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INVITED PAPER

Defending Scientific Realism Without Relying on Inference to the Best Explanation

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Abstract Explanationist strategies for defending epistemological scientific realism (ESR) make heavy use of a particular version of inference to the best explanation known as the no-miracle argument. I consider ESR to be a genuinely philosophical—non-naturalistic—thesis which contends that there are strong arguments to believe in some non-observational claims made by scientific theories that are partially observationally correct. In this paper, I examine the grounds of the strength of these arguments from what I call a *contemplative* perspective which focuses on the end products, *i.e.* theories, of the scientific activity as opposed to the pragmatist view which considers science to be primarily an activity. I briefly rehearse the main difficulties of the no-miracle argument and of inference to the best explanation in general. I argue that a convincing defence of ESR should be based on the empirically ascertained reality of causal connections between theoretical entities which possess properties that are in principle observable (OP properties) and the results of measurements or observations. The knowledge of those causal connections may well deliver an—even the best—explanation of the appearances. But belief in the existence of some unobservable entities is mainly justified by their empirically attested causal role, not on their possible explanatory function.

Keywords Causal explanation · Explanation · Inference to the best explanation · No-miracle argument · Scientific realism

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1 What are We Talking About?

As van Fraassen recently reminded us, the debate on scientific realism (SR) is not about the existence of unobservable entities such as electrons, atoms, genes…

> Do electrons exist? Are atoms real? These are not philosophical questions. Whether electrons exist is no more a philosophical question than whether Norwegians exist, or witches, or immaterial intelligences. Questions of existence are questions about matters of brute fact, if any are, and philosophy is no arbiter of fact. (van Fraassen 2017, p. 95)

True, before espousing belief in the existence of electrons, it is advisable to consult scientists rather than philosophers.[1] But as far as scientific realism is concerned, the philosophical questions hinge on the cogency of the reasons or arguments which justify (or not) belief in the reality of unobservable entities posited by scientific theories and the truth of statements about them. This is an epistemological, and therefore philosophical, question which deserves to be thoroughly examined. Why? Simply because it bears upon the degree of confidence we are entitled to confer to our best scientific theories, in general. Even if the arguments in favour of the existence of electrons are obviously distinct in their contents from the arguments developed to establish the existence of mitochondria, they have some common traits that make them forceful and therefore convincing. The philosophical task consists in elucidating what makes some purportedly truth-conducive scientific arguments cogent and others not.

Undoubtedly, such investigation will shed light on what science is. Science is supposed to be knowledge. How far does it reach? Is justified belief in scientific theories confined to what is (directly, immediately, straightforwardly…) observable or can its limits be extended beyond what is accessible to our unaided perceptual abilities? Determining the limits of scientific beliefs surely is a philosophical endeavour, which echoes major Kantian concerns.

Since disagreements among philosophers are not uncommon, a wide variety of definitions of scientific realism have been proposed. Many philosophers have stressed that science is an *activity*, and it certainly is. To characterize an activity, we must describe its *aim* and the *commitments* embraced by its practitioners. As an activity, scientific realism can be portrayed thus:

> *Science aims at giving 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*. (van Fraassen 1980, p. 8)

This famous passage in *The Scientific Image* is immediately followed by the laconic sentence: "This is the correct statement of scientific realism".

The originality and challenging tenor of van Fraassen's statement triggered considerable gloss in the philosophical literature. Since van Fraassen's formulation of scientific realism is offered as a statement, we might rightly ask: what are the grounds for its correctness or truth? Science is practiced by human agents, namely

[1] See also Ghins (2002).

scientists. Do they seek to discover truths about the world? When they accept a theory, are they committed to belief in their truth? These are factual—not philosophical—questions. To answer them, we must conduct polls among scientists and ask them what aims they pursue and what beliefs they entertain as practitioners of a scientific discipline when they accept a theory. If all (or most) participants in the poll reply "yes" to both questions, we could assert that science indeed aims at producing true theories and that their practicioners do believe in the truth of the theories they accept. On the other hand, if they confess that they just want to save appearances or data,[2] and that their acceptance of a theory is limited to a practical commitment to use it in their scientific activity, then scientific realism has been falsified.

This is certainly *not* what van Fraassen has in mind. Philosophical positions are not to be argued on the basis of polls or, generally, facts. van Fraassen's aim is to characterize a philosophical position, an attitude or a practical "stance" (van Fraassen 2002). The "statement" of SR in *The Scientific Image* is not intended to be a correct or true statement about matters of fact but a possible answer to the central question: *what is science?* Or, more precisely: *what is science, primarily construed as an activity?* Neither is this statement of SR intended to be a definition like those of "circle" or "bachelor". For van Fraassen, scientific realism is not a philosophical doctrine, but a characterization of scientific activity in terms of normative (not descriptive) distinctive aims and commitments of its practitioners.

