Can Berkeley Be an Instrumentalist? Towards a Reappraisal of Berkeley's Philosophy of Science

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According to Doug Jesseph, there are two kinds of interpretation of Berkeley's theory of force: "Commentators have regarded [Berkeley] as holding that the theory of forces can be replaced by talk about observed motions, or see him as expressing an instrumentalism in which scientific theories are acceptable for their predictive value, but not regarded as true."[1] But it seems that the instrumentalist interpretation is now so widespread that one would think that the debate has concluded. Indeed, now that this interpretation is the received view, it is not examined anymore but simply taken as evident.[2] In this essay, though, I will examine critically the view that Berkeley is an instrumentalist in order to open once again a discussion that has been closed too quickly.

Of course, it would be nonsense to try to prove that Berkeley is a kind of Cartesian realist. But as Jesseph acknowledges, it is nonetheless possible to admit that forces do not exist in bodies and still not conclude that Berkeley is an instrumentalist. For example, we could think that talk of forces is simply a way to refer to motions without thinking that that commits us to instrumentalism.[3] Or we might come up with yet a third interpretation. In any event, fleshing out such alternatives is not my concern here, since my aim is

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[1] Editor's Introduction, *Berkeley*: *De Motu and The Analyst*, ed. and trans. D. M. Jesseph (Dordrecht: Kluwer Academic Publishers, 1992), 35.

[2] For example, Sébastien Charles writes that the task of the physicist is to "account for phenomena—Berkeley accordingly adopts an instrumentalist position" ["Berkeley occasionnaliste malgré lui? De la causalité et de la volonté chez Berkeley et Malebranche," in *Science et épistémologie selon Berkeley*, ed. Sébastien Charles (Québec: Presses de l'Université Laval, 2004), 81]. Even if the question of whether Berkeley is an instrumentalist is not the point of Charles' essay, it is introduced in a strikingly abrupt way. Charles takes it as evident that for Berkeley "to account for the phenomena" means "to save the phenomena." I won't discuss the fact that even for a realist, science has "to account for" phenomena. The question is just to know what "to account for" means, and for Charles it means "to save" the phenomena. As with all the papers in this volume—at least those that mention that point—he takes it for granted that Berkeley is instrumentalist.

[3] Jesseph defines instrumentalism in this way: "Instrumentalism, broadly speaking, is the doctrine that a theory can accepted and applied for reasons of utility, even if the claims made in the theory or its application are not accepted as literally true" [*Berkeley's Philosophy of Mathematics* (Chicago: University of Chicago Press, 1993), 76]. But this definition is too broad, because it does not clearly define what "literally true" means in at least two ways. First, it is not clear what "literally true" means in the case of mathematical laws. Newton's second law of motion, for example, does not refer to vectors of course, so we cannot say that it is "literally true." Second, when a theory allows for different ways of speaking or metaphors, then it should be interpreted instrumentally. According to Jesseph, in this weaker sense of instrumentalism, a theory relying on a simplification is construed instrumentally, and in this sense Galileo would be an instrumentalist. But such a use of the term seems quite confusing. In any event, according to Jesseph, Berkeley "tends to" adopt the stronger version of instrumentalism as Jesseph defines it.

simply critical. By showing how the influential instrumentalist interpretations proposed by Karl Popper and W. H. Newton-Smith are implausible, I want only to highlight the need for resuming the search for an alternative view.

The Instrumentalist Interpretation

Both Popper and Newton-Smith support the view that Berkeley maintains a "semantical instrumentalism" (Newton-Smith's term). For them, theoretical propositions are neither true nor false. Instead, the role of such propositions is to facilitate predictions:

> Theoretical sentences are held not to have been provided with the kind of meaning that gives them truth-value. Theoretical sentences are not hypotheses which are either true or false. Their role is not to express facts about the world but to facilitate the business of making correct observational predictions. A theory is just a tool which can be used to derive observational predictions about the state of a system from statements giving its observational state at another time.[4]

According to Berkeley, Newton's mechanics would be an efficient tool to predict phenomena, but it would not say anything about the world; that is, Newton's mechanics and Leibniz's dynamics do not describe facts. The only reason to say that Newtonian mechanics is better than other theories would be its superior predictive power.

This interpretation relies on two main arguments:

- The first appeals to Berkeley's idea of mathematical hypothesis. Theoretical sentences using such hypotheses are also hypotheses, because they too lack empirical content: "The part of the theory involving the concepts which are above criticised [e.g., the concept of force] is not true since these are mathematical hypotheses. As such they should not, however, be rejected if they work well."[5]

- The second uses the idea that, according to Berkeley, it is at least possible that different theories predict the same phenomena. The choice between them would be experimentally impossible. According to Newton-Smith, Berkeley sustains the thesis of the underdetermination of theory by data: "The same appearances *may* be successfully calculated from more than one mathematical hypothesis, and two mathematical hypotheses which yield the same results concerning the calculated appearances may not only differ but even contradict each other (especially if they are

[4] W. H. Newton-Smith, "Berkeley's Philosophy of Science," in *Essays on Berkeley*, eds. J. Foster and H. Robinson (Oxford: Clarendon Press, 1985), 150. See also Karl Popper, "Berkeley as a Precursor of Mach," *British Journal for the Philosophy of Science* 4 (1953): 29.

