1] he has much to say that bears upon it, particularly in his discussions of the capacities of the blind and the relations of visible to tangible figure. My goal in this essay is to ascertain and evaluate Reid's answer.

On a first reading, it can seem that Reid gives two inconsistent answers. I shall argue, however, that the inconsistency goes away once we distinguish different versions of what is being asked. I shall also argue that Reid's answer of *yes* to one important Molyneux question is more plausible than Berkeley's answer of *no*.

1. Molyneux's question

Molyneux posed his famous question in a letter to Locke, which Locke quoted along with an endorsement of Molyneux's answer in the second edition of the *Essay Concerning*

[1] Molyneux's name does not occur at all in the index to the Derek R. Brookes edition of Reid's *An Inquiry into the Human Mind* (Edinburgh: Edinburgh University Press, 1997). It occurs in the Brookes index to the *Essays on the Intellectual Powers of Man* (University Park, PA: Pennsylvania State University Press, 2002) only in connection with moral topics.

Human Understanding (1694).[2] Berkeley in turn quoted Locke in his *Essay Towards a New Theory of Vision*. Here is the question as it appears in Berkeley's text, differing only in capitalization and inessential punctuation from Locke's:

> *Suppose a man born blind, and now adult, and taught by his touch to distinguish between a cube and a sphere of the same metal, and nighly of the same bigness, so as to tell when he felt one and the other, which is the cube, which the sphere. Suppose then the cube and sphere placed on a table, and the blind man to be made to see:* quaere*, whether by his sight, before he touched them, he could now distinguish and tell which is the globe, which the cube?*
>
> To which the acute and judicious proposer [Molyneux] answers: *Not. For though he has obtained the experience of how a globe, how a cube affects his touch; yet he has not yet attained the experience, that what affects his touch so or so must affect his sight so or so; or that a protuberant angle in the cube, that pressed his hand unequally, shall appear to his eye as it does in the cube.* I [Locke] agree with this thinking gentleman, whom I am proud to call my friend, in his answer to this his problem; and am of opinion, that the blind man, at first sight, would not be able with certainty to say which was the globe, which the cube, while he only saw them. (NTV 132)[3]

There are several clarifications or possible amendments to Molyneux's question that we ought to consider. The first, proposed by Diderot in his *Letter on the Blind*, is that we change the question from *globe vs. cube* to *circle vs. square*.[4] His reason is that a subject gaining sight for the first time might not be able to perceive depth (as Berkeley maintained), in which case he would not yet be able to see *anything* as a three-dimensional globe or cube, but he could nonetheless still presumably distinguish from one another two-dimensional figures like circles and squares.[5] I agree with Gareth Evans

[2] Locke quoted from the second of two letters on this topic from Molyneux, dated March 2, 1693. An earlier letter from 1688, to which Locke never responded, had raised as well the question whether a newly sighted man could see things as being at a distance from him.

[3] I shall refer thus by section number to passages in George Berkeley, *Essay Towards a New Theory of Vision* (1709).

[4] Denis Diderot, *Letter on the Blind* (1749). Substantial portions of the letter are translated and discussed in Michael J. Morgan, *Molyneux's Question: Vision, Touch, and the Philosophy of Perception* (Cambridge: Cambridge University Press, 1977). According to Marjolein Degenaar, the first thinker to propose the circle/square substitution was actually the Dutch philosopher Bouillier in 1737; see her *Molyneux's Problem* (Dordrecht: Kluwer, 1996), p. 46.

[5] Incidentally, it is arguable that Locke's reason for answering Molyneux's question *no* (even though he does not state this as the reason) is that he thinks the newly sighted would be unable to recognize three-

that Diderot's substitution leaves us with a question that still poses the key issues about the recognition of shapes presented in different sensory modalities.[6]

The next two twists are due to Leibniz, who discussed Molyneux's question in the section of the *New Essay Concerning Human Understandings* dealing with Locke's treatment of it. Here is Leibniz's spokesman Theophilus:

> I believe that if the blind man knows that the two shapes which he sees are those of a cube and a sphere, he will be able to identify them and to say without touching them that this one is the sphere and this the cube. . . . I have included in [my answer] a condition which can be taken to be implicit in the question: namely that it is merely a problem of telling which is which, and that the blind man knows that the two shaped bodies which he has to discern are before him and thus that each of the appearances which he sees is either that of a cube or that of a sphere. Given this condition, it seems to me past question that the blind man whose sight is restored could discern them by applying rational principles to the sensory knowledge which he has already acquired by touch.[7]

There are two amendments here that we should distinguish. One is that the subject be *told* that one of the objects before him is what he formerly knew by touch as a cube and the other what he knew as a globe. Molyneux's own formulation is silent on whether the subject is to be given this information. The other is that the subject be given the opportunity to *work things out* "by applying rational principles to the sensory knowledge which he has already acquired by touch" (for example, that a cube but not a sphere has eight distinguished points). In his discussion of Leibniz's version of the question, Evans gives prominence to the "let him work it out" aspect, but Leibniz's own emphasis is on the "give him a hint" aspect—that the subject be told "that, of the two appearances or

---

dimensional shapes in their two-dimensional visual ideas. How else can we explain his placement of Molyneux's question in the *Essay* at the end of section II.ix.8, whose main point is that past associations with tangible ideas make us confuse our two-dimensional visual ideas with ideas of three-dimensional solids?

[6]Gareth Evans, "Molyneux's Question," in his *Collected Papers* (Oxford: Clarendon, 1985), pp. 364-99.

[7] G.W. Leibniz, *New Essays on Human Understanding,* translated and edited by Peter Remnant and Jonathan Bennett (Cambridge: Cambridge University Press, 1981), pp. 136-37. A draft of this work was complete in 1704, but it was not published until 1765.

perceptions he has of them, one belongs to the sphere and the other to the cube."[8] It seems to me that the hint is the more significant factor of the two (or at any rate, that the hint together with the time to work things out is more significant than the time alone).

A fourth clarification is due to Evans. The interesting question according to Evans is not whether a newly sighted subject will be able to *distinguish* circles and squares, but whether a subject who *can see* circles and squares will recognize them as shapes he formerly knew by touch. Perhaps his visual field at first will be a chaos in which no distinct figures stand forth. But once the subject *is* able to see figures, will he be able to recognize them as the very figures he previously knew by touch? That is what we really want to know, and when the question is so understood, some apparently negative results involving subjects whose sight was restored are seen to be irrelevant.

To these four qualifications or amendments, I propose to add a fifth: let the question concern the subject's ability to recognize the shapes that now *visually appear* to him for the first time, regardless of whether these shapes are actually possessed by the objects before him. If we do not make this stipulation, the answer to Molyneux's question threatens to be negative for an irrelevant reason. Suppose that because of some systematically distorting property in the conditions of observation or in the subject's sensory transducers, tangible globes appear to his vision with corners poking out and tangible cubes appear with their corners smoothed away.[9] If the subject in a Molyneux

[8] Leibniz, p. 138.

