The Debates on Scientific Realism Today: Knowledge and Objectivity in Science

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ABSRACT

Debates on realism in science concern two main questions: whether theoretical knowledge is possible, and whether it is objective. Today, as in the past, the possibility of theoretical knowledge is often denied because of the empirical underdetermination of theories. Realists rely on explanatory power, theoretical virtues, and instrumental but theory-free observation to solve this problem. Besides, they use the *no miracles argument* for the truth of successful theories. Antirealists, however, deny that explanation is either necessary or possible, and that is a cue to truth. Moreover, they reject realism and the cogency of the no miracles argument by the pessimistic induction from the falsity of past successful theories. Some realists reply that there is a radical discontinuity between past science (largely off-track) and current science (basically sound). But this reply is at best insufficient, and most realists prefer to restrict their commitment to selected parts or features of theories, both past and present. Forms of "selective realism" are *entity realism*, *structural realism*, *deployment realism* and *semirealism*, but also the *verisimilitude* research program and the *restricted-domain* approach. Realists need criteria to identify the true components of theories, and a noteworthy candidate is essential involvement in functionally novel and surprising predictions. The second main question is a special instance of the old debate between realists and relativists or idealists: according to antirealists science cannot be objective, because of its inherently "perspectival" nature, characterized by a priori and subjective factors. On the contrary, *perspectival realists* argue that the specific "viewpoints" within which scientists must work do not prevent them to discover objective features of reality.

1. Knowledge, objectivity, and the earlier debates on scientific realism

Debates on realism in science have taken place since the Antiquity (Alai 2008), and they concern two different questions: (I) whether the unobservable entities posited by theories can be known (that is, whether we can have beliefs about them, and these beliefs can be both true and justified); and (II) whether any knowledge we have of them is objective or not. Question (I) arises from the doubt that since observation is the basis of all our factual knowledge, unobservable entities cannot be known; questions (II) arises from the doubt that since scientific representations are inextricably laden with, or distorted by, the subjective, idiosyncratic and a priori features of human cognition and scientific practice, they cannot convey any reliable information on how their objects are "in themselves". The same two questions arise even concerning different subjects (ordinary material objects, the mind, etc.), and antirealism on knowledge is generally termed 'skepticism', while antirealism on objectivity is called 'idealism', 'subjectivism' or 'constructivism'.

These two questions are logically independent, so they have been mostly discussed separately, and one can be realist on one and antirealist on the other. For instance Putnam held in (1978b) and (1981) that we can know unobservable entities quite as we know ordinary observable objects, but this is not knowing an absolutely mind-independent reality; a similar stance has been taken by some "perspectival realists" (see below). Nevertheless, certain powerful arguments for realism work in both debates, so that one may find it hard to be realist on knowledge but not or objectivity, or vice-versa (Alai 2005, 2006: 214-216, 2009, forthcoming). Moreover, as we shall see, the two debates somehow converge, in the sense that some of the most plausible forms of realism on each question (selective realisms on the one hand, and perspectival realisms on the other) basically agree on the possibility of objective but partial knowledge. Here I shall examine the state of contemporary debates mainly on the knowledge question, and more briefly on the objectivity question.

Since the Antiquity and up to the beginning of the last century, the typical antirealist stand about scientific knowledge was that we can at most aim at "saving the phenomena", i.e. form true and justified beliefs about observable entities. A frequent corollary was instrumentalism: to the extent that theories apparently describe unobservable entities, they should not be interpreted literally as descriptions, because they actually are (or should be) just practically useful instruments, computing devices to predict future phenomena or directions for technological applications, and as such neither true not false.

However, instrumentalists should explain why theories are so useful and successful, if they don't offer an at least approximately true description of the underlying reality; moreover, what justifies the claim that the literal interpretation of scientific statements as descriptions is wrong, and that scientists which aim at finding the truth are wrong and should pursue a different goal? (Alai 2006: 220). The only possible justification would be the claim that saving the phenomena is the best we can do, but even so instrumentalism seems to be a dubious and idle corollary. In fact, it isn't a live option anymore, except perhaps as concerns quantum mechanics (e.g., Wigner 1967).

Logical positivists tried to bypass the epistemological debate on truth and justification by a "linguistic turn": their verificationist theory of meaning entailed that, appearances notwithstanding, scientific theories did not actually speak of unobservable entities; the meaning of theoretical terms was entirely reducible to possible observations, so in principle they could be replaced by a purely observational vocabulary (Carnap 1923).

However, eliminativism was abandoned when it became clear that non-observational terms cannot completely defined by observational terms, as it happens already with simple "dispositional" terms (such as 'soluble' and 'fragile') and the irrational values of physical measurements (Carnap, 1936: § 7; Hempel 1952: II). Moreover, non-observational terms play a necessary role in systematizing experience, predicting future observations, and leaving the way open to the discovery of new properties (see Hempel 1958); moreover, their elimination would leave numberless unexplained coincidences (Smart 1963: 39; Psillos 1999: 72-73). Finally, verificationism itself was abandoned in the second half of the XX Century, especially as a consequence of Quine's (1951) criticisms.

Therefore van Fraassen (1980: II,1) explained that linguistic questions are no longer live issues between scientific realists and antirealists: antirealists may grant that theories must be read literally, as purportedly true descriptions of unobservable entities, but deny that we have compelling reasons to believe that they are true. According to his "constructive empiricism" all we need to believe is that a theory is empirically adequate, i.e., it "saves the phenomena". Thus he brought back the debate to the epistemic question on which it had focused for about 20 centuries, and in so doing he set the agenda for most subsequent discussions on the knowledge question up to this day (Alai 2006: 217-220).

2. The empirical underdetermination of theories

Antirealists believe that since observation is the only basis for factual knowledge, unobservable entities cannot be known. This is made evident by the empirical underdetermination of theories (see Stanford 2013): they are introduced as explanations of the observable phenomena, but for any set of given phenomena many incompatible explanations can be found; therefore, it is claimed, we cannot know which one is true. This has been one of the main arguments against realism since the Ancients up to our days (Celsus 1935: §§ 28-29; Alai 2008: §§ 3-4).

The history of science, however, would seem to refute this argument: cases of actual competition among empirically equivalent theories have been very few, and eventually they have been decided to everybody's satisfaction (e.g., undulatory vs. corpuscular theory of light; Ptolemaic vs. Copernican vs. Tychonian cosmology). There are mathematically intertranslatable theories, like Newtonian mechanics (based on force) vs. Lagrangian or Hamiltonian mechanics (based on a principle of minimal action); or theories introducing fields and theories using action at distance with retarded potentials (Putnam 1978b: 133), but they look more like different formulations of the same theory. There seems to be a genuine conflict between Heisenberg's matrix mechanics and Schrödinger's wave mechanics (Putnam 1978c: 555; Friedman 1983: 165 ff.; Fano 2005: 166), and between standard quantum mechanics and Bohmian mechanics; but one day we might find new empirical consequences which allow to decide between them. In general, "it has never been shown that for any theory there exist non trivial and minimally plausible alternatives" (Psillos 1999: 168); underdetermination does not look like a concrete problem in scientific practice, but a merely in-principle risk not to be taken too seriously (see Laudan and Leplin 1991, 1993).

Yet, isn't it a logical fact that there are infinitely many possible theories compatible with any body of data? So, it has been suggested that our failure to consider many of those theories (including perhaps the true one) has to do with our conservatism and lack of imagination, and that many unconceived alternatives "exceed our grasp" (Stanford 2006).

3. Explanation and theoretical virtues

This, however, is just part of the story: scientists do not look simply for theories that are compatible with the phenomena, but that are *true*, *explanatory* and *fecund*. So, theories must be *empirically testable, plausible* (i.e., consistent with the largest possible background of accepted beliefs), *simple*, *non-ad hoc*, and provide models of the underlying causes, which at the same time *explain* the phenomena, *unify* them, and *predict* new phenomena as effects of the same causes. So many possible theories which could be *imagined*, are not even considered because they lack these *theoretical virtues*.

In other words, the evidence for a theory does not coincide with its empirical consequences: for instance, trivial and *ad hoc* consequences don't confirm (Laudan 1996, ch. 3; Laudan and Leplin 1991, 1993; Psillos 1999, 169-176), and certain phenomena can confirm a hypothesis even if not entailed by it (Psillos 1999: 170). In Bayesian terms, different hypotheses have different prior probabilities, so even if supported by the same data, they get different posterior probabilities (Fano 2005: 166; Psillos 1999: 163). Theoretical virtues have confirmatory power (Glymour 1980, Kosso 1992, Psillos 1999: 171-176) and supply the necessary guidance to all *ampliative* inferences: even the mere inductive projection presupposes the uniformity of nature, and follows the rule of simplicity. The same holds for analogical reasoning, while abductive reasoning exploits the explanatory power of hypotheses.

Antirealists deny the confirmatory power of theoretical virtues: simplicity is evidence for truth only if nature is simple, or *uniform*; but this can be shown only by circularly assuming that our theories, which depict it as simple, are true. Assuming that consistence with other accepted theories is evidence for truth presupposes the *petitio principii* that accepted theories are themselves true: the entire body of our scientific beliefs might be coherent, but false.

This reminds of Humean criticism of ampliative reasoning: its validity cannot be proved *a priori*, since it is a factual question, nor *a posteriori*, since an inductive proof (from the past success of these methods) would be circular. Yet, this is not enough, if scientific antirealism is must differ from general Humean skepticism (Alai 2006: 228-230). Moreover, we use basic patterns deductive of inference, like *modus ponens* and *modus tollens*, without requiring a non-circular proof of their validity; so, why shouldn't we do the same for equally basic forms of inductive inference? Besides, many antirealists use them in inferences from observed facts to yet unobserved ones; in particular, only ampliative methods allow to claim that a theory is *empirically adequate* - as opposed to merely compatible with known data (Lipton 1991, 154 ff.). Some reply that ampliative methods are reliable when inferring from observed to *unobserved-but-observable* entities, but not when inferring to *unobservable* entities. However, this discrimination is unjustified, since the validity of inferential patterns does not depend on the subject-matter (Alai 2010: § 3).