[5] "Berkeley as a Precursor of Mach," 29. Because Newton's mechanics rests entirely on the concept of force, at best it is hypothetical (in a sense which is compatible with Newton-Smith description of semantical intrumentalism).

> misinterpreted as describing a world of essences behind the world of appearances); nevertheless, there may be nothing to choose between them."[6]

This is the general structure of the instrumentalist interpretation. Johnston adds one further argument, namely, that in a key section of the *Siris* Berkeley implies that he is an instrumentalist:[7]

> It is not known what other different rules or laws of motion might be established by the Author of nature. Some bodies approach together, others fly asunder, and perhaps some others do neither.[8]

Like other commentators, Johnston takes this to mean that it is possible that what we take to be the laws of nature do not correspond to the "real" laws as established by God himself. Or at least, we do not know if they correspond.

But according to this instrumentalist interpretation of Berkeley's philosophy, Berkeley would have sustained a quite paradoxical position: by looking backwards to Bellarmine and Osiander, before Galileo and Newton, he would have anticipated the most contemporary instrumentalism of Duhem, Quine, and even Van Fraasen. Not surprisingly, this way of reading the history of the philosophy of science is characteristic of Duhem in general.[9]

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[6] "Berkeley as a Precursor of Mach," 30. Note that Popper seems to consider that the theorems are themselves mathematical hypotheses, even though Berkeley does not say anything like this. For Popper, mathematical hypotheses are concepts used by a theory (see below); cf. also Newton-Smith, "Berkeley's Philosophy of Science," 157. Lisa Downing doubts that Berkeley sustained the Quinean thesis of underdetermination; however, she admits that there is a "plurality of equally adequate dynamic theories" as "a consequence of the fact that force terms do not refer to any underlying entities." Accordingly, she proposes that Berkeley sustains a "semantical instrumentalism" ["Berkeley's Case Against Realism About Dynamics," in *Berkeley's Metaphysics, Structural, Interpretive and Critical Essays*, ed. Robert G. Muehlmann (University Park: Pennsylvania State University Press), 212.

[7] G. A. Johnston, *The Development of Berkeley's Philosophy* (London: MacMillan, 1923), 249-50.

[8] *Siris: A Chain of Philosophical Reflexions and Inquiries*, in *The Works of George Berkeley Bishop of Cloyne*, eds. A. A. Luce and T. E. Jessop (9 vols.; London: Thomas Nelson and Sons, 1948-57), 5: § 235. [Hereafter W.]

[9] However, even before examining closely the arguments I have mentioned, one should note that Popper and Newton-Smith did not really write their papers as historians of philosophy, but as philosophers. They have something to prove. Popper begins his paper saying that he does not share the kind of position sustained by Berkeley; and Newton-Smith ends his own paper by showing easily that Berkeley's argument does not work (there was no real concurring theory to Newton's mechanics). But their aim is not to prove that Berkeley's own position is false. Their aim is more general: it is to prove that instrumentalism is false. They try to prove this point by making Berkeley an instrumentalist. Such a Berkeley is thus just the ghost of the real target of Popper and Newton-Smith.

A Critique of the Instrumentalist Interpretation

I will now criticize the instrumentalist interpretation by appealing to two kinds of arguments. First, I will examine the historical context; and second, I will examine Berkeley's own texts. In both cases, it will appear that the instrumentalist interpretation is not plausible.

The historical context

Newton-Smith focuses on how Berkeley's instrumentalism is close to the standard interpretation of pre-Galilean astronomy. His references to Bellarmine and Osiander links Berkeley to a tradition and gives some historical plausibility to the instrumentalist interpretation, in that Berkeley would have been drawing on some philosophical commonplaces. However, it is not certain that such an instrumentalism has been held before the twentieth century, especially by Osiander and Bellarmine. I propose that Berkeley could not have shared their philosophical positions and thus does not rely on a pre-Galilean philosophy of science. If anything, his alleged instrumentalism would be a pure anticipation of most contemporary philosophy of science.

For example, in his *Preface* to Copernicus, Osiander affirms that it is impossible to know the true causes of celestial motions. But he adds, "Maybe the philosopher demands more likelihood, but [neither he nor the astronomer] can know or teach anything certain, unless it has been revealed by God."[10] This version of instrumentalism rests on a kind of scepticism, which grounds philosophy and astronomy to Revelation—which, for Berkeley, is complete nonsense.

