



## A MODEST REFUTATION OF MANIFESTATIONALISM\*

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### ABSTRACT

In their recent “A modest defense of manifestationalism” (2015), Asay and Bordner defend this position from a quite famous criticism put forward by Rosen (1994), according to which while manifestationalism can be seen as more compatible with the letter of empiricism than other popular stances, such as constructive empiricism, it fails nonetheless to make sense of science. The two authors reckon that Rosen’s argument is actually flawed. In their view, manifestationalism could in fact represent a legitimate thesis about the nature of scientific inquiry. In this paper, I will show that Asay and Bordner’s criticisms to Rosen are actually off target. Moreover, they rest upon an understanding of what the aim of science is that might serve to their purposes, but that does not seem to be in line with the scientific enterprise. Perhaps constructive empiricism still represents the best compromise so far presented between strict empiricism and the acknowledgment of the rationality of science.

*Keywords:* Asay and Bordner; constructive empiricism; manifestationalism; Rosen; van Fraassen

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## UNA MODESTA REFUTACIÓN DEL MANIFESTACIONALISMO

### RESUMEN

En su reciente artículo “A modest defense of manifestationalism” (2015), Asay y Bordner defienden la posición de una famosa crítica presentada por Rosen (1994), según la cual, aunque el manifestacionalismo puede ser más compatible con el espíritu del empirismo que otras vertientes bastante conocidas, tales como el empirismo constructivo, no logra, sin embargo, interpretar de manera efectiva la ciencia. Los dos autores consideran que, en realidad, el argumento de Rosen falla. Según ellos, el manifestacionalismo puede de hecho representar una tesis legítima acerca de la naturaleza de la investigación científica. En este trabajo mostraré que las críticas de Asay y Bordner a Rosen en realidad no dan en el blanco. Además, reposan sobre una interpretación del objetivo de la ciencia que puede servir para los propósitos de los autores, pero que no es aceptable. Quizás el empirismo constructivo todavía sea el mejor compromiso encontrado hasta la fecha entre un empirismo riguroso y el reconocimiento de la racionalidad de la ciencia.

*Palabras clave:* Asay y Bordner; empirismo constructivo; manifestacionalismo; Rosen; van Fraassen

IN THEIR RECENT “A modest defense of manifestationalism” (2015), Jamin Asay and S. Seth Bordner try to defend this empiricist position from a quite famous case that Gideon Rosen put forward against it—and that will be presented in section 2—, arguing that “the primary argument against manifestationalism fails to hit its target” (p. 147).

I am not sure that they succeed, though, as I will try to show. Moreover, since their main concern is to refute what seems to be the sole case against manifestationalism, it is my contention that even if one concludes that Asay and Bordner succeeded in rebutting Rosen’s example, it is quite easy to formulate other ones that are fatal to the idea that manifestationalism (henceforth M, following the two authors’ use) does make sense of scientific practice.

Finally, the concept of *aim of science* that is used in the paper does not seem to make justice to what is usually held in literature either. What is more, in at least one passage it gives the impression of being suited to the authors’ goal—which Asay and Bordner think they have achieved—but actually depends on an *ad hoc* premise. Since the aim of science cannot be what they would like it to be, it is *their* argument that fails to hit its target.

### 1. Manifestationalism, models and domains

ACCORDING TO CONSTRUCTIVE EMPIRICISM (CE), the well-known view on science and its aim introduced by Bas C. van Fraassen in his seminal book *The Scientific Image* (1980), “*science aims to give us theories which are empirically adequate; and acceptance of a theory involves as belief only that it is empirically adequate*” (van Fraassen, 1980, p. 12; emphasis in the original). Roughly speaking, a theory is empirically adequate if what it says about the observable things and events in this world is true; in other words, if it “saves the phenomena” (see van Fraassen, 1980, p. 12). In the same book, van Fraassen also offers a characterization of scientific realism (SR): “*science aims to give us, in its theories, a literally true story of what the world is like; and acceptance of a scientific theory involves the belief that it is true*” (1980, p. 8; emphasis in the original).<sup>1</sup>

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1 This formulation seems to be considered satisfactory by most scientific realists (for exceptions, see Sicha, 1992, p. 522).

Following van Fraassen's description of SR and CE, Asay and Bordner define manifestationalism (M) in terms of the aim of science. According to M, they say, "science aims to give us theories that are comprehensively manifestly adequate [and] acceptance of a theory involves only the belief that it is comprehensively manifestly adequate" (2015, p. 149). The locution *comprehensively manifestly adequate* applies to theories that capture "all truths about *all* the observed phenomena, whether past, present, or future" (2015, p. 149).

Similarly, according to van Fraassen, a true theory speaks *all* truths, while an empirically adequate one captures *all* the (observable) phenomena. The reason is well known: the so-called "semantic approach", notoriously advocated by the Dutch philosopher, identifies a theory with a class of mathematical models. One can think of models as representing the possible worlds allowed by the theory, explains the originator of constructive empiricism. One of the consequences is that "to believe the theory is to believe that exactly one of its models correctly represents the world (not just to some extent, but in all respects)" (van Fraassen, 1980, p. 47).

Asay and Bordner interpret the above passage—and others contained in *The Scientific Image*<sup>2</sup>—as if any scientific theory were, in fact, a "theory of everything" (see 2015, p. 149). No branch of science is that comprehensive, however. As a matter of fact, specific areas of studies focus on specific domains. For instance, astroparticle physicists are interested in knowing if a certain theory correctly describes phenomena associated with the emission of gamma rays by a distant star, but not if the very same model they are considering is also a faithful representation of, say, the behavior of a child when their parents argue or if it explains why the particular kind of moss discovered in Sweden last year is capable of removing arsenic from contaminated water.

Actually, the semantic view does not even entail "theories of everything". In fact, it can be considered as a "localized" approach to scientific theories, for it emphasizes specific features of the domains to which the models of a theory

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2 Asserting a theory to be true, for instance, says van Fraassen, means affirming that it has a model "which is a *faithful replica, in all detail, of our world*" (van Fraassen 1980, pp. 68-69; my emphasis).

apply.<sup>3</sup> For the sake of the argument, however, I'll stick with what seems to be Asay and Bordner's interpretation. If one thinks of the models as "maps of everything", but bears in mind that scientists focus on (and only work with) very specific portions of them, corresponding to the intended domains of the theories they deal with, then perhaps, at the end of the day, this does not make much difference. The problem is that Asay and Bordner do *not* take into account the fact that scientific theories *do* have an intended domain.