Some antirealists deny that we need explanations, or that explaining is a task for science (Duhem 1906). However the search for explanations is natural for human beings and a spur to inquiry (Aristotle *Metaphysics,* 982b); it is typical of ordinary knowledge, and science is just a development of ordinary knowledge (Rescher 1987: 36-41). The desire to explain is not mere curiosity, but a search for consistency and self-correction: not for any event we ask why or how it happened, but only for those which in the light of our preconceptions should not or could not have happened. Hence, finding an explanation involves correcting some mistake or learning some new crucial information. In fact, the search for explanations produces new discoveries and is a key to the self-corrective character of science.

Van Fraassen (1980: 25) claimed when explaining observable regularities through unobservable ones we leave the unobservable regularities unexplained; therefore we should limit our search for explanation to the observable level. However unobservable regularities may in turn be explained by deeper unobservable regularities, and these by even deeper ones, etc. At each level explanation gains generality, simplicity and information: fewer laws, entities and properties are employed. This does not necessarily launch an infinite regress, since wherever we stop we have a correct, informative and interesting explanation even if the *explanans* is left itself unexplained. The longest the explanatory chain gets, the more it becomes informative and interesting. Furthermore, often observable regularities are not explained by further regularities, but by postulating *entities*, such as genes, viruses, etc. (Aronson 1984).

4. The *No Miracles Argument*

If we accept the ampliative methods, in particular the Inference to the Best Explanation, then not only we can hope that our selection of theories among the infinitely many logically possible ones is reliable, but we have a meta-argument to the effect that best theories are in fact true or partly true, and scientific method is truth-conducive, hence scientific realism is right: as noted by Putnam (1975a, 73), this is the only explanation of the stunning predictive and applicative success of science which does not make it a miracle (or if you will, a miraculously lucky coincidence). This *No Miracles Argument* (NMA) has been used in various versions also by Smart (1968: 150), Grover Maxwell (1970:12), Musgrave (1985), Lipton (1991), Niiniluoto (1999: 197), etc. (See Psillos 1999: 70-81).

It has been objected that the NMA is circular or question-begging, for it presupposes the reliability of abductive reasoning, which is rejected by antirealists (Laudan 1981: 45-46; Fine 1984: 85-86: see Ladyman 2002: 218-219). In this sense, the choice between realism and antirealism is a *stance* (van Fraassen 2002; Psillos 2011a, 2011b). But as argued above, realism seems to be better grounded and more consistent with our epistemic practices at large.

4.1 Novel predictions

The NMA presupposes that truth is the only (non-miraculous) explanation of success. But certain forms of success can be explained otherwise: success in accounting for known phenomena might be simply due to the theorist’s puzzle-solving ability (ingenuity, imagination and patience). This explanation is not available for the prediction of novel phenomena. However, if the predicted phenomena are similar to already known phenomena, the theorist might have inductively extrapolated them from the previously known ones, and then built the theory with an eye to accommodate them. For instance, it would be hardly surprising that a theory T based, among other data, on certain chemical and physical properties of carbon-12 and carbon-13, predicted analogous properties for carbon-14. Again, if T predicts new and heterogeneous phenomena, but they a priori probable, this success might be simply credited to good luck. In all these cases there is no need to assume that T is true. For example, many false astronomical theories could predict the existence of a new unknown planet somewhere in the universe. But the prediction of a new planet (Neptune) with precise mass and orbit in a particular region, as licensed by Newton’s gravitation theory, showed that there was some truth in it. Equally, quantum electrodynamics predicted the magnetic moment of the electron was $1159652359 \times 10^{-12}$, while that obtained by experiment is $1159652410 \times 10^{-12}$ (Wright 2002), and such concordance cannot be due to luck (Alai 2014a §§ 3.2-3.3; 2014b, §§ 4-5).

Therefore the NMA properly applies only to the prediction of novel, surprising (heterogeneous from all that was known before) and bold (a priori improbable) phenomena (Musgrave 1988, Psillos 1999). Further examples of predictions fulfilling these constraints are Fresnel's bright spot and dark spot prediction; the new chemical elements predicted by Mendeleev; Einstein's predictions of the retard of clocks in motion and the bending of light; the background radiation predicted by the Big Bang theory; etc.

This means that novel predictions confirm more than mere *a posteriori* accommodation; but many objected that confirmation is a logical relation between a hypothesis and its empirical consequences; therefore prediction should have no advantage over accommodation. The ensuing debates (see Alai 2014a: §§ 1-3) have shown that in order to count for the NMA a new phenomenon NP needs not be *historically* novel (i.e., not known by the theorists, or not used by them in building the theory): even if NP was used, it is enough that it was not used *essentially*. That is, it is enough that the theory T was plausible independently of NP, i.e., that it was already be the best possible account of a body of *old* phenomena OP not including NP. In fact, in that case it would be a miraculous coincidence if T, build to accommodate OP, were false, yet at the same time predicted the highly improbable NP, radically heterogeneous from OP. Since this notion of novelty does not concern a relation between NP and the theorist, but the relations of NP to T and OP, it is compatible with the logical nature of confirmation. Novel predictions so understood confirm for the same reason for which the consilience of non-*ad-hoc* predictions by independent theories confirm those theories: the unlikeliness that they are just a lucky coincidence (ibi: § 4).

4.2 Objections to the NMA

Since success is essentially the truth of the (novel) prediction 'NP', it has been objected that it has a trivial explanation, which entails nothing about T: 'NP' is true simply because things are as it states (White 2003; Rees 2012: 302). Yet, we have a real puzzle: how could the theorist find, among the numberless possible theories compatible with the old phenomena OP, one predicting the heterogeneous and improbable new phenomenon NP, without essentially using it? This is the real *explanandum* in the NMA (White 2003; Laudan 1984a: 92; Alai 2014a: 299; 2014c: 50). It has also been noted that the *explanans* cannot be that the theorist found a true theory: for the *true* theories entailing NP are an just a tiny subset of those entailing NP, so we would explain a puzzling fact by an even more puzzling one. On a careful analysis, however, the actual *explanans* is that scientific method is truth-conducive (because the uniformity and causal structure of nature makes ampliative inferences reliable), *hence* the theorist found a true theory (Alai 2014a: § 1; 2014c: § 5).

Problems with the NMA were raised also by Lipton (1991), Howson (2000) and Magnus and Callender (2004), for whom the impossibility to know the relevant base rate prevents to claim that successful theories are probably true. Paul Hoyningen-Huene (2011) argued that the version of the NMA based on use novelty is falsified by what he called "transient underdetermination".

Some antirealists claim that the success of T can be explained differently: for instance, because T is empirically adequate, i.e., compatible with all the phenomena (van Fraassen 1980, 12); or because all phenomena are *as if* T were true (Fine 1984, Leplin 1987). But this does not explain why T predicts NP, for T may be *compatible* with all phenomena without predicting any of them, in particular without predicting NP. For instance, a theory with no empirical content would be empirically adequate. For Stanford (2000, 272 ff.) T is successful because it makes predictions very similar to those of *the* true theory; but this would be like saying that T predicts NP because it predicts a number of things including NP. So, it would not be an explanation, but just a repetition of the *explanandum*. Other variants of the empirical adequacy explanation, like Lyons' *modest surrealism* (2002, 78), and Fine's *instrumental reliability* explanation (1986, 1991), have the same flaw. In general, no "as if" account really explains: "the hypothesis that it is raining explains why the streets are wet—but 'The phenomena are as if it were raining' does not" (Musgrave 2006-2007).

Besides, none of these *expalanantia* explains how the theorist found a theory that predicts NP without essentially using it. Realists explain that T was found thanks to the scientific method, which is fecund and truth-conducive; but antirealists cannot answer in a similar way. In fact, there are just two *methods* to find theories predicting new phenomena: one is by finding true and fecund theories (the realist method); the other is by inductive extrapolation from known phenomena (Laudan 1984a, § 5; Rees 2012, 302; Wright 2013, chs. 3, 4; Alai 2014d, 125-126), but this is impossible if the new phenomena are radically heterogeneous from the old ones (Alai 2014c: §6).

According to Fine (1984) and Hacking (1983: 64) the success of T is just the obtaining of the phenomena predicted by T, hence it is already explained by T itself. However, advancing a hypothesis as an explanation is accepting it as true, hence their "deflationistic" explanation cannot dispense with the assumption that T is true. Moreover, it doesn't explain how T was found (Alai 2014c §4).

Stanford suggests that even a radically false theory which saves the phenomena is likely to make novel predictions, thanks to "the systematic relationship among phenomena within the same domain of inquiry" (2000: 281). However, if "systematic relationship" means that the phenomena are homogeneous, there is no novelty in that field; if instead it means that they are connected by unobservable underlying mechanisms, then it would be a miracle if T predicted NP without getting those mechanisms right.

Van Fraassen's (1980, 40) "natural selection" explanation was that our theories are successful, simply because the unsuccessful ones were dropped. However, this explains just (1) why we have *only* successful theories, not (2) what makes these theories successful nor (3) how they were found. When asked "why birds can fly?" one cannot just answer (1) "because those which did not fly were wiped out by natural selection". It should also be explained that (2) birds fly thanks to wings, feathers, hollow bones, etc. (Kitcher 1993; Alai 2014c, § 7).

5. Indirect but theory-free observation.

Theoretical hypotheses are a creation of human mind, introduced as the best explanation of certain phenomena, and tested by their observable consequences. Antirealists object that (a) any explanation is underdetermined and (b) confirmation by consequences is the invalid scheme of *adfirmatio consequentis*. If one points out that some theoretical entities are *instrumentally* observed (e.g., by the electronic microscope), they reply that since the reliability of those instruments can be established only on the basis of theories, theories cannot be confirmed by them.

