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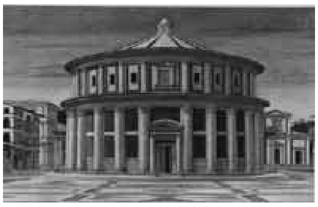

“Σώζειν τά φαινόμενα”

Realistic and antirealistic attitudes in natural science*

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Abstract

Throughout history scientists and philosophers have discussed whether the theoretical descriptions of unobservable entities and facts were credible or not. The problem, I claim, is not due to an epistemic boundary demarcating the observable from the unobservable, but to methodological problems concerning theories, both local (as incompatibility with background theories, or lack of independent evidence) and global (as empirical underdetermination and basic human fallibility). This is seen by reviewing Celsus’ account of controversies in Hellenistic medicine, selected contemporary sources on ancient, medieval and modern cosmology, some outlines of developments in atomistic theory, wave theories, classical and relativistic mechanics, and the most recent debates on scientific revolutions.

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1. Introduction

Since its beginning, science tried to make the most from sensory experience employing *reasoning* to collect, compare, organize and synthesise empirical data. Even more importantly, reasoning also directs our search for new observational data[1] and extends our belief beyond actual observations in two ways: by *explaining* them and by inferring from them to non observed facts. Of course, we do the same in ordinary everyday knowledge, but science begins when this is done systematically, purposefully and critically. Nonetheless, the extension of our beliefs by *reasoning* has always been challenged by radical empiricists, like the ancient sceptics and Hume, who held that since observation is our only source of information on the external world, no belief can be warranted except by observation, and reasoning cannot extend the range of our warranted beliefs.

But this position is considered too radical by many empirically oriented philosophers and scientists, for in daily cognitive practices we confidently infer from observed to unobserved facts, for instance by analogy, trusting in the uniformity of nature, or through the cause-effect relation. In all ages, therefore, some have taken the compromise view that inferences to unobserved facts may be warranted as long as they concern ordinary physical objects and properties, but not if they concern unobservable entities unknown to commonsense and postulated by scientific theories[2]. Thus, the epistemological question of which role, if any, reason can play in extending and warranting our beliefs is turned into a question of philosophy of science, whether belief in the theoretical entities of science can be warranted or not. Throughout history, as we shall briefly see, there have been *scientific realists*, answering 'yes', and *scientific antirealists*, answering 'no' to this question. The dispute, however, has become all the more important in recent times, as theoretical entities are assuming an ever more decisive role in contemporary science.

Van Fraassen (1980, § 2) has provided an epistemological foundation for the scientific antirealist attitude by claiming that beliefs in unobserved facts may be warranted only if they are directly observable by humans (i.e., if they could be observed by unaided human senses, in favourable conditions)[3]. However, if we hold the extreme position that actual observation is a necessary condition for warranted beliefs, there is no reason to make an exception, believing in certain non observed facts, only because they concern entities of a particular physical structure and size (i.e., the structure or size that make them directly observable to us: say, being larger than 0,01 cm., or it reflecting light in the visible spectre). On the other hand, if there are inferential patterns warranting our beliefs even beyond actual observation, why should they work only for entities within certain sizes and with a particular physical structure[4]? In fact, we shall see that in many historical circumstances scientific antirealist doubts on certain facts or entities did not depend on their unobservable character, but on the lack of proper or sufficient evidence, both empirical or theoretical, concerning them.

2. Hellenistic medicine: the *rationalists*

From the very beginning of western medicine physicians tried to reason about supposed unobservable causes of observable symptoms in order to *explain* and *cure* them, and this raised the question whether and to which extent such reasoning could be warranted. This debate was so conscious and articulate already in the Alexandrine age, that the account Celsus gave of it in *De Medicina* (I century A.D.) makes for a small treatise of scientific method, with some interesting anticipations of today's arguments. In the *Prooemium* of this work, Celsus distinguishes two opposite positions: the supporters of the role of reasoning (*rationalists,* or scientific realists, as we would say), and those who rely exclusively on observation («*empirici*», or scientific antirealists):

> There is a primary difference of opinion, some holding that the sole knowledge necessary is derived from experience, others propounding that practice is not efficient enough except after acquiring a reasoned knowledge of human bodies and of nature […] *13* They, then, who profess a reasoned theory of medicine, propound as requisites, first, a knowledge of *hidden causes* involving diseases, next, of evident causes, after these of natural actions also, and lastly of the internal parts. *14* They term hidden, the causes concerning which inquiry is made into the principles composing our bodies, what makes for and what against health.[5]

Interestingly, the same term 'hidden' used here, today is applied (often with a derogative connotation) to the unobservable factors postulated by realists interpretations of quantum mechanics in the spirit of Einstein, Podolski, Rosen (1935). Greek physicians were not interested in etiology *per se*, but merely for therapeutic purposes, so medicine was more a technique than a science (as in part it is still today); however, the rationalists believed that the search for hidden causes of diseases is necessary in order to achieve what we now consider as the two main goals of science, beyond description: prediction (which in this case amounts to therapy) and explanation:

> For they believe it impossible for one who is ignorant of the origin of diseases to learn how to treat them suitably. They say that it does not admit of doubt *that there is need for differences in treatment*, if, as certain of the professors of philosophy have stated, some excess, or some deficiency, among the four elements, creates adverse health; *15* or, if all the fault is in the humours, as was the view of Herophilus; or in the breath, according to Hippocrates; or if blood is transfused into those blood-vessels which are fitted for pneuma, and excites inflammation which the Greeks term φλεγμόνη, and that inflammation effects such a disturbance as there is in fever, which was taught by Erasistratus; *16* or if little bodies by being brought to a standstill in passing through invisible pores block the passage, as Asclepiades contended—his will be the right way of treatment, who has not failed to see the primary origin of the cause.