Now, if acceptance of a theory involves the belief that it is true with respect to (all) the *observed* phenomena, rather than to the *observable* ones, then M certainly represents a stricter version of empiricism than CE—"an exceptionally strict empiricist perspective on science", say Asay and Bordner (2015, p. 148)—, for a theory being empirically adequate entails that it is (comprehensively) manifestly adequate, but not vice versa. For instance, an adept of M would not believe that a theory she accepted correctly describes a flower grown on an extrasolar planet in 1492, while a constructive empiricist would believe so. Nonetheless, believing that a certain theory captures all truths about all the observed phenomena means sticking our neck out too, no differently from CE or SR. As a matter of fact, not only does M consider past and present phenomena (i.e., the so-far-observed ones), but also observations that will be performed in the next millennium—provided the human species and the world will still be there. Being so, believing that a theory is manifestly adequate involves making a leap of faith too; this is not a peculiarity of SR or CE.

A way to avoid this would be adopting an even stricter version of empiricism, such as a version of manifestationalism that Asay and Bordner seem to think that some of the philosophers they try to refute have in mind, i.e., one that maintains that the aim of science is to provide theories adequate only to the observed phenomena *so far*. Perhaps this would be more adherent to what the two authors call "the guiding idea of empiricism", namely, the idea that only experience gives us knowledge about the world (Asay & Bordner, 2015, p. 148). But of course, the aim of science cannot be giving us theories that only describe some past or

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3 I would like to thank Professor Otávio Bueno, of the University of Miami (e-mail messages of April 28<sup>th</sup>, 2019), and an anonymous referee of this Journal for clarifying my view on this aspect of the semantic approach to scientific theories.

present states of affairs and for which making predictions is optional. Hardly anyone would call these theories *scientific*—not even *theories*, probably.

Since this version of manifestationalism, such as any other even stricter versions of empiricism, cannot make sense of scientific activity, it cannot be considered a view on science either. Asay and Bordner think that M (*their* formulation of manifestationalism), instead, *does* make sense of scientific activity and therefore could represent “*the* principled stopping point between full-blown realism and extreme skepticism” (2015, p. 150, fn. 2), something that they are allegedly cautious not to claim, but not very convincingly.

The term ‘manifestationalism’, Asay and Bordner remind us, was coined thirty years ago by Peter Railton. What Railton had in mind, however, was a version of manifestationalism allegedly different from the one Asay and Bordner defend in the paper that is being addressed here. In his “Explanation and Metaphysical Controversy” (1989), in fact, the author describes a *manifest theory*, “which says all that the observational theory says about observed observables— past, present, or future—but which is altogether silent about unobserved observables” (p. 235). According to Railton, whereas other theories say things like, for instance, “All swans are white” as well as “All swan observations are white swan observations”, the manifest theory says only the latter.

Asay and Bordner are right in pointing out that M is somehow different from Railton’s definition of manifestationalism<sup>4</sup>, for, according to them, “manifestationalists may accept the very same theories as scientific realists, though accepting a theory, for a manifestationalist, means something different than for a realist” (Asay & Bordner, 2015, p. 151, fn. 3). But this does not prevent them from trying to address Rosen’s thought experiment as if he had M in mind and not the position held by someone who accepts only the manifest theory.

This means that, for practical purposes (regarding the case put forward by Rosen, at least), the two versions amount to the same thing. Or else, addressing Rosen’s example while having a different formulation of manifestationalism in mind would not make much sense.

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4 “Presumably, a manifestationalist for Railton is one who accepts only the manifest theory” (Asay & Bordner, 2015, p. 151, fn. 3).

As a matter of fact, if an adept of M believes only in that part of a theory she accepts that describes *observed* states of affairs, whether past, present, or future, having only a manifest theory at hands would not make any difference, for Railton is explicit about the manifest theory being one that covers all observed observables—past, present, or future. Indeed, here is how Rosen describes Railton’s manifestationalism: “The manifestationalist holds that acceptance of a scientific theory involves only the belief that it is adequate to the actually observed phenomena, past, present and future, and that science aims to produce theories that possess this feature” (1994, p. 161). Where is the difference with M?

## 2. The case of the archaeologist

ASAY AND BORDNER CALL THEIR WORK “a modest defense of manifestationism” because their main focus is just to show that the sole argument against this version of empiricism fails to hit its target—and therefore that it is not irrational to adopt M as a view about the nature of scientific inquiry.

Rosen’s 1994 thought experiment was meant to show that M cannot make sense of one of the most prominent features of the scientific activity: generating new phenomena to be saved by our theories. Rosen thinks that, according to M, there is no reason for an archaeologist to search for new evidence if the theory he holds has not proven to be manifestly inadequate up to that moment. “But one of the hallmarks of good scientists is that they perform experiments pushing beyond the limits of what has been observed so far. Manifestationalism fails to capture our idea of what it is to do good science”, conclude Monton and van Fraassen (2003, p. 407).<sup>5</sup>

Rosen agrees that M involves less belief, and is therefore more compatible with the letter of empiricism, if compared with van Fraassen’s constructive empiricism, for it refuses to take into account the observable parts of reality that are never in fact examined. “However—he adds—it is also a revisionary stance” (1994, p. 162), for the reason already mentioned. Therefore, it cannot replace CE as the minimum empiricist position capable of making sense of science.

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5 Asay and Bordner consider the case of a female archaeologist, while in Rosen’s thought experiment the archaeologist is a man. I decided to stick with Rosen’s original case, despite the common use.

Here is Rosen's passage:

Consider, for example, an archaeologist whose theory covers all the evidence so far collected about Etruscan urns. He has sole license to dig in the last uninspected patch of ground, but it is also in his power to destroy the site so that no one will ever have a chance to inspect it. What should he do? A real scientist would dig, of course. And this is just what we should expect if his aim were to produce empirically adequate theories in van Fraassen's sense. For then he would regard his theory as responsible to the urns that are now still underground—because they are observable things—whether or not anyone actually observes them. But what sense can the manifestationalist make of this digging? From where he sits, the archaeologist who digs risks turning an adequate theory into an inadequate one by unearthing the sole extant counterexample. On the other hand, by destroying the site he will have guaranteed that his theory is ideal by his lights, i.e., adequate to the actually observed phenomena in its intended domain. Manifestationalism therefore motivates an ostrich-like ducking at crucial moments which is clearly incompatible with the imperative to observe as much as possible that informs all real science. Actual science does not proceed as if manifestationalism were a true account of the aims and attitudes of scientists. A theorist who adopts it must therefore regard much of what scientists do as irrational given his conception of the aim of the practice; and to this extent he must either seek to reform it or to opt out (1994, p. 162).