However, some theoretical entities can be observed by instruments which do not presuppose theories (Alai 2010: §4). Van Leeuwenhoek, a draper, invented the optical microscope by observing that the lenses he used to ascertain the quality of his fabrics magnified small but observable objects by 200 or 300 times. Thus, when through his lenses he saw things which escaped the naked eye (like bacteria, spermatozoa, and muscular fibers), he could safely assume that those were real entities, and approximately measure their actual size. In so doing he relied merely on his eyes, the uniformity of nature (e.g., the assumption that the ratio of the lens images to their objects did not vary at different scales), and elementary mathematics.

Perrin was able to measure Avogadro's number, hence the volume and size of molecules, by procedures which also presupposed nothing but the uniformity of nature and easy computations. For instance, he dropped a droplet of oil on a water surface covered by talc powder: the drop expanded, pushing the talc aside. Eventually it reduced to the thickness of one molecule and became invisible, but its surface coincided with the talc-free area, and could be measured. Thus, by dividing the volume of the droplet times the surface, he found the value of about 1μμ for the diameter of molecules (Perrin 1913: § 32). With similar methods Millikan was able to measure the charge of the electron, and those results finally dispelled all the doubts on the atomic structure of matter.

Another example is the chemical composition of the stars: it cannot not be observed directly, but it can be recognized by observing the same spectra which are emitted by samples of different elements in the laboratory. Probably cases like these are more common than it might seem, and in front of them stubborn antirealists must pay a high price: doubt the uniformity of nature, and give up induction in science and everyday life (Alai 2010: §4).

Perrin's measurement of Avogadro's number also exemplifies the confirming power of Whewell's "consilience of inductions", because he reached the same results by different methods (Kosso 1992: ch. 9). Those procedures presupposed various theories, but, since they were mutually independent, the agreement of their results would be a miraculous coincidence unless they had some real grasp of the world. So, this reasoning is just another variant of the NMA.

6. The historical objections to realism

The most severe objection to realism, beside empirical underdetermination, is that in the history of science most theories sooner or later have been found to be false, and none that is still accepted is older than 100 years or so. This is the premise of the so called "pessimistic meta-induction" (PMI) (Putnam 1978a: 25):

(PMI_1) all past theories were completely false, even the successful ones;
(PMI_2) there is no radical epistemic or methodological difference between past and present theories; therefore
(PMI_3) most likely also current and future theories are and will be completely false; therefore
(PMI_4) unobservable entities cannot be known.

(PMI_2) is often left implicit, although it is crucial to the argument. (PMI_1) also works as a premise to Laudan's (1981) refutation of the NMA, called by Lyons (2002) the "meta-modus tollens" (MMT):

(MMT_1) past successful theories were completely false; therefore
(MMT_2) truth is not the only explanation of success; therefore
(MMT_3) the NMA is flawed.

Even the restricting the notion of success to novel predictions will not save the NMA from the MMT: as pointed out by Lyons (2002), many ancient false theories made important novel predictions.

7. Resisting the historical objections: the discontinuity strategy

Some scholars denied the cogency of the PMI: for Lewis (2001), Lange (2002) and Magnus and Callender (2004) it is based on inductive fallacies, like the NMA. Doppelt claimed that it is incoherent (2011: 310; 2013: 48–49; 2014: 282–283), but he seems to be wrong (Alai 2016: §7). More often the PMI is countered by denying either of its premises: the "discontinuity strategy" rejects (PMI_2), while the "selective strategy" rejects (PMI_1).

The discontinuity strategy adds that since there are radical differences between past and present science, no inductive inference from the former to the latter is possible; therefore also (MMT_2) applies only to past science, and the NMA remains cogent for current science.

Hardin and Rosenberg (1982) claimed that many false theories cited by Laudan were not products of *mature science*, so from their falsity we cannot infer to the falsity of theories in mature science. Yet, there were many false theories even in mature science (Newton's gravitation theory, Fresnel's wave theory of light, Rutherford's model of the atom, etc.). Devitt (1984: 143-149) argued that progress in scientific methodology, observation instruments and experimental technology continuously improve the reliability of theoretical science, hence past failures should not be projected onto the present or the future. Still, even from the Antiquity to the XVIII Century scientific methodology, instruments, etc., had greatly improved, but all theories accepted in the XVIII Century have been subsequently rejected.

Gerald Doppelt (2007, 2011, 2013, 2014) argues that old superseded theories were radically false and their success cannot be explained by assuming that they were partly true, while current best theories are completely true and will never be superseded or corrected by better theories. However, this "is an illusion owing to our particular historical perspective" (Nickles, this volume), which prevents us to see that progress began already in the past, and it will go on in the future. *Pace* Hegel, Marx or Fukuyama, we are not at the end of history (Nickles forthcoming[b]), and science is always fallible. Moreover, Doppelt can't account for the success of past theories, nor for the failures of current ones (Alai 2016).

Nonetheless, the discontinuity strategy might be right that contemporary science is *much more* mature than Newton's theory (or any of the false theories cited by Laudan), exhibiting a *greater degree* of sophistication, unification and coherence, and that from the XVIII Century to now there have been much greater methodological and technological improvements than from the antiquity to the XVIII Century. For instance, ether and phlogiston were never measured, while we can measure many properties of unobservable entities (Dorato 2007: 181).

Fahrbach (2011: 1283) argued that "current best theories enjoy far higher degrees of success than any of the successful but refuted theories of the past". In fact "three-quarters of all scientific work ever done was done in the last 30–40 years", and the exponential increase in the amount, diversity, and precision of scientific data and computing power marks a sharp difference between science today and only a few decades ago. Therefore present theories are incomparable to earlier ones as to the number and diversity of the tests successfully passed.

Still, it is questionable whether this difference between past and present science is radically qualitative or merely quantitative, and in the latter case and it couldn't block the PMI: at most it might require some caution in the inductive extrapolation from past failures, because "successful science" is not a unitary kind, but rather a set of practices bearing family resemblances (Bird 2015, §3); or it might suggest that current theories will be superseded in a much longer time than past ones. At any rate, it cannot block the MMT, because if past theories were false but successful, truth cannot be the only explanation of success, even for current theories. Therefore the discontinuity strategy needs in any case to be supplemented by the *selective strategy*.

8. Resisting the historical objections: the selective strategy

The selective strategy rejects PMI_1 by holding that even in radically false and discarded theories there were *some* truths, which are still preserved today; hence even in current theories there are some (and presumably more) truths. Thus, selective realists are committed only to the truth of some parts of certain theories, and they stress the continuity between past, present and future science. If it can be shown that all the novel predictions of past false theories were due to their true parts, then also the MMT is blocked, and the NMA allows to argue that also the parts of current theories which produced novel predictions are true. There are different versions of selective realism, among which at least: *entity realism*, *structural realism*, *deployment realism*, *semirealism*, the *verisimilitude* strategy, and the *restricted-domain* approach.

8.1 Referential continuity and entity realism

Realists employed Kripke's and Putnam's (1975c, 1975d) causal theories of reference to argue that discarded theories were not completely off-track, because they posited the same entities we now believe in, although with different descriptions and sometimes with different names. For instance, the term 'ether' referred to whatever caused its introduction—say, the electromagnetic field (Hardin and Rosenberg 1982).

But this does not seem possible when the core descriptions associated to a term are completely wrong, or there exists nothing even slightly similar: there is nothing to which 'phlogiston' or 'caloric' might refer (Putnam 1978a: 25; Laudan 1984b: 160-161; Psillos 1999, 290-

293); understanding ‘ether’ as ‘electromagnetic field’ may be overstretched (Worrall 1995). Holding that a term refers to whatever is the cause of the phenomena it is supposed to explain would trivialize reference: for example, Aristotelian natural places, Newton’s gravitation force and Einstein’s curvature of spacetime, all were supposed to be the cause of gravitational phenomena. A shared explanatory agenda cannot be confused with a shared explanatory ontology (Laudan 1984b: 161).

Psillos raised further problems (1999: 286-287), and proposed instead a causal-descriptivistic account, by which a term refers to the unique natural kind having the core causal properties assigned to it by the description, provided that the actual kind-constitutive properties are the causal origin of such description (ibi: 295). More recently, Schurz (2011) proved a correspondence theorem showing that a theoretical term originally intended to refer to an inexistent entity may *indirectly* refer to a real counterpart of that entity. For instance, in phlogiston theory ‘dephlogistication’ indirectly refers to the process of oxidation.

So, noting that different theories of reference have been proposed, motivated by conflicting intuitions, Votsis (2011a) suggests that perhaps the very concept of reference is not a monolithic one. At any rate, what ‘reference’ means is conventional, and scientists may intend to pick their referents in different ways: no doubt Mendel understood ‘gene’ very liberally, as *whatever* played a certain causal role, while Bohr may have understood ‘atom’ as something very close to the description he gave. Besides, probably the crucial question is not whether T’s terms refer, but how much truth is found in T. It doesn’t help much that T’s terms refer, if everything T says on their referents is wrong. If a relaxed definition of reference is used, T can be radically false although its terms refer; if instead a strict definition is chosen, there can be a lot of truths in T even if its terms fail to refer (more on this below). So, reference may help, but it is neither necessary nor sufficient to resist the PMI (Alai 2006: 239).

According to a different approach, however false some theories may be, the entities they postulate must exist, because we currently *manipulate* them. For instance, in certain experiments electrons are sprayed to reveal the existence of quarks with fractional charges. We may have widely different beliefs concerning those electrons, but their existence is out of question: they are “here in front of us” (Hacking 1983: ch. 16). Equally, protons are produced and used to bomb the nuclei and study the trajectories of neutrons so expelled (Giere 1988: ch. 5).