Alexandrine rationalist physicians seem to have practiced a sort of hypothetico-deductive method, whereby observation is coupled to reasoning as *a posteriori* test of hypotheses; they also entertained considerations analogous to those by which Popper (1935, 30) held that observation and experiment need to be directed by theory:

> They do not deny that experience is also necessary; but they say it is impossible to arrive at what should be done unless through some course of reasoning. *17* For the older men, they say, did not cram the sick anyhow, but reasoned out what might be especially suitable, and then put to the test of experience what conjecture of a sort had previously led up to.

Moreover, they used a variant of one of today's most powerful arguments for realism, the so called *no miracle argument*[6]: if event A is similar to event B, of which we already know the effects (the cause), the effect (the cause) of A can be predicted merely on the basis of past observation and reliance on the uniformity of nature. But if there is nothing similar to A of which we know the effects (the cause), predicting the effects (the cause) of A is a genuinely *novel* prediction, and requires assumptions which transcend past (or even *possible*) observation. Further, if such a prediction succeeds, it strongly confirms those assumptions: for it would be a miraculous coincidence if true unsuspected consequences could be drawn from false hypotheses:

> Again they say that it makes no matter whether by now most remedies have been well explored already. . . Frequently, too, novel classes of disease occur about which hitherto practice has disclosed nothing, and so it is necessary to consider how such have commenced, without which no one among mortals can possibly find out whether this rather than that remedy should be used; this is the reason why they investigate the occult causes.

But explanation, even more than prediction, is what calls for a realist attitude toward theories: for it is at least in principle possible that a false licence a true prediction, while one cannot hold that a theory is the actual explanation of a given phenomenon without holding that it is true. In fact, all scientific antirealists deny that explanation in terms of unobservable facts is possible and/or desirable. On the opposite, rationalist physicians

> inquire *why* our blood-vessels now subside, now swell up; what is the *explanation* of sleep and wakefulness […] *20* Among these natural actions digestion seems of most importance, so they give it their chief attention. Some following Erasistratus hold that in the belly the food is ground up; others, following Plistonicus, a pupil of Praxagoras, that it putrefies; others believe with Hippocrates, that food is cooked up by heat. In addition there are the followers of Asclepiades, who propound that all such notions are vain and superfluous, that there is no concoction at all, but that material is transmitted through the body, crude as swallowed.

Finally, in order to find out about the «hidden» causes of diseases, they resorted to autopsy and vivisection:

> *23* Moreover, as pains, and also various kinds of diseases, arise in the more internal parts, they hold that no one can apply remedies for these who is ignorant about the parts themselves; hence it becomes necessary to lay open the bodies of the dead and to scrutinize their viscera and intestines. They hold that Herophilus did this in the best way by far, when they laid open men whilst alive—criminals received out of prison from the kings—*24* and while these were still breathing, observed parts which beforehand nature had concealed, their position, colour, shape, size, arrangement, hardness, softness, smoothness, relation, processes and depressions of each, and whether any part is inserted into or is received into another.

3. Hellenistic medicine: the *empirici*

> *27* On the other hand, those who are called «*empirici*» because they have experience, do indeed accept *evident causes* as necessary; but they contend that inquiry about *obscure causes* and natural actions is *superfluous*, *because nature is not to be comprehended*.

This is to say that even Hellenistic antirealists were ready to infer from what is observed to what is not observed, as long as it is observable (an «evident cause»), but not to what is unobservable (an «obscure cause»). Their argument (the search for hidden causes is superfluous *because* it is impossible) resembles the reasoning of Aesop's fox (who, unable to get to the grape, concluded that it wasn't ripe, yet). But perhaps, they meant that inquiry into hidden causes was useless since it was hopeless: not that if, counterfactually, it could succeed, its result would not be useful. Even for Galileo the search for the «essence» (the deep nature of things) was «*vana*» (useless) because «impossible», not because its successes could not, counterfactually, be extremely useful[7]. At any rate, in order to show that hidden causes cannot be found, they anticipated quite a number of today's antirealist arguments. For instance, they argued, there are wide theoretical disagreements among practitioners and theoreticians, which cannot be settled either by reasoning or by observation. Not by reasoning, since it only yields inner consistency, which does not warrant truth nor therapeutic success. And they cannot be settled by observation, because different and mutually incompatible theories may entail the same empirical consequences, and so enjoy equal practical success (an argument known today as *argument from the empirical underdetermination of theories*):

> *28* That nature cannot be comprehended is in fact patent […] from the disagreement among those who discuss such matters; for on this question there is no agreement, either among professors of philosophy or among actual medical practitioners […] *29* If one wants to be guided by reasoning, they go on, the reasoning of all of them can appear not improbable; if by method of treatment, all of them have restored sick folk to health [...] Even philosophers would have become the greatest of medical practitioners, if reasoning from theory could have made them so; as it is, they have words in plenty, and no knowledge of healing at all.