Like Osiander, Bellarmine affirms that is it possible to calculate appearances in different ways. But at the conclusion of his famous letter to Foscarini, he makes his views on astronomy quite clear:

> I say that if there were a true demonstration that the sun was in the center of the universe and the earth in the third heaven, and that the sun did not travel around the earth but the earth travels around the sun, then it would be necessary to proceed with great caution in explaining the passages of Scripture which seem to tell the contrary, and we would rather have to say that we did not understand them than to say that something was false which has been demonstrated. *But I will not believe that there is such a demonstration, until it is shown to me.* It is not the same thing to show that the appearances are saved by assuming that the sun is in the center and the earth in the heavens and to demonstrate that the sun really is in the center and the earth in the heavens. For I believe that it is possible to demonstrate the first proposition, *but I have grave doubts about the second*, and in a case of doubt, one may not depart from

[10] Osiander, Preface to Copernicus, *De Revolutionibus Orbium Coelestium* (Wittemberg, 1543), I-II: "Philosophus fortasse, veri similitudinem magis requirer, neuter tamen quicquam certi compraehendet, aut tradet, nisi divinitus illi revelatum fuerit" (my translation). All translations into English are mine.

> the Scriptures as explained by the holy Fathers. I add that the words 'the sun also rises and the sun goes down, and hastens to the place where he arises, etc.' were those of Solomon, who not only spoke by divine inspiration but was a man wise above all others and most learned in human sciences and in the knowledge of all created things, and his wisdom was from God. Thus it is not likely that he would affirm something which was contrary to a truth either already demonstrated or likely to be demonstrated. And if you tell me that Solomon spoke only according to the appearances, and that it seems to us that the sun goes around when actually it is the earth which moves, as it seems to one on a ship that the beach moves away from the ship, *I shall answer that one who departs from the beach, though it looks to him as though the beach moves away, knows that he is in error and corrects it, seeing clearly that the ship moves and not the beach*. But with regard to the sun and the earth, no wise man is needed to correct the error, since *he clearly experiences that the earth stands still* and that his eye is not deceived when it judges that the sun moves, as it is not deceived when it judges that the moon and stars move. And that is enough for the present.[11]

Note how Bellarmine is not even sure that it possible to prove that the Copernican hypothesis can be demonstrated. At least, in 1615, it is not the case. He also clearly doubts whether one could demonstrate that the sun is really in the center of the universe. He gives a physical reason, refuting a classical argument relying on the relativity of motion. To a man on a ship, the shore seems to recede, and if the motion of the ship is regular enough, he cannot decide what is moving, the shore or the ship. According to Bellarmine, this is false: the sailor corrects what appears to be, according to the cardinal, an optical illusion. But even in that case the analogy between the sailor on the ship and the man on the earth does not hold. According to Bellarmine, our eyes are not deceived at all. He emphasizes how we experience that the earth stands still, because we do not feel it moving (this is probably the ground on which the sailor corrects the illusion: he can feel

[11] Bellarmine, Letter to Foscarini, 12 April 1615, in *Le opere di Galileo Galilei*, ed. A. Favaro (Firenze: Giunti Barbera, 1890-1909), XII, 172: "Dico che quando ci fusse' vera dimostratione che il sole sti nel centro del mondo e la terra nel 3° cielo, e che il sole non circonda la terra, ma la terra circonda il solo, alhora bisognera andar con molta consideratione in explicare la Scritture che paiono contrarie, e piu tosto dire che non l'intendiamo, che dire che sia falso quello que si demostra. *Ma io non credero che ci sia tal dimostratione, fin che non mi sia mostrata* ; nè è l'istesso dimostrare che supposto ch'il sole stia nel centro e la terre nal cielo, si salvano le apparanze, e dimostrare che in verità il sole stia nel centro e la terra nel cielo; perchè la prima dimostratione credo che si possa essere, ma *della 2^{a} ho grandissimo dubio*, et in caso di dubbio non si dee lasciare la Scrittura Santa, esposta da'Santi Padri. Aggiungo che quello scrisse : 'Oritur sol et occidit, et ad locum suum revertitur etc.', fu Salomone, il quale non solo parlo inspirato da Dio, ma fu huomo sopra tutti gli sapientissimo e dottissimo nelle scienze humane e nelle cognitione delle cose dreaten e tutta questa sapienza l'hebbe da Dio ; onde non è verisimile che affermasse una cosa che fusse contraria alla verita dimostrata o che si potasse dimostrare. E se mi dira che Solomone parla secondo l'apparenza, parendo a noi ch'il sole giri, mentre la terra gira, come a chi si parte dal litto pare che il litto si parta da lui, *nondimeno conosce che questo è errore e lo correffe, vedeno chiaramente che la nave si muove e non il litto* ; ma quanto al sole e la terre, nessuno savio è che habbia bisogno di correggere l'errore, perchè *chiaramente esperimenta che la terra sta ferma* e che l'occhio non s'inganna quando giudica che il sole si muove, come anco non s'inganna quando giudica che la luna ele stelle si muavano. E questo basti per hora" (emphasis mine).

the boat moving).[12] From all this, it is quite clear that Bellarmine does not see any real equivalence between hypotheses: there is one true physical thesis.

