HOW SHOULD WE JUDGE CURRENT SCIENTIFIC THEORIES?

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1. Two equally wrong extreme views

The scientific realism-antirealism debate concerns theories in general. However, as soon as the discussion draws arguments from the historical development of science, some issues emerge concerning how we should regard current theories in particular, as opposed to past and future ones.

Positions here range between two extremes: on the one hand, a radical version of the pessimistic meta-induction would have it that since all past theories older than 100 - 150 years or so have been proven radically false and rejected, all present and future theories will also be rejected within 100 -150 years or so. As a consequence, in science there can be no truth but, at most, empirical adequacy or the like.

On the opposite, Fahrbach (2011) stressed that in the 20th-century science has undergone an exponential quantitative and qualitative improvement as to the number and education of researchers, methodology, instruments, facilities, funding, available data, and communication; all of this makes the theories of the last 100 years or so dramatically different and incomparable with those of all the past centuries. Thus, some radical optimists like Doppelt (2007, 2011, 2014) or Park (2017b, 2018) hold that current best theories are almost completely and exactly true, and that further progress, besides adding new knowledge, can at best correct minor details of present day theories: they will not be refuted, because they have a "unique status" in the history of their discipline, distinguishing them from the older theories, and they "stand alone at the pinnacle of the entire field of inquiry" (Doppelt 2014, p. 285). For instance, "no scientific revolution will oust the special theory of relativity, and the special theory of relativity will only be augmented by infinitely many unconceived methods. Hence, there is no need to distinguish between stable and unstable posits of the special theory of relativity" (Park 2017a, p. 8).

Evidence, however, is against both extremes. To begin with, consider extreme pessimism: it cannot explain why even the ancient and now rejected theories were predictively successful: some of their predictions were as precise as utterly unforeseen and unforeseeable; therefore, they couldn't be gotten right by chance, except by a miraculous coincidence. The only plausible explanation of those successful achievements is that those theories had some true components which were sufficient to derive their predictions; current research in history and philosophy of science has spotted many of those components and shown that they are still accepted today.[1] Therefore, the best past theories were not *completely* false. Thus, one cannot inductively conclude that current theories are completely false: the pessimistic meta-induction is based on a wrong premise. Moreover, radical pessimism cannot explain the rapidly increasing rate of success of science, which instead is explained by the assumption that the true components of older theories are typically preserved in current ones.

As for the opposite extreme, it is implausible that *just now* we have reached the "end of history" in scientific research, a sort of promised land of pure truths, or Peirce's ideal limit of research, and that our science is infallible. While we see the mistakes of past science, obviously we cannot see those of present science, and this ingenerates the illusion that there are none. But even in the past, age after age, people have been prey to the same illusion, which was then regularly deluded: in the 18th century they thought Newton had finally and definitely discovered God's blueprint of the Universe. In 1874 Philipp von Jolly advised a young Max Planck against studying

[1] Psillos 1999, Alai 2018, Alai 2021.

physics, because "In this field, almost everything is already discovered, and all that remains is to fill a few holes" (Lightman 2005, p. 8). In addition, we positively know that there are mistakes in current theories, because even two of the most successful ones, quantum mechanics and relativity, are at variance with one another and are beset by unsolved riddles. Besides, many basic fundamental problems are still unsolved, and we still lack a grand unified theory: therefore, we are not at the end of the road, yet, and future research will introduce basic changes in currently accepted views.

> Why should we think that today's best science is true when past scientists believed the same of their science – which we reject today as badly wrong? The answer some strong realists give is that today's science is now mature, whereas theirs was not. After all, we now know of flaws in their theories, their instrumentation, their experimental design, their goals and standards, etc. But wait! What is to keep our distant successors from saying the same about us? Just because today's most successful theoretical claims seem practically flawless to us does not mean that they really are (Nickles 2017, p. 153).