The purported correctness of such formulation is to be understood in contrast with the alternative anti-realist attitude favoured by van Fraassen, namely "constructive empiricism":

> *Science aims to give us theories which are empirically adequate*[3]; *and acceptance of a theory involves as belief only that it is empirically adequate.* This is the statement of the anti-realist position I advocate; I shall call it *constructive empiricism*. (van Fraassen 1980, p. 12)

Bas van Fraassen is wary of falling into the trap of caricaturing the attitude of his opponent as an unsustainable position. The scientific realist shouldn't be depicted as a straw man that would be easily defeated. van Fraassen proposes a "minimal" formulation of scientific realism, which is weaker and thus harder to criticize than alternative—more restrictive—formulations. But at the same time, he carefully preserves the core contention of scientific realism, namely the centrality of the role of truth. "Correct" here means "minimal". More restrictive formulations would be

[2] Following van Fraassen, I distinguish appearances, measurements and data from phenomena. The latter are directly observable, whereas appearances are what phenomena "look like in given measurement or observation set-ups" (van Fraassen 2008, p. 284).

[3] "(…) the theory is empirically adequate if it has some model such that all appearances are isomorphic to empirical substructures of that model" (van Fraassen 1980, p. 64). In the model-theoretic approach, a theory is empirically adequate if all appearances fit in some submodel of the theory or, in syntactic language, if all its predictive statements about measurement results are true. For van Fraassen, theories are in the first place classes of models (which occupy "centre stage"). But they also contain statements. In this paper, theories will be mainly viewed as sets of statements. Such point of view will not impair the examination of the arguments in favour of scientific realism.

incorrect to the extent that they would be indefensible—or at least much harder to defend—against the attacks launched by the empiricists.

Why should one preferentially adopt the attitude of the constructive empiricist? To support his position, van Fraassen implements the following strategy. First, he shows that the arguments typically used by scientific realists, such as Inference to the Best Explanation (IBE), also called "abduction", are not rationally compelling. They allow for rational disbelief or suspension of belief about the existence unobservable entities and their properties. Second, given that belief in empirical adequacy is weaker than belief in truths about unobservable entities, it is less risky and therefore *pragmatically* more rational to adopt the constructive empiricist's attitude. Even if it is not irrational to embrace scientific realism, since "(…) what is rational to believe includes anything that one is not rationally compelled to disbelieve" (van Fraassen 1989, pp. 171–172), as a matter of practical prudence, it is more reasonable to be a constructive empiricist. Accordingly, science is an activity dedicated at saving appearances and acceptance of a theory is limited to belief in truths about appearances. But it also involves a commitment to conduct research within the framework of the theory (van Fraassen 2017, p. 100).

Far from denying of course that science is also an activity, I will propose a significantly different approach to the main question—*what is science?*—which I will call *contemplative* as opposed to *pragmatic*. For van Fraassen, to answer this central question we must identify the aim of scientific inquiry and specify the basic criterion for attaining this aim, that is, the basic criterion of success (van Fraassen 2017, p. 98). As a matter of fact, predictively successful scientific theories exist today and are, also as a matter of fact, practically accepted by a vast majority of scientists—and non-scientists as well—for a wide variety of reasons.

The philosophical question I want to address is this: *Is it more rational to believe than not to believe in the truth of some scientific statements about unobservable entities, which belong to a theory which is at least partially observationally and predictively correct?* The epistemological scientific realist gives an affirmative answer to this question. There are *more* reasons to believe than not in selected parts of theories that have been falsified or discarded by scientists today.[4] But then, the challenge to which the scientific realist is confronted consists in specifying the sort of arguments which make belief in selected non-observational parts of a theory more rational than disbelief or agnosticism about them.

Pondering the strengths and weaknesses of arguments advocated to sustain scientific realism is an epistemological task. Such approach is contemplative, since it focuses on the end products of the scientific activity, namely theories, and the justification of beliefs in their partial truth, while leaving (at least provisionally) aside the aims and the commitments involved in scientific practice. Within such contemplative approach, the main concern is normative, not descriptive, and thus genuinely philosophical. It has to do with the justification of beliefs. And belief in a statement implies belief in its truth. Moreover, belief is a sufficient (but not

[4] It is useful to distinguish between theories that have been partially falsified, such as classical mechanics and optics (which are still taught, and rightly so) and theories that have been discarded such as Ptolemaic astronomy and Pristley's chemical theory.

necessary) condition for allegiance to a theory. Belief implies acceptance, rather than the other way around. Beliefs do have practical consequences, but assessing the strength of the arguments adduced if favour of the truth of some beliefs is prior to identifying the criteria of the practical success of scientific activity.

I thus propose the following characterization of epistemological scientific realism (ESR):

> Rather than disbelieving or suspending belief in unobservable entities posited by a partially predictively correct scientific theory and the truth of statements about them, it is more rational to believe in some such entities and the truth of some statements about them.