My point is that Osiander is a skeptic and Bellarmine is not clearly an instrumentalist—at least it is impossible to say that he sustains the kind of semantical instrumentalism that Newton-Smith attributes to Berkeley. In a sense, it would be better to say that they are "frustrated realists."[13] In both cases, the knowledge of truth depends on the Scriptures or a Divine Revelation. For obvious reasons, this is very far from Berkeley's philosophy. This implies that, if Berkeley really were an instrumentalist, he would be quite unique. Of course, that is not impossible; after all, Berkeley's immaterialism seems to be original. But Berkeley always tries to prove that his immaterialism follows from agreed-upon principles; and secondly, he develops and explains his immaterialism at length, even though for him it is obvious. There is nothing comparable about his instrumentalism: he presents his original position in a few lines, in what he calls in a letter to Johnson "a tract" (W 2: 83).[14] Maybe this means that he does not see his own originality. Maybe he read sixteenth astronomy in the same way as Duhem. He would then have anticipated not only Duhem's philosophy but also his way of reading historical texts. That I do not find really plausible.

Berkeley's texts

I now turn to Berkeley's texts. My only aim is to see if Berkeley's explicit positions are compatible with instrumentalism. I will examine the two arguments I have mentioned. Before doing that, though, just a word about the claim that Berkeley did not recognize that he was an instrumentalist. It is clear that Johnston's reading of *Siris* 235 is nonsense, for like Newton, Berkeley states that there are different laws in nature.[15] Obviously, the laws of gravity, electricity and magnetism are not the same; but then, one might guess that there are other domains or "provinces" in nature that we do not know yet. That is to say that God could have established other laws than those we already know. After all, it

[12] Of course, such arguments are false, and it is easy to refute them: we do not feel the motion of the earth because we move with it. However, it is necessary to note that Bellarmine wrote his letter in 1615, and Galileo does not refute this kind of argument until seventeen years later, in the second part of his *Dialogue on the Two Systems of the World*, where he states the first formulation of the principle of inertia. In this historical context, Bellarmine's argument perfectly makes sense: he could not know the new physics needed to resolve the issue.

[13] Cf. P. Barker and R. Goldstein, "Realism and Instrumentalism in sixteenth Century Astronomy: A Rappraisal," *Perspective on Science* (1998), 253**.**

[14] In this passage Berkeley seems to oppose the "treatise" (*The Principles of Human Knowledge*) and the "tract," suggesting that the second is shorter (but of no less importance).

[15] Cf. Isaac Newton, *Opticks: Or, A Treatise of the Reflections, Refractions, Inflexions and Colours of Light. The Second Edition, with Additions* (London, 1717), Query 31, 350-351: "Have not the small Particles of Bodies certain Powers, Virtues or Forces, by which they act at a distance, not only upon the Rays of Light for reflecting, refracting, and inflecting them, but also upon one another for producing a great part of the Phænomena of Nature? For it's well known, that Bodies act one upon another by the Attractions of Gravity, Magnetism and Electricity; and these Instances shew the Tenor and Course of Nature, and make it not improbable but that there may be more attractive Powers than these. For Nature is very consonant and conformable to her self."

seems quite reasonable to suppose that we do not know everything (even the most dogmatic realist would admit that). But this does not prove that our present knowledge is not true knowledge.

So on to the two arguments in favor of the instrumentalist interpretation. According to the first, a physical theory is and can only be hypothetical. But Berkeley's declarations directly seem to contradict this assertion. Here, *De Motu* 28 is crucial:

> Action and reaction are said to be in bodies, and that way of speaking suits the purposes of mechanical demonstrations; but we must not on that account suppose that there is some real virtue in them which is the cause or principle of motion. For those terms are to be understood in the same way as the term attraction; and just as attraction is only a mathematical hypothesis, and not a physical quality, the same must be understood also about action and reaction, and for the same reason. *For in mechanical philosophy the truth and the use of theorems about the mutual attraction of bodies remain firm, as founded solely in the motion of bodies, whether that motion be supposed to be caused by the action of bodies mutually attracting each other, or by the action of some agent different from the bodies, impelling and controlling them*. Similarly the traditional formulations of rules and laws of motions, along with the theorems thence deduced remain unshaken, *provided that sensible effects and the reasonings grounded in them are granted,* whether we suppose the action itself or the force that causes these effects to be in the body or in the incorporeal agent. (Emphasis mine)