No doubt, contemporary science has made astonishing progresses with respect to past science. Nonetheless, even in the past the quantity of available data had increased steadily, better and better instruments had been introduced, methodology had progressively improved: for instance, there had been an unquestionable progress from the year 1000 AD to 1700 AD, yet many wrong theories were still held at that date, and even thereafter. So, it is hard to think that any improvement of background empirical knowledge, methods and technology can at some point make scientists practically infallible, and it is even more difficult to believe that *we* have already reached that point.

Moreover, Brad Wray pointed out that, just like at any time there are unconceived alternatives to the current theories (Stanford 2006), there are also unconceived methods and instruments, by which those theories could be overthrown: for instance, the discovery of the astronomical telescope contributed to the rejection of geocentrism and the discovery of the microscope undermined the theories of spontaneous generation.[2] Such methodological and technological advances are themselves largely the product of scientific progress. Hence, the very excellence of contemporary science will probably yield even newer methods and instruments, which in turn will likely undermine today's theories.

2. Revolutions ahead

The history of science has witnessed many ruptures, or "revolutions", and we may expect that this keep happening even in the future, for both subjective and objective reasons. On the one hand, in fact, scientists are still humans, using the same fundamental cognitive tools (reason and the five senses), and subject to the same cognitive limits; besides, scientific method is not radically different from the past. On the other hand, nature itself, contrary to accepted wisdom, makes jumps and has ruptures, because it is very complex and because it works in different ways at different scales and at different locations in space or time. For instance, nature is (roughly) deterministic at large scales, but indeterministic at small scales; the physical laws today are probably different from those a few instants after the Big Bang; entropy increases over time in the universe as a whole, but it may decrease in local areas or over short time spans; etc.[3]

Humans start their inquiry about nature from the time and area in which they are located, and from the scale of magnitude and range of energies to which they have most direct access. However, as they move forward to explore what lies farther in space or time, or what happens at different scales and ranges of energies, often what humans have learned about their earlier targets is no longer valid for the new targets. This may happen just because the new targets have a different nature or work in a different way, so what we discover about them is simply added to what we knew about the old targets, as information about different subjects. Otherwise, it may be the case that

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[2] Wray 2016, Park 2017a, § 2.
[3] Alai 2017, p. 3282.

even our earlier targets actually had this newly discovered nature or worked in the newly discovered ways, but we could not notice it within that restricted location or at that particular scale.

For instance, the principle of inertia works on the Earth just like in the empty space, but due to the ubiquitous presence of frictions it was recognized only with great difficulty; similarly, mass varies with velocity, but this could not be appreciated at ordinary velocities. So, by extending our research to different scales or locations we may learn that our previous theories were strictly speaking wrong: for instance, we discovered that there is no gravitational attraction at distance, that mass is not unalterable, and that there is no radical opposition between matter and energy.

What we learned at the previous scales or locations may still be approximately correct within those limits, not only at the empirical level, but also at the theoretical level: for instance, the planetary model of the atom is still an approximately true description of the unobservable behavior of the atom within a certain range of phenomena. Yet, in these cases the old models are replaced by, and embedded into, radically different models, which explain the success of the old theories and show which of their assumptions were true and which ones were false. For instance, the heliocentric model supplanted the geocentric one, and the curvature of space supplanted gravitational attraction.

Although we have already greatly expanded our understanding of nature, there are still undiscovered territories in front of us (think, for instance, of dark matter and energy). Therefore, like in the past, any breakthrough into a new location, scale or range of energies will probably teach us some new fundamental features of nature, showing that current theories have certain basic mistakes and must be substituted by more fundamental and comprehensive accounts. In fact, the increasingly powerful resources of current science may make such revolutionary changes even more frequent than in the past.

On the other hand, as we proceed farther and farther from the areas, scales, and ranges of energies with which are more familiar, research becomes more and more difficult and demanding. For instance, the energies required to probe more and more basic particles, or earlier and earlier states of the universe, grow exponentially, up to the point of escaping present or foreseeable technology. Therefore, even the unprecedented improvement of our scientific resources and methods is insufficient to get us a completely safe grasp of the phenomena on today's frontiers of research and to assure that current theories won't be superseded.