Such statement of ESR immediately prompts several remarks. First, scientific realism is a genuinely epistemological philosophical thesis: it bears upon the reasons to believe (while allowing the possibility of error) in some scientific assertions about unobservable entities. As a statement, it could be true or false. It might be that all the arguments adduced so far by scientific realists are all ineffective. I will argue that some realist reasonings are indeed flawed, but I will elaborate an argument for scientific realism that I deem to be cogent. Second, since my approach is contemplative, the notion of practical scientific success in producing true or empirically adequate theories becomes marginal to my main purpose. An activity may succeed or fail, in accordance whether its aim is achieved or not. I'm convinced that scientific activity is successful at producing partially true theories, since I will defend that we have more reasons to believe in their partial truth. Belief in success in this practical sense is seen as a consequence of an epistemological argumentation.

Theories are not activities: they are the outcomes of the scientific activity. Often, a theory is judged successful when it accurately predicts and explains appearances, including novel, unexpected ones. There is no doubt that the vast majority of scientists strive to construct theories which make correct observational predictions and which account for these predictions by means of—typically causal—explanations. This is a fact that I will take for granted. But to avoid confusion, I will call "predictive truth" or "predictive correctness" what is usually referred to as the "predictive success" of theories, since success is more adequately attributed to action rather than contemplation.[5] Observational predictive correctness or truth is akin to what van Fraassen calls "empirical adequacy". Yet, it is more restrictive. Whereas for van Fraassen, an empirically adequate theory is predictively correct for *all*—present *and future*—appearances or data, predictive correctness is limited to data which are available now. A current predictively true theory remains falsifiable. As far as explanation is concerned, instead of the expression "explanatory success" of a theory, I will use "explanatory power".

Avoiding explicit reference to success is not an innocent change in terminology. This move indicates a shift in perspective, from the pragmatic to the contemplative approach. It implies concentrating on the end products of the scientific activity, namely theories, and by evaluating the arguments which support their credentials

[5] "Θεωρια" in ancient Greek means "vision" or "contemplation".

with respect to truth about reality. Unlike the pragmatic perspective, which is diachronic, the contemplative approach is synchronic. The contemplative perspective accords with characterizations of epistemological scientific realism articulated by philosophers such as Stathis Psillos:

> The thrust of this [epistemic] realist thesis is that science can and does attain theoretical truth, where by 'theoretical truth' we understand the truth of what scientific theories say about unobservable entities and processes (…) (Psillos 1999, p. xx)

The challenge for the scientific realist is thus to make his case in favour of the truth of ESR, not by considering the scientific activity and its success in achieving some objectives, but by defending that there are good arguments for belief in their partial truth on the basis of the fact that predictively correct and explanatorily powerful theories are already available.

2 What is the Explanationist Strategy in Favour of Scientific Realism?

Typically, scientific realists have argued that the best, even unique, *explanation* of the predictive correctness of a scientific theory is that it is at least partially true. Given this, it is more reasonable to believe than not in its partial truth. The explanationist strategy in favour of scientific realism is famously exemplified by Hilary Putnam:

> When they argue *for* their position realists typically argue *against* some version of idealism – in our time, this would be positivism or operationalism (…) the typical realist argument against idealism is that it makes the success of science a *miracle* (…) The modern positivist has to leave it without explanation (the realist charges) that 'electron calculi' and 'space-time calculi' and 'DNA calculi' correctly predict observable phenomena if, in reality, there are no electrons, no curved space-time, and no DNA molecules. If there are such things, then a natural explanation of the success of theories is that they are *partially true* accounts of how they behave. (…) But if those objects don't really exist at all, then it is a *miracle* that a theory which speaks of gravitational action at a distance successfully predicts phenomena; it is a *miracle* that a theory which speaks of curved space-time successfully predicts phenomena. (Putnam 1978, p. 18)

Clearly, the existence of some unobservable objects and true statements about them is heralded as the only reasonable explanation of the success of a scientific theory, understood as its observational predictive truth. Yet, the no-miracle argument (NMA), even raised by Putnam to the status of *the* "ultimate'" argument in favour of scientific realism, is plagued by several difficulties which have abundantly been discussed in the literature. I'll briefly rehearse those which are more relevant to my main purpose.

How does the NMA work? To begin, notice that the NMA is a *meta*-argument. A good scientific theory predicts measurements or data and explains those data by means of hypotheses about unobservable entities ("entity" is a more general term than "object")[6] such as electrons etc. *First-level facts* F are described by the correct predictive statements of the theory. A *second-level fact* F* is that a particular theory actually predicts and explains some appearances or data (*i.e.* first-level facts). Partisans of the NMA argue that the only rational explanation of this second-level fact is that parts of the theory are true. Therefore, the theory is partially true.