First, I think that it is necessary to credit Berkeley with consistency. In this section, Berkeley says that the theorems of mechanical philosophy are true and that these theorems rest on suppositions or hypotheses. This undercuts the first argument. Berkeley does not draw the conclusion Popper draws: the fact that mechanical philosophy uses mathematical hypotheses does not entail, according to Berkeley, that it is entirely hypothetical. Indeed, the knowledge provided by natural philosophy remains firm, whatever are the suppositions on which it rests, whether they are in accordance with the nature of things or not. In fact, according to Berkeley, even if the theorems use mathematical hypotheses, they are not strictly grounded on mathematical hypotheses: "the reasonings are grounded in [the sensible effects]," that is, on the motion of bodies.[16]

[16] It seems then that mathematical hypotheses are only one particular way to talk of real motions. Indeed, Berkeley proposes such a translation in *De Motu* 68: "Let us lay down that the new motion in the body struck is conserved either by the natural force by reason of which any body persists in its own uniform state of motion or of rest, or by the impressed force, received (while the percussion lasts) into the body struck, and there remaining; it will be the same in fact, the difference existing only in name. Similarly when the striking moveable body loses motion, and the struck body acquires it, it is not worth disputing whether the acquired motion is numerically the same as the motion lost; the discussion would lead into metaphysical and even verbal *minutiæ* about identity. And so it comes to the same thing whether we say that motion passes from the striker to the struck, or that motion is generated *de novo* in the struck, and is destroyed in the striker. In either case it is understood that one body loses motion, the other acquires it, and besides that, nothing." According to Berkeley, then, understanding the transmission of force must be limited to what is perceivable. Downing does not take

However, it may be possible to interpret the notion of truth as meaning something like "predictive power." I think that such an interpretation of *De Motu* 28 (which is never mentioned by defenders of the instrumentalist interpretation) would be far-fetched but not necessarily impossible. For what is at issue is how Berkeley understands the *truth* of mechanics, the description of the world and its state, or its order. He writes that "the theorems of mechanical philosophy . . . unveil the recesses of nature" (DM 66), but that unveiling needs to be understood in the context of how mechanics is related to metaphysics. As Berkeley observes:

> To treat of the good and great God, creator and preserver of all things, and to show how all things depend on supreme and true being, although it is the most excellent part of human knowledge, is, however, rather the province of first philosophy or metaphysics and theology than of natural philosophy which to-day is almost entirely confined to experiments and mechanics. And so natural philosophy either presupposes the knowledge of God or borrows it from some superior science. Although it is most true that the investigation of nature everywhere supplies the higher sciences with notable arguments to illustrate and prove the wisdom, the goodness, and the power of God. (DM 34)

On the one hand, natural philosophy provides "notable arguments" to theology, in order to reveal the divine Wisdom, Power and so on. This supposes that mechanics describes something of the world; otherwise, it could not illustrate the Wisdom of God. On the other hand, if mechanics were entirely hypothetical, it would not have to "borrow" from theology the certainty that God acts in a regular manner. If for the instrumentalist, the order that may be found in nature is only the order we introduce into it (merely for calculative or predictive purposes), then there would be no need (as Berkeley insists there is) for highlighting the intrinsic relation between natural philosophy and theology.

Admittedly, Berkeley often, if not always, mentions the utility of science. In *De Motu* 28, for example, he acknowledges that it is not only the truth but also the usefulness of the theorems of mechanics that are fixed. Usefulness cannot be the sole criterion of acceptability for a scientific theory, because a completely acceptable scientific theory should be both useful and true. Some scientific theories are useless: for example, parts of arithmetic and even natural history are true but a waste of time. Others (e.g., analysis) are false but useful in that they allow us to identify good results. But no legitimate scientific theory is acceptable independently of how it is linked to the truth. And that is the point at issue in challenging the instrumentalist account.

DM 68 into account when she dismisses this "reductionist interpretation" (according to which "Berkeley would hold that dynamics is reducible to kinematics, that is, he would be committed to the possibility of translating any statement apparently invoking forces into a statement merely about the motions of bodies"). In her view, "Berkeley always *justifies* the use of mathematical hypotheses by the *utility* of dynamics, never by the *translatability* of dynamic terms into kinematic ones, nor does Berkeley offer anything like a manual for translation." See Lisa Downing, "Berkeley's Philosophy of Science," in *The Cambridge Companion to Berkeley*, ed. Kenneth P. Winkler (Cambridge: Cambridge University Press, 2005), 263. Cf. Luc Peterschmitt, "Berkeley et les hypothèses mathématiques," *Archives Internationales d'Histoire des Sciences* (2003), 192-94.