Of course, we now know that all the theories about hidden causes held at the time were wrong. However, the *empirici* could not assume this without circularity, while apparently the rationalists were able to draw the right consequences from the wrong premises (obviously, by *a posteriori* accommodation). In fact, the *empirici* granted that all theories were successful, but held for sure that all (perhaps all but one) were false, since they were incompatible. As for those unable even to accommodate their theory enough to make it practically efficacious, quite reasonably they considered them not as physician, but as philosophers. The *empirici* also argued that

> *30* […] the methods of practice differ according to the nature of localities, and that one method is required in Rome, another in Egypt, another in Gaul; but that if the causes which produce diseases were everywhere the same, the same remedies should be used everywhere […].

In a quite similar vein, at our days, Nancy Cartwright (1983) has claimed that there are no deep theoretical laws of physics, since by postulating them we are not able to predict effects with precision and regularity. In fact, the *empirici* were well aware that in the actual world causal relations are mediated by such a complex array of contingent factors that often the exact effect cannot be predicted even for apparent causes; much less, therefore, can we infer from observed phenomena to their supposed hidden causes:

> […] often, too, the causes are apparent, as, for example, of ophthalmia, or of wounds, yet such causes do not disclose the treatment: *31* […] if the evident cause does not supply the knowledge, much less can a cause which is in doubt yield it.

In any case, medicine cannot be deduced *a priori*: initially it was merely based on observation and analogy, and only after physicians empirically found the remedies of diseases they sought to discover their causes by *abduction* (i.e., reasoning back from effects to causes):

> *33* Even in its beginnings […] the Art of Medicine was not deduced from [reasoning], but from experience; for of the sick who were without doctors, some in the first days of illness, longing for food, took it forthwith; others, owing to distaste, abstained; and the illness was more alleviated in those who abstained. […] etc. When this and the like happened day after day, careful men noted what generally answered the better, and then began to prescribe the same for their patients. […] *36* It was afterwards […] when the remedies had already been discovered, that men began to discuss the reasons for them: the Art of Medicine was not a discovery following upon reasoning, but after the discovery of the remedy, the reason for it was sought out.

In fact, since what really matters in medicine is not explaining diseases, but finding the right therapy, the most straightforward method is inferring by analogy from what is observed to what is not observed, but observable: once observed that symptom A is relieved by remedy B, in presence of a similar symptom A' we assume that it will be relieved by a similar remedy B', without any need to conjecture hidden causes:

> *37* For even if there happened nowadays some unknown form of malady, nevertheless the practitioner had not to theorize over obscure matters, but straightway would see to which disease it came nearest, then would make trial of remedies similar to those which have succeeded […] it does not matter what produces the disease but what relieves it; nor does it matter how digestion takes place, but what is best digested, whether concoction comes about from this cause or that, and whether the process is concoction or merely distribution. *39* We have no need to inquire in what way we breathe, but what relieves laboured breathing; not what may move the blood-vessels, but what the various kinds of movements signify. All this was to be learnt through experiences.

Thus, the *empirici* held an instrumentalistic view, according to which medicine has a merely pragmatic value. Moreover, their argument is quite similar to what Hempel (1958) calls «the theoretician's dilemma»: since the purpose of science is merely practical, the theoretician's task is to find a network of theoretical concepts and laws T such that there is a function F_1 from observed phenomena to T, and another function F_2 from T to future observable phenomena, so that the latter may be reliably predicted on the basis of past observed phenomena. Now, if theoretical concepts and laws do not allow this, they are useless, but if they allow this, they are also useless: for it means that there exists a function F_3 directly from observed phenomena to future observable phenomena (it is the compound function $F_1 \circ F_2$, whose arguments are the arguments of F_1, and whose values are the values of F_2). Now, F_3 is all we need, so we can dispense with F_1 and F_2, hence also with T, which is useless in any case.

Nowadays, however, we know that the theoretical apparatus is not eliminable, as the dilemma presupposes: Carnap has shown that theoretical concepts cannot be completely defined in observational terms, even in simple cases as *dispositional* terms (such as 'soluble' and 'fragile') or the irrational values of physical measurements[8]. Moreover, Hempel (1958) himself points out that only theoretical concepts allow theories to systematize experience, predict future observations, and be open to the discovery of new properties. In fact, even if in principle (by Craig's theorem) for any theory T involving non-observational concepts there is another theory T' with the same empirical consequences of T, but no non-observational concepts, T' consists of an *infinite* number of axioms[9], and the mathematical function connecting its consequences to possible observations would be too complex to be grasped by humans[10].

Because of their disinterest for causes, Hellenistic antirealist physicians rejected not only «superfluous» theorizing, but even autopsy and vivisection, holding that it was

> cruel as well, to cut into the belly and chest of men whilst still alive, and to impose upon the Art which presides over human safety someone's death, and that too in the most atrocious way. Especially is this true when, of things which are sought for with so much violence, some can be learnt not at all, others can be learnt even without a crime. *41* For when the body had been laid open, colour, smoothness, softness, hardness and all similars would not be such as they were when the body was untouched […] Nor is anything more foolish […] than to suppose that whatever the condition of the part of a man's body in life, it will also be the same when he is dying, nay, when he is already dead […].