[9] Could there be a situation in which what feels like a globe (i.e., feels how globes normally feel in our world) looks like a cube (i.e., looks how cubes normally look in our world) and vice versa? Judith Thompson (in "Molyneux's Problem," *The Journal of Philosophy*, 71 (1974), 637-50) apparently takes this sort of possibility to be at the root of Molyneux's and Locke's negative answers to Molyneux's problem. She questions whether the possibility is a genuine one by noting various anomalies that would result if it obtained—for example, that one could not stick one's finger in the visible gap between two contiguous visible circles if they were produced by contiguous tangible squares. As I am proposing to understand Molyneux's question, possibilities of the sort Thomson discusses would not support a negative answer to it.

experiment could not rule out such a hypothesis about how tangible globes and cubes affect his sight, he would not be in a position to say which of the objects before him is a globe and which a cube. But this is an utterly boring reason for answering *no*. It would also be a reason for answering *no* to the following question: would a man who had become acquainted with red and white things in London be able to recognize them again when he saw them in Amsterdam? For all he knows, there are strange lights in the new city that make white things look red and red things look white, leaving him unsure or mistaken in his judgments about the colors of objects. Let it be so; it is still a question of interest whether he could recognize the *visual presentations* of red and white things. A parallel question about presentations is what of interest in the Molyneux problem: could the subject recognize visually presented shapes as the shapes he previously knew by touch?

Diderot was aware of the complication I have just raised, but he does not reformulate the instructions to the subject so as to remove it. In consequence, he says he would expect the following response in a Molyneux experiment if the subject were philosophically minded: "This seems to me to be the object that I call square, that to be the object I call circle; however, I have been asked not what seems, but what is: and I am not in a position to answer that question." I would take such a response as warranting an answer of *yes* to the Molyneux question as I conceive of it.[10] Diderot's philosophical

[10]Here is another reason for insisting on the qualification I am proposing: even *after* attaining the experience that what affects his touch so or so normally affects his sight so or so—in other words, even after learning what it takes according to a Berkeleian to perform Molyneux tasks--the subject could not authoritatively identify tangible objects on the basis of their visible appearances. For how could he be sure that the conditions of observation are normal and not such as to distort the visual appearances he has come through past experience to expect?

subject would show by his words that he connects one visual presentation with tangible squares and the other with tangible circles.

We have, then, five possible variations or specifications of Molyneux's question: (1) replace globe/cube in the problem posed to the subject by circle/square; (2) tell the subject one of the objects now before him is what he formerly knew by touch as a globe (circle) and the other is a cube (square); (3) let him work out his answer by reasoning rather than insisting on immediate recognition; (4) assign the recognitional task only after the subject is able to see circles and squares as distinct figures; (5) let the question concern the shapes presented to the subject's vision rather than the shapes actually possessed by the objects before him.[11]

In what follows, I always take stipulations (2), (4), and (5) for granted. I give separate consideration to how Molyneux's question should be answered depending on whether stipulations (1) and (3) are in place.

2. Empirical evidence bearing on the question

Though merely a thought experiment when first propounded, Molyneux's question is apparently a straightforwardly empirical question. One might think that it ought to have been decided by now by actual cases of persons born blind and made to see. Nonetheless, three centuries after the question was first asked, the evidence drawn from cases of restored vision does not conclusively settle it. Such, at any rate, is the

[11]Does the Leibniz hint in condition (2) obviate the need for my condition (5)? I don't think so, because cross-wiring could make the appearance normally produced by a cube or square proceed in the test situation from a globe or circle.

conclusion of three writers who have surveyed the evidence: Morgan, Evans, and Degenaar.[12]

There have been scores of reported cases of persons blind from birth or a very early age who were restored surgically to sight, most often by the couching or removal of cataracts.[13] Some of these patients were explicitly given Molyneux tasks and others not (though their relevant reactions were recorded). Some of them could perform Molyneux tasks (for example, name figures or answer which-is-which questions) and others not.[14] The evidence thus points in divergent directions and is often simply ambiguous. Many questions have been raised about the reported cases. Could the postoperative patients really see? How blind were they initially, and for how long? Had they really been denied any opportunity to learn by association? Were they asked any leading questions? Evans notes that many of the "can't tells" may have been "can't sees" and that many of the "can tells" may already have had relevant experience.[15]

One of the best known cases of a cataract patient restored to sight is that of William Cheselden in 1728. Cheselden's patient was a thirteen-year-old boy who had lost sight so early that he had no memory of it. Cheselden's published report of the boy's experiences was commented upon by Berkeley, Reid, Voltaire, and others. Some, including Berkeley and the optical writer Robert Smith, cited the Cheselden case as supporting their own negative answer to Molyneux's question, but others questioned its relevance, noting that

---

[12] Cited above in notes 4 and 6 (verify numbers).
[13] Couching, the earliest treatment for cataracts, consists in moving them aside rather than extracting them.
[14] Degenaar reports that the first researcher explicitly to pose a Molyneux task with sphere and cube was J.C.A. Franz in 1841. Franz's patient could not recognize the sphere and cube as such, but identified them instead as circle and square. Some sixty years later, A.M. Ramsay conducted a Molyneux test with a ball and a brick, telling his newly sighted patient which two objects he would be shown. The patient was able to identify each correctly. These cases highlight for me the significance of Diderot's variation and Leibniz's hint. See Degenaar, pp. 87-97, for further details.
[15] Evans, pp. 380-82.

the Cheselden lad apparently could not at first distinguish figures at all. It seems clear to me that the questioners are right. Cheselden says that when the boy first saw, "he knew not the shape of anything, nor any one thing from another, however different in shape."[16] He thus belongs in the class of "can't sees" rightly deemed irrelevant by Evans.

Degenaar concludes her book reviewing three centuries of empirical evidence bearing on the Molyneux question with the following sentence: "We have not answered Molyneux's question—and, indeed, we think that it cannot be answered because congenitally blind people cannot be made to see once their critical period is passed."[17] She is referring to a period early in life during which if there is no appropriate retinal stimulation, there is no formation of the feature analyzer cells that are needed for subsequent discrimination of shapes.[18] In that case, one may wonder, how can there have been *any* positive results in Molyneux experiments? Perhaps they involved persons blind from an early age, but not from birth.