Hence, the unprecedented rate of progress of contemporary science allows to block the pessimistic meta-inductive conclusion that current theories are completely false, but it does not show that they are immune from revision or substitution.[4] Every great advancement in science has uncovered a new unsuspected, deeper and more basic layer of the structure of Nature, and we don't know how many of those still lie ahead. Any of those discoveries exposed some basic mistake in our understanding of certain mechanisms of nature, spurring a big or small revolution, and nothing says we have probed all of this unfathomable complexity, yet.

3. A moderate intermediate view

Therefore, there are good arguments against both extreme pessimism and extreme optimism. Moreover, these arguments are mutually compatible, and converge in suggesting a moderate intermediate position: current theories are partly true, in fact more (perhaps much more) largely true than past ones, yet probably they still include important false components. Those false components may be replaced by future revolutionary changes, more or less like Newton's absolute space and time and gravitational theory were replaced by Einstein's spacetime and by its curvature, or the dichotomy of matter and energy, still surviving in late 19th-century electromagnetism and statistical mechanics, was replaced by the convertibility of one into the other in quantum mechanics and special relativity. Contrary to Kuhn's view, revolutions and progress go hand in hand.

[4] Alai 2017, pp. 3282-3283.

Induction as such is a good inference pattern, and it may correctly be applied to past science, on condition of taking a correct image of past science as a premise. If this is done, the conclusion is a more balanced judgment of current and future theories, neither completely pessimistic nor implausibly optimistic. What we observe in past science is that (1) every theory has been found to be (partly) mistaken and replaced; yet (2) mistaken but predictively successful theories had some true components (those essential to deriving their successful novel predictions); moreover, (3) those components were typically preserved in subsequent theories, which therefore were more largely true.

Since there is no reason to deny that this will keep happening, we should conclude that current theories are more largely true than earlier ones, but still partly false. Even if not all the content of current theories exceeding old ones is probably true and will be preserved in the future, still we can appreciate one by one many new pieces of information and many corrections introduced by current theories.

4. Can we discriminate the true from the false content in current theories?

According to Psillos' (1999) *deployment realism*, our best theories, both past and present, are at least partly true because the assumptions which were essential in deriving their novel successful predictions are probably true (the riskier those predictions, the most probably true those assumptions).[5] Thus, it might be suggested that by checking which hypotheses were essentially deployed in novel predictions we should be able to discriminate what is true and what is false in our theories, so getting rid of the latter.

This, however, is clearly impossible, because otherwise we could anticipate future scientific progress, getting rid of our false assumptions, with no need to wait for future scientists to identify and reject them. Also, our negative heuristics would become much easier than they are, since, in face of any experimental failure of a theory, we would know right away precisely which (ones) of its claims should be modified or abandoned. On the contrary, throughout the history of science mistakes have been eliminated only in hindsight, and scientists today, while trusting that current theories are largely true, grant that some of their assumptions are probably wrong, but only future research will tell which ones.

Distinguishing exactly and securely between true and false contents by checking which claims were deployed in novel predictions is impossible for at least two reasons: first, essential deployment in a novel prediction is reliable evidence that a hypothesis is true, but it is not a necessary condition for being true: various hypotheses in our theories may well be true even if they haven't passed such an acid test (or not yet). Second, not all the hypotheses actually deployed in a novel prediction were essential to it. For instance, suppose I hold the hypothesis that

(H) *John has become terminally ill because of a voodoo rite performed by a shaman*:

from this I infer the prediction that

(NP) *John will die*,

and unfortunately, he actually dies. However, to predict NP I did not need to assume H: only the weaker hypothesis that

(H*) *John is terminally ill*

was essential to this prediction. Therefore, the truth of NP is not evidence for the truth of H, but at most of H*. In general, whenever a hypothesis H entails a prediction NP, there may be a weaker hypothesis H*, entailed by H, which also entails NP. In such case H is not essential to deriving NP, and the prediction is not evidence of its truth.

[5] Alai 2014a, § 3.2; Alai 2014b, § 4; Alai 2021.