With the first point made here, concerning what today are known as *ethical constraints* upon research, we obviously agree. The second point claims that experiment, as an attempt to manipulate nature in order to learn something beyond what mere observation can teach us, is bound to fail, since it *perturbs*—or even destroy—the very properties we are trying to learn about. Or put more generally, that it is impossible to disentangle the various factors which together produce an observable effect, in order to know the individual role of each of them. Actually, modern science was born in the XVII century on the very rejection of this point, i.e., on the presupposition that in measuring a given property it is possible to abstract from the effect of our own action on the measured property. On the other hand, Heisenberg and many others gave a *perturbative* reading of the indeterminacy principle of quantum mechanics, claiming as a consequence that the main difference between classical and contemporary physics is that the latter is once again characterized by the kind of impossibility put forward by the *empirici*. Now, although the perturbative reading is questionable if assumed as a general epistemological principle, it holds at least as a consequence of the quantum theory for quantum objects: see (Fano 2002).

It is interesting to notice that while some of the methodological questions and principles debated by Hellenistic physicians are still relevant to us, the very problem they disagreed about has lost any interest: nobody questions anymore that medicine should discover *hidden causes*. Why? One would say, because they are no longer *hidden*, since we can investigate them through microscopes, chemical analyses, X-rays, etc. A contemporary antirealist like van Fraassen would claim that since we cannot see them directly by our eyes, the causes of diseases are still *unobservable*, so we should not make conjectures about their reality; but this only shows that he is no longer dealing with a problem in philosophy or methodology of science, but with the epistemological problem whether beliefs concerning unobservable facts can be warranted, and what is to be considered as observable. That the *empirici* were not after van Fraassen's problem is also clear from the fact that they had no interest in autopsy or vivisection: for what is laid open by autopsy for van Fraassen counts as observable, hence as a legitimate object of beliefs. Instead, what mattered for them was not being observable or not, but being actually observed or not. Moreover, they thought medicine did not need to inquire the causes of diseases, even when they could be observed by autopsy or vivisection, since observation and analogy were enough to establish a reliable match of symptoms to remedies, and this was all they needed.

4. Ancient and medieval cosmology

According to the Pithagoreans, the Earth was a sphere (a perfect solid), and all the heavenly bodies moved around the Earth along spherical surfaces. In the *Timaeus* (XI, 38c-39e), Plato describes the solar system, explaining that the fixed stars, the Sun and the Moon appear to move around the Earth in smooth circles, while Mercury, Venus, Mars, Jupiter and Saturn (*planets*, i.e., wandering bodies) appear to move in a complex irregular way (including, for instance, retrograde motions). Moreover, the Moon appears to orbit just above the Earth, the Sun next to it, and Mercury and Venus on the same orbit and with the same velocity as the Sun, but in the opposite sense. Mars, Jupiter and Saturn, instead, have more complex motions, which Plato declines to describe. (All this is indeed the most immediate way of projecting into a three-dimensional arrangement the bi-dimensional motions we perceive in the sky). Since Plato also held that the Divine Craftsman, or Demiurge, shaped the world in perfect geometrical forms, later philosophers read this passage as a challenge to figure out which combination of spheres and circular motions could produce those appearances. In his comment to *De Coelo* II,12 Simplicius (VI century a.D.) writes that Plato sets for the mathematicians the task to find a model which *save these appearances* while keeping spherical motions[11]. But it is hard to tell whether Plato would have been satisfied by pure geometrical fiction, as Duhem (1908) thought, or would have asked for nothing less than the *true* model, the structure actually given to the world by the Demiurge.

In the IV century B.C. Eudoxus taught that all bodies move on concentric spheres around the Earth. Outermost is the sphere of fixed stars, inside are four spheres for each of the planets, three for the Sun and three for the Moon (for a total of 27 spheres). Each body takes part in the movement of all the spheres of its own group. In the same century, in order to increase the accuracy of match of the model to observations, Callippus raised the number of spheres to 34, and Aristotle to 55[12]. According to Duhem (1908), Aristotle recommends that models be compatible with physical principles, for it is supposed to be *true*, not a purely mathematical fiction.

In the II century B.C. Ipparcus advanced a different model: each planet moves on a sphere (*epicycle*) whose centre is fixed on the surface of a larger sphere (*deferent*) rotating around the Earth. The Earth does not occupy the center of the deferent, but an eccentric position, and the angular velocity of the deferent is constant with respect to a point symmetric to the Earth with respect to the center (the *equant*). This model accounted nicely for the astronomic observations. In particular, the epicycles explained the apparent changes in velocities, direction (including the retrograde motions) and luminosity of the planets (the planet was supposed to appear brighter when on the side of its epicycle close to the Earth and dimmer when on the opposite side).

Geminus of Rhodes (I century B.C.) distinguished between physical and astronomic method: the former deduces the motions of the bodies from their physical nature, while the latter deduces it from geometrical models: astronomers do not care what is *by nature* fit to rest or motion, but they frame models as hypotheses, deduce the apparent motions they would yield, and then check if they correspond to the observed phenomena[13]. So, the method of astronomy, as understood by Geminus, seems to be at once hypothetico-deductive and instrumentalist: astronomers do not care for what is by nature fit to rest or motion, but seek for models whose consequences fit observations, because their goal is not a *true* description of the heavens, but only *σώζειν τά φαινόμενα*, saving the phenomena, i.e., correctly describing and predicting the *apparent* motions.