All agree that the NMA is *not* deductively valid if it is an instance of "inference to the best explanation" (IBE) or "abduction", which can be generally formulated thus:

1. F is a fact
2. Hypothesis H explains F
3. No better explanation of F than H is available

Conclusion: H is true

Is there a way to render the NMA argument deductively valid—and sound—by adding a—true—premiss, in the same manner as some attempted to turn induction valid by adjoining the premiss of the uniformity of nature? In this vein, Alan Musgrave proposes:

1* "*It is reasonable to believe that* the best explanation of any fact is true [added premiss]

1. F is a fact
2. Hypothesis H explains F
3. No available competing hypothesis explains F as well as H does

Therefore, *it is reasonable to believe that* H is true." (Musgrave 2017, p. 80)

The scientific realist who resorts to the NMA must show that it gives *better* reasons to believe than not in the truth of some unobservational statements of a predictively correct theory. We thus have, together with premiss 1* above:

1. The predictive correctness of H is a meta-fact F*
2. The truth of H [the truth of H = H*] explains F*
3. H* is the best available explanation of F*

Therefore, it is reasonable to believe H*, *i.e.* that H is true.

Here, H* is the *only* reasonable explanation of F, thus the best. This is what Putnam contends since the only possible alternative explanation of the predictive correctness of theory T (which contains H) is the occurrence of a miracle, which surely is not a receivable explanation.

However, as van Fraassen acutely remarks, it is not the mere rationality of a belief that is at stake here. The realist must prove more, namely that "it is unreasonable *not* to believe that H is true." (van Fraassen 2017, p. 102) Without asking that much (which would be a daunting challenge), I will only try to show that it is *more* reasonable to believe in the truth of some theoretical statements about unobservable entities than suspending belief about them.

[6] Fields are entities, but not objects in the usual sense.

Before doing that, I wish to draw a distinction between two kinds of explanations, which is akin to the distinction between valid and sound arguments. A sound argument is a valid argument whose premisses are all true. Similarly, a *correct* explanation is a *satisfactory* explanation which is also true. An explanation is satisfactory if it complies with rational requirements. For example, the stability of some motions of the planets is satisfactorily explained by the regular motions of hard crystalline spheres. A more satisfactory explanation has more explanatory power. Failing to distinguish between these two kinds of explanations fosters confusion. This must be conceded by the advocates of IBE, since they acknowledge that there can be several explanations in competition, some better than others, and yet only one—the best, *i.e.* the most satisfactory—is true.

If only one scientific hypothesis (or set of hypotheses) which predicts and explains the data is available, there is more reason to believe in its truth than in hypotheses which give incorrect predictions. But what happens if we have several hypotheses which correctly and perhaps also explain the data we are interested in? This is the issue of theory-choice. (I prefer the expression “hypothesis-choice” since the scientific realism I advocate is selective). As is well-known, van Fraassen has a straightforward pragmatic criterion of choice. Choose to *work* with the hypotheses which have more virtues that you deem to be important. Explanation is one of these virtues. Therefore, accept to work with the hypotheses which give the best explanation of data. Yet, it is more rational to refrain from believing in their truth. Why? Even if we knew all possible explanations and how to rank them (which is of course impossible to accomplish in practice), the main reason is that explanatory power is *not* truth-conducive or truth-tropic. This is the painful itch. It certainly is not irrational to believe in an explanatory predictively correct set of hypotheses (unless they are incompatible with the truth of other hypotheses we already believe in), as we saw. But is it *more* rational to believe that the world is organized in such a way that it is explainable by us? Do our requirements on what counts as a satisfactory explanation have any impact on reality? In Peter Lipton’s phrasing, is the “loveliest” explanation, that is, the one which provides most understanding, also the most likely to be true (Lipton 2004, p. 61). Is there a pre-established harmony between our demands for rationality and the order of the actual world? Such Leibnizian harmony might perhaps be in place (who knows?) but what convincing philosophical argument can we muster in favour of such a view?[7] This is what Lipton calls “Voltaire’s objection”:

> (…) supposing that loveliness is as objective as inference (…) What reason is there to believe that the explanation that would be the loveliest, if it were true, is also the explanation which is the more likely to be true? Why should we believe that we inhabit the loveliest of all possible worlds? (Lipton 2004, p. 70)

[7] Some naturalistic evolutionary theories of knowledge attempt to argue in favour of such harmony, but they remain highly controversial.

This is, I think, the *major objection* against explanationist strategies for scientific realism. There could be explanations which are most satisfactory yet false.[8] What are the grounds of the connection between explanatory power and truth? I here agree with van Fraassen (2005, 2017) that such grounds are found wanting.