The second argument in favor of Berkeley's supposed instrumentalism concerns his view about empirically equivalent theories. Berkeley certainly knew that different hypotheses could be used to explain the same phenomena. In the *Dialogues between Hylas and Philonous* he alludes to the issue when he writes:

> And if it pass for a good argument against other hypotheses in the sciences, that they suppose Nature or the divine wisdom to make something in vain, or do that by tedious round-about methods, which might have been performed in a much more easy and compendious way, what shall we think of that hypothesis which supposes the whole world made in vain? (DHP II, W 2: 214)[17]

This question was especially important in the context of Cartesian science, since mechanist principles may provide different explanations.[18] But when it is impossible to choose between two hypotheses on empirical grounds, there are metaphysical reasons to keep one hypothesis, relying on the way in which God acts (viz., the easiest and most compendious way, as Berkeley puts it). So that even if physics remains uncertain and only probable, the possibility of concurring hypotheses is not understood in an instrumentalist way, because of the link between physics and metaphysics.

However, Berkeley separates physics and metaphysics in the *De Motu*. He could have (maybe he should have) addressed the question about their relation, but he does not. For

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[17] As Richard Glauser has shown, this is an *ad hominem* argument: it is not an indication that Berkeley endorses the position. See Richard Glauser, *Berkeley et les philosophes du XVIIe siècle. Perception et scepticisme* (Sprimont: Mardaga, 1999), 287. Malebranche makes the point: "As God acts only by using the simplest ways, it seems unreasonable to explain how we know things by admitting the creation of an infinite number of beings, since this difficulty can be solved in an easier and more natural manner." See Nicolas Malebranche, *De la recherche de la vérité*, in *Œuvres*, éd. G. Rodis-Lewis and G. Malbreil (2 vols; Paris: Gallimard, 1979-1992), 3.2.4; 1: 334; and "God never does by very difficult ways what can be done in the easiest and the simplest ways: God does not do anything uselessly and without reason" (*ibid.*3.2.6; 1: 338). My mention of Berkeley's appeal to Malebranche's argument is intended primarily to highlight his strategy regarding alternative hypotheses.

[18] See for example J. Rohault, *Traité de Physique* (Amsterdam, 1672) or Pierre-Sylvain Régis, *Système de philosophie, contenant la logique, métaphysique, physique et morale* (Lyon, 1691). The latter writes: "As its parts [of the body] are not perceptible, we cannot perceive their order or arrangement, and all that we can do is make guesses through its effects . . . . Although peculative physics can be treated only in a problematic way, and nothing demonstrative pertains to it, one must grant, however, that this part of physics, as uncertain it is, holds one of the highest ranks in human knowledge: because even if we are not completely assured of what it teaches us, we can believe that we know everything that the human mind can about a physical body if we can distinctly conceive a disposition, figure, and arrangement of its parts in such a way that all the effects depending of this body can easily be deduced . . . and since mathematics should admit only what is certain and demonstrative, physics should admit all that is probable, provided that it is deduced from one system grounded on the first truths of nature. . . . Since nature always acts in the simplest ways, we are sure that its action is to be explained by one system only. By SYSTEM, we do not understand one hypothesis, but a mass of several hypotheses that depend on one another, linked to the first truths in such a way that they are as necessary consequences and deductions from them. Purely arbitrary hypotheses cannot do that" (2: 4-5).

example, he does not seem to admit that there are equivalent hypotheses in astronomy. Compare these two texts:

> It is one thing to arrive at general laws of nature from a contemplation of the phenomena, and another to frame an hypothesis, and from thence deduce the phenomena. Those who supposed epicycles, and by them explained the motions and appearances of the planets, may not therefore be thought to have discovered principles true in fact and nature. (*Siris* 228)

> And if, by considering this doctrine of force, men arrive at the knowledge of many inventions in Mechanics, and are taught to frame engines, by means of which things difficult and otherwise impossible may be performed; and if the same doctrine which is so beneficial here below serveth also as a key to discover the nature of the celestial motions; shall we deny that it is of use, either in practice or speculation, because we have no distinct idea of force?" (*Alciphron* VII. 7; W 3: 295-296)

In the first text, Berkeley does not say a word about those who do not use epicycles (i.e., the Copernicans). And in the second he clearly says that the Newtonian theory of force allows us to discover the true celestial motions.[19] But Newton's theory supposes that the sun is a center of force—that is, it is in the "center" (at least of the motion of the earth).[20] So it seems that Berkeley reverses Bellarmine's position: for the Cardinal, the Copernican hypothesis allows us to save the phenomena, but it is false; for the Bishop, if the Ptolemaic hypothesis accounts for the motion, it is nevertheless false. In neither case do I see any semantic instrumentalism.

In any event, commentators generally rely on one text to justify the instrumentalist interpretation, *De Motu* 67. There Berkeley seems to adopt the thesis according to which there equivalent theories:[21]

> It remains to discuss the cause of the communication of motions. Most people think that the force impressed on the moveable body is the cause of motion in it. However that they do not assign a known cause of motion, and one distinct from the body and the motion is clear from the preceding argument. It is clear, moreover, that force is

[19] This is not a question of knowing the nature of motion (which is the task of metaphysics). It is just a question of determining the true motions of celestial bodies.