In the II century A.D. Ptolemy **c**ompleted and perfected Ipparcus' model, making it the accepted theory for 14 centuries. In *Almagest* XIII, 2 he writes:

> One should not object to the complexity of the astronomical models we introduce […] One must look for the simplest possible hypotheses, and if they are not compatible with the phenomena one should introduce more complex hypotheses; even if the resulting model seems too complex, if it allows to deduce exactly the observed phenomena and *if there is nothing in the nature of thing contrary to it*, one should not object to such complexity, since models which seen here on earth seem complex, in the heaven, where they do not hinder each other, work very well [my emphasis].

In fact, his model would be physically impossible if the spheres were made of earthly elements, for they intersect one another, so their motions would be blocked; but heavenly spheres consist of ether, so they offer no resistance, and are free to move intersecting each other. Since Ptolemy requires that models, complex as they may be, be physically possible and compatible with the nature of the heavenly bodies, his attitude is realistic: his model is supposed to be the simplest one compatible with observation, *and* to provide a possibly true account. Even more explicitly, in the *Hypotheses Planetarum* he states that the geometrical structures of planetary models may be real components of the universe[14].

On the contrary, Proclus (V century A.D) remarked in the *Hypotyposis* that astronomers are unable to deduce cosmological models from self-evident axioms (in particular from the Aristotelian principle of a natural motion around the center of the universe), but make up hypotheses with the only constraint that they save the appearances. Hence, those models are mere computational device, devoid of physical significance, and the impossibility to do better is limit placed by God upon the human mind: Jones (1996); Losee (2001, 32).

An equally clear instrumentalist view of cosmology is expressed by Thomas Aquinas, based on an explicit and rather general formulation of the argument from empirical underdetermination: for he sees that for any body of empirical data an alternative theoretical account is always possible in principle, even if not presently available:

> The hypotheses which [the astronomers] have found are not true necessarily; for given that, certain hypotheses having been made, the appearances [of the irregularities of celestial

> motions] are saved, it need not be said that these hypotheses are true; for perhaps the appearances relative to the celestial bodies could be saved in another manner which men have not yet grasped.[15]

As remarked by Evandro Agazzi, considerations of this kind made it common in the Middle Ages to distinguish between mathematician-astronomers and philosopher-astronomers, more or less along the line of Geminus' distinction between astronomers and physicists:

> The former were occupied merely with proposing what today we would call ever more simple and efficacious 'computational models' for calculating the paths of the planets and for making predictions (to save the phenomena), without thereby claiming to furnish a true ontological description of the structure of the universe; the latter, on the other hand, endeavoured to determine just such a structure.[16]

5. Modern cosmology

Empirical underdetermination provided an even stronger argument for antirealism in the XVII century, when three radically different models, compatible with all the astronomical observations, became available: Ptolemy's geocentric system, Copernicus' eliocentric system, and Tycho Brahe's system, in which the Sun rotates around the Earth, but all the other planets rotate around the Sun. The same argument, however, was used in a rather different way by St. Robert Bellarmino:

> Your Reverence and Sig. Galileo have acted prudently in being satisfied with speaking in terms of assumptions [*ex suppositione*] and not absolutely, as I have always believed Copernicus also spoke. For to say that the assumption that the earth moves and the sun stands still *saves all the appearances* better than do eccentrics and epicycles is to speak well, and contains nothing dangerous. But to wish to assert that the sun is *really* located in the center of the world and revolves only on itself without moving from east to west, and that the earth is located in the third heaven and revolves with great speed around the sun, is a very dangerous thing, not only because it *irritates all the philosophers and scholastic theologians, but also because it is damaging to the Holy Faith by making the Holy Scriptures false*.[17]

Implicit in the italicized passage is the idea that Copernicus' theory was false, because it contradicted both the accepted sense of the Bible, and the strong "philosophical" arguments (i.e., arguments based on Aristotelian physics) which at the time stood against the motion of the Earth. But Bellarmino points out that however false, this theory saves the phenomena even better that the alternative accounts, hence it can be held on condition of interpreting it instrumentalistically; i.e., of understanding it not as a work in philosophical astronomy, purporting to offer a true description of the universe, but as a work in mathematical astronomy, offering only a simple and accurate account of observations. So, an antirealist reading of the Copernican theory is coupled to a realist conception of philosophical astronomy (i.e., physical cosmology) as a discipline, in which there are criteria (like those implicitly used against Copernicanism) allowing to distinguish true from false descriptions. Paradoxically (or not so paradoxically, after all), on the latter point Galileo agrees with Bellarmino:

> […] philosophical astronomers who, going beyond the demand that they somehow save the appearances, seek to investigate the true constitution of the universe, the most important and most admirable problem that there is. For such a constitution exists; it is unique, true, real, and could not possibly be otherwise; and the greatness and nobility of this problem entitle it to be placed foremost among all questions capable of theoretical solution.[18]

There are three different forms of realism, professed by Galileo in this quotation: ontological realism, when he writes that there exists one objective constitution of the universe; methodological realism, when he writes that physicists are trying to find it, and epistemic realism, when he writes that it is possible to find it. Of course, unlike Bellarmino, he also holds a realistic interpretation of Copernicus' theory: he emphasizes that in his previous writings Copernicus had tried to save the appearances by making computations according to the theory of Ptolemy, but then