Although tests on adults or adolescents restored to sight would be the only direct tests of Molyneux's original question, there have been recent experiments on infants that potentially bear on the underlying issue. I have in mind the various experiments on "Molyneux babies"--infants too young to have learned any associations between sight and touch who are given Molyneux-like tasks. In one such experiment, conducted by Streri and Gentaz, days-old infants were allowed to grasp either a cylinder or a triangular prism out of sight in their right hand. They were then shown these objects for the first time, whereupon they gazed longer and more often at the object they had *not* previously grasped. Since independent experiments have shown that novel objects rather than

[16] Quoted in Morgan, p. 19.
[17] Degenaar, p. 132.
[18] The theoretical possibility that the congenitally blind may never be able fully to recover their vision was pointed out already by Adam Smith; see Degenaar, p. 97.

familiar objects tend to capture an infant's attention, Streri and Gentaz took their results to indicate that the infants recognized one of the seen shapes as what they had already felt and regarded the other as new.[19] As the authors put it, "This is experimental evidence that newborns can extract shape information in a tactual format and transform it in a visual format before they have had the opportunity to learn from the pairings of visual and tactual experience."[20]

Also worth mentioning are the experiments of Meltzoff and his collaborators, who have found that days-old infants can imitate facial gestures they see, such as forming the lips into a circle. Since this happens before the infants have had any opportunity to learn any correlations between the look and the feel of a round mouth, it suggests that the same shape information may be accessible through separate sensory modalities.[21]

There is thus some prospect that Molyneux's question (or a question in the same vicinity) may finally be answered on empirical grounds. Nonetheless, those who addressed the question early in its history could only offer theoretical or *a priori* grounds for their answers to it. I turn now to the answers of Berkeley and Reid. Before I am done, I shall consider whether there is any understanding of the question under which their attempts to answer it in *a priori* fashion are legitimate.

---

[19] Arlette Streri and Edouard Gentaz, "Cross-Modal Recognition of Shape from Hand to Eye in Human Newborns," *Somatosensory & Motor Research*, 20 (2003), 11-16. I thank Brian Glenney for bringing this research to my attention. Glenney has pointed out to me that it does not matter for purposes of the experiment whether novelty or familiarity draws an infant's attention. Just so long as the experimental group exhibits preferential looking and the control group (which was not habituated to shapes by touch) does not, that indicates that some sort of cross-modal transfer is taking place.

[20] Streri and Gentaz, p. 11 (from the abstract).

[21] Andrew N. Meltzoff, "Molyneux's Babies: Cross-modal Perception, Imitation and the Mind of the Preverbal Infant," in *Spatial Representation*, edited by Naomi Eilan, Rosaleen McCarthy, and Bill Brewer (Oxford: Blackwell: 1993), pp. 219-35. I mention another possible construal of Meltzoff's findings in the appendix.

3. Berkeley's answer

The fourth main thesis of Berkeley's *New Theory of Vision* is that there is no idea or kind of idea common to sight and touch—the objects of these senses are entirely heterogeneous (NTV 121 and 127). He says that the extension and figure perceived by sight are "specifically distinct" from those perceived by touch, by which he means that they are different in species or kind. He agrees with Molyneux and Locke in their answer to Molyneux's question, and he takes the supposed correctness of their answer as an important confirmation of his heterogeneity thesis. As he puts the point in his table of contents entry summarizing NTV 133, the Molyneux problem "is falsely solved, if the common supposition [of ideas common to sight and touch] be true."

Berkeley, in other words, offers us the following *modus tollens*:

1. If visible squares resemble tangible squares, the answer to Molyneux's question is *yes*.
2. But in fact the answer is *no*.
3. Therefore, visible squares do not resemble tangible squares.

He explains as follows why he takes the first premise to be true

> Now, if a square surface perceived by touch be the same sort with a square surface perceived by sight, it is certain the blind man here mentioned might know a square surface as soon as he saw it. It is no more but introducing into his mind, by a new inlet, an idea he has been already well acquainted with. (NTV 133)

He goes on to say that since the blind man is supposed to have known by his touch that a cube is terminated by square surfaces and a globe is not, he could know (on the supposition of similarity between visible and tangible squares) "which was the cube, and which not, while he only saw them." It is clear, then, that although Berkeley is ultimately addressing Molyneux's original question about globes and cubes, he bases his negative

answer to the original question on a negative answer to the Diderot variant about circles and squares.

In Berkeley's view, a seen square and a felt square have nothing more in common with each other than do a square of either variety and the *word* 'square'. (See NTV 140.) The connection between them is a brute correlation of the sort that can be learned only through repeated experience. One could no more expect a newly sighted man to know that the square figure he sees belongs to a tangibly square object than one could expect a neophyte in English to know that 'square' is the name for squares.

4. Reid's answer(s)

We find material relevant to Molyneux's question in four sections of Reid's *Inquiry into the Human Mind*: sections 5.6, 6.3, 6.7, and 6.11

I begin with *Inquiry* 5.6, in which Reid discusses how we obtain the conception of extension through touch. By touching things we obtain both tactile sensations and a conception of hard, extended, and figured objects.[22] But the conception (or the extended object that we conceive) does not in any way resemble the sensations; nor could it have been derived from them by any process or abstraction or ratiocination. We obtain the conception we do not because of any internal relation (say, of resemblance or necessary connection) between the conception and the tactile sensations, but only because it is a law of our constitution that such-and-such sensations are the occasion for the formation of such-and-such conceptions. In the absence of such a law, no diet of sensations alone

[22] Reid's term 'conception' is another name for what he also calls 'simple apprehension', that is, the apprehension of a thing or quality without any judgment about it. Despite the Kantian overtones of the term, it is more akin to acquaintance à la Russell than to conceptualization à la Kant.

would ever give us any conception of extension. Such is the moral of the thought experiment Reid calls his *experimentum crucis*.[23]

I mention this aspect of Reid's philosophy because it shows that he would repudiate a reason some thinkers have for answering Molyneux's question in the negative. Lotze and von Senden hold that the blind do not have spatial concepts at all, because (a) spatial concepts must be concepts of things existing side by side simultaneously, and (b) the blind do not have any such concepts of the simultaneously side by side, but only concepts of what *sequences* of tactile sensations they would obtain if, for instance, they ran their fingers around the edge of a table.[24] If the blind do not have spatial concepts at all, then they presumably would not recognize a square as such upon seeing it for the first time. Reid might agree with point (a), but he would certainly dispute point (b). The touch-derived conception of extension that the sighted and the blind alike possess is not the conception of any sequence of sensations, because it is not the conception of anything sequential, nor even of anything sensational. It is the conception of points arrayed thus and so in space. It is at least an open possibility (so far as what Reid says in 5.6 goes) that the conception of extension given to the blind as a hardwired response to cutaneous stimulation is the same conception that is given to the sighted as a hardwired response to retinal stimulation.[25]

I turn now to *Inquiry* 6.3, where Reid discusses the perspectival variation in the figures given to sight. A round coin seen at an angle will look elliptical; a rectangular table seen from one end will appear trapezoidal. These facts are well known to painters,

[23] It is so called (at *Inquiry*, p. 70) because Reid regards extension as a crucial test case for Hume's principle that all our conceptions or ideas are derived from sensations or sensory impressions.
[24] See Evans, pp. 366-68, for discussion.
[25] Reid would say that there is at least this difference, however: touch can give a conception of both two and three-dimensional objects, whereas sight originally gives the conception of two-dimensional objects only.

who must reproduce the appearances of things on canvas, but they are easily overlooked by common folk, who may swear (if they do not give the matter sufficient attention) that the seen shape of a table is constant as they walk around it. (Reid is here giving notice to the phenomenon psychologists nowadays call 'shape constancy'.) The important point for our purposes comes in the final paragraph of the section:

> To a man newly made to see, the visible appearance of objects would be the same as to us; but he would see nothing at all of their real dimensions, as we do. He could form no conjecture, by means of his sight only, how many inches or feet they were in length, breadth, or thickness. He could perceive little or nothing of their real [three-dimensional] figure; nor could he discern, that this was a cube, that a sphere . . . . (pp. 84-85)

Nor could he discern, that this was a cube, that a sphere: that sounds like a definite *no* to Molyneux's question. Before we draw any hasty conclusions, however, let us read on.