The case put forward by Rosen is quite clear and seems to have persuaded other authors (such as Monton & van Fraassen, 2003, among others). But Asay and Bordner are not convinced and raise a list of criticisms, to the effect that “even if manifestationalism is not the correct view of science, the objections levied against it so far can be met” (2015, p. 148). In the next section, I will try to show that the two authors' criticisms are off target.

### 3. Criticism of the criticisms

ASAY AND BORDNER RECKON that “there are (at least) five problems with Rosen's argument, any one of which vitiates its conclusion” (2015, p. 154). In this section I will address each one of their objections.



The first one is that a theory which is adequate *only* to the phenomena observed so far is not (of course) *comprehensively* manifestly adequate—unless the human species disappears right now. Therefore, they say, there is not the risk of turning an adequate theory into an inadequate one, contrary to what Rosen claims (Asay & Bordner, 2015, pp. 154-155). However, Rosen does not say that that the archaeologist's theory covers *only* the phenomena observed so far. Instead, he invites us to imagine “an archaeologist whose theory covers all the evidence so far collected about Etruscan urns” (1994, p. 162). For all we know, and unlikely as it is, the theory could very well be *comprehensively* manifestly adequate too. It is not correct, then, to claim that “the archaeologist does not start with a manifestly adequate theory” (Asay & Bordner, 2015, p. 155). There is simply no way of knowing—so far, at least.

Worse, suppose the theory says that the urns still underground are now yellow, because of some chemical reaction with the ground. In case the archaeologist digs and the urns turn out to be yellow, his theory remains adequate (so far) *with respect to Etruscan urns*. But the same is true even in case no digging is performed or if the site is blown up. Suppose, in alternative, that the so-far-adequate theory predicts something more bizarre, such as that one hour after being taken off the ground the urns will emit a melodious sound for exactly 48 minutes and then will turn green. The archaeologist will probably judge that by unearthing the urns that are still underground he risks turning an up-to-that-moment-adequate theory into an inadequate one. Being aware that, by destroying the site, the theory will keep manifestly adequate in its intended domain, he might actually choose to blow up the place—provided he agrees that the aim of science is what M says it is, of course. Which is exactly Rosen's point.

Moreover, Asay and Bordner claim that “so long as human beings continue to survive, there will continue to be new observations for theories to account for” (2015, p. 155). Therefore, the according-to-M aim of science is not fulfilled by the archaeologist's theory and for this reason there is not the risk of turning an adequate theory into an inadequate one. As said before, however, this objection rests upon the idea that the archaeologist's theory is adequate *only* to the phenomena observed so far, which is not what Rosen has in mind. But even if it were, an archaeologist is not interested in *any* observation performed by the members of the human species. He is probably not even capable

of judging the adequacy of the most part of the models of his own theory—provided they are *comprehensive* as Asay and Bordner take them to be. Or should an archaeologist be capable of evaluating whether his theory explains why Don Juan Pond’s water refuses to freeze even at -50 degrees Celsius (it is situated in Antarctica) or correctly predicts the behavior of a tiger in captivity?

Rosen appropriately talks about a theory “adequate to the actually observed phenomena *in its intended domain*” (1994, p. 162; my emphasis). In his thought experiment, the domain is restricted to the Etruscan urns, and one could even say that it is a theory about these objects, meaning that even if one interprets van Fraassen’s claim that its models represent “the possible worlds allowed by the theory” (van Fraassen, 1980, p. 47) as implying that they are *comprehensive*, the focus is very narrow: only Etruscan urns matter. Again, this is true of any branch of science. For instance, suppose that the Big Bang theory’s models should also describe how Hurricane Andrew formed from a tropical wave that crossed from the west coast of Africa to the tropical North Atlantic Ocean on August 14, 1992, as Asay and Bordner seem to think. Even if that were the case, of course physicists and astronomers would never check this fact (and meteorologist would probably rely on other models, not on the Big Bang theory’s ones). Still, they might very well believe that the theory is comprehensively manifestly adequate, in case they judge it adequate to the actually observed phenomena *in its intended domain*.

Neglecting the quite obvious fact that while their models might even be comprehensive, scientific theories do have intended domains, Asay and Bordner raise criticisms to Rosen’s argument that cannot be effective. Or worse, that are off target.<sup>6</sup> The next paragraphs will make this clearer.

The second objection is a bit confused, but apparently relies on a wrong interpretation of Rosen’s example as well. According to Asay and Bordner, the archaeologist does not know whether there are urns underground in the last un-inspected patch of ground, the theory does not presuppose the existence of urns there, and, in any case, “if there are urns heretofore unknown to archaeologists

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6 An anonymous referee of this Journal made the interesting point that it is against the intended domain of scientific theories that M, CE or SR must be judged; otherwise, the issue is not an epistemological one (about theories), but rather an ontological one (about entities).

at *L*, then those urns were created by human hands, and so are a source of many *already observed* phenomena from the past that the archaeologist's theory does not currently account for" (2015, p. 155).

Again, in Rosen's thought experiment, the archaeologist has a theory that "covers all the evidence so far collected about Etruscan urns" (1994, p. 162), which means that if there are urns at *L*, then they are accounted for—and that what counts to the archaeologist is that the theory is adequate to the actually observed phenomena *in its intended domain*.<sup>7</sup> But the theory *does* in fact presuppose the existence of urns at *L*, for Rosen speaks of "urns that are now still underground" (1994, p. 162). The consequence is that the archaeologist *does* know that there are urns underground at *L*. It is his reason for considering it risky for the theory's adequacy to unearth them that we do not know. In the above answer to the first objection I have put forward a couple of addenda to Rosen's "short story" that could explain why the archaeologist might think that digging might not be worthy or, worse, even risky to his theory's adequacy. Of course, they are fictional addenda, such as Rosen's case is, but I think they fit the thought experiment under consideration. The point, however, is that Asay and Bordner's second objection seems completely off target, since they assume that the archaeologist does not know about the presence of urns underground and, especially, whether his theory is even so-far manifestly adequate; but he does, *ex hypothesi*.