> wearing the philosopher's dress, and considering whether such a constitution of the parts of the universe could really exist in *rerum natura,* and having seen that this was not the case, and also estimating that the problem of this true constitution was worth being investigated, he engaged himself in the investigation of such a constitution, recognizing that, if a disposition of the parts of the universe was able to satisfy the appearances in spite of being fictitious and not true, much better would this result be obtained from the true and real disposition.[19]

Two points are made here, both on the presupposition, shared with Bellarmino, that physics should study the true disposition of the universe: first, the underdetermination argument is at once acknowledged and countered, by suggesting that although different theories account for the same appearances, heliocentrism explains them *better* than the others (as granted by Bellarmino), and this may suggest that it is *true*. Galileo is thus advancing what has been called an *inference to the best explanation*: see Lipton (1991). Second, he raises a philosophical (i.e. physical) objection against geocentrism, symmetrical to those raised by Bellarmino against heliocentrism: contrary to Ptolemy's above quoted claim, the parts of his model could not possibly coexist in the actual world.

In fact, as is well known, since Galileo defended Copernicanism and Newton underpinned it by his gravitation theory, instrumentalism on cosmology, a live option from Plato onwards, completely disappeared (except in the fiction of catholic authors who for another century formally declared they assumed heliocentrism *ex suppositione* (as a mere hypothesis), in order to comply with the church's prescriptions). Why did this happen? Because a 2000 years old *philosophical* problem had found a *scientific* answer? This is unlikely. Or because nowadays we can directly see celestial movements from spaceships? No, since the Copernican theory was definitely accepted long before space travels. Perhaps because the movements of heavenly bodies are *observable* in van Fraassen's sense, so they are not *hidden causes*? No, because they were observable, in principle if not in practice, even before Galileo.

The reason is rather that for 2000 years no cosmological model had been fully compatible with accepted *philosophical* (i.e. physical) theories. In particular, the motion of the Earth was incompatible with trivial observations in the light of Aristotelian mechanics, but it became perfectly compatible with them in the light of Galileo's principles of relativity and inertia. Further, Kepler's and Newton's work allowed to do precisely what Proclus complained one could not do, viz. deducing a cosmological model from accepted axioms, hence no reasonable doubt on the Copernican theory was possible any longer[20]. This shows that reasoning may make theories practically certain, even about unobservable entities. The unquestionable confirmation of the Copernican hypothesis by direct spaceships observation should dispel doubts about this. 2000 years of discussions had been motivated by concrete and particular problems in philosophy and methodology of science, not by the epistemological doubt whether it is in principle possible to gain knowledge of unobservable entities.

6. Atomic theory, observation, explanation, and prediction

A similar lesson is taught by the destiny of the atomic theory. The question whether matter was ultimately continuous or discrete had been discussed for more than 2000 years as a typically metaphysical problem, for which supposedly no empirical evidence was foreseeable in either sense. It became a problem in philosophy of science in the XVIII and XIX century, when the existence of atoms was assumed as an explanatory hypothesis in chemistry by Prout, Dalton, Avogadro, and in physics, especially by Maxwell's and Boltzmann's statistical thermodynamics. Although confirmed in the hypothetico-deductive way by various empirical laws, it was not supported by any direct empirical evidence; hence, it was surrounded by widespread scepticism until the end of XIX century, when Mach rejected the very notion of the atom as a scientific concept.

However, at the beginning of the XX century, Jean Perrin (1913, § 32) and others were able to measure Avogadro's number, and hence the volume and weight of molecules, by means of experiments involving only straightforward inductive extrapolation from observable measurements; in a similar way Robert Millikan determined the charge of an electron[21]. At that point, scepticism suddenly give way to almost universal acceptance. In this case, uncertainties were not caused by incompatibility with background physical theories, like in the case of cosmology, but by the lack of empirical evidence independent of the phenomena supposedly explained by the atomic hypothesis: in other words, scepticism was due to the fact that atoms had not been *observed*, not that they were *unobservable*. Perrin's and Millikan's experiments were considered as *bona fide* observations of (respectively) molecules and electrons, and that ended the properly scientific debate. Granted, since they do not count as observations in van Fraassen's sense (i.e., by unaided human senses), one could still hold on to the epistemological question whether belief in atoms is warranted; but that would resemble more the question whether beliefs in the external world or in other minds are warranted, than the original debate of the end of XIX century.

7. Scientific revolutions and partial realism

In his *Principia*, Newton postulates what could properly be termed *hidden causes*: the force of gravitation (so hidden, in fact, he confesses he neither knows anything nor can make any conjecture on its nature: «*hypotheses non fingo*»[22]), absolute space, absolute time, mass. Apparently, he might have offered just a body of empirical laws, directly connecting observable causes (i.e., initial position and momentum of physical systems) to observable effects (position and momentum at any future time), in the spirit of the *empirici* physicians and of the theoretician's dilemma, and that would have been enough for all technological applications. Why instead did his theory postulate unobservable entities? in order to *explain why* bodies move in that way, i.e., *because* of gravitational force. In fact, it *explains* both Galileo's earthly mechanics and Kepler's heavenly mechanics.