In *Inquiry* 6.11 Reid has us imagine what amounts to a Molyneux scenario involving Dr. Nicholas Saunderson, a blind mathematician whose acquaintance Reid made on a visit to Cambridge in 1736:

> [L]et us suppose such a blind man as Dr Saunderson, having all the knowledge and abilities which a blind man may have, suddenly made to see perfectly. Let us suppose him kept from all opportunities of associating his ideas of sight with those of touch, until the former become a little familiar; and the first surprise, occasioned by objects so new, being abated, he has time to canvass them, and to compare them, in his mind, with the notions which he formerly had by touch; and in particular to compare, in his mind, that visible extension which his eyes present, with the extension in length and breadth with which he was before acquainted. . . .
>
> . . . if Dr Saunderson had been made to see, and had attentively viewed the figures of the first book of Euclid, he might, by thought and consideration, without touching them, have found out that they were the very figures he was before so well acquainted with by touch. (Inq. 6.11, pp. 117-18).

That sounds like a definite *yes*! What gives? Has Reid given us two conflicting answers to Molyneux's question?

There is no conflict, as I think the reader will surmise even from my brief quotations if he keeps in mind the different versions of the Molyneux question I distinguished at the outset. Reid is not returning answers of *yes* and *no* to one and the same question. There are three relevant differences between the 6.3 and 6.11 passages that make this clear.

The first difference is that 6.3 is dealing with globes and cubes, whereas 6.11 is dealing with circles and squares. Here is 6.3 again, with a bit of Reid's explanation:

> [A man newly made to see] could perceive little or nothing of their real figure; nor could he discern, that this was a cube, that a sphere; that this was a cone, that a cylinder. His eye could not inform him, that this object was near, and that more remote. (p. 85)

Reid agrees with Berkeley that depth, or distance outwards in the third dimension, is not an original object of sight. We come to be able to perceive by sight the outward distances of things only after we have learned associations between visual cues and information gained by touch and locomotion. For that reason, a newly sighted person could apprehend only two-dimensional shapes, not the three-dimensional shapes Reid refers to as "real figure." Upon seeing a cube for the first time, he would not recognize it as the cube he formerly knew by touch because he would not see it as a cube at all. That, of course, was precisely Diderot's reason for recommending that we replace the globe/cube version of Molyneux's question by the circle/square version. He wanted us to be able to raise questions about cross-modal shape recognition independently of questions about depth perception.

In 6.11, Reid is addressing the circle/square version of the question. This is clear for two reasons. First, he speaks of Dr. Saunderson's viewing the figures of "the first book of Euclid." Book I of the *Elements* is concerned with two-dimensional figures only; solids do not make their appearance until Book XI. Second, in the paragraph explaining

why he thinks Dr. Saunderson could recognize such figures (elided in my quotation from p 118 above), Reid discusses plane figures explicitly, emphasizing how great he takes the similarity between visible plane figures and tangible plane figures to be.

The second salient difference between the passages is that the subject of Reid's thought experiment in 6.11 is not just any blind person (as it was in 6.3), but Dr. Saunderson (1682-1739), the Lucasian professor of mathematics at Cambridge. Saunderson had been blinded by smallpox at the age of one, but that did not keep him from teaching a remarkable range of subjects, including not only algebra and geometry, but optics, the nature of light and colors, the theory of vision, and the effects of lenses.[26]

The third salient difference between the two passages is that in 6.11 the subject is allowed to use "thought and consideration," whereas in 6.3 there is no such stipulation.[27]

From now on, I am going to lump the second and third differences together, since it seems to me that they work only in concert. "Thought and consideration" probably would not enable a newly sighted plain man to recognize a cube by sight if he lacked Dr. Saunderson's sophistication, and sophistication might not suffice for Saunderson if he were not given time to exercise it. That gives us the following two-by-two matrix, in which we can enter Reid's answers to two versions of Molyneux's question—one with both of stipulations (1) and (3) in place and the other with neither:

	Plain man on first view	Dr. Saunderson after thought and consideration

[26] For more on Saunderson, see Degenaar, pp. 49-50, and Morgan, ch. 3, especially pp. 42ff. Incidentally, it was Diderot's attribution (in the *Letter on the Blind*) of a blasphemous deathbed remark to Saunderson that got Diderot thrown into jail for a month in 1649.

[27] In "Thomas Reid on Molyneux's Question" (forthcoming in *Pacific Philosophical Quarterly*), Robert Hopkins identifies the same three differences that I do between the passages in 6.3 and 6.11. He and I agree that there is no contradiction between Reid's answers in 6.3 and 6.11. However, as will emerge below, we differ on the main point of Reid's discussion in 6.11.

Globe-cube	6.3: No	
Circle-square		6.11: Yes

When I say ‘on first view’, I do not mean that quite literally. The subject is to be given time to get over any dazzlement and confusion. But once things have settled down enough that he can differentiate figures before him, we are to ask what strikes him rather than what he can work out.

The blank spots in the table naturally prompt the question what Reid would say about the remaining two cases. Although he does not address either case explicitly, I shall now argue that he provides us with the basis for answers of *yes* in each of the two unfilled slots.

To take the upper right slot first, could Dr. Saunderson, upon gaining sight, tell which is the globe and which the cube? I think Reid provides us with a clear case for *yes* in his discussion in *Inquiry* 6.7 of the relation of visible figure (which is always two-dimensional) to real figure (which may be either two or three-dimensional). Reid distinguishes the two types of figure as follows: “as the real figure of a body consists in the situation of its several parts with regard to one another, so its visible figure consists in the position of its several parts with regard to the eye” (p. 96). Position with regard to the eye is direction out from the eye, regardless of distance: points that lie on the same straight line drawn from the center of the eye have the same position with regard to the eye. If lines are drawn from the eye (considered as a single point) to all points on an object, the intersections of all these lines with a sphere centered on the eye (or any other

intercepting surface) will mark out the object's visible figure. The visible figure of a tilted round coin will be an ellipse, while that of a cube will be a network of polygons.[28]

The visible figure of an object is mathematically deducible from its real figure plus its distance and orientation, as Reid explains in the following passage from *Inquiry* 6.7:

> The visible figure, magnitude, and position, may, by mathematical reasoning, be deduced from the real . . . . Nay, we may venture to affirm, that a man born blind, if he were instructed in mathematics, would be able to determine the visible figure of a body, when its real figure, distance, and position are given. Dr. Saunderson understood the projection of the sphere, and perspective. Now, I require no more knowledge in a blind man, in order to his being able to determine the visible figure of bodies, than that he can project the outline of a given body, upon the surface of a hollow sphere, whose centre is in the eye. This projection is [determines] the visible figure he wants . . . . (p. 95)[29]
>
> [F]rom these principles, having given [to him] the real figure and magnitude of a body, and its position and distance with regard to the eye, he [our blind mathematician] can find out its visible figure and magnitude. He can demonstrate in general, from these principles, that the visible figure of all bodies will be the same with that of their projection upon the surface of a hollow sphere, when the eye is placed in the centre. (p. 96)

Reid is telling us that Dr. Saunderson could have worked out, in advance of ever seeing them, what visible figures are normally presented by globes and cubes when seen from various perspectives.[30] This knowledge is not fully tantamount to knowing *how they*

---

[28] Intersections or projections on a nonspherical surface will look just the same to the eye and will demarcate visible figure equally well, but Reid chooses the sphere because the real angle magnitudes of figures drawn upon a sphere model the geometry of visible figures. For further elucidation of this point, see my "Thomas Reid's Geometry of Visibles," *The Philosophical Review*, 111 (2002), 373-416.

[29] Reid should have said that the projection determines, specifies, or represents the visible figure, not that it *is* the visible figure. The projection is curved and at a certain distance from the eye; the visible figure has neither of these properties.

[30] In a paragraph on p. 97, Reid distinguishes two routes to the conception of a figure such as a parabola or a cycloid: by means of a mathematical definition and by means of seeing or feeling a specimen of it drawn on paper or cut out in wood. He says the blind mathematician forms his conceptions of visible figure in the first way, the mathematically naïve sighted person in the second. He does not say the resulting conceptions are different. I think Reid would be suspicious, as I am, of C.D. Broad's distinction between geometrical properties and sensible forms as set forth in *The Mind and its Place in Nature* (London: Kegan Paul, 1925), pp. 171-72.

At any rate, I think Reid would acknowledge the possibility of a blind mathematician whose conceptions of visible figure are the same in all ways as those of the sighted, except that they are not got through the channels of sight. I have in mind someone whose cortical structures are the same as in the sighted (and who can therefore visualize a square) but whose blindness is due to a problem somewhere in the visual pathways to the cortex and not in the cortex itself. Such a person could visualize squares as readily as the

would look, if only because it omits any information about color. But it ought surely be enough to enable him to know, by reflection after he sees them, that the object that looks this way is a cube and the object that looks that way is a globe. At any rate, Dr. Saunderson could surely know this if he had been given the Leibniz hint (which I am taking for granted) that globe and cube are the only choices.[31] Without the hint, he could suppose that the round figure he is now seeing for the first time is the visible figure of any number of objects—a cylinder or an egg viewed endwise, perhaps, or even just a two-dimensional circle. Visible figure by itself does not uniquely determine the real figure of the object that projects it.

Let us turn now to the plain man. Lacking the sophistication of Dr. Saunderson, he may be unable to answer the question about globes and cubes, but there is a good Reidian basis for thinking that he could answer Diderot's question about circles and squares. The reason for *yes* this time comes out in Reid's discussion (in *Inquiry* 6.9 and 6.11) of the *similarity* between visible and tangible figures. Reid is very much opposed to Berkeley's doctrine of the radical heterogeneity of the objects of sight and touch. According to Berkeley, a visible square and a tangible square are no more alike than a tangible square and the *name* 'square' (NTV 140). But according to Reid, a visible square and a tangible square, though not "strictly and mathematically" alike as regards shape (6.9, p. 106) may be "to all sense the same" (6.11, p. 118).

To appreciate the subtlety of Reid's position, we must venture a bit into his geometry of visibles. Reid believes that visible figures and tangible figures obey different

sighted, but could have only tactile or other nonvisual cues for doing so.

[31] Saunderson is reported to have had an opinion on the Molyneux question himself—that the subject could indeed know, if given the Leibniz hint (Degenaar, p. 49). I daresay that if Saunderson was capable of having this opinion, it must have been right!

geometries, that of tangibles being Euclidean and that of visibles non-Euclidean. Any visible triangle, Reid believes, has an angle sum greater than 180 degrees, and as a corollary, any visible rectangle (defined for present purposes as an equiangular quadrilateral) has an angle sum greater than 360 degrees.[32] For confirmation of this point, look up successively at the four corners of a rectangular ceiling from the center of a room; each angle will be visibly obtuse, implying that the visible rectangle overhead has an angle sum of greater than 360 degrees. Of course, in a normal-sized room, you cannot take in all four corners of the ceiling at once. If a rectangle is small enough or far enough away that you *can* take in all four corners at once, the departure from Euclidean values will be negligible. A small rectangle might, for example, have each of its angles equal to 90.1 degrees, which is too small a difference from a right angle for the eye to discern.

It is in light of the foregoing that Reid makes the following observation:

> [I]t is true, that of every visible triangle, the three angles are greater than two right angles; whereas in a plain [plane] triangle, the three angles are equal to two right angles: but when the visible triangle is small, its three angles will be so nearly equal to two right angles, that the sense cannot discern the difference. . . .
>
> Hence it appears, that small visible figures (and such only can be seen distinctly at one view) have not only a resemblance to the plain tangible figures which have the same name, but are to all sense the same. (6.11, p. 118, lines 18-29; see also 6.9, p. 106, lines 3-23)

This, then, is the reason Reid gives for saying that Dr. Saunderson could recognize the figures of Euclid when first seen in a book as the figures he formerly knew by touch: they are "to all sense the same."[33] It is true that the visible figures are colored and the

[32] Set forth in *Inquiry* 6.9 and expounded in my "Thomas Reid's Geometry of Visibles."

[33] In saying that a visible triangle and a tangible triangle are "to all sense the same," is Reid saying outright that they are the same no matter in what sense modality presented? No, his immediate point is simply that they are sensibly indiscriminable despite not being exactly congruent. But that paves the way for the view that they are nearly enough the same *for all senses*.

tangible not, but "they may, notwithstanding, have the same figure; as two objects of touch may have the same figure, although one is hot and the other cold" (6.11, p. 118).

But if this is the reason in Dr. Saunderson's case, it seems an equally good reason in the plain man's case.[34] Visible squares and tangible squares are as much alike in point of figure as hot squares and cold squares. What better reason to think that a subject acquainted with them in one modality or medium would be well placed to recognize them in the other?

To sum up, then, I think the rest of our table should be filled in like this:

	Plain man on first view	Dr. Saunderson after thought and consideration
Globe-cube	6.3: No	6.7: Yes (implicitly)
Circle-square	6.11 Yes (implicitly)	6.11: Yes

If I am right about this, Reid's answer of *yes* in the lower right cell is doubly determined. The considerations that yield *yes* at the upper right and those that yield *yes* at the lower left are each sufficient on their own to yield *yes* at the lower right.[35]

5. Is Berkeley's *modus tollens* Reid's *modus ponens*?

The reasons I have just attributed to Reid for answering *yes* to the circle-square question apparently involve attributing to him an argument like the following:

1. If visible squares resemble tangible squares, the answer to Molyneux's question is *yes*.
2. Visible squares *do* resemble tangible squares.