The third criticism goes straight to the heart of the question of the *intended domains*, that has already been addressed and to which Asay and Bordner seem to give no attention at all. Here is a passage from their objection:

The archaeologist who chooses not to dig does not make an observation regarding the presence of urns in *L* but she does make other observations that her theory, or other theories, will have to account for. There simply is no way—short of every last one of us committing mass, coordinated suicide—of *not* creating new observed phenomena. The only way to avoid bringing about new observed phenomena (and thus end the need for science, according to

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7 The second criticism raised by Asay and Bordner seems to be immune to the counterargument that focuses on the fact that any scientific theory has an intended domain, since the "*already observed* phenomena from the past that the archaeologist's theory does not currently account for" (Asay & Bordner, 2015, p. 155) are in fact part of the archaeologist's theory's intended domain.

manifestationalists) is to cease having experiences altogether. On the minimal assumption that we survive as a species, plenty more phenomena will be observed. Manifestationalism requires that scientific theories account for those observations, whatever they turn out to be. But, importantly, there is no way to know in advance *or ensure* that the phenomena that will be observed will or won't threaten theories that are so-far adequate to the observed phenomena (2015, p. 155).

The first line says it all. Choosing not to dig, the archaeologist avoids the creation of new evidence about Etruscan urns and the risk of turning his so-far-adequate theory into an inadequate one *in that domain*. To him, that is all that matters. But this is not the kind of behavior one should expect from an archaeologist (and that probably will never be witnessed). *Any* archaeologist, in a situation like the one described in Rosen's thought experiment, would dig. Most definitely. Unless something exceptional prevents him from doing it—but nothing in Rosen's description suggests this would happen. Again, Asay and Bordner's objection seems off target.

In their fourth objection, the two authors claim that “M does not encourage the archaeologist not to dig for that last Etruscan urn because M does not hold that the aim of science is to preserve the adequacy of the theories we already hold” (2015, p. 155). According to them, if the archaeologist chooses to dig, the worse than can happen is that he is forced to accept a different theory (in case the old one turns out to be inadequate). If, on the other hand, he chooses not to dig, his theory will continue to be manifestly adequate with respect to the urns, though he will open himself up “to different adequacy worries concerning all sorts of other matters (such as psychological theories about the effects on archaeologists from destroying their own archaeological sites)” (p. 156).

But it is immediately clear that, if it were for M, the archaeologist might very well decide not to dig—as the authors implicitly admit. And this should suffice to refute it as an adequate view of science, since, as said before, *any* archaeologist would dig in that situation.<sup>8</sup>

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8 Or should the archaeologist have scruples about digging, for the decision of unearthing the urns would prevent him from collecting evidence useful in the psychology field?

What's more, the problem is not just that Asay and Bordner neglect the issue of the intended domains (as a matter of fact, the psychological theories about the effects on archaeologists from destroying their own archaeological sites that they mention concern psychologists and not archaeologists), but that M does not encourage the archaeologist to dig either, while both CE and SR do. Even if Asay and Bordner took into consideration the issue of the intended domains, in fact, the archaeologist might still choose not to dig, which would ensure him that at the end of the day he will have a manifestly adequate theory about Etruscan urns (once again, this is exactly Rosen's point)—and therefore fulfill the according-to-M aim of science.

Were he a scientific realist or a constructive empiricist, on the other hand, this option would not be available to him, since in order to test the (potential) empirical adequacy of his theory (again, *in its intended domain*) he would have no other choice than to dig and unearth the urns<sup>9</sup>. In other words, according to SR or CE, the archaeologist *should* dig—which is exactly what invariably happens (in normal conditions).

It is important to note, also, that this has nothing to do with these views on science being normative—which they are not.<sup>10</sup> Yet, since *any* archaeologist, in a case like the one described in Rosen's thought experiment, would dig, then there would be nothing wrong in trying to give a "lawlike description" of the situation. Think of the motion under gravity. As we all have learned at school (and can easily see), if we lift a body and then let it free, it falls towards the earth and its motion presents always the same features. Newton's famous law of gravity perfectly describes and explains free fall motion (together with other phenomena). Does it mean that physics is normative? Did Newton dare telling Mother Nature what she should do? Of course not! *Mutatis mutandis*, something similar can be said

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9 Since we are dealing only with observables here (Etruscan urns), empirical adequacy and truth amount to the same thing (see van Fraassen, 1980, p. 72).

10 One might rightly observe that they stand somewhere halfway between pure descriptive positions and normative ones. But sure, neither the scientific realists nor van Fraassen have the pretension of telling the scientists what to do!

about both SR and CE, when they “prescribe” that an archaeologist in a “Rosen-like” situation should dig and unearth the urns that are still underground.<sup>11</sup>

Asay and Bordner consider their fifth objection as the most substantial. According to them, Rosen’s view is that “M entails that we should do everything in our power to reduce the number of observations, so as to minimize the chance of science not reaching its aim” (p. 156). It seems to me that Rosen thinks in terms of permission rather than obligation instead—and that this suffices for him to refute M. As a matter of fact, M is compatible with an ostrich-like ducking at crucial moments such as the one the archaeologist is experiencing. More than this, it would provide a rationale for behaviors that any non-supporter of M would most certainly classify as utterly non-scientific, such as destroying the archaeological site. Last, but not least, from the point of view of a theorist who adopts M, normal scientific practice appears irrational (for performing a new experiment could put the adequacy of a theory at risk, for example; if one has an adequate theory at hand, in fact, she then has already fulfilled the according-to-M aim of science and not only collecting new evidence would bring no benefits at all, but could put the theory’s adequacy in jeopardy as well). M is then clearly at odds with science and its practice.

Asay and Bordner, on the contrary, interpret Rosen’s case as implying that M entails normative claims and that this is a consequence of what manifestationalists holds as being the aim of science. This gives them the opportunity of pondering on this concept, which is crucial for their defense of M. However, despite mentioning van Fraassen famous example of the chess player, which is meant to draw a distinction between the aim of science intended as a collective enterprise and the personal goals of the scientists engaged in its practice, the two authors seem to make some confusion among the personal and the collective plans.<sup>12</sup>

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11 It might be objected here that making an analogy between scientific investigation and philosophical investigation is problematic. But I take it that, in this particular case, it works. *Mutatis mutandis*, of course.

12 “The aim of science is of course not to be identified with individual scientists’ motives. The aim of the game of chess is to checkmate your opponent; but the motive for playing may be fame, gold, and glory. What the aim is determines what counts as success in the enterprise as such; and this aim may be pursued for any number of reasons” (van Fraassen, 1980, p. 8).