Musgrave (2006-7) objected that “entity realism is a hopeless form of realism”, because the existence claims are empty without some description of properties and behaviour. Moreover, the manipulation argument is question begging: who says you are actually manipulating an unobservable entity, rather than merely performing certain macroscopic operations with certain macroscopic effects? (see van Fraassen 1985: 298; Nola 2002: 9).

To avoid these problems the argument can be reinterpreted as an inference to the existence of such entities as the best explanation of their observed effects (Nola 2002: 9-14), but then it supports also some theoretical assumptions about them. Otherwise, it can help to solve the Duhem-Quine indeterminacy of empirical control: machines incorporate the less problematic of our beliefs, which can be used to test the more problematic ones (Giere 1988). Even so, however, entities and beliefs about them go hand in hand. Summing up, the partial truth of theories cannot be confined to the existence of the entities they postulate (see also Dorato 2007: 183-184; Nanay 2016).

8.2 *Structural realism*

In opposition to *entity realism*, *structural realism* (StrR) holds that only structures can be known, while entities cannot (Frigg, Votsis 2011). According to different versions, we cannot know the individual entities, but we can know their properties and relations; or not their intrinsic properties, but their first-order relational properties; or none of these, but the second-order structure of their relational properties. This last was Russell’s (1927) and Carnap’s (1928) view (Ladyman 2014; French and Ladyman 2011).

Poincaré adopted StrR in reaction to the PMI: as theory T_1 is replaced by theory T_2, and T_2 by T_3, etc., the entities postulated by T_1 are substituted by different ones postulated by later theories (for instance, ether was substituted by the electromagnetic field); but the basic equations, tracking the underlying structure of things (e.g. Maxwell's equations) are preserved and are approximately true (Poincaré 1902: 160-162). Also the logical positivists maintained that we know only forms, not content (Schlick 1938), or structures, not objects (Carnap 1928, §§ 1, 6, 11, 16 etc.). Similar views were also held by Arthur Eddington, Grover Maxwell, Hermann Weyl (see Psillos 1999, 621-663; Ladyman 2014), and Ernan McMullin (1984).

Lately this position has been advocated by Worrall: "Fresnel completely misidentified the *nature* of light, but nonetheless it is no miracle that his theory enjoyed the empirical success that it did … because Fresnel's theory had ... more or less the right *structure*". Thus, "there was continuity or accumulation in the shift [from Fresnel to Maxwell], but ... one of *form* or *structure*, not of *content*" (Worrall 1989. See also Worrall 1994, 1995: 92-94). Therefore, showing that the success of discarded theories was due to their structural claims, StrR also vindicates the NMA against Laudan's MMT refutation. Besides, since incompatible but empirically equivalent theories may share the same structure, it has been suggested that StrR can also solve the underdetermination problem (Worrall 2011, Lyre 2011, French 2011).

More recently, the merely *epistemic* thesis that we can know only structures (EStrR), has been reinforced by the *ontic* thesis that there exist no objects but only structures (OStrR). The latter may be further detailed as claiming that (1) there are no individuals but only relational structures; or (2) relations do not supervene on the intrinsic spatio-temporal properties of their relata, or (3) individual objects have no intrinsic natures or properties; or (4) the identity of objects is ontologically dependent on their relations; or (5) individual objects are just constructs used to build approximate representations of the world (Ladyman 1998; French, Ladyman 2003a, 2003b; Ladyman and Ross 2007; Ladyman 2014).

OStrR is strongly suggested by contemporary physics: in the entangled states of quantum mechanics relations do not supervene on the properties of particles, and particles seems to lose their individuality, since there are no properties, even spatiotemporal, which allow to distinguish them from one another. Although they may be weakly discernible, Muller (2011) noted that this can be reasonably understood via a relationist conception of objects that supports OStrR.

Moreover, the traditional ontology of individuals, intrinsic properties and relations seems to be at odds with the nature of space, time and matter. The proper objects of contemporary physics are rather symmetries and invariants, and "elementary particles are hypostatisations of sets of quantities that are invariant under the symmetry groups of particle physics" (Ladyman 2014), or excitations of fields.

Many objections have been raised against StrR: do the mathematical equations preserved in theory change tell something about the underlying structure of the world, or merely about empirical regularities? (Laudan 1981: 237). Isn't the retention of equations just a convenient and labor saving pragmatic feature of scientific practice, due to the conservativeness of the scientific community? (Fano 2005). That equations tell us something about the structure of the world can only be shown by the NMA, which however supports at the same time the theoretical claims about entities (Psillos 1999: 152). In fact, for StrR structures are the only responsible for success. But mathematical equations by themselves can license predictions only when theoretically interpreted and supplemented with auxiliary hypotheses; so, the success of predictions confirms also those theoretical hypotheses (ibi: 153-155).

Besides, we cannot distinguish "between the *nature* of an entity and its *structure* such that we can ... know its structure but not its nature", because "to say what an entity *is* is to show *how this entity is structured*" (ibi: 155-156). For instance, there was no structure of light on which Fresnel was right while being wrong on its nature: there simply were properties of light on which he was right and others on which he was wrong (ibi: 159).

Granted, the properties on which he was right were behavioral and relational properties (the ways of propagation) while he was wrong that the physical nature of light was molecules of ether. So, we can know a lot about the relations and behavior of the unobservable entities without knowing much about their physical nature. For example, Mendel built his theory of genes without having any idea about their physical instantiation. Yet, as research proceeds, we can often discover the very nature of our objects, as Einstein did with light, or Watson and Crick with genes (Psillos 160-161). That the contents of perception are not actual properties of thing, as stressed by Russell and Carnap, is clear; but the same may not hold for theoretical properties.

Moreover, structural realists must specify what exactly is the "structure" preserved across theories, and how is it represented. For Grover Maxwell, Worrall, and Cruse and Papineau (2002), it is the relevant equations, represented by the theory's "Ramsey sentence" (a formulation where all the theoretical terms are substituted by existentially quantified variables). However a number of problems arise concerning the adequacy of such representation (Demopoulos and Friedman 1985: 635; Psillos 1999: 63-69; Ladyman 1998; Ladyman 2014). This is why, instead, Ladyman and Ross (2007) and French (2014) hold that what is preserved are symmetries and the associated group-theoretic structures, represented by the semantic formulation of theories.

Opponents of StrR also charged that it cannot account for the difference between physical reality and mere mathematical structures; that often also structure is lost in theory change; and that StrR only applies to physics (Ladyman 2014). Specific objections have been raised against the ontic versions of StrR, especially concerning plausibility of the existence of relations without relata, and the extent to which physics univocally support this view (ibid.). There is no room here to discuss them; besides, even if OStrR were wrong, one could still use EStrR as a form of selective realism able to resist the PMI.

Summing up, on the one hand the basic role of structures (in some sense of 'structure') in modern physics seems undeniable, and there are striking examples of structure preservation in theory change; on the other hand it has not been shown that no theory has ever correctly described the nature of entities, or that entities were never essential for novel predictions; moreover, although there have been attempts to apply StrR to biology (French 2011) and to the social sciences (Ladyman and Ross 2007, Kincaid 2008), it is far from clear that it can apply to all scientific disciplines. Therefore, while these questions are still hotly debated (Ladyman 2014), at present it may be reasonable to assume that StrR can account only in part for our realist commitments, and
we should take a liberal on which kinds of features (entities, or laws, or structures, or particular properties, etc.) theories can get right even if otherwise false.

8.3 *Deployment realism*

Kitcher's and Psillos' "deployment" realism is not concerned with the *kind* of theoretical components which survive scientific change and deserve realist commitment (whether they are ontological or structural), as with their role: they are those essentially deployed in novel predictions. "No sensible realist should ever want to assert that the idle parts of an individual practice, past or present, are justified by the success of the whole" (Kitcher 1993: 142). In fact if a component C were not deployed essentially in deriving prediction NP, there would be a different component C' not implying C, from which NP could have been derived; hence success could equally be credited to C', and the truth of C would no longer be the only plausible conclusion of the NMA.

Lyons (2002) criticized deployment realism by listing a number of novel predictions derived from components we now recognize as false. For instance, the idea of absolute acceleration was used in derivations from Newton's theory; the claims that phlogiston is the principle of heat and that "sulfuric acid was dephlogisticated sulfur" were involved in Stahl's prediction that the synthesis of phlogiston and sulfuric acid would result in sulfur; the postulate that charcoal is "high in phlogiston" and that inflammable air is pure phlogiston, were used in deriving Priestley's prediction that inflammable air would, like charcoal, turn calx into metal; the prediction that the rate of expansion is the same for all gases was derived from a number of false claims about caloric; etc. (Lyons 2002: 80-81).

But it has been replied that some of those predictions were true only under a charitable interpretation (e.g., by understanding 'phlogiston' as deprivation of oxygen), under which, however, also the claim used in deriving them turns out to be true. Other predictions were actually derived from false claims by chance, but this was possible because they were a priori probable (and we saw that only improbable predictions are evidence for truth). All the other false claims were not actually *essential* in the prediction, because they entailed some weaker and true claims which were enough derived the same prediction (Alai 2014b). Lyons (2006) argued that the essentiality requirement should be dropped, but this would obviously deprive the NMA of its cogency.

8.4 *Semirealism*

Another version of selective realism is Anjan Chakravartty's (1998) *semirealism*. Like entity realism, he holds that we can know the existence of the unobservable entities with which we establish causal interactions; but unlike entity realism, he also grants that we can correctly describe their "detection" properties: "Detection properties are causal properties one has managed to detect; they are causally linked to the regular behavior of our detectors. Auxiliary properties are any other putative properties attributed to particulars by theories" (Chakravartty 2007: 47). So, we should be realist about detection properties, but agnostic about auxiliary properties (see Ivanova (ed.)). Bence Nanay (2013) proposed instead *singularist semirealism*, according to which "science is mostly right, not only about which unobservables exist, but also about their property tokens, but not their property types".