[34] I am assuming that the circle and the square are presented head-on rather than obliquely to the plain man's sight. If they were presented obliquely (so as to present an ellipse and a trapezoid), the subject might need something of Dr. Saunderson's sophistication (along with the Leibniz hint) in order to know which is the circle and which the square.

[35] So why didn't Reid explicitly say that Dr. Saunderson could "get it" with globe/cube and that the plain man could "get it" with circle/square? I do not have a good answer. But the bases for saying both of these things are clearly there in his text.

3. Therefore, the answer to Molyneux's question is *yes*.

That would be replacing Berkeley's *modus tollens* with his own *modus ponens*. Does Reid really wish to argue in that way? The question breaks in two: does Reid affirm the antecedent, and does he affirm the conditional itself?

I think it is clear that Reid does affirm the antecedent of Berkeley's conditional (the second premise above). On this point, I may be in disagreement with Robert Hopkins, who questions whether two-dimensional shape is a common object of sight and touch for Reid. Hopkins notes that according to Reid, visible and tangible figures obey different geometries.[36] This is true, as discussed above. It seems to me, however, that in the passages where Reid discusses the differences, his main intent is to emphasize the similarities: to affirm that visible and tangible triangles, though not strictly and mathematically identical, may be "to all sense the same." His discussion of what is in effect the Molyneux question in 6.11 is embedded in a critical discussion of Berkeley's heterogeneity thesis. It closes with these words: "Berkeley therefore proceeds upon a capital mistake, in supposing that there is no resemblance betwixt the extension, figure, and position which we see, and that which we perceive by touch" (p. 119).

So Reid affirms the antecedent of Berkeley's conditional, but would he affirm the conditional itself? And is that a reasonable thing for a philosopher to do? Here some may have doubts, not unconnected, on both scores. One may think that the Molyneux question is a thoroughly empirical question, even if not settled by the evidence available to date, and that it is bad strategy for a philosopher to have a stake in the eventual outcome. If Reid affirms Berkeley's conditional along with its antecedent, he unwisely gives hostage to future empirical enquiry. This concern has led some friends of Reid to

[36] Hopkins, op. cit.

question whether he is really committed to the conditional.[37] Let us therefore reflect further on the sorts of questions that are properly pursued from the philosopher's armchair.

6. Would, could, or should?

"If visible squares are like tangible squares," runs Berkeley's conditional, "the answer to Molyneux's question is *yes*." But *yes* to exactly what question? To assess the conditional, we must get clearer on what question it refers to. Is it a question about *woulds, coulds,* or *shoulds*? We are now contemplating a new dimension of variation in the Molyneux question, one that cuts across the five dimensions distinguished above.

Is the Molyneux question a question about what the subject *would* say or do in the envisaged setup?[38] If so, it is no doubt an empirical question, and philosophers should make no bets. However similar visible circles and squares may be to their tangible counterparts, there can be no *a priori* assurance about what a Molyneux subject would actually say. Cognitive dysfunction or an idiosyncratic quality space might prevent someone from recognizing even a *perfect replica* of what he has been presented with many times before—he might not judge a red square seen now to be the same as a red square seen earlier. It is risky to affirm Berkeley's conditional, then, if its consequent makes a claim about anyone would actually do.

Is the Molyneux question a question about what a subject in the envisaged situation *could* do? No, for then the answer to Molyneux's question would too easily be *yes* (with

[37] This point has been urged forcefully by Ben Jarvis in (cite his manuscript).
[38] Recall that the setup itself may vary in the five ways we have discussed. I am taking it as including conditions (2), (4), and (5), with condition (1) sometimes added for the benefit of the plain man and condition (3) for the mathematical sophisticate.

or without the help of the antecedent of Berkeley's conditional). There is nothing impossible about a subject's being asked which seen figure belongs to the object he used to know by touch as a square and then proceeding to give the correct answer—if only as the result of a lucky guess.

In asking the Molyneux question, we do not merely mean to ask whether there is a possibility of a subject's being set a Molyneux task and subsequently getting the answer right. We mean to ask whether he would be in a position to give an answer *knowingly*—with adequate warrant or a good rational basis or something of that sort. But this is to shift to our third alternative, in which 'could' gives way to 'should' or some kindred normative notion.

Under our third construal, to say that the answer to Molyneux's question is *yes* is to say that in the envisaged setup, something along the following lines would be true of the subject:

--he *should* be able to tell which seen figure is a circle and which a square;

--he would have *sufficient evidenc*e for judging that this figure is a circle, that a square;

--he would be *warranted* in judging that this figure is a circle, that a square, whether he actually so judges or not;

--he would be in a position to *know* which seen figure is a circle and which is a square (unless he were suffering from some cognitive dysfunction), etc.

These formulations may not all be equivalent, but they have something in common: they all make the consequent of Berkeley's conditional a *normative* assertion.[39] As such, it hazards no predictions about what any subject would actually do in the envisaged setup.

[39]Note that Locke states his opinion thus: "that the Blind Man, at first sight, would not be able *with certainty* to say, which was the Globe, which the Cube" (emphasis mine). If this certainty has to do with strength of evidence rather than strength of conviction, it may be that Locke, too, understood the question in a normative way. For further discussion of what "certainty" might involve, see Janet Levin, "Molyneux's Question and the Individuation of Perceptual Concepts," forthcoming.

It only makes a claim about what the subject would be *warranted* in judging, whether he actually so judges or not. So it is safer now to affirm Berkeley's conditional, and Reid's commitment to the consequent of it is more defensible than it would be on an empirical construal.

It must be acknowledged that if we insulate Reid's answer to Molyneux's question from experimental disproof by the strategy above, we also renounce the right to claim experimental confirmation for it. Consider again the experiments with Molyneux babies described in section 2. The infants apparently recognize the seen cylinder as something they have touched, but who can say whether the mechanisms subserving this recognition are such as to make it a warranted phenomenon?

7. Berkeley's argument is backwards or circular

Although the support I have just offered for Berkeley's conditional may help Reid's cause, it does not help Berkeley's, for reasons I must now explain.

Berkeley proposes the Molyneux question as a test question for the fourth main thesis of the *New Theory of Vision*—the thesis that the objects of sight and touch are heterogeneous, and in particular that there is nothing but a name common to visible and tangible figure. He proposes similar test questions about the abilities of the newly sighted in connection with the other main topics in the *New Theory*. Is outward distance perceived immediately by sight? For this issue, the test question is whether a man born blind and made to see would think the things he saw at any distance from him, and Berkeley is confident that the answer is no: one enjoying sight for the first time would think the sun and stars "as near to him as the perceptions of pain or pleasure, or the most

inward passions of his soul" (NTV 41). Is the up or down orientation of things a proper object of sight? For this issue, the test question is whether a newly sighted man would be inclined to call any visible object high or low, erect or inverted (NTV 92-95). Berkeley is again confident that the answer is no: visible objects gain appellations such as 'erect' and 'inverted' only after being associated with the tangible objects that properly bear them (NTV 97-100). Returning to the case at hand, are the shape properties exemplified in things we touch also exemplified in things we see—that is, is there a resemblance in respect of figure between things felt and things seen? No, says Berkeley, relying this time on the Molyneux test: a man given sight would not recognize by his new sense the shapes now before him--as he must if they are the very shapes he was previously acquainted with, let in now by a new channel.