Of course specifying the aim of science does not provide a motivating reason for someone to engage in science (Asay & Bordner, 2015, p. 157), but once one has become a scientist, whatever her personal reason for embarking in this career, she then acts according to the aim of science. And this might suffice to explain her as-a-scientist actions. Asay and Bordner, conversely, think that if we ask why a physician gives a patient a certain drug, the answer cannot be “*because medicine aims at a patient’s health*” (p. 156; emphasis in the original).<sup>13</sup> I disagree and consider this an appropriate answer instead.

Suppose a child watches a chess game between a layman and Garry Kasparov. She asks her father why the Russian chess grandmaster is constantly eating his opponent’s pieces. Wouldn’t “because the aim of the game of chess is to check-mate your opponent” be an appropriate answer? I take it that it is. Whatever reason Kasparov has to be playing that game, we don’t need to know and does not really matter. This is even truer when it gets to science, for it is a collective enterprise, in which a very large number of individuals are engaged—and many were in the past. All of them perform observations all the time. Why is that so? One might answer that Isaac actually wanted to impress his parents, Nikola in fact was seeking glory for his country, Marie wanted to promote a positive image of women in science, etc. I do not claim that there is not something right to these answers. But does it mean that if one wants to know why scientists do invariably perform observations while “doing their job”, the answer is a set of one million different answers? Of course not. Evoking the aim of science, instead, would be a perfectly legitimate and satisfactory way of giving one single and definite answer to that question.<sup>14</sup>

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13 According to the two authors, evoking the aim of medicine cannot provide an answer because “it is our *desire* for these goods that ultimately explains why we do these things” (p. 156). But if this is right, then why stopping there? Why not explaining the reason(s) behind such desire? And so on. Perhaps Asay and Bordner forgot that, according to van Fraassen (1980, p. 23), the rejection to an unlimited demand for explanation is one of the characteristic features of empiricism. Isn’t M supposed to be a stricter-than-CE version of it?

14 One might want to elaborate on this point and distinguish among “first order” answers (when they concern the scientific community as a whole) and “second order” ones (when they concern the individuals that compose the scientific community). This might help identifying a principled stopping point in order to avoid an unlimited demand for explanation.

In sum, while it is correct that it is her desire to know that explains one being a scientist, not the other way round, I consider inaccurate Asay and Bordner's contention that "whether the archaeologist should dig or not depends on *whether she wants to know if there is an urn at L*, not on her role as a scientist or on her acceptance of M (or CE or SR) as the correct description of the aim of science" (2015, p. 157; emphasis in the original)—and see no real connection between the two claims: think of an archaeologists who digs because it is his job to, but who has completely lost interest in it after his fiancée told him that he does not look like Indiana Jones at all. But I guess we have considered enough examples at this point and need not add any other.

Asay and Bordner, however, elaborate on their fifth objection (it is, in fact, the most substantial) and go on putting on a par M, CE and SR with regard to "observation-minimizing normative conclusions". According to them, if such normative conclusions follow from M, then the same should be true of CE and SR as well. Leaving aside the fact that, to me, Rosen seems to think in terms of permission rather than obligation (which means that I might agree with the two authors when they deny that any normative conclusions follow from M), I am not sure that Rosen-style cases can easily be constructed against either CE or SR, as Asay and Bordner claim instead. The examples they put forward, for instance, are incorrect.

The two authors maintain that "we *could* organize a selective breeding program to progressively diminish the perceptual abilities of human beings, changing our community of observers into a species of deaf and blind beings with no senses of taste or smell" (p. 157). This, according to them, would favor CE, for what counts as empirical adequacy is a function of the sensory limitations of the human community (van Fraassen, 1980, p. 17). What Asay and Bordner neglect, however, is that even if our species will turn into a community of insensible people over the coming years, this is not the case at the present moment. As a consequence, what is observable now will maintain this feature even if in the future no one will be able to observe it.

This term "observable" is very much like such other common words as "portable" and "fragile". They are, so to speak, anthropocentric terms, for they refer to our limitations. They are not person-centric, however; laptop computers are portable and wine glasses fragile, even though some people are too weak to carry or break either (van Fraassen, 1992, pp. 18-19).



The second objection against CE goes on the opposite direction: “since van Fraassen was considering the impact of the *addition* of more perceptive beings to our community of observers, we might at the very least avoid steps that make our meeting such beings more likely” (Asay & Bordner, 2015, p. 157). Something even stronger was stated by André Kukla in 1996: “Clearly, what van Fraassen has to do in order to avoid the collapse of his antirealism is not allow any flexibility in the composition of the epistemic community. If you’re in, you’re in, and if you’re out, you’re going to stay out no matter what happens” (Kukla, 1996, p. 208).<sup>15</sup>

I take it that van Fraassen answered satisfactorily in his “The Day of the Dolphins. Puzzling over Epistemic Partnership” (2005).<sup>16</sup> How do we know, for example, that there are more perceptive beings? A constructive empiricist does not believe that, say, dogs hear ultrasounds.<sup>17</sup> She might consider them reliable indicators of ultrasounds, but that’s it (van Fraassen, 1985, p. 257). Any argument to the effect that if we admit dogs among our epistemic community then the range of the observable phenomena will increase rests upon a premise that a constructive empiricist can merely accept, but not believe: that dogs can observe entities that are unobservable to humans. Then again, says van Fraassen, “there is no great threat in the reflection that in the future we shall give up some beliefs we hold now and replace them by contrary beliefs” (2005, p. 116).

As a matter of fact, suppose one day we decide to admit dogs among “us”. Before that day, we were agnostic about the existence of the unobservable (to us) entity *Y*. As a consequence, we did not hold the belief that dogs could observe *Y*. All we can say, before union, is that after “they” become part of “us”,

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15 And in the next page: “you’ll never encounter any reasons that rationally compel you to enlarge your epistemic community. So there’s really no question that antirealists have to be inflexible about who gets into the epistemic club. Speaking technically, this conclusion spells trouble for antirealism” (Kukla, 1996, p. 209).

16 Not forgetting his “Empiricism in the Philosophy of Science” (1985), where he addressed similar objections by Grover Maxwell (1962), Paul Churchland (1982, 1985) and Jeff Foss (1984).