It might be that the NMA (without Musgrave's additional premiss), although not deductively valid, could avail itself of the privilege of being a scientific argument and then enjoy the prestige and credibility of a scientific argumentation. The NMA has the same logical form of IBE, which is a form of argumentation widely employed in science and which even has led to correct—sometimes spectacular—novel predictions (but also, not unsurprisingly, to false ones). It has been argued by Levin (1984) and myself (Ghins 2002) that truth cannot perform an explanatory function in the same way as, say, the behaviour of electrons and DNA molecules explain some data. Surely, truth by itself is not a scientific explanatory factor. Therefore, SR is not on the same footing as a scientific hypothesis which would explain some measurement results. Moreover, the meta-claim that some theoretical statements about unobservable entities are true does not in any way improve on the predictive correctness of the theory to which these theoretical statements belong. Thus, scientific realism cannot be endowed with the same status as a scientific hypotheses as Putnam would have it when he says that "(…), realism must itself be an over-arching scientific *hypothesis*." (Putnam 1978, p. 18)[9] Scientific realism is a genuinely philosophical thesis which is not designed to explain facts, whether these are first or second level.

Naturalist philosophers such as Richard Boyd (1981), Stathis Psillos (1999) and Howard Sankey (2008) dispute this claim. They have developed a naturalistic argumentation in favour of the reliability of IBE based on the fact that scientific theories are predictively correct. But the argument is based on the premiss that "The (first-order) instances of explanatory reasoning involve the claim that is *reasonable* [my underlining] to accept that *particular* theories are relevantly approximately true. NMA is, then, based on these instances to defend the more general claim that science *can* deliver *theoretical truth*. NMA is a kind of *meta*-abduction" (Psillos 1999, p. 79). (Notice that Psillos' first-level corresponds to my second-level above. His version of NMA lies at what I would call a third level).[10] Psillos ingeniously attempts to solve the well-known problem of circularity by making the distinction between "rule-circularity" and "premiss-circularity" introduced by Braithwaite (1953). Certainly, the assertion that IBE is reliable does not occur in the premiss of the argumentation, and his vindication of abduction is not viciously circular. However, IBE is used in establishing the claim that it is *reasonable* to accept that particular theories are partially true and also in the meta-abductive move to the more general claim that scientific methodology, which makes use of abduction, is reliable. Such attempt is not immune of difficulties (Douven 2017). But, more importantly

[8] Satisfactory explanations might be true or false. My notion of satisfaction differs from Liptons's loveliness. For him, the loveliest explanation would be the one that provides the most understanding, *if true* (Lipton 2004, p. 59). I wish to sharply separate satisfaction from truth. A false explanation could remain the most satisfactory and provide the most understanding even if proven false.

[9] For a detailed discussion of Putnam's argument, see Ghins (2002). For a rejoinder, see Alai (2012).

[10] For a discussion of various versions of the NMA see Ghins (2002).

for our purpose, Psillos and others must prove not only that it is reasonable to believe in the unobservable parts at the theory which are relevant for its predictive correctness, but also that belief in them is *more* rational than belief suspension.

One might try to rescue IBE by pointing out the effectiveness of IBE within the domain of observable entities such as mice.[11] We have directly observed many mice and seen that they all have grey hair which they lose sometimes, eat cheese, make specific noises etc. By induction, we obtain a few crude causal generalizations: all mice loose grey hair, all eat cheese etc. These are observational generalizations that are *causal* in the minimal sense that we have observed sequences of events in which event *a* (begin of cheese eating) is regularly followed by event *b* (disappearance of some cheese). We also have seen other animals, *e.g.* rats, and they do not share all those properties. Now, we (directly) observe some grey hair on the floor, the disappearance of cheese and specific noises. Perhaps with dismay, we conclude: there is at least one mouse in the cellar, because such facts are best explained by the presence of a mouse. In this case, we do have more reasons to believe in the presence of a mouse, which is tantamount to belief in the empirical adequacy of this hypothesis since mice are observable. Later on, after patient watching, we do (directly) see a mouse, as expected.

Furthermore, we have made this kind of inference a number of times and (more often than not) we have been able to ascertain its effectiveness, namely its ability to lead to correct previsions. Making an inductive inference over many uses of IBE, we conclude that IBE *albeit* not valid is, at least more often than not, a reliable form of inference. IBE allows to infer the existence of observable, but as yet unobserved, entities and make true assertions about them. We then have more reasons to believe in the truth of some conclusions reached by IBE rather suspending belief. In the realm of observable things IBE is truth-conducive.

Given this, it is tempting to extend the reliability of IBE to the unobservable as several scientific realists have contended. In doing so, we would make an inductive reasoning that only pushes further the limits of the effectiveness of IBE. As long as we believe in the reliability of induction (which is not at issue here), wouldn't such a move be perfectly legitimate? The answer is "no", simply because when we are trying to defend belief in the existence of entities such as electrons, which unlike mice we haven't seen before. An immediate response would be that the distinction observable/unobservable is relative to us, and has nothing to do with external existence. The reliability of a *form* of reasoning surely doesn't depend on its contents.

Let us not let ourselves to be misled. We are doing philosophy. The main issue isn't whether mice or electrons exist, but the cogency of the reasons to believe in their existence. And we are now assessing the merits of a form of inference, namely inference to the best explanation. Does the reliability of IBE in the domain of observable entities justify our belief in its reliability in the realm of unobservable entities. And if not, we must show why.