8.5 The *verisimilitude* research program

Two traditional and influential approaches can also be considered as forms of selective realism. One is the "verisimilitude" research program initiated by Popper (Popper 1963; Oddie 1986; Niiniluoto 1987, 1998). Its key idea is that even false theories may be more or less "verisimilar", or "close" to the truth, since they include some true statements, or false statements with some true content. More verisimilar theories have more truth content and/or less false content. For instance, (1) 'All swans are white' and (2) 'All swans are black' are both false, but part of the content of (1) is the entailed statement (3) 'All swans except Australian swans are white', which is true and explains the predictive success of (1) (Musgrave 2006–2007). Also (2) entails a true claim, viz. (4) 'Australian swans are black', but that is weaker than (3), so (1) is more verisimilar than (2). "Approximate truth is a species of partial truth, since the approximations in question are logical parts of what we began with. 'It is 4 o'clock' logically implies 'It is approximately 4 o'clock' as well as 'It is 4 o'clock give or take 5 minutes'" (ibid.). Rescher (1987: Ch. 5) seems to follow a similar line when he distinguishes between forefront science, which is precise and never true, and "schoolbook science", which though vague and imprecise includes the true core of the forefront science.

8.6 The *restricted-domain* approach

The other traditional and influential idea which may be interpreted as a version of partial realism is that theories must not be taken as true for all the phenomena and with absolute precision, but only for certain intended ranges of phenomena, or levels of approximation, or domains of entities, which are defined by the theory itself.[1] Therefore, even when rejected, they remain true within those

[1] See Heisenberg 1955: 20; Agazzi 1969: 361, 368, 372 ff.; Agazzi 2014: 310–311, 403–407; Toraldo di Francia 1976; Dalla Chiara, Toraldo di Francia 1999: 93-96; Dorato 2007: 172, 203-204; Fano, Macchia 2015: 74. According to Kuhn (1962: ch. 9) this idea was "prevailing" in his time.

limits. For instance, “The fact that we can use classical mechanics in creating many machines or for sending rockets into space certainly means that this mechanics is *true of its objects* and therefore ‘tells a true story’ about *certain aspects of reality*” (Agazzi 2014: 310-311).

If taken to an extreme this position might imply that all theories are analytic (Kuhn 1962, ch. 9), or that they are reducible to their empirical claims, or that they don’t describe the actual world, but different worlds of our making, as claimed by Goodman (1978, chs. I, VII); for instance, it might mean that phlogiston theory was true of phlogiston, ether theory was true of ether, etc. On the contrary, theories and laws are intended to be true—period, true of everything. For instance, ‘electrons have negative charge’ is not intended to be true only of electrons, but of everything, for it has the universal form ‘$\forall x(Ex \rightarrow NCx)$’. If the class of intended applications is delimited a priori, it precludes the extension of the theory to new phenomena, depriving it of fecundity and heuristic power; if instead it is delimited a posteriori, following empirical failures, then it is *ad hoc*, and the theory risks to lose its empirical content. In order to restrict the domain of a law we need good reasons, beside an experimental failure; in particular, we need to find a different (specialized) law, which however has a universal scope, and above all must be embedded in a different theory. Moreover, the theories which Einstein called “principles theories” don’t delimit any field of intended applications.

This approach is fine, however, if interpreted as the selective realist thesis that theories which are radically false overall, nonetheless include descriptions - not merely empirical, but theoretical as well - which are approximately true for the certain domains, or aspects of reality, or scales, or levels approximation. There is no delimitation of the intended applications, either a priori or a posteriori, and when a claim is empirically refuted it is declared false. However, it may be found that it was partly true, since it entailed a weaker but true claim. Still, the latter must be supported and explained by a new theory. For example, the assumption that mass is inalterable was proven false—false of everything. However, it entails the true claim that mass is approximately inalterable at low speeds; yet, this limitation cannot be explained by Newton’s theory, but only by Relativity. This is why Newton’s theory is considered false—period. The theory of luminifer ether was intended for all the phenomena of light, but it is true of none. Rather than conceiving theories as completely true of a restricted domain, they may be seen as partially true of a universal domain. Partial truth also explains why different theories can be all true in the same field: they do not concern different realities, but different aspects of reality (Agazzi 2014, 405).

8.7 *Local* realisms?

In view of so many different ways and versions of realism, Magnus and Callender (2004) argued that we shouldn’t look for “wholesale” arguments for realism, but for a case-by-case defense. Saatsi (2016) suggests that realists should give up the idea of a general “recipe ... capable of distilling the trustworthy aspects of a theory, applicable to *any* good, predictively successful mature theory”. Instead, they should settle for an “exemplar realism” which focuses on specific, “local” reasons for realism. All these different recipes shouldn’t be seen as competitors, but “as capturing the different possible ways that a theory can ‘get the world right’” (French, this volume). Yet, why are all these versions forms of realism, what do they have in common? (this was Plato’s problem of the universal). For Saatsi it is the general idea that science is successful because it gets something right about the world, but this needs not be exactly the same in all theories, contexts or disciplines.

We noted that *deployment realism* already is already enough flexible as to which *kind* of components can be right, focussing more on how the particular true claims can be identified. But also different identification criteria have been proposed, and perhaps they are compatible, or suited to different contexts: being essential to derive novel predictions (for deployment realism); being confirmed by indirect but theory-free observation (as explained above); being the “minimally interpreted mathematical parts” of successful theories (Votsis 2011b); being the minimal sub-theories which are presupposed by the successful predictions and not empirically refuted (Peters

(2014); being involved in predictive success, resistant to hostile probing and with outside support (Cordero, this volume); attributing properties that are in principle observable, measurable by distinct independent methods and causally produce the observed data (Ghins, this volume).

Votsis (2011b) and Peters (2014) argue that in order to save the NMA from the MMT we should be able to identify the particular true components of discarded theories *prospectively*, from the authors' point of view. In fact, if we could identify them only retrospectively, as those which are preserved in today's theories, we would beg the question of the cogency of the NMA by assuming that the currently accepted theories are true (see Stanford 2006: Ch. 6).

However, if we could do this for past theories, we should also be able to distinguish in current theories precisely which claims will be preserved forever and which ones will be discarded, which is impossible (Alai 2016: § 3; Nickles, this volume). Yet, realists can still make their point if they can (a) argue *in general* that a successful prediction must be derived from some true assumption, even without being able to pinpoint exactly which one; (b) for any particular successful prediction NP, show that indeed the theory includes certain assumptions $A_1...A_n$ from which NP can be derived, and that for all we know $A_1...A_n$ may be true, because they fulfill the just mentioned criteria; and (c) explain away each putative counterexample of true predictions apparently derived from false assumptions as sketched above.

9. The objectivity question

Thus far we have dealt with the former question dividing realism and antirealism, whether unobservables entities can be known. This question arises from the limits of our senses and cognitive apparatuses. There is however a second question: whether unobservables can be known *objectively*. It arises because knowledge is a function of two arguments, objective reality and the subjective factors which precede and "shape" our experience and conceptions; among these factors are on the one hand the peculiar scope and mode of operation of our senses, and on the other hand the different conceptual schemes, frames of reference, background beliefs, cultural biases, methodological allegiances, technological or environmental conditions of belief formations, etc. Antirealists maintain that these factors distort or completely filter out any objective information about reality, and knowledge is purely subjective.

The objectivity question arises for knowledge in general, not just for science. Illustrious antirealist doctrines are Protagoras' and the skeptics' relativism; Kant's critical idealism; Nietzsche's doctrine that there are no facts, only interpretations (1886: §14, 1901: §540) ; the Sapir-Whorf thesis that language shapes the world (Whorf 1956: 213); post-modern gnoseology (Vattimo, Rovatti 1983; Ferraris 2012). Special arguments have been used to deny the objectivity of scientific knowledge in particular. Foucault (1966) claimed that man is built by human sciences, and Latour (1998) denied that Ramses II was killed by the bacilli of tubercolosis, because they were discovered only in 1882; in the late 1950's and 1960's Hanson, Kuhn and Feyerabend, the "new philosophers of science", held that meanings and experience are thoroughly theory-laden; Putnam (1978b, 1981) rejected "metaphysical realism"; Goodman (1978) claimed that we make the world, or actually, many worlds.

9.1 *Perspectival realism*

Today "perspectivist" philosophers stress that science is always carried out within a "perspective" characterized by a priori factors like the above mentioned ones. Nickles considers "an illusion" the idea that we can gradually eliminate every human perspectival element and finally reach a completely objective science (Nickles forthcoming[a]). However, perspectivists need not be subjectivist, relativist, or antirealist: one can acknowledge the perspectival character of knowledge while recognizing that objective reality plays a role at least as important as subjectivity. In fact, the two are not incompatible nor they limit each other, precisely as each argument of a function determines exactly the values without preventing the other to do the same. Of course, *just* looking at the values we cannot distinguish the two arguments; but in knowledge we are given both the value and the subjective factor, and this gives us the objective factor (Alai 1994: §§ 2, 3.6, 3.7). No wonder that subsequent research in the history and philosophy of science has shown that the radical relativism of the "new philosophers of science" was at best exaggerated, and since1994 Putnam has retracted his antirealism (Putnam 1994: 489-494, 502-506, 516-517; Putnam 2012: chs. 1-4).

Thus it is possible to embrace forms of *perspectival realism* (roughly what Putnam called "sophisticated metaphysical realism"), as also urged by Votsis (2012). Basically, a perspective may determine either (a) the particular aspects of reality that are selected for representation, or (b) how those aspects are represented, or (c) both (Giere 2006: 14, Votsis 2012: 90). (a) is perfectly compatible with realism, and if (b) can be shown to boil down to (a) then we have a realist answer to the objectivity question. Moreover, although the knowledge question and the objectivity question are in principle independent, many take a realist stand on both (e.g., Devitt 1984: 22; Sankey, 2008: 12-18).