17 According to F. A. Muller, “dogs hear ultrasounds” is an unempirical proposition, for it is not about actual and observable entities only. Therefore, a constructive empiricist keeps a neutral attitude with regard to it (Muller, 2004, pp. 639-642).

our common opinion will initially be vague, varying between the two extremes “*Y* is unobservable” and “*Y* is observable”. Afterward, as a result of a common epistemic policy, the opinion should converge on the position that one of the two groups—humans or dogs—held before union, according to the available evidence. But getting to a conclusion *now* would be incoherent and irrational (van Fraassen, 2005, p. 130). No danger for CE, then.

Asay and Bordner have something to say about SR too, however: “supposing certain quantum phenomena are literally indeterminate until measured, it would follow from a similar understanding of SR that such measurements should not be done at all” (2015, p. 157). To do so, in fact, according to them, would mean creating new facts for which our theories should then account, while there would be no facts at all “if we chose the ‘safe’ route of avoiding such messy, work-increasing experiments” (p. 158). But this does not seem correct either. The simple fact that, admittedly, “certain quantum phenomena are literally indeterminate until measured” (p. 157) is something that a scientific realist would feel compelled to prove, if her aim is to get to a true theory about the world surrounding her. Not to mention that, most probably, such theory will also make predictions about the results of measuring certain quantum phenomena (in terms of probability, at least), which is also something that scientists (especially the ones having sympathy for SR) will most certainly be willing to test.

In sum, it does not seem to be easy at all to produce Rosen-style cases against either CE or SR. And even if it is, Asay and Bordner have not succeeded in constructing any. Or so I argue.

It is my contention that in this section Asay and Bordner’s objections to Rosen’s case against manifestationalism have been addressed. The consequence is that unless other, more consistent criticisms are put forward, Rosen got it right: M can even be a version of empiricism more compatible with its letter, though it fails to make sense of scientific practice. But it seems to me that, even in case I got it all wrong and Asay and Bordner’s objections are actually sound, other Rosen-like cases against M could easily be constructed—which will not be done here, however.

Moreover, the two authors claim that “views like M, CE, and SR do not entail conclusions about which observations should or shouldn’t be carried out” (p. 158). I do not agree and hope I have made it clear that there is a sense in

which one can actually conclude that, even if such views are not (exactly) normative, they *do* entail conclusions about what scientists should do. Along the same lines, Asay and Bordner also maintain that “it is not the aim of *science* that directs us toward the observations we are to make; it is *our* personal aims that inform our observational choices” (p. 158). I disagree and have explained why above. What is more, I take it that their remark rests upon an interpretation of what the aim of science is that may be useful to their purpose, but perhaps is not adequate. The next section will clarify this last remark.

#### 4. What the aim of science is not

“IS IT POSSIBLE TO COME UP with one right view on the aim of science?,” asks Maarten van Dyck (2007, p. 28, fn. 10). I have already claimed that the meaning Asay and Bordner assign to *aim of science* might be useful to their purpose, but perhaps is not adequate. Let us see how they interpret the locution.

The aim of science is to be thought of from a long view about what science aims to have accomplished when it is completed. Suppose that scientific inquiry continues right up to the heat death of the universe. How might we describe the aim of science, looking back at its history from its conclusion? Considered this way, the manifestationalist holds that the aim of science is—and always has been—to produce theories that are true of *all* the phenomena that end up being observed, and that accepting a theory requires only—and has always required only—believing that a theory is true of all those observed phenomena. The manifestationalist holds that believing further in the *truth* or *empirical adequacy* of a theory with respect to the unobservable or the merely unobserved is (and always has been) supererogatory (Asay & Bordner, 2015, p. 150).

Even admitting that the aim of science is tantamount to what science aims to have accomplished when it is completed, as Asay and Bordner maintain, is it looking back at its history (once it is “completed”) the appropriate way of establishing what its goal was? Suppose I replace the layman from the previous example and play chess against Kasparov for the rest of my life; if once my life is completed one looks back at my history she will probably conclude that my aim in playing against the Russian chess master was losing all the games, which of course was not.

Worse, suppose that, in a parallel universe, planet Earth\* is completely identical to ours, so that the course of events is exactly the same, with the only exception that on Earth\* men are excluded from the scientific community, for they are considered too dumb. When the heat death of the universe comes (suppose it comes for the parallel universe too), the planet has exactly the same number of inhabitants as our Earth. The number of observations performed through history on the two planets is highly different, however, being extremely lower on Earth\*—for men’s “observations” do not count; such as dogs’ “observations” do not count on our Earth. If one looks back at the history of science on Earth\*, is she allowed to conclude that its aim was to get to a theory that covered exactly (and perhaps only) *those* observations? Suppose scientists both on (our) Earth and on Earth\* actually got to a *final* comprehensively manifestly adequate theory. The two theories are of course identical with regard to the part that covers the observations performed by women. Suppose they differ on the rest, however. Can one maintain that science has fulfilled its aim on both planets anyway?

According to Asay and Bordner, “the correct account of the aim of science needs not only to be epistemologically sound, but also able to make sense of the practice of actual science” (p. 153). However, they consider that “thinking in terms of the ‘aim’ of science can easily mislead. ‘Aim’ is ambiguous between ‘goal’ or ‘end product’ on the one hand, and the intention to achieve a goal on the other” (p. 156), which allows them to focus on the end-product side of the question and drive home their point. Here is another passage from their paper:

One might argue that the aim of an enterprise is closely related to, if not constituted by, what an ideal community of practitioners of that enterprise would accomplish under ideal circumstances. So imagine an ideal scientific community. This is a set of people like us, who share our sensory abilities and limitations, as well as our place and time in the universe. But they are always in “scientist mode”, incessantly collecting and recording *all* of their observations. Regardless of what else they do in their scientific inquiries (and regardless of which phenomena they choose to investigate), they never miss recording an observation; all observed phenomena make it into their theories. Their data recording is exhaustive and never flawed, and the summation of their findings is, by definition, comprehensively manifestly adequate. The end product of this ideal community’s labors is guaranteed to be manifestly adequate, though

not necessarily empirically adequate or true. (...) If the aim of an enterprise is something that is in principle attainable by the ideal practitioners of that enterprise under ideal circumstances, then *only* manifestationalism has a potential claim to having captured the aim of science. So perhaps manifestationalism has a fighting chance after all in the philosophy of science (p. 160).

Then again, this would mean that, even if the ideal scientific community collected only half the observations, the aim of science would be accomplished anyway. And this would be true whichever amount of phenomena had been recorded. Is this acceptable? Recall that “manifestationalism holds that science will have accomplished its aim when and only when we are in possession of theories that are true of all the actually observed phenomena, past, present, or future” (Asay & Bordner, 2015, p. 159).