[20] Of course, it is not a center in a geometrical sense, since the orbs are not circular (cf. Kepler's first law). However this law clearly states that the earth moves around the sun. Berkeley makes an allusion to Kepler's laws (the third) in the *Dialogues*: "Raise now your thoughts from this ball of earth, to all those glorious luminaries that adorn the high arch of heaven. The motion and situation of the planets, are they not admirable for use and order? Were those (miscalled erratic) globes ever known to stray, in their repeated journeys through the pathless void? Do they not measure areas round the sun ever proportioned to the times? So fixed, so immutable are the laws by which the unseen Author of Nature actuates the universe" (DHP II; W 2: 210-211). If he admits the third law, it would be difficult for him to refuse the first. Indeed, the general tone of this passage is clearly Copernican.

[21] For my argument, the question whether this is an argument in favor of instrumentalism or a consequence of Berkeley's semantics does not matter. My aim is to show that Berkeley does not maintain such a position at all.

> not a thing certain and determinate, from the fact that great men advance very different opinions, even contrary opinions, about it, and yet in their results attain the truth. For Newton says that impressed force consists in action alone, and is the action exerted on the body to change its state, and does not remain after the action. Torricelli contends that a certain heap or aggregate of forces impressed by percussion is received into the mobile body, and there remains and constitutes impetus. Borelli and others say much the same. But although Newton and Torricelli seem to be disagreeing with one another, they each advance consistent views, and the thing is sufficiently well explained by both. For all forces attributed to bodies are mathematical hypotheses just as are attractive forces in planets and sun. But mathematical entities have no stable essence in the nature of things; and they depend on the notion of the definer. Whence the same thing can be explained in different ways. (DM 67)

According to Popper, Newton-Smith and others, Berkeley says that it is possible to explain phenomena in different ways, so it would be possible to contrast Newton and Torricelli. But such a reading rests on a surprising shift. See how Newton-Smith paraphrases Berkeley. Berkeley's sentence is: "they each advance consistent views, and *the thing* [in the singular] is sufficiently well explained by both" ["*singuli sibi consentanea proferunt, res satis commode ab utrisque explicatur*"].[22] Here is Newton-Smith's reading: "both 'attain the truth', both 'explain' things" [in the plural]. This shift from "the thing" to "things" misinterprets Berkeley. Luce correctly translates the Latin "res" as singular. But then it cannot signify "phenomena." And once again, at the end of the section, Berkeley speaks of "the same thing" which may be explained ("*unde eadem res diversimode explicari potest*"). In both cases, the "thing" in question is not a phenomenon, but what Newton or Torricelli conceive when they speak of forces (that is to say when they try to explain the cause of the communication of motion). The term here refers to the supposed cause of the communication of motion. One should then distinguish two levels of explanation: (a) the explanation of the primitive concepts or notions of a theory (the very first part of Newton's *Mathematical Principles of Natural Philosophy*); and (b) the explanation of the phenomena.[23] The equivalence of explanations concerns only the first level of explanation. At this level, natural philosophers try to explain their conceptions or notions (e.g., what they understand by a

[22] DM 67, W 4: 29.

[23] Berkeley describes the work of the physicist in this way: "The human mind delights in extending and expanding its knowledge; and for this purpose general notions and propositions have to be formed in which particular propositions and cognitions are in some way comprised, which then, and not till then, are believed to be understood. Geometers know this well. In mechanics also notions are premised, i.e. definitions and first and general statements about motion from which afterwards by mathematical method conclusions more remote and less general are deduced. And just as by the application of geometrical theorems, the sizes of particular bodies are measured, so also by the application of the universal theorems of mechanics, the movements of any parts of the mundane system, and the phenomena thereon depending, become known and are determined. And that is the sole mark at which the physicist must aim" (DM § 38). First, the physicist gives the definitions, then the most general statements, then the mathematical theorems, and *in fine* the application to the phenomena. This is the very structure of Newton's *Mathematical Principles of Natural Philosophy*. Berkeley discusses only the first definitions.

word). But as Berkeley repeatedly insists regarding the obscurity of terms such as "corporeal force," this is a difficult task.

Moreover, Berkeley does not mention here any theorem bearing on the communication of motions, but only Newton's definition of impressed force.[24] This is the important point made in the last sentence of the section: "Whence the same thing can be explained in different ways." This sentence is ambiguous. It is possible that the word "thing" refers to the notion of force that the philosophers of nature frame. But given the differences between the concepts, it is difficult to say that they share anything. It would be better to say that the "thing" is the communication of motion. The problem here rests on the verb "to explain." Obviously, since Berkeley mentions the definitions of force given by Newton and Torricelli, we should not take the term in his technical sense (viz., to reduce a phenomenon to general laws). If Berkeley had intended that, then he would have had to admit something like a causal explanation (even if it were only a hypothetical explanation). However, giving such an explanation is not the task of natural philosophy.