If we look carefully at the mouse argument above, we quickly notice that its strength crucially rests on the truth of statements which describe *causal* processes

[11] See van Fraassen (1980, pp. 19–20). I revisited this example in Ghins (2017, p. 126).

(that is, causal sequences of events; an event is the occurrence of properties at moments of time): All mice lose grey hair, eat cheese etc. The truth of these causal statements is established on the basis of induction on observed things. We have thus a first level induction over a series of observations. And then a second level induction over the uses of IBE in the observational domain. In a (perhaps tedious) attempt to make things crystal clear, let us spell out the mouse argument:

1. All and only mice lose grey hair, eat cheese, make specific noises etc.
2. Fact F: grey hair etc. is observed
3. The presence of the mouse is the best causal explanation of F

Therefore, a mouse is present

Sadly enough, this argument is not valid. A malevolent demon (or neighbour) could have put the clues in place to trick me. However, we here have more reasons to believe in the existence of the mouse because of premiss 1 and because alternative causal connections can be eliminated on the basis of other observations (my neighbour always displayed a friendly behaviour etc.).

Coming back to direct observations, if we grant that properties possessed by entities are real, their observation provides evidence for the existence of entities which possess them.[12] Doesn't the direct observation of the properties of mice provide *evidence for* the existence of a mouse? Don't we have reasons to believe that there are causal chains that link the unobserved mouse with the observation of the clues, even if we don't know the details of these causal connections? Instead of following the explanationist path which starts from a top hypothesis to move downwards to what it best explains, and then afterwards climbing up to establish the existence of the explanatory entities, why not take evidence as our departure point and investigate what kind of observational evidence would give us more reasons to believe in the existence of some unobservable entities rather than not? Why not abandon the top-down explanatory strategy to embark in a bottom-up defence of scientific realism instead?

3 The Bottom-Up Evidential Strategy in Favour of Scientific Realism

Paying tribute to empiricists, let's begin with ordinary observational experience. Why do I have more reasons to believe than not that there is now a cup on my desk? It wouldn't be irrational to believe that there is no cup or to suspend my judgment about its existence. After all, the philosophers who have defended radical scepticism or solipsism were far from being irrational. The world could be just a dream, a hypothesis that was seriously considered (but also refuted) by philosophers such as Descartes.

How do I attest that what I see on my desk is a cup? I have seen many cups before and I know that they have a number of properties. They are hard, hollow, have a closed curve edge and can contain a liquid, to mention just a few of their properties. I can assure myself that the observed entity has the property of hardness by touching

[12] I assume throughout this paper that real properties are instantiated by real entities.

it. I can also hit it and hear a specific sound. Moreover, it has visual characteristics similar to those of hard objects. Three distinct and independent perceptual modalities (touch, hearing, sight) give me cognitive empirical access to the property of hardness. Within each modality, I can repeat my observations in various ways. There are different manners of touching to check hardness. Similarly, by varying the ways of hitting the cup, I hear different sounds which all reveal hardness. I can apply the same methods to attest the presence of other properties indicating the existence of a cup, instead of a vase. Such considerations lead to a first requirement for having more reasons to believe than not in the existence of an observable thing.[13]

1. *Requirement of concordance (or invariance)*: distinct and independent perceptual modalities of the same property must give (approximately) identical results.

The rationale behind this requirement is the truth of generalizations which state a causal connection between the observation of a thing, in suitable circumstances, and the actual possession of a particular property by the observed thing. Several observations underpinned by various causal connections, which give concordant results, indeed increase our confidence in the instantiation of a particular property by the observed thing. Why? Simply because we have inductively verified that such concordance makes it less likely that beliefs warranted by this procedure will be falsified.

What about observable entities which are not immediately present, such as an unobserved mouse? As we saw, empirically attested causal connections must exist between the properties (clues) that are directly observed and the properties of the entity whose existence is suspected. Such causal links cannot be flatly postulated; they must have been empirically attested by previous observations. Leaving mice aside, consider the more relevant scientific example of the discovery of the planet Neptune. Early in the nineteenth century Bouvard observed—through a telescope—that the trajectory of Uranus did not obey Newton's second law—which is a causal law[14]—together with the formula of the gravitational force. Since those laws were abundantly confirmed by numerous observations, Adams and Leverrier independently inferred the existence an unobserved planet, which was the cause of the perturbations (appearances or data) in the orbit of Uranus, namely Neptune, which was observed in 1846. Other planets had been observed before, and it was known that they possess the properties of motion and mass, and that their behaviour was governed by Newton's laws. We are thus led to introduce another requirement, namely the *requirement of causality*.

2. *Requirement of causality*: the observable properties of the entity whose existence is inferred must be related to the observed data by means of causal

[13] Ghins (1992).