According to an American astrologer, the Armageddon will take place in 2020. Suppose we somehow get to know that she is right. In order to fulfill what M takes the aim of science to be, we might then simply sit down, record everything that has been observed so far and keep doing this until the end of this year, so that all observed phenomena will make it into one theory (or more than one)—as if the whole world turned into a big accounting firm. The end product of this activity is guaranteed to be manifestly adequate; but should we call this *science*?

Perhaps it would be better to think of the aim of science as a concept that guide scientists’ actions and plans, something like a North Star or even an à-la-Aristotle final cause (since it can be evoked to explain why a physician gives a patient a certain drug). Van Fraassen merely considers that “what the aim is determines what counts as success in the enterprise as such” (1980, p. 8), but he is perfectly aware that even if we get to an empirically adequate theory, the most that we will ever be able to safely say is that it has not proved inadequate so far, for there will always be unobserved phenomena that nonetheless the theory accounts for—still, one might believe that a certain theory is empirically adequate; which, according to CE, implies a series of consequences. But, of course, this requires a leap of faith.<sup>18</sup>

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18 As said before, according to M, say Asay and Bordner, “accepting a theory requires only—and has always required only—believing that a theory is true of all [the] observed phenomena. The manifestationalist holds that believing further in the *truth* or *empirical adequacy* of a theory with respect

One might like to compare the concept of *aim* (of any activity) with the mathematical notion of *limit*: when a function moves towards a certain limit, in some cases it actually reaches it; other times, however, it does not, for the limit presents a certain value while the function ends up reaching another. The according-to-CE aim of science might be thought of as being the limit of this human enterprise: getting to an empirically adequate theory (or more than one). The function (scientific activity) moves towards it. At the heat end of universe, however, the most that we can know is that we have reached a manifestly adequate theory. Still, getting to this was not the aim—or else, science could have stopped before (or “moved slower”).

### 5. A modest defense of constructive empiricism

THE AIM OF THIS WORK is definitely not to defend CE, which has probably been the most prominent empiricist/anti-realist view of science for the last decades and does not need to be defended at all—not by this author, at least. Still, since Asay and Bordner do not seem to make justice of it, I felt a couple of words are in order.

According to van Fraassen, there is nothing to be actually gained in believing, as scientific realists do, that a certain theory is true rather than “only” empirically adequate. “Since there is no possible evidence for the former that isn’t also evidence for the latter, there is no possible evidence that could ever tell between empirically identical theories” (Asay & Bordner, 2015, p. 152). Therefore, while it is not irrational to believe that a certain theory is true, the extra commitment

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to the unobservable or the merely unobserved is (and always has been) supererogatory” (2015, p. 150). I have already stated that, however, believing that a theory is manifestly adequate involves making a leap of faith too, for not only does M take into account past and present phenomena (i.e., the so-far-observed ones), but also observations that will be performed in the future—which then are *unobserved* (so far, at least) phenomena. Curiously enough, in the very same paper the two authors claim that “Manifestationalism permits an individual’s belief in the unobserved; it simply stresses that such belief is supererogatory from the perspective of science” (p. 159). Of course, by ‘the unobserved’ they here mean phenomena that will never be observed, but I still take this claim as emblematic; perhaps the authors have not reflected enough on the fact that believing *now* (we cannot certainly wait for the heat death of the universe!) that a theory is manifestly adequate means believing (also) in *unobserved* (so far) phenomena.

in favor of the unobservable entities postulated by it can never find any empirical support and constructive empiricists consider it supererogatory.

Asay and Bordner think there is a perfect analogy if one compares the SR-CE relation with the CE-M one.

Simply replace “true” with “empirically adequate” and “empirically adequate” with “comprehensively manifestly adequate” and we have an exactly parallel argument that any belief in unobserved observables is just as extraneous and supererogatory as any belief in unobservables. By definition, the only phenomena that could serve to distinguish between competing manifestly adequate theories are ones that will never be observed. The very empiricist argument used to motivate CE is, it turns out, better suited to motivate M (p. 152).

Of course, one might go on and suggest replacing “comprehensively manifestly adequate” with “so-far manifestly adequate” and perhaps have a better-suited-to-empiricism argument. But even then, Asay and Bordner’s claim that believing in unobserved observables (such as the flower grown on an extrasolar planet in 1492 mentioned in a previous example)<sup>19</sup> is exactly parallel to believing in unobservables would certainly be rejected by van Fraassen. See the following passage from *The Scientific Image*:

With this new picture of theories in mind, we can distinguish between two epistemic attitudes we can take up toward a theory. We can assert it to be true (i.e. to have a model which is a faithful replica, in all detail, of our world), and call for belief; or we can simply assert its empirical adequacy, calling for acceptance as such. In either case we stick our necks out: empirical adequacy goes far beyond what we can know at any given time. (All the results of measurement are not in; they will never all be in; and in any case, we won’t measure everything that can be measured.) Nevertheless there is a difference: the assertion of empirical adequacy is a great deal weaker than the

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19 What about not-yet-observed observables then? If “any belief in unobserved observables is just as extraneous and supererogatory as any belief in unobservables” (Asay & Bordner, 2015, p. 152), isn’t believing in an observable event that will happen and allegedly be witnessed in 2492 just as extraneous and supererogatory? Or worse, how does one even know that it will be observed?

assertion of truth, and the restraint to acceptance delivers us from metaphysics (van Fraassen, 1980, pp. 68-69).

True, the assertion of manifest adequacy is, in turn, a great deal weaker than the assertion of empirical adequacy, but the parallel stops here, for there is an important difference between the two cases, which van Fraassen quite rightly stresses—as the passage above shows—but Asay and Bordner neglect. Indeed, in the case highlighted in *The Scientific Image*, van Fraassen can legitimately claim that asserting the empirical adequacy of a theory delivers us from metaphysics. Limiting the belief to that part of the theory that represents the observable phenomena, in fact, means maintaining an agnostic attitude with regard to electrons, quarks, ribosomes and the like. The unobservables are completely different kinds of entities, if compared with the observables. Not only believing in their existence is supererogatory, according to the constructive empiricist, but implies carting a metaphysical (quite heavy) baggage as well.

Limiting the belief to that part of the theory that represents the observed phenomena (past, present, or future), on the other hand, as M recommends, means reducing the number of tokens in which one believes, but not of types.<sup>20</sup> Which means that there is not something like a deliverance from metaphysics if one chooses to adopt M instead of CE. This is a difference that makes all the difference, as they say. The parallel Asay and Bordner draw does not work.