Initially the postulation of *hidden* gravity encountered strong objections. But soon it was universally accepted, not only because of the admirable explanations it provided for many and disparate known phenomena, but also because it predicted in a remarkably exact way new and unexpected phenomena. Most notably, in the early XIX century astronomical observations did not exactly conform with Newton's predictions for a solar system consisting just of the seven planets known by then. Astronomers Adams and Le Verrier, taking the theory seriously, assumed that there existed a new planet, of which the theory also allowed to calculate the mass and the orbit, and their prediction were confirmed with the discovery of Neptune in 1846.

Philosophical discussion has been raging again since Einstein advanced a radically different (and more successful) model, with his general relativity theory (1915): there is no mutual attraction among physical bodies, but they move along trajectories (geodetics) determined by the space's curvature. So, exactly like Aristotelian mechanics has been superseded by classical (Galileo's and Newton's) mechanics, the latter has been superseded by relativity; but then, it seems a compelling inductive conclusion that sooner or later even relativity will be rejected, and there is no hope to ever reach a definitely true theory. Besides, since Newtonian mechanics possessed great explanatory and predictive power, it seems that explanatory and predictive power cannot be taken as reliable indicators of truth.

The logical empiricists took an optimistic approach to this problem: although Newton's theory is strictly speaking false, it *nearly* true *within* a certain domain: when dealing with bodies moving at low velocity Newton's laws yield values extremely close to Einstein's laws, and it is still true that bodies have mass, move in space and time, etc. Einstein's theory is probably even closer to truth, and in a wider domain, and if it will be superseded, it will be by an even better one, so that science may be seen as continuously approaching the truth. But Thomas Kuhn (1962, ch. 9, 126-132) replied that if theories are taken to be true only within the domain of known data, first, they risk to become immune to falsification, and second, they cannot be used to make predictions about new phenomena. Moreover, Newton's theory is radically false, because the entities it describes simply do not exist: there is no absolute space and time, and no Newtonian mass, defined as constant (as opposed to relativistic mass, varying with acceleration). Besides, Kuhn argues that the history of science in general is a sequence of periodic «revolutions» (1962, ch. 1, 9), hence this *pessimistic meta-induction* does not hold only for mechanics, but for all of our theories. Of course, this is a new powerful argument against scientific realism; but it may be noticed that it does not hinge on worries about observability, but on the fallibility of our epistemic procedures, no matter whether they concern observable or unobservable facts.

Hilary Putnam tried to counter the pessimistic meta-induction by his historic-causal theory of reference: the reference of scientific terms is not fixed by their theoretical definition, but by causal connection with the referred object or property. For instance, by his terms 'mass' Newton intended to refer to *whatever* is responsible for observable effects (such as the fall of stones, the oscillation of pendulums, the movement of planets, etc.), independently of the way he himself described it. So, if the cause of such effects is relativistic mass, he referred to relativistic mass, in spite of having partly wrong ideas about it[23].

Unfortunately, in some cases there exists nothing even vaguely similar to the entities postulated to account for certain effects. For instance, in Fresnel and Maxwell's wave theory light was described as a vibration of *ether*, a medium transparent, with no weight and no resistance to penetration, diffused everywhere. The theory explained all the known phenomena of light diffusion, reflection, refraction, diffraction, etc., and besides, it predicted a previously unknown and unexpected phenomenon: a bright white spot appearing in the centre of shadow projected by an opaque disk (see Worrall 1989b). Hence, it seemed as well confirmed as any theory can be. Nonetheless, Michelson's and Morley's experiments showed (and the relativity theory confirmed) that no medium even vaguely similar to ether exist. Hardin and Rosenberg (1982, § 5) claimed that the term 'ether' actually referred to the electromagnetic field, which is altogether different from ether, yet plays roughly the causal role previously attributed to ether. But this proposal seems too far fetched: Stahl and other eighteenth century chemists assumed *phlogiston* (an element supposedly without colour, odour, taste, and with negative weight) as responsible for combustions and other processes in which we now believe the essential role is played by oxygen. But surely, by 'phlogiston' they didn't refer to oxygen: for instance, phlogiston was supposed to be lost by the burning bodies, while we know oxygen is acquired by them. Thus, Putnam's causal theory of reference is of no help in these cases, the way is open for the pessimistic meta-induction, and the confirmatory virtue of explanations and novel predictions is undermined.

An alternative rescue strategy was adopted by Poincaré and some logical empiricists[24], and revived in recent years by Worrall (1989a; 1994; 1995): science does not purport to describe the *nature* of things (as perhaps metaphysics does), but only their *structures*. So, it is no problem if the entities to which a certain behaviour is attributed turn out to be inexistent, as long as there exists something behaving as described by the equations of the theory: Fresnel and Maxwell correctly saw that light and electromagnetic phenomena are vibrations, and they also found the laws of these vibrating motions, laws we still accept as true:

> 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, as science later saw it, more or less the right *structure*. [Thus,] there was continuity or accumulation in the shift [from Fresnel to Maxwell], but the continuity is one of *form* or *structure*, not of *content*.[25]

A problem with this *structural realism* is that there is no sharp distinction between entities (which we can be mistaken about) and structures (about which a successful theory should supposedly be right): for structures are relations among *entities*, and *what* an entity is, depends on what its *structure* is; on the other hand, if Worrall's distinction were to be interpreted as a distinction between the mere mathematical structure of a theory and its theoretical interpretation, preservation of the former would not be enough for realists, since they claim that theories offer a correct representation of the world, and mere mathematical formalism cannot be said to represent anything: see Psillos (1999, 152-156). Another problem is that structural descriptions may vary between rich and detailed, and very abstract and schematic; but the most concrete details of a structural description are likely to be rejected by subsequent research, and only the most abstract levels of description may be hoped to endure over time. But the more a structural description is rendered abstract and schematic, the closer it comes to the network of empirical laws. So, the more structural realism is *structural*, the less it is *realist*, and eventually it coincides with instrumentalism.