Perspectival realism, however should be distinguished from doctrines which are misleadingly called 'realism', like Kant's "empirical realism" or Putnam's "internal realism" (Alai 1989, 1990), for they keep objectivity only by weakening it until it becomes compatible with subjectivism and antirealism (Agazzi 2014: 51-57).

9.2 Agazzi and Dilworth

A truly realist perspectivism, instead, was put forth by Agazzi in (1969). He called it "Gestalt view", a term also used for the "new philosophies of science" (Suppe 1977), but he showed that the a priori features of science highlighted by them could be taken into account without jeopardizing realism. This view has been presented and discussed in depth again in (Agazzi 2014). In a nutshell, each scientific discipline 'clippes out' its objects by particular empirical operations, which characterize its specific point of view, or "Gestalt" (Agazzi 1979: 42-44; 2014: ch. 2, pp. 83, 97, passim). Hence, "one and the same 'thing' can become the object of a new and different science every time a new specific point of view ... is taken on it" (ibi: 84).

Thus we always work from within some particular perspective and on already structured materials; scientific objects are abstract and constructed, but the interactive operations by which they are "clipped out" ensure their reference to independent reality and an objective criterion of truth for claims about them. The intervention of the human subject simply brings to light different aspects of reality. "Under different conditions [and through different operations] reality would manifest itself under different aspects ... but these too would be real" (ibi: 229). Therefore "(a) science attempts to represent a reality independent of science itself…; (b) what science states is an adequate representation of this reality 'as it is'" (ibi: 263).

A similar conception was proposed by Craig Dilworth in (1981). It was called 'perspectivism', it borrowed some of Agazzi's key insights, and in reaction to the new philosophers of science it provided criteria for evaluating scientific progress (1981: 84-88). On Dilworth's perspectivism scientific laws can be true and provide knowledge, while theories are not true or false, but serve the primary aim of science, i.e. *understanding* the laws (2015: 23).

9.3 Giere

New discussions on perspectivism have been spurred by Sosa (1991) and Giere, R. (2006), and the issue 84 (2012) of *Philosophica* collects various essays on them. Giere's "perspectival realism" is meant to provide "a genuine alternative to both objectivist realism and social constructivism…" (2006: 14-15). On this view perspectives play a key role both in scientific observation and in theorization. In each of them perspectives affect scientific outputs in two ways: first, both the human visual system and instruments are sensitive only to some kinds of input. Second, "the output is a function of both the input and the internal constitution of the instrument" (ibi: 14).

As noticed by Votsis (2012), the first way has no bearings on the realism/antirealism discussion, it simply entails that different theories have access to different aspects of reality (like for Agazzi). From the second way, however, Giere infers that science does not describe *objective* features of the world, both as concerns everyday macroscopic objects and theoretical unobservable entities: "truth claims are always relative to a perspective" (2006: 81). "So even the claim that the sky is blue is not an absolutely objective truth. Rather, the sky appears blue to normal human trichromats" (ibi: 123). We can only say: "According to this highly confirmed theory (or reliable instrument), the world seems to be roughly such and such, [but not] 'This theory (or instrument) provides us with a complete and literally correct picture of the world itself'" (ibi: 6). Yet, Giere rejects ontological constructivism, granting that the facts under investigation are objective (ibi: 81-82). Thus, his position resembles Kant's, holding that we cannot observe things in and of themselves, but things-*from-the-perspective of human-visual-apparatus, or things-as-represented-by such-and-such-instrument* (ibi: 43, 56).

However, he faces a dilemma: either the output of each perspective is just an alternative projection of the same content (like different maps of the same territory), but then they are mutually compatible, without any threat to objectivity; or one may be correct and other incorrect, but then this can be decided by empirical evidence, and if it cannot we get just another instance of the general underdetermination problem (Votsis 2012: 101-ff).

9.4 Some latest proposals

A more distinctly realist position is taken by Sosa (1991), followed by Massimi (2012). According to them we can know non-perspectival facts, but we know them perspectively, because the *justification* of our beliefs is perspectival: it "always takes place within an epistemic perspective, including not only first-order beliefs about body *x*, but also beliefs ... about our perceptual system, cognitive faculties, measurement devices, and their reliability as sources of beliefs (Massimi 2012: 40-41; see Sosa: 145, 210, passim). The result is a sort of "perspectival coherentism", which makes justification and knowledge context-relative: "J. J. Thomson was justified to believe [that cathode rays were not electrons] from *his own* epistemic perspective as much as we are justified in not believing [that] from *our own* perspective" (Massimi 2012: 47). But this conception "does not open the door to epistemic relativism of Rortian type or to Kuhnian incommensurability, because it is objective facts that make those beliefs true or false" (ibi: 48). So, it is realist about truth, even if antirealist about knowledge. In (2016) Massimi further tries to show how truth itself can be 'perspectival' while remaining correspondence to objective states of affairs.

Teller's "panperspectival realism" maintains that "the world is too complicated for us to succeed in attaching specific referents to our terms" and "to get things exactly right". However, "our representations tell us about an independent world without securing reference by showing that the world is very like the way it is represented in a range of different, often complementary modeling schemes". Each of these schemes gives us understanding "from one or another 'angle'", so, "though never exact, these representations are of something extra-representational because they present the world modally as going beyond what is represented explicitly" (Teller: 1). Therefore "This counts as knowledge of how the world is (really)", and from this point of view "the theoretical and perceptual are on the same (inexact) footing" (ibi: 10).

In fact, Teller's perspectivism provides a further argument for realism on the knowledge question: since perception is as perspectival as theorization, and the former provides knowledge, so does the latter (ibi: 7). Thus Teller agrees with selective realisms that best theories are true, although not exactly (i.e., not completely) true, and he agrees with structural realism that only structures are objective, while entities are features of our representations.

Interestingly, the discussions on knowledge and those on objectivity converge to the conclusion that both can be achieved, but only partially. In fact, this kind of modest realism is compatible even with another form of perspectivism, Nickles' "nonrealism". According to Nickles (this volume, forthcoming[a]), we don't have sufficient evidence to believe that our best theories are true or nearly true, in the sense of providing us with a complete and final understanding of "what is really going on". A modest realist may agree, granting that probably no accepted theory is finally and completely true, so we are still far from "the whole truth" (if there is one), and perhaps we'll never get there. Yet, such a realist may hold that some partial truths about unobservables have been found by science in the past and they still keep accumulating, probably at an increasing rate.

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REFERENCES

Agazzi, Evandro. 1969. *Temi e problemi di filosofia della fisica.* Milano, Manfredi (2nd ed. Roma: Abete 1974).

Agazzi, Evandro. 1979 Proposta per una nuova caratterizzazione dell'oggettività scientifica. In E. Agazzi, *Problemi di epistemologia contemporanea*, Lanciano: Itinerari, pp. 31-61.

Agazzi, Evandro. 2104. *Scientific Objectivity and its Contexts.* Cham: Springer.

Alai, Mario. 1989. *A Critique of Putnam's Anti-realism.* Ann Arbor, Mi.: U.M.I. Online: http://www.proquest.com/products-services/dissertations/.

Alai, Mario. 1990. L'equivoco del "realismo interno" di Hilary Putnam. *Rivista di Filosofia* LXXXI, 2: 263-290.

Alai, Mario. 1994. *Modi di Conoscere il Mondo.* Milano: Franco Angeli.

Alai, Mario. 2005. Dal realismo scientifico al realismo metafisico. *Hermeneutica*: 167-189.

Alai, Mario. 2006. Science and Non-Observable Reality. In *The Controversial Relationships between Science and Philosophy: A Critical Assessment*, ed. G. Auletta: 211-250. Vatican City: Libreria Editrice Vaticana.

Alai, Mario. 2008. Σώζειν τά φαινόμενα. Realistic and Antirealistic Attitudes in Natural Science. *Isonomia*, http://isonomia.uniurb.it/vecchiaserie/2008alai.pdf .

Alai, Mario. 2009. Realismo scientifico e realismo metafisico. In *Fisica e metafisica*, ed. Mario Alai, *Giornale di Fisica* 50, Suppl.1: 19-27.

Alai, Mario. 2010. Van Fraassen, Observability and Belief. In *New Essays in Logic and Philosophy of Science*, ed. Marcello D'Agostino, Giulio Giorello, Federico Laudisa, Telmo Pievani and Corrado Sinigaglia, 663-675. London: College Publications.

Alai, Mario. 2014a. Novel Predictions and the No Miracle Argument. *Erkenntnis* 79, 2: 297-326.

Alai, Mario. 2014b. Defending Deployment Realism against Alleged Counterexamples. In *Defending Realism. Ontological and Epistemological Investigations*, ed. G. Bonino, G. Jesson, J. Cumpa: 265-290. Boston-Berlin-Munich: De Gruyter.

Alai, Mario. 2014c. Why Antirealists Can't Explain Success. In *Metaphysics and Ontology Without Myths*, ed. F. Bacchini, S. Caputo and M. Dell'Utri: 48-66. Newcastle upon Tyne: Cambridge Scholars Publishing.

Alai, Mario. 2014d. Explaining the Novel Success of Science. *Metascience* 23,1: 125-130.

Alai, Mario. 2016. Resisting the Historical Objections to Realism: Is Doppelt's a Viable Solution? *Synthese*: 1-24, DOI 10.1007/s11229-016-1087-z.

Alai, Mario. Forthcoming. The Metaphysical Scope of the New Realism: Confronting Eco's and Parrini's Strictures". In *New Perspectives on Realism*, ed. L. Taddio, K. W Molìn: 9-31. Milano: Mimesis International.

Alai, Mario, Buzzoni, Marco and Tarozzi, Gino (eds.). 2015. *Science between Truth and Ethical Responsibility*. *Evandro Agazzi in the Contemporary Scientific and Philosophical Debate*. Cham: Springer.