The point I wish to make is this. Suppose the questions in the previous paragraph about the newly sighted are questions about what they should be able to do, or about what is within their rational competence, as I am now proposing in regard to Molyneux's question. They are then normative questions (in a broad construal of 'normative'), and the answers to such questions supervene on matters of natural fact. Is Nelson Mandela a good man? Yes, because he was patient, courageous, and a force for peace. Should the Molyneux subject be able to tell that this is a circle, that a square? Yes, because there is a close resemblance between these objects and the objects he formerly knew as circles and squares; no, because there is no such resemblance, the objects being utterly new. You see where I am heading: normative questions are both ontologically and epistemically posterior to the factual matters on which they supervene. On the normative construal of Molyneux-style questions, there is therefore something backwards about Berkeley's

procedure in the *New Theory*. The questions initially of interest to him are all questions about which features of things are among the proper and immediate objects of sight. To focus or dramatize these questions, he asks what would be seen or recognized by a subject given sight for the first time. But the latter questions, if normative in the way I have suggested, cannot give us independent leverage on the former set of questions.[40] We must answer the questions about the newly sighted by answering the questions about what is proper to sight and not vice versa. So it is wrong for Berkeley to announce that the Molyneux question is "falsely solved" on the hypothesis of common sensibles, as though he knew the answer to the Molyneux question independently. On the normative construal of the question, Berkeley is in the position of one who claims to know that the strawberry is not sweet and juicy because it is not good.

Reid is not open to a similar criticism. If he replaces Berkeley's *modus tollens* with his own *modus ponens*, he at least has got the direction right—from natural facts about resemblance to normative claims about what Dr. Saunderson ought to be able to do.

In fairness to Berkeley, I should note that he himself seems to have construed the Molyneux question as empirical rather than normative. In the appendix to the *New Theory of Vision*, he mentions a case reported in 1709 of a man made to see and says that if anyone should get an opportunity to interrogate him, "I should gladly see my notions either amended or confirmed by experience." By the time Berkeley wrote the *Theory of Vision Vindicated* in 1733, Cheselden's case had become widely known, and Berkeley cites it as confirming by experiment what he had discovered by reasoning.[41] As noted above, I think Berkeley was too hasty about this, since Cheselden's patient could not even

[40] I thus agree with Hopkins's observation that the Molyneux question may be used to focus Berkeley's question about common sensibles, but not to settle it--though he makes the point in the belief that the relevant Molyneux question is empirical.
[41] *The Theory of Vision Vindicated and Explained*, sec. 71.

distinguish shapes by sight and thus did not satisfy one of the conditions I have placed on Molyneux experiments.[42] But Berkeley's use of the case nonetheless suggests that he took the question to be an empirical one.

Even if Molyneux's question is taken to be empirical, however, a criticism remains to be made of Berkeley's procedure in the *New Theory*. By what "reasoning" was Berkeley led to think the answer to Molyneux's question is negative? One suspects that it can only have been the following argument:

1. If the answer to Molyneux's question is *yes*, visible squares resemble tangible squares,
2. Visible squares do *not* resemble tangible squares.
3. Therefore, the answer to Molyneux's question is *no*.

This argument employs the converse of what I have been calling Berkeley's conditional, adds to it the thesis of the heterogeneity of visible and tangible figure, and concludes that the answer to Molyneux's question is *no*. The trouble, of course, is that when that argument is set alongside Berkeley's other *modus tollens*--his invocation of Molyneux to reinforce the heterogeneity thesis—the entire ensemble is a piece of circular reasoning. The answer to Molyneux's question is *no* because visible shapes are unlike tangible shapes; visible shapes are unlike tangible shapes because the answer to Molyneux's question is *no*. Berkeley cannot rely on both of these arguments simultaneously.

Berkeley's converse conditional—if the answer to Molyneux's question is *yes*, then visible squares resemble tangible squares--raises interesting issues about innate wiring that I discuss in the Appendix.

---

[42] Berkeley may have been right in claiming that the Cheselden case confirms *some* of his tenets, however —for example, that depth is not given to sight prior to associations with touch. The Cheselden lad said that at first everything he saw seemed to touch his eye.

8. The one-two Molyneux question

Aristotle ranked figure, magnitude, motion, and number as "common sensibles," that is, features that can be perceived by more than one sense. In developing his thesis of the radical heterogeneity of the objects of sight and touch, Berkeley explicitly denies that the first three are common sensibles. What would he say about number?

This question is connected with another: how would Berkeley answer the one-two Molyneux question, a question we might frame in Molyneux's style as follows?

> *Suppose a man born blind and taught to distinguish by his touch a single raised dot from a pair of raised dots (as Braille readers can do). Suppose the blind man made to see, and presented with a single visible dot on the left and a pair on the right.* Quaere: *whether by his sight and before he touched them, he could now distinguish and tell, which is the single and which the pair?*

In the case of figure, we have seen that Berkeley offers the following argument:

1. If visible squares resemble tangible squares, the answer to Molyneux's question is *yes*.
2. But in fact the answer is *no.*
3. Therefore, visible squares do not resemble tangible squares.

Would he give the following parallel argument about number?

1. If visible pairs resemble tangible pairs, the answer to the one-two question is *yes*.
2. But in fact the answer is *no*.
3. Therefore, visible pairs do not resemble tangible pairs.

Most people to whom I have presented the latter argument think there would be something bizarre or incredible about a subject who was stymied by the cross-modal number recognition task. But the number task seems to me in principle no more difficult than the shape task. I am therefore tempted to regard the argument as a *reductio ad*

absurdum of Berkeley's original *modus tollens* (and indirectly, as a recommendation of Reid's *modus ponens*).

Before we draw these morals, however, we should consider whether it is legitimate to extend Berkeley's views in the way I am suggesting. Is he really committed to saying that the Molyneux man would not know by sight which is the single and which the pair?

There are more and less radical ways of interpreting Berkeley's philosophy as it bears on this point, but either way, I think the answer is yes. Among the attributes discussed in the *New Theory* are outward distance, size, orientation, figure, and number. The radical line about any of these attributes says that it is a proper object of touch alone, not really manifested in vision at all. The moderate line about one of the attributes says that vision displays an analog or counterpart of the tactile attribute, but that the counterpart is heterogeneous from the tactile attribute and only contingently connected with it.

About distance or depth, Berkeley clearly takes the radical line. There is no such thing as visual depth; there are only visual cues from which we may infer degrees of distance once we have correlated the cues with distance as gauged by locomotion and touch. I believe he also takes the radical line about orientation.