Unfortunately, unlike in this trivial example, in most real-life scientific cases it is very difficult to tell whether a particular hypothesis H was essential in deriving a novel prediction or not. From a purely logical point of view, one could always ask whether there is any H* entailed by H and entailing NP. But first, often H may seem such a natural reason to predict NP, that one just doesn't feel the need to look for a weaker reason H*. Certainly, one doesn't even dream of checking *all* of the possible implications of H, in order to see whether there are some which also would entail NP. Second, even if scientists realized that there is some weaker H* *logically* sufficient to derive NP, they might believe that still H is *physically* required, because of certain presuppositions explicitly or implicitly held by them. Here are a few examples.

(1) Fresnel and Maxwell derived various novel predictions from the hypothesis that

AV: ether vibrates.[6]

Today we know that **AV** is false, for ether does not exist. However, we have also understood that **AV** was inessential in those derivations, because it can be substituted by its weaker consequence

VM: there is a vibrating *medium* (which today we call 'electromagnetic field').

Fresnel and Maxwell did not realize that **AV** was not essential, therefore possibly not true, probably because they presupposed that

PR1: all mediums are material,

and/or that

PR2: all vibrations are produced by the oscillations of particles.

Hence, given their presuppositions, *any* vibrating medium couldn't be but a *material medium composed of particles* (i.e., either water, or air, or *ether*). In this perspective, therefore, VM counted as physically equivalent to AV, hence AV appeared to be essential.

(2) Laplace predicted the speed of sound in air starting from a hypothesis we now know to be false:

H: the propagation of sound is an adiabatic process, in which some quantity of caloric contained by air is released by compression.

Now we know that H was not essential to Laplace's prediction, which could also be derived from the weaker hypothesis

H*: the propagation of sound is an adiabatic process, in which some quantity of latent heat contained by air (whatever be the nature of heat) is released by compression.[7]

However, at that time they presupposed that

PR3: gases can be heated without exchanges with the environment only if they contain heat in a latent form,

and

PR4: only material substances can be contained by material substances in a latent form.

But the material substance of heat was just caloric, hence it seemed that adiabatic heating could only be explained as the disengagement of caloric from ordinary matter, caused by mechanical compression.[8] In other words, given PR3 and PR4**,** H* entailed H, hence H seemed essential.

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[6] Lyons 2002, p. 72; Doppelt 2011, pp. 304, 306; 2013, § 2; 2015, p. 275.
[7] See Psillos (1999, pp. 119– 121)
[8] Chang 2003, p. 904.

(3) Bohr predicted the spectral lines of ionized helium by assuming that

H: the electron orbits the nucleus only on certain specific orbital trajectories, each characterized by a given quantized energy.

H turns out to be false, but the same prediction could have been derived by the weaker hypothesis that

H*: the electron can only have certain, specific, quantized energy states.

But at that time it was natural to suppose that

PR5: quantized energies are the result of orbital trajectories,[9]

therefore, H was thought to be essential.

The same may happen with current theories: although a novel prediction NP was derived from a hypothesis H, H may entail a weaker hypothesis H* from which NP could also have been derived; however, given certain presuppositions PRS implicitly or explicitly accepted today, H may "appear to be conceptually or metaphysically entailed by" H*,[10] hence essential to NP, hence probably true. If, however, in the future those presuppositions were discovered to be false, we would learn that H was not essential, after all, therefore we could no longer be practically certain of its truth.

In fact, the revision of our beliefs may also follow the reverse order: suppose we fail to experimentally confirm H, or find some prima facie experimental disconfirmation, or notice that it is contradicts other accepted hypotheses: then, we would begin to doubt that H is true. However, if H is false, it cannot have been essential in predicting NP, because the hypotheses essentially deployed in novel predictions are most probably true. In this case, therefore, we would question the presuppositions which make H seem essential, and perhaps eventually reject them.[11]

Of course, we cannot tell which of the presuppositions we hold today will be rejected in the future, hence whether any hypothesis we now consider as essential to a novel prediction, hence probably true, is actually so or not. In due course, *some* hypotheses will be discovered, retrospectively, to be inessential, but of no hypothesis at any time will we be certain that it was essential to some novel prediction. That is, even if we drop a hypothesis H which we no longer believe to be essential to predicting NP and keep in its place a weaker hypothesis H* which still entails NP, we cannot ever be certain whether H* is essential, or it can be dispensed with in favor of a still weaker hypothesis H** entailed by H*.