A recent and more promising approach is the *partial realism* of Kitcher (1993, 144-149), Musgrave (2006-2007) and Psillos (1999, 108-114): even if we cannot discriminate between true structural descriptions and false ontological assumptions, within a once successful theory we can distinguish claims or assumptions which are now rejected from others which have not been shown false and we still accept. Typically, the latter claims are those which may be credited for the explanatory and predictive success of both older and newer theories; hence, there are good reason to believe they are in fact true. For instance, we have now rejected the claim that light is a vibration of an elastic solid, but we still hold that light involves a vibration perpendicular to the direction of propagation, and as far as we know this is true. Equally, we still believe in the basic claim implicit in Newton's idea of *central forces*: that bodies exert a radiating dynamical influence on their environment proportional to their mass. This is likely to be true, even if it is false that such influence has the form of an attractive force (as opposed to a modification of the curvature of the space itself).

Thus, the best contribution of empiricist and antirealist cautions to a correct understanding of the scope of science has not been delimiting the range of facts or entities on which we can have warranted beliefs, but the number of beliefs we can trust within each field or theory: a sensible realism is a partial realism, since we are inherently fallible. But just as there are no privileged objects on which our beliefs are ipso facto warranted, so there are no outcast objects on which there is no hope to have warranted beliefs.

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Footnotes

[1] See Popper (1934, § 30).
[2] On this see Alai (forthcoming).
[3] In fact, the logical empiricists were even more radical: by their verificationist doctrine, the meaning of any sentence consisted in a set of possible observations; hence, beliefs had to concern directly observable entities, not just in order to be warranted, but to be possible at all. See Schlick (1932, § 2), Schlick (1936), Carnap (1928a, 336, 352), Carnap (1928b, II, 7).
[4] See Alai (forthcoming).
[5] Celsus (1935, 9-23). My emphases. At least a hint at this distinction was already in the *Corpus Hippocraticum* (V-IV century B.C., books: *Diseases II*, *On Regimen in Acute Diseases*), in polemics with the school of Cnidus: see Jouanna (1974), Fano (2008).
[6] See Psillos (1999, ch. 4).
[7] "Il tentar l'essenza, l'ho per impresa non meno impossibile e per fatica non men vana nelle prossime sustanze elementari che nelle remotissime e celesti": "Lettera a Marco Velseri circa le macchie solari, Venere, Luna e Pianeti Medicei, e nuove apparenze di Saturno", in Galilei (1929-, V 187).
[8] See Carnap (1936, sect. 7), Hempel (1952, ch. II).
[9] Hempel (1958), Nagel (1961), Putnam (1965), Suppe (1977, 32).
[10] Tolman (1936) and Spence (1944).
[11] Simplicius (1548, 74-76), Verdet (1990, 36).
[12] Toomer (1996, 93-95), Dreyer (1906, 96).
[13] Losee (2001, 30-31). Geminus is quoted in Simplicus (1948, 91).
[14] Ptolemaeus (1820, 151), Toomer (1996, 117).
[15] *In Libros Aristotelis de coelo et mundo expositio,* bk. 2, lectio 17, in Thomas Aquinas, (1886-, 186-7). Quoted in Agazzi (2006, 90).
[16] Agazzi (2006, 78).
[17] *Letter to Paolo Antonio Foscarini,* Rome, 12 April 1615, Galilei (1929-1939, XII, 171**)**. Quoted in Agazzi (2006, 90, footnote 12). My emphases.
[18] Galilei (1929-1939, V, 102), Drake (1997, 97). Quoted in Agazzi (2006, 91, footnotes 13, 14).
[19] Letter addressed to P. Dini 23 March 1615, in Galilei (1929-1939, V, 297-298**)**. Quoted in Agazzi (2006, 91, footnote 13).
[20] Of course, on condition of reading Copernicus' claim that the Sun is still not as absolute, but as relative to the center of mass of the solar system. But this condition captures very naturally the spirit of his heliocentrism as opposed to geocentrism, even if Copernicus himself and his contemporaries could not suppose it was necessary.
[21] Millikan (1911, 349-397); see also Perrin (1913, § 100).
[22] Newton (1713, *Scholium Generale*, 506).
[23] See Putnam's articles "Is Semantics Possibile?", "Explanation and Reference", "The Meaning of 'Meaning'", "Language and Reality", all collected in Putnam (1975b).
[24] Poincaré (1902, 160-162). See also Carnap (1928, § 6): "It is in principle possible to characterize all objects through merely structural properties […] and thus to transform all scientific statements into purely structural statements"; Carnap (1928, § 1, § 11, § 16 etc.); Schlick (1938); etc.
[25] Worrall (1989a). See also Worrall (1994), Worrall (1995, 92-94).