Aronson, Jerrold L., 1984. *A Realist Philosophy of Science*. London: The Macmillan Press, New York: St. Martin's Press.

Bird, Alexander. 2015. Is there meta-scientific knowledge? Against both the no-miracles argument and the pessimistic meta-induction. Draft.

Carnap, Rudolf. 1923. Uber die Aufgabe der Physik und die Andwendung des Grundsatze der Einfachstheit. *Kant-Studien* 28: 90-107.

Carnap, Rudolf. 1928. *Der logische Aufbau der Welt*. Berlin-Schlachtensee: Weltkreis.

Carnap, Rudolf. 1936. Testability and Meaning. *Philosophy of Science* 3.

Celsus, A. C., 1935, *De Medicina*, Prooemium, Loeb Classical Library edition, vol. II., *http://penelope.uchicago.edu/Thayer/L/Roman/Texts/Celsus/Prooemium*.html*.

Chakravartty, Anjan. 1998. Semirealism. *Studies in History and Philosophy of Science Part A* 29 (3): 391-408.

Chakravartty, Anjan. 2007. *A Metaphysics for Scientific Realism: Knowing the Unobservable*. Cambridge: University Press.

Churchland, Paul M. and *Hooker*, Clifford A. (eds.) *1985*. *Images of Science: Essays on Realism and Empiricism*. Chicago: University Press.

Clarke, Steve and Lyons, Timothy D. eds. 2002. *Recent Themes in the Philosophy of Science. Scientific Realism and Commonsense*. Dordrecht: Kluwer.

Cruse, Pierre, and Papineau, David. 2002. Scientific Realism Without Reference. In *The Problem of Realism*, ed. Michele Marsonet: 174-189. Aldershot-Burlington: Ashgate.

Dalla Chiara, M. Luisa and Toraldo di Francia, Giuliano. 1999. *Introduzione alla filosofia della scienza*. Roma-Bari: Laterza.

Demopoulos, William, and Friedman, Michael. 1985. Critical notice: Bertrand Russell's *The Analysis of Matter*: its Historical Context and Contemporary Interest. *Philosophy of Science* 52: 621–639.

Devitt, Michael. 1984. *Realism and Truth*. Oxford: Basil Balckwell.

Dilworth, Craig. 1981. *Scientific Progress*. Dordrecht: Kluwer (Springer 2008[4ed.]).

Dilworth, Craig. 2015. The Perspectivist Conception of Science. In Alai, Buzzoni, and Tarozzi, (eds.) 2015: 21-25. Cham: Springer.

Doppelt, Gerald. 2007. Reconstructing Scientific Realism to Rebut the Pessimistic Meta-induction. *Philosophy of Science*, 74: 96–118.

Doppelt, Gerald. 2011. From Standard Scientific Realism and Structural Realism to Best Current Theory Realism. *Journal for General Philosophy of Science*, 42: 295–316.

Doppelt, Gerald. 2013. Explaining the success of science: Kuhn and scientific realists. *Topoi*, 32: 43–51.

Doppelt, Gerald. 2014. Best Theory Scientific Realism. *European Journal for Philosophy of Science*, 4: 271–291.

Dorato, Mauro. 2007. *Cosa c'entra l'anima con gli atomi*? Roma-Bari: Laterza.

Duhem, Pierre. 1906. *La théorie physique. Son objet et sa structure*. Paris: Rivière.

Fahrbach, Ludwig. 2011. Theory Change and Degrees of Success. *Philosophy of Science*, 78: 1283–1292.

Fano, Vincenzo. 2005. *Comprendere la scienza. Un'introduzione all'epistemologia delle scienze naturali*, Napoli: Liguori.

Fano, Vincenzo and Macchia, Giovanni. 2015. Scientific Progress. In Alai, Buzzoni, and Tarozzi, (eds.) 2015: 65-80. Cham: Springer.

Ferraris, Maurizio. 2012. *Manifesto del nuovo realismo*, Roma-Bari: Laterza.

Fine, Arthur. 1984. The Natural Ontological Attitude. In J. Leplin (ed.) 1984: 83-107.

Fine, Arthur. 1986. Unnatural Attitudes: Realist and Instrumentalist Attachments to Science”, *Mind* 95: 149.
Fine, Arthur. 1991. Piecemeal Realism. *Philosophical Studies* 61: 79.
Foucault, Michel. 1966. *Les mots et les choses.* Paris: Gallimard 1966.
French, Steven. 2011. Metaphysical underdetermination: Why Worry? *Synthese* 180, 2: 205-221
French, Steven**.** 2014. *The Structure of the World.* Oxford: University Press.
French, Steven and Ladyman, James. 2003a. Remodelling Structural Realism: Quantum Physics and the Metaphysics of Structure. *Synthese* 136: 31-56.
French, Steven and Ladyman, James. 2003b, The Dissolution of Objects: between Platonism and Phenomenalism. *Synthese* 136: 73-77.
French, Steven and Ladyman, James. 2011. In Defence of Ontic Structural Realism. In *Scientific Structuralism,* ed. Alisa Bokulich and Peter Bokulich: 25-42. Dordrecht: Springer.
Friedman. Michael. 1983. *Foundations of Space-Time Theories,* Chicago: University Press.
Frigg, Roman and Votsis, Ioannis. 2011. Everything you Always Wanted to Know about Structural Realism but Were Afraid to Ask. *European Journal for the Philosophy of Science,* 1 (2): 227-276.
Giere, Ronald N. 1988. *Explaining Science: A Cognitive Approach*. Chicago: University Press.
Giere, Ronald N. 2006. *Scientific Perspectivism*. Chicago: University Press.
Glymour, Clark 1980. *Theory and Evidence*. Princeton, NJ.: University Press.
Goodman, Nelson. 1978. *Ways of Worldmaking*. Indianapolis-Cambridge: Hackett.
Hacking, Ian. 1983. *Representing and Intervening*. Cambridge: University Press.
Hardin, Clyde L., and Rosenberg, Alexander. (1982). In Defence of Convergent Realism. *Philosophy of Science,* 49: 604–615.
Heisenberg, Werner. 1955. *Das Naturbild der heutigen Physik*. Hamburg: Rohwolt.
Hempel, Carl G. 1952. *Fundamentals of Concept Formation in Empirical Science*. Chicago: University of Chicago Press.
Hempel, Carl.G. 1958. The Theoretician’s Dilemma. *Minnesota Studies in the Philosophy of Science* 2: 37-98.
Howson, Colin. 2000. *Hume's Problem: Induction and the Justification of Belief*. Oxford: Oxford University Press.
Hoyningen-Huene, Paul. 2011. Reconsidering the Miracle Argument on the Supposition of Transient Underdetermination. *Synthese* 180: 173-187.
Ivanova, Milena (ed.) Scientific Semi-Realism. *PhilPapers*. http://philpapers.org/browse/scientific-semirealism.
Kincaid, Harold, 2008. Structural Realism and the Social Sciences. *Philosophy of Science*, 75: 720–731.
Kosso, Peter. 1992. *Reading the Book of Nature. An Introduction to the Philosophy of Science,* Cambridge: University Press.
Kitcher, Philip. 1993, *The Advancement of Science,* Oxford: University Press.
Kuhn, Thomas S. 1962. *The Structure of Scientific Theories*. Chicago: The University Press.
Ladyman, James. 1998. What is Structural Realism? *Studies in History and Philosophy of Science* 29: 409–24.
Ladyman, James. 2002. *Understanding Philosophy of Science*, London, New York: Routledge.
Ladyman, James. 2011. Structural Realism versus Standard Scientific Realism: the Case of Phlogiston and Dephlogisticated Air. *Synthese* 180: 87–101.
Ladyman, James. 2014. Structural Realism. *Stanford Encyclopedia of Philosophy*. http://plato.stanford.edu/entries/structural-realism/
Ladyman, James and Ross, Don, with Spurrett, David and Collier, John 2007. *Every Thing Must Go: Metaphysics Naturalised*. Oxford: University Press.
Lange, Marc. 2002. Baseball, Pessimistic Inductions and the Turnover Fallacy. *Analysis*, 62: 281–285.