[14] Newton's second law is causal since it contains a time derivative, which refers to effect, while the forces refer to causes (Blondeau and Ghins 2012).

connections described by previously empirically verified generalizations or laws.

Notice that the causal connections at stake in this requirement are distinct from the causal links between the property of a directly observed thing and the actual observation of it; the latter were involved in the first requirement.

So far, I have been talking of observable properties. For the sake of clarity, it is appropriate to divide properties in two classes[15]: the properties that are observable in principle and the purely theoretical properties. Some properties are observable, either directly or indirectly by means of instruments which enhance our perceptive abilities and have been shown to be reliable. Other properties are currently beyond possible observation but could be rendered perceptually accessible by means of instruments which might become available later. These properties are *observable in principle* (OP). I'll include in the class of OP properties, properties such as mass, charge, temperature etc. This is controversial, since the terms "mass", "charge", "temperature" etc. belong to a theory-laden language. Moreover, these properties sometimes require very sophisticated instruments to be accurately measured. Their meanings were fixed only after a long and convoluted historical process. However, once the meaning of *e.g.* inertial mass is clear, we can—in favourable circumstances—perceptually ascertain that bodies have different or approximately equal values of resistance to acceleration, provided they are not too small and their masses differ by a sizable amount.[16]

The other class contains properties which are not observable, even in principle. In elementary particle physics, for properties such as (internal) spin, strangeness, charm etc. there is no way to confirm their presence in ordinary perceptual experience. These are *purely theoretical (PT) properties*. By now, it is uncontroversial that in the context of a scientific theory all properties are theoretical, even at the observational level, in the innocuous sense that the *terms* used to refer to them are theory-laden. This is why I qualify properties that are *de dicto* beyond our perceptual reach as *purely* theoretical. I'm now in a position to formulate a third requirement:

3. *Requirement of observability in principle*: entities that are not directly observable must possess some properties (at least one…) which are identical or similar to directly perceptible properties of everyday observable things. These properties are observable in principle (OP) with the aid of suitable scientific instruments.

This requirement seems to bring us back to empirical adequacy only. This is not so, because van Fraassen wouldn't accept that what is seen through a microscope[17] counts as evidence for the existence of mitochondria. The appearances to be saved are the phenomena immediately seen in the microscope, like colour patches, spatial

[15] Ghins (2017, p. 123).

[16] More on this in Ghins (2017).

[17] van Fraassen (2008, p. 100ff).

forms etc. Unlike planets, mitochondria lie beyond our possible direct observation. If satisfied, this third requirement supports belief in the existence of unobservable entities if we have more reasons to believe than not that they instantiate OP properties, in virtue of the requirement of causality.

How could we defend that this third requirement must be satisfied by an argumentation in favour of unobservable entities? After all, access to OP properties can only be obtained by means of instruments which involve causal connections in their functioning. Some PT properties could be cognitively accessible in an analogous way. Why not admit, as many scientific realists do, that PT properties accessible by a variety of independent methods which give concordant results are real and instantiated by existing entities? On top of obvious empiricist motivations, the rationale behind the third requirement is, I suggest, that forceful argumentations of favour of the reality of some entities rely on previously verified empirical causal generalizations, in accordance with the second requirement. In order to verify such generalizations by empirical methods such as Mill's (1911), these generalizations must refer to observable properties. Then, step by step and by relying on previously empirically ascertained regularities, we could confirm other regularities which contain OP properties instantiated by unobservable entities.

To illustrate this point, think of Perrin's argumentation in favour of the existence of molecules.[18] Among other hypotheses, Perrin's reasoning crucially depended on Newton's causal second law of motion since the grains of mastic or gamboge in Brownian motion were supposed to incessantly collide with the molecules of the liquid in the emulsion. Another example is the discovery of the electron by J. J. Thomson in 1897 on the basis of his observations of the behaviour of cathode rays in vacuum tubes. Relying on previously known causal laws, he concluded that cathode rays are beams of very light charged particles: the electrons.[19]

Since in most scientific contexts it is impossible to accurately determine the values of OP properties without instruments, a fourth requirement must be added.

4. *Requirement of measurability*: the OP properties must be quantitatively measurable by means of appropriate already available instruments.

The reliability of an instrument or measuring device must have been previously empirically attested, either by the knowledge of the laws which govern its functioning or by means of empirical checks (for example, the reliability of Galileo's telescope can be established without knowing the laws of optics simply by comparing indirect distant observations with close direct observations of the same thing, a ship for instance). Satisfaction of the fourth requirement is a condition of possibility for the satisfaction of the first when crude perceptual measures cannot be directly performed. Even if mass is an OP property, the value of the mass of an elementary particle such as the electron cannot obviously be obtained through our unaided perceptions.

[18] See Ghins (2017) and Psillos (2011, 2014).

[19] Again, it could be objected that electromagnetic fields etc. are not directly observable. This is true, but the presence of such fields can be detected by means of everyday experiences with, for example, magnets. For detailed discussion of Thomson's experiments, see Nola (2008).