Another allegation regarding CE has already been addressed in the section where I tried to rebut the two authors' criticisms to Rosen's argument against M. Asay and Bordner suggested that we *could* organize a selective breeding program in order to turn our community of observers into a species of insensible

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20 One might object here that believing in the empirical adequacy of a theory implies believing in the (past) existence of dinosaurs and other species of animals that no man has ever seen and that, therefore, limiting one's belief to the manifest adequacy of the theory only *does* actually deliver us from metaphysics as well. But I think it is better keeping a "nonstickler" attitude here, otherwise one can reply that comprehensively manifestly adequate theories certainly describe other species of animals that no man has ever seen so far but that will be "discovered" in the future—it happens all the time, actually. I say these are neglectable exceptions, if compared to the whole universe of entities that scientific realists believe in. "Do the concepts of the Trinity, the soul, haecceity, universals, prima matter, and potentiality baffle you? They pale beside the unimaginable otherness of closed space-times, event-horizons, EPR correlations, and bootstrap models" (van Fraassen, 1985, p. 258).



beings (2015, p. 157). This would allegedly favor CE, for “what counts as an observable phenomenon is a function of what the epistemic community is” (van Fraassen, 1980, p. 19). However, Warren Bourgeois brilliantly answered to similar objections more than three decades ago: “Of course different members of a given community will have different capacities to observe, but *observability for one is observability for all*” (Bourgeois, 1987, p. 307; my emphasis). Nothing would change, in sum, in the set of the observables, in case Asay and Bordner’s selective breeding program were executed.

The issue is more general and has to do with what van Fraassen takes the limits of *observability* to be. As he explains in “Empiricism in the Philosophy of Science” (1985), observability presents *special limits*, that are due to the physiology of the human species (our epistemic community)<sup>21</sup>, and *general limits*, which, in turn, do not depend on the human physiology: these are spatial and temporal limits determined by Einstein’s relativity theory. Interplanetary travels of human crews certainly fit within these limits and so there is no problem in imagining contexts (models) in which astronauts are in the vicinity of Jupiter—as van Fraassen does in *The Scientific Image* (p. 16).

Asay and Bordner perhaps are unaware of this, for they claim that “constructive empiricists maintain that science aims to capture the truth about the observable entities in those too-distant regions of the universe [outside our species’ light cone], even though it’s impossible for us to make contact with them” (2015, p. 160). But this is wrong. Anything outside our light cone is beyond what van Fraassen considers the general limits of observability. It follows, trivially, that if there are entities outside our light cone, they are *unobservable* (even in case they are not microscopic). Constructive empiricists *do not* maintain that science aims to capture the truth about entities in those regions of the universe. In “The Day of the Dolphins” van Fraassen explained that, according to his view on science and its aim, “what the sciences say about the observable parts of the world is true, the rest need not matter” (2005, p. 111); entities outside the human species’ light cone are part of “the rest”.

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21 It is because of them that *observable* is, as the originator of CE maintains, an *indexical* term (short for *observable-to-us*).

## 6. What manifestationalism?

IN THE LAST SECTION OF THEIR PAPER, called “Manifestationalism defended”, Asay and Bordner address an hypothetical criticism to M—as if it appeared to present science as nothing more than a perpetually self-congratulating enterprise—, reiterating that “manifest adequacy is not simply adequacy to the observed phenomena *so far* (a goal that already no scientific theory can claim to have achieved), but rather to all observed phenomena *all told*, whatever they turn out to be” (p. 159). They think, in fact, that “critics of manifestationalism have seemed to treat manifest adequacy in its weakest formulation, namely, that a theory is manifestly adequate just in case it is true to all of the phenomena observed *so far*” (p. 149; emphasis in the original).

True, in his 2004 paper James Ladyman mentions Monton and van Fraassen’s reply to Marc Alspector-Kelly about the possibility that a constructive empiricist embraces a stricter version of empiricism—namely, manifestationalism—and have the latter say that “if constructive empiricists were really motivated by epistemic modesty, that is unwillingness to go beyond the deliverances of experience, then they would stop short of belief in empirical adequacy and believe only what theories say about the actually and indeed already observed” (Ladyman, 2004, p. 757). But, as a matter of fact, what Alspector-Kelly (2001) actually wrote was that one “might slide all the way down the slippery slope and advocate the weakest assertion consistent with the observed evidence, namely, the observed evidence itself, and refuse to endorse anything beyond what has been or *will be* in front of our eyes”, adding that “Peter Railton (1990) calls this position ‘manifestationalism’” (p. 416; my emphasis).

Then perhaps Ladyman has not been fair to manifestationalism, but it does not seem right to claim that critics of this position treat manifest adequacy as being solely about the phenomena observed so far. More importantly, this is not how Railton defined the so-called “manifest theory” and Rosen considered manifestationalism when he put forward the case of the archaeologist. As explained in previous sections of this work, in fact, both Railton and Rosen *do* take into account, in their formulation of manifestationalism, the so-far-unobserved phenomena that will actually be observed in the future.

Again, were this not the case, addressing a case against manifestationalism while having in mind another formulation of it would be tantamount to claiming that Garry Kasparov is actually not such a great player after defeating a six-years child with that name in a chess game.

## 7. Conclusion

IN THIS PAPER I have shown that Asay and Bordner are right to consider Rosen's thought experiment as a key argument against M and that this is true even if the original formulation of manifestationalism, due to Railton, is slightly different from theirs. If we stick to the archaeologist case, in fact, there is no practical difference between the two versions: if it works for Railton's formulation, then it works for M too—and vice versa.

The two authors, however, take Rosen's case to be against a stricter version of manifestationalism, that allegedly recommends limiting belief to that part of an accepted theory that describes what has been observed *so far*. Since this is not the case, however, as has been reiterated in the last section, part of their criticisms to what they take to be the primary argument against M is simply off target. In a section devoted especially to their objections to Rosen's argument, I have actually argued that none of their criticisms really works.

The consequence is that, unless Asay and Bordner put forward more effective objections—in their paper they claim that there are *at least five* problems with Rosen's argument (p. 154)—, the case against M stands. As a result, van Fraassen's CE—which I have very modestly defended in this paper against some wrong allegations put forward by Asay and Bordner—can still be considered the best compromise so far presented between strict empiricism and the acknowledgment of the rationality of science.

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