The "empiricist concept"[25] of causality is of no use here: the problem is not to say that a body is the cause of the motion of another, but what happens in or during an impact. In fact, in their definitions of force, Newton and Torricelli try to explain how they conceive that a body may cause the motion of another body.[26] As this is not a real explanation (in mechanics), what is precisely conceived does not really matter: such conceptions affect neither the fact of the communication of motion nor its laws. Indeed, it is possible to see how there might not be any real difference between Newton and Torricelli: both maintain that after an impact a body keeps its motion in a straight line at a constant speed.[27] Is there any real difference in saying (a) that it is the state of motion that is modified (and then explain the conservation of motion by a force of inertia), or (b) that a body retains an *impetus* that moves it? In both cases, to know the direction and speed of the impacting body may help to explain the direction and speed of the impacted body, given the general laws of motion. As far as mechanics alone is concerned, it is possible to conceive that there are little men (or something comparable) in bodies that push and pull them, as long as they know perfectly the general laws of motion and act according to them. Once again, it would not change anything regarding the laws, which are the object of mechanics. Of course, from a metaphysical point of view, such a hypothesis is certainly false, just as are those of Newton and Torricelli.

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[24] Newton, *Mathematical Principles of Natural Philosophy*, definition IV: "Impressed force is the action exerted on a body to change its state either of resting or of moving uniformly straight forward" [Newton, *Mathematical Principles of Natural Philosophy*, trans. by I.B. Cohen and A. Whitmann (Berkeley: University of California Press, 2000), 405].

[25] See Bertil Belfrage, "Vers une nouvelle interprétation de la Théorie de la Vision de Berkeley," in *Berkeley, langage de la perception et art de voir*, ed. Dominique Berlioz (Paris: Presses Universitaires de France, 2003), 159-212.

[26] Indeed, Berkeley specifically mentions (DM 67) the commentary that Newton added to his definition: "This force consists solely in the action and does not remain in a body after the action has ceased. For a body perseveres in any new state solely by the force of inertia" (Newton, *Mathematical Principles*, 405).

[27] See note 16.

Conclusion

Berkeley is not an instrumentalist. At least, he is not explicitly an instrumentalist in the strong sense of Popper and Newton-Smith. The argument they and their followers give to support this interpretation does not seem to be conclusive. Still, it is possible that his conception of mathematical hypotheses could lead him to adopt an instrumentalist position; but then he would be inconsistent. This means that if the instrumentalist interpretation is right, then there is something rotten in Berkeley's philosophy. To put it in another way: proof that instrumentalism is a necessary consequence of Berkeley's philosophy would be an argument against him.

This is not to say that, just because Berkeley is not an instrumentalist, he must be a realist—at least, a "realist" in the sense that Newton or Leibniz could be. Instead of thinking of Berkeley in terms of this contrast, perhaps we could consider him more a conventionalist, closer to Poincaré's conventionalism than to Duhem's instrumentalism.[28] Here I am not being (or at least, not only being) ironic. If a comparison is really necessary, then this is the one that should be drawn. It is surprising that it has not been drawn, as if contemporary philosophy of science oscillates only between the strongest versions of instrumentalism and of realism. However, I am not sure that such a way to read old texts is cogent: it amounts to building ghosts of contemporary philosophy. It is more interesting to evaluate Berkeley's own position by comparing it to Newton's *Principles*, in which case we should instead ask how Berkeley gives a plausible account for the work of natural philosophy in the beginning of the eighteenth century. Maybe it doesn't (and that would be a good argument against the good bishop). But then again, maybe it does. In both cases Berkeley would still be of philosophical interest. However, to answer these questions requires that we examine more precisely what was Berkeley's philosophy of science, and in particular the role he attributes to mathematical hypotheses.

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[28] Here I can only give an indication of what I have in mind by pointing to Poincaré's introduction to his *La science et l'hypothèse*. He writes that mathematical sciences rest on "disguised definitions or conventions": they are "decrees" of our spirit—decrees that impose themselves on our science, but not on nature. Despite this fact, the scientist does not only know a world that he created himself (we could say that the order he finds in the world is not only the order he puts in it). Poincaré adds: "If it were the case, science would be powerless. But we can see it acting every day. This could not happen if it did not make us know something of reality; however, it cannot know things in themselves, as the naïve dogmatic thinks, but only the relations between things; there is nothing knowable about reality but these relations." See Henri Poincaré, *La science et l'hypothèse* (Paris: Flammarion, 1968), 25. I do not claim that there is a perfect analogy. However, there are resemblances. In particular, both Poincaré and Berkeley affirm that there is something useful in the "principles" of a theory, and that, nevertheless, the theory may be true. It may entail a comparable tension between "usefulness" and truth.