About size, Berkeley takes the moderate line. There is such a thing as visible magnitude, as shown by the fact that a being endowed with sight alone could tell that one visible line was longer than another. However, visible magnitude has no resemblance to tangible magnitude, and it is only after learning correlations between the two that anyone can judge of tangible magnitude from visible magnitude.

What line does Berkeley take about number?

Reid evidently interpreted Berkeley as taking the radical line about number (as well as figure and orientation). Under Reid's understanding, it is Berkeley's view "that we do not originally, and previous to acquired habits, see things either erect or inverted, of one figure or another, single or double, but learn from experience to judge of their tangible position, figure, and number, by certain visible signs" (*Inquiry* 6.11, p. 117). As an instance of this general position, "if the visible appearance of two shillings had been found connected from the beginning with the tangible idea of one shilling, that appearance would as naturally and readily have signified the unity of the object, as now it signifies its duplicity" (p. 116). On this reading of Berkeley, his views clearly imply that the Molyneux man would be unable to recognize a pair of visible dots as a pair. Number would have no meaning as applied to visible things except by courtesy of association with numbered tangible things, and the Molyneux man has had no opportunity to learn the required associations.

I myself think that Berkeley took the moderate line about number, assimilating it to size rather than to orientation or depth. We find evidence of this in his reply to an objection to his view that a newly sighted man, upon first seeing heads and feet, would not connect these visible objects in any way with the tangible objects called 'head' and 'feet'. The objection is that the subject could infer that because the visible feet of the man are two, they correspond to the man's tangible feet rather than to his head. Here is Berkeley's reply:

> In order to get clear of this seeming difficulty we need only observe that diversity of visible objects does not necessarily infer [imply] diversity of tangible objects corresponding to them. . . . I should not therefore at first opening my eyes conclude that because I see two I shall feel two. How, therefore, can I, before experience teaches me, know that the visible legs, because two, are connected with the tangible legs? (NTV 108)

Here Berkeley is granting that I do indeed see the feet as two; it is just that I cannot draw any inferences regarding the number of tangible feet.

For present purposes, however, it does not matter which interpretation of Berkeley (Reid's or mine) is correct, since the moderate line and the radical line have the same implications. On the moderate line, there is such a thing as visible number, but it is wholly disparate from tangible number and only contingently connected with it. In short, visible number is related to tangible number as visible figure to tangible figure. But the disparity in the case of visible and tangible figure is enough to make Berkeley confident that the answer to the circle-square Molyneux question is *no*. He therefore has just as much reason to insist that the answer to the one-two Molyneux question is *no*. If his position is incredible in the latter case, it must also be so in the former.

9. Concluding confession

I have taken for granted throughout this essay that whether a property apprehended previously has been presented again is a matter of objective fact, capable of rationalizing a subject's opinion about it rather than being constituted by it. Some philosophers take the opposite position. According to Quine, whether the same predicate applies to each of two things is as much a function of the subject's innate quality space as it of any features of the things.[43] According to Wittgenstein in the *Brown Book*, whether two things are the same in some respect is a matter of whether they strike the speaker as the same.[44]

[43] W.V. Quine, *Word and Object* (Cambridge, Mass.: MIT Press, 1960).

[44] 'Striking the subject as the same' is perhaps ambiguous between 'S's response to x is the same as S's response to y' and 'S judges that x is the same as y'. These are not equivalent. If the former is made a necessary condition of sameness, it gives rise to a vicious regress, since 'same' occurs in the statement of the condition. The latter does not necessarily give rise to a regress, since 'same' occurs in the statement of the condition only inside a propositional attitude context. It nonetheless makes urgent the question: what is

According to some contemporary theorists of *radical response-dependence*, whether a given concept is instantiated always depends on what a subject is disposed to say or do when confronted with a putative instance of it.[45]

If we review the questions asked in this essay, persuaded of these principles, what havoc must we make? Obviously, I am not myself persuaded of the principles, but I must leave the proper examination of them for another occasion.

APPENDIX: MOLYNEUX'S QUESTION AND INNATE CONNECTIONS

Gareth Evans has asked whether Molyneux's question is a crucial experiment for the hypothesis that the concept of square exercised in touch is the same as the concept of square exercised in vision. His answer is *no*: although a negative answer to Molyneux's question would refute the hypothesis of a common concept, a positive answer would not necessarily prove it.[46] In effect, Evans is saying that although Berkeley's conditional is true--if visible squares resemble tangible squares, the answer to Molyneux's question is *yes*--its converse need not be true.

Why is the converse conditional less secure? It is because an affirmative answer to Molyneux's question has more than one possible explanation. It might be explained by the commonality of shape in touch and vision, but it might also be explained by innate or

---

the content of the subject's judgment of sameness?

[45] Michael Smith and Daniel Stoljar, "Global Response Dependence and Noumenal Realism," *The Monist*, 81 (1998), 85-111.

[46] See Evans, pp. 377-78, and the chart on p. 381.

hardwired connections between disparate concepts of the shape properties given to the two sensory modalities.[47]

The possibility of innate connections between disparate conceptual or experiential states, though routinely ignored by Berkeley, is often endorsed by Reid. The propensity of human beings to respond to certain sorts of tactile sensations with conceptions of hard, extended objects, which Reid regards as innate, is a case in point.

It seems to me that there is confirmation of the possibility of innate connections between disparate deliverances of separate modalities in Meltzoff's finding, cited above,[48] that days-old infants can imitate the facial gestures they see in adults, such as sticking out one's tongue. The explanation of this can hardly lie in any cross-modal resemblance between the sight of another's protruding tongue and the interior feel of the infant's own protruding tongue. The explanation more plausibly lies in a preprogrammed ability to do what you see.

Should we conclude that positive results in Molyneux experiments would never unequivocally support the hypothesis that shape is a common sensible? That would be too hasty. The question whether Berkeley's conditional holds in the converse direction must be revisited once we construe the consequent of the original conditional (= the antecedent of the converted conditional) as normative. Under the hypothesis of hardwiring, even if visible shape and tangible shape had nothing to do with each other, the presentation of one could evoke a response appropriate to the other. No doubt such hardwiring could explain how a subject can *say* which is which in a Molyneux test. But could it explain how he can *know* or *rationally judge* which is which, should that be the

[47] Jesse J. Prinz has made a similar suggestion to protect his empiricist hypothesis of "modal-specific representations" from positive results in modal transfer experiments; see *Furnishing the Mind* (Cambridge, MA: MIT Press, 2002), pp. 132-37. .

[48] Cited earlier in n. 00.

outcome? Put yourself in the place of such a subject: "You know, I want to say that that one is the square, but damned if I know why. It doesn't seem anything at all like the squares I have felt with my fingers." There is something incongruous about the situation of such a subject. Incongruous or not, it seems clear to me that his position is not one of knowing which presented shape is the square. He can tell which is which in the sense of *saying*, but not in the sense of *knowing*. Knowing would require some rational basis for his response, such as the basis I have claimed to be available if shape is a common sensible.