Latour, Bruno. 1998. Ramses II est-il mort de la tubercolose? *La Recherche*.
Laudan, Larry. 1981. A Confutation of Convergent Realism. *Philosophy of Science* 48: 19-49. Repr. in D. Papineau (ed.) 1996: 107-138.
Laudan, Larry. 1984a. Explaining the Success of Science. In *Science and Reality: Recent Work in the Philosophy of Science*, ed. James T. Cushing, Cornelius F. Delaney and Gary M. Gutting: 83–105. Notre Dame: University Press.
Laudan, Larry. 1984b. Discussion: Realism without the Real. *Philosophy of Science*, 51: 156–162.
Laudan, Larry. 1996. *Beyond Positivism and Relativism*. Boulder, Co.: Westview Press.
Laudan, Larry, Leplin, Jarrett. 1991. Empirical Equivalence and Underdetermination. *Journal of Philosophy* 88: 449-472.
Laudan, Larry, Leplin, Jarrett. 1993. Determination Underdeterred: Reply to Kukla. *Analysis* 53: 8-16.
Leplin, Jarrett (ed.). 1984. *Scientific Realism*, Berkeley: University of California Press.
Leplin, Jarrett. 1987. Surrealism. *Mind* 96: 519.
Lewis, Peter. 2001. Why the Pessimistic Induction is a Fallacy. *Synthese*, 129: 371–380.
Lipton, Peter. 1991. *Inference to the Best Explanation*. London: Routledge (2004[2]).
Lyons, Timothy D. 2002. The Pessimistic Meta-Modus Tollens. In Clarke, Lyons (eds.): 63-90.
Lyons, Timothy D. 2006. Scientific Realism and the Stratagema de Divide et Impera *Brit. J. Phil. Sci*. 57: 537–560.
Lyre, Holger. 2011. Is Structural Underdetermination Possible? *Synthese* 180, 2**:** 235–247.
Magnus, P.D. and Callender, Craig. 2004. Realist Ennui and the Base Rate Fallacy. *Philosophy of Science* 71 (3): 320-338.
Massimi, Michela. 2012. Scientific Perspectivism and its Foes. *Philosophica* 84: 25–52.
Massimi, Michela. 2016. Four Kinds of Perspectival Truth. *Philosophy and Phenomenological Research.* doi:10.1111/phpr.12300.
Maxwell, Grover.1970. Theories, Perception, and Structural Realism. In *Nature and Function of Scientific Theories*, ed. R. Colodny: 3-34. Pittsburgh: University Press.
McMullin, Eranan. 1984. A Case for Scientific Realism. In Leplin (ed.): 8-40.
Muller, Frederick A. 2011. Withering Away, Weakly. *Synthese* 180, 2**:** 223-233.
Musgrave, Alan. 1985. Realism vs. Constructive Empiricism. In Churchland and Hooker (eds.) 1985: 197-221.
Musgrave, Alan. 1988.The Ultimate Argument for Scientific Realism. In *Realism and Relativism in Science*, ed. R. Nola: 229-252. Dordrecht: Kluwer.
Musgrave, Alan. 2006-7. The 'Miracle Argument' for Scientific Realism. *The Rutherford Journal* 2. http://www.rutherfordjournal.org/article020108.html
Nanay, Bence. 2013. Singularist Semirealism. *British Journal for the Philosophy of Science* 64 (2): 371-394.
Nanay, Bence. 2016. Entity Realism and Singularist Semirealism. *Synthese* online. DOI 10.1007/s11229-016-1179-9
Nickles, Thomas. Forthcoming[a]. Perspectivism versus Copernicanism. *Axiomathes*.
Nickles, Thomas. Forthcoming[b]. Strong Realism as Scientism: Are We at the End of History? In *Science Unlimited? The Challenges of Scientism*, ed. M. Boudry, M. Pigliucci. Chicago: University Press.
Nietzsche, Friedrich. 1886. *Jenseits von Gut und Böse. Vorspiel einer Philosophie der Zukunft.* Leipzig: C. G. Naumann.
Nietzsche, Friedrich. 1901. *Der Wille zur Macht. Versuch einer Umwerthung aller Werthe.* hrsg. v. Ernst Horneffer, August Horneffer und Peter Gast. Leipzig: C. G. Naumann.
Niiniluoto, Ilkka. 1987. *Truthlikeness*, Dordrecht: Reidel.
Niiniluoto, Ilkka. 1998. Verisimilitude: the Third Period. *The British Journal for the Philosophy of Science* 49.1: 1–29.
Niiniluoto, Ilkka. 1999. *Critical Scientific Realism*, Oxford: University Press.

Nola, Robert. 2002. Realism Through Manipulation. In Clarke, Lyons (eds.): 1-23.
Oddie, Graham. 1986, *Likeness to Truth*, Dordrecht: Reidel.
Papineau, David (ed.). 1996.*The Philosophy of Science*. Oxford: University Press.
Perrin, Jean. 1913. *Les atomes*. Paris: Alcan.
Peters, Dean. 2014. What Elements of Successful Scientific Theories are the Correct Targets for “Selective” Scientific Realism? *Philosophy of Science* 81: 377–397.
Poincaré, Henry. 1902. *La science et l’hypothèse*. Paris: Flammarion. Engl. transl. *Science and Hypothesis*. New York: Dover Publications 1952.
Popper, Karl R. 1963. *Conjectures and Refutations: The Growth of Scientific Knowledge.* London: Routledge and Kegan Paul.
Psillos, Stathis. 1999. *Scientific Realism: How Science* Tracks Truth. *London-New York: Routledge.*
Psillos, Stathis. 2011a. Choosing the Realist Framework. *Synthese* 190: 301-316.
Psillos, Stathis. 2011b. The Scope and Limits of the No-Miracles Argument. In *The Philosophy of Science in a European Perspective*, ed. Friedrich Stadler et al. Vol. II:23-35, Cham: Springer.
Putnam, Hilary. 1975a. *Mathematics, Matter and Method. Philosophical Papers vol.1*. Cambridge: University Press.
Putnam, Hilary. 1975b. *Philosophical Papers*, Vol. II: *Mind Language and Reality*. Cambridge: University Press.
Putnam, Hilary. 1975c. The Meaning of ‘Meaning’. In Putnam 1975b: 215-271.
Putnam, Hilary. 1975d. Language and reality. In Putnam 1975b: 272-290.
Putnam, Hilary, 1978a. *Meaning and the Moral Sciences*, Boston-London-Henley: Routledge & Kegan Paul.
Putnam, Hilary, 1978b. Realism and Reason. In Putnam (1978a): 123-140.
Putnam, Hilary. 1978c. Equivalenza. In *Enciclopedia Einaudi*, vol. V: 547-564. Torino: Einaudi. Engl. transl.: Equivalence. In Putnam 1983: 26-45.
Putnam, Hilary. 1981. *Reason, Truth and History*. Cambridge: University Press.
Putnam, Hilary. 1994. Sense, Nonsense, and the Senses: an Inquiry into the Powers of the Human Mind. *The Journal of Philosophy* XCI, n. 9, 445-517.
Putnam, Hilary. 2012. *Philosophy in an Age of Science*. Cambridge, Mass.- London: Harvard University Press
Quine, Willard V.O. 1951. Two Dogmas of Empiricism. *Philosophical Review* 60: 20-43.
Rees, Paul. 2012. *A Critique of The Arguments for Scientific Realism*. Reading: Cranmore Publications.
Rescher, Nicholas. 1987. *Scientific Realism: A Critical Reappraisal*. Dordrecht: Reidel.
Russell, Bertrand.1927. *The Analysis of Matter*. London: Routledge Kegan Paul.
Saatsi, Juha. 2016. Replacing Recipe Realism. *Synthese*, First Online, 2016. DOI:10.1007/s11229-015-0962-3.
Sankey, Howard. 2008. *Scientific Realism and the Rationality of Science*. Aldershot and Burlington, VT: Ashgate.
Schlick, Moritz.1938. *Form and Content. An Introduction to Philosophical Thinking*. In Schlick, *Gesammelte Aufsätze* 1926-1936, ed. F. Waisman. Wien: Gerold.
Schurz, Gerhard. 2011. Structural Correspondence, Indirect Reference, and Partial Truth: Phlogiston Theory and Newtonian Mechanics. *Synthese* 180: 79-85.
Smart, John J.C. 1963. *Philosophy and Scientific Realism*, London: Routledge & Kegan Paul.
Smart, John J.C. 1968. *Between Science and Philosophy*: New York: Random House.
Sosa, Ernest. 1991. *Knowledge in Perspective. Selected Essays in Epistemology*. Cambridge: University Press.
Stanford, Kyle P. 2000. An Antirealist Explanation of the Success of Science. *Philosophy of Science* 67: 266-284.

Stanford, Kyle P. 2006. *Exceeding Our Grasp. Science, History, and the Problem of Unconceived Alternatives*. Oxford: University Press.
Stanford, Kyle P. 2013. Underdetermination of Scientific Theory. *Stanford Encyclopedia of Philosophy*. http://plato.stanford.edu/entries/scientific-underdetermination/
Suppe, Frederick.1977. *The Structure of Scientific Theories*. Urbana, Ill.: University of Illinois Press.
Teller, Paul R. Pan-Perspectival Realism Explained and Defended. http://philosophy.ucdavis.edu/people/fzteller/site/truth-page .
Toraldo di Francia, Giuliano. 1976. *L' indagine del mondo fisico*. Torino: Einaudi.
Van Fraassen, Bas C. 1980. *The Scientific Image*. Oxford: Clarendon Press.
Van Fraassen, Bas C. 1985. Empiricism in the Philosophy of Science. In *Churchland* and *Hooker* (eds.) 1985: 245-307.
Van Fraassen, Bas C. 2002. *The Empirical Stance*, Yale: Univerity Press.
Vattimo, Gianni and Rovatti, Pier A. (eds.). 1983. Il pensiero debole. Milano: Feltrinelli.
Votsis, Ioannis. 2011a. Saving the Intuitions: Polylithic Reference. *Synthese* 180: 121-137
Votsis, Ioannis. 2011b. The Prospective Stance in Realism. *Philosophy of Science* 78:1223–1234.
Votsis, Ioannis. 2012. Putting Realism in Perspectivism. *Philosophica* 84: 85–122.
White, Roger. 2003. The Epistemic Advantage of Prediction over Accommodation. *Mind* 112 (448): 653.
Whorf, Benjamin Lee. 1956, *Language, Thought, and Reality*, Cambridge: MIT Press.
Wigner, Eugene P. 1967. Remarks on the Mind-Body Question. In Wigner, *Symmetries and Reflections*. Bloomington, In.: Indiana University Press.
Worrall, John. 1989. Structural Realism: The Best of Both Worlds? *Dialectica* 43:99-124.
Worrall, John. 1994. How to Remain (Reasonably) Optimistic: Scientific Realism and the 'Luminiferous Ether'. *PSA* 1994. vol. 1., ed. M. Forbes and D. Hull: 334-342. East Lansing, Mi.: Philosophy of Science Association.
Worrall, John. 1995. Il realismo scientifico e l'etere luminifero. In *Realismo/antirealismo*, ed. Alessandro Pagnini: 167-203. Firenze : La Nuova Italia.
Worrall, John. 2011. Underdetermination, Realism and Empirical Equivalence. *Synthese* 180, 2: 157–172.
Wright, John. 2002. Some Surprising Phenomena and Some Unsatisfactory Explanations of Them. In Clarke and Lyons (eds.): 139-153.
Wright, John. 2013. *Explaining Science's Success. Understanding How Scientific Knowledge Works*. Durham: Acumen.