I submit that argumentations complying with these four requirements give *more* reasons to believe than not that unobservable entities possessing OP properties exist and that some assertions involving OP properties are true (within the limits of accuracy of measuring devices). Before concluding, I wish to reply to two objections.

The first objection concerns the requirement of invariance. Isn't this requirement a disguised appeal to some form of inference to the best explanation? It would be some kind of miracle or very improbable coincidence if a large quantity and variety of observations or measurements giving concordant results would not be caused by the presence of an entity possessing the requisite properties. However, if we directly observe something like a cup, there is no IBE, even implicit. We have immediate perceptual evidence for the presence the thing observed. Moreover, in the case of an entity such as an unobserved mouse, planet, particle etc. the available evidence permits to infer its existence on the basis of already empirically ascertained causal connections. Surely, in this situation, the presence of a mouse does causally explain the clues, but we don't infer its existence because it provides the best explanation in a pool of satisfactory explanations. Within our causal approach, we don't search for possible hypotheses from which we could descend to account for the appearances, but we try to climb up the ladder of causal connections. If the details of the causal connections are available, then we also have a causal explanation, which is also the best possible one. But the knowledge of the details isn't indispensable to have sufficient evidence for the presence of a causal link. In other words, we have more reasons to believe than not in the existence of an entity because we are justified in believing in the existence of a causal link (which as a bonus, could sometimes also deliver an explanation) and not because after having considered a number of explanations, we deem one of them to be the best, *i.e.* the most satisfactory, and conclude that it is true. It is not the satisfactory nature or explanatory power of a hypothesis which gives us reason to believe in its truth; rather it is the existence of a causal link which is truth-tropic and, as a consequence, could perhaps (if the underlying causal mechanism is known)[20] furnish the true explanation.

This brings us to a second objection, which bears on the requirement of causality. The discovery of Neptune and the electron are textbook examples of the alleged reliability of IBE (Douven 2017). Aren't there several possible causal explanations of the deviation of the orbit of Neptune and the behaviour of cathode rays? Certainly. As Lipton remarks:

> Inference to the best explanation does not require that we infer only one explanation of the data, but that we infer only one of *competing* explanations. (Lipton 1991, p. 65, 2004, p. 62)

Within the contemplative approach, we aim at assessing the merits of the existence and truth claims made by predictively correct theories. To be forceful, a claim about the existence of an entity which possesses an OP property must be buttressed, among other requirements, by the attested presence of a real causal connection between the appearances and the possession of that particular property or set of

[20] Ghins (2018).

properties instantiated by the purportedly real entity. Such approach is bottom-up since a cogent argumentation must start from the effects (the appearances) and ascend to the causes by means of previously empirically verified causal generalizations or laws. Alternative causal connections must be empirically eliminated through such a procedure (using typically Mill's method of difference), which however doesn't not warrant the elimination of error.

The NMA isn't grounded on a causal link between truth and predictive correctness, as we saw.[21] What is more, IBE operates in a quite different way from our inference from effects to causes. First of all, IBE reasoning is not restricted to causal explanations. As far as the issue of scientific realism is concerned, causal connections matter most. Second, inference to the best explanation is an inference from a pool of satisfactory explanations. Let's restrict ourselves to causal explanations and then weight the merits of each competing explanation. With respect to what? To explanatory power, *i.e.* to their satisfactory nature or what Lipton calls "loveliness". Thus, the merit of an explanation is assessed by examining how well it explains the appearances. This is a top-down approach. In such a procedure, what counts is the relation between what we consider to be a satisfactory explanation—its loveliness—and the truth or correctness of the explanation. The challenge for the explanationist is, as Lipton acknowledges, to "show how likeliness is determined (at least in part) by explanatory considerations" (Lipton 2004, p. 61). Agreeing with van Fraassen (2005, 2017), I think that Lipton has not shown that such determination is (at least sometimes) present and has sufficient force to justify belief in the existence of unobservable (explanatory) entities.

4 Conclusion

Epistemological scientific realism (ESR) is the philosophical thesis according to which we sometimes have more reasons to believe that not in the truth of theoretical claims about unobservable entities. Such reasons are grounded on our empirical ability to ascertain the reality of causal chains which relate appearances to some entities which possess properties which are observable in principle (OP properties). The invariance or concordance of the measurements of OP properties reinforces our theoretical beliefs in the same way as the quantity and variation of observations of the same directly observable property of an ordinary thing furnishes supplementary warrant for belief in its reality. The offered defence of ESR doesn't depend on the use of inference to the best explanation whose reliability is disputable. In this sense, our defence of epistemological scientific realism is not explanationist.

[21] Significantly Lipton concedes that the NMA isn't a cogent application of IBE, even though he maintains that IBE "can be used to provide some arguments for adopting a realist (…) stance toward scientific theories" (Lipton 2004, p. 209).

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