In some cases we might be able to distinguish between the assumptions that from a purely logical viewpoint are strictly necessary to derive NP, and those that they entail in the light of our factual presuppositions, hence distinguish between our reason for believing H* (i.e., its essential role in predicting NP), and our reasons for believing the content of H exceeding H*, thus realizing that H were not as secure as H*. For instance, perhaps Bohr might have distinguished the extra-content of H from H*, so adopting a more guarded attitude toward the former.[12] In other cases, however, the presuppositions which make H indispensable may be too apparently obvious or deeply entrenched to be doubted or even noticed, as it happened for PR1, PR2, PR3, PR4, and PR5. Further

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[9] Stanford 2006, 171.
[10] Vickers 2016, §4.
[11] Even in this case, however, declaring that H was inessential would be independent of discovering that it was false (even if spurred by that discovery), because its inessentiality could be established only if independent reasons to reject those presuppositions were found. Therefore, the "no miracle" argument (NMA) can be defended from Lyons' (2002) "meta-modus tollens" without begging the question. In fact, Lyons claimed that the NMA is falsified by the fact that false hypotheses were (essentially) involved in successful predictions. The NMA can be rescued from this objection by claiming that it didn't commit us to the truth of those hypotheses because they were not essential. But if the only reason to claim that they were inessential was the discovery that they were false, that would be tantamount stipulating that the NMA is correct. However, this is not the case if those hypotheses are discovered to be inessential for independent reasons, as in the above examples (Alai 2021, p. 203).
[12] Vickers 2016, §4.

such presuppositions may be the principles of conservation of energy and mass, isotropy and homogeneity of space, physical causal closure, etc. Perhaps we unconsciously presuppose many other very general principles, which may render essential certain hypotheses that are not such in a purely logical sense.

Therefore, the pessimistic meta-induction is probably right only in the sense that none of our theories is *completely* true (while it is wrong that they are *completely* false). On the other hand, Peters (2014), Votsis (2011) and Cordero (2017a, 2017b) are right that we can recognize whether a hypothesis H is true, but only in the sense that we can recognize whether it is *at least partly* true: if H was deployed in a novel prediction, either it was deployed essentially, hence it is completely true, or it was deployed inessentially, but then it is at least partly true, since it entails a weaker hypothesis H* that was actually essential to the prediction, and is completely true. But we cannot be sure which one is the case, nor which is the weaker hypothesis H*.

Therefore, we are entitled to believe that there is *some* truth in current theories, and more precisely that there is some truth in some hypotheses which *appear* to have been essential to certain novel predictions; still, we cannot be certain of what exactly is true in them. However, the hypotheses deployed in novel predictions (or, at least, the parts of those hypotheses which are considered as essential) are typically preserved in successor theories; hence, science is cumulative, and we may trust that, *overall*, current theories are more largely true than past ones (and future ones will be even more largely true).

5. We cannot measure the percentage of truth in current theories

Nonetheless, since we are unable to circumscribe the strictly essential part of hypotheses, we cannot tell what in our theories is (almost) certainly true and what is not, hence what is the proportion of truth vs. falsity in them. A fortiori, we cannot tell how larger is their proportion of truth than that of past theories. Even less, of course, can we tell what proportion of the whole truth on its particular subject a theory contains, since we don't know what the whole truth is.

This uncertainty is also due to the fact that speaking of the "parts" of theories and hypotheses is vague and somewhat metaphorical. Since theories and hypotheses may be sorted out into parts in many equally legitimate ways, it is unclear what counts as a part, in particular as an elementary part, and it is even less clear how we should compare the "size" of different parts.

To take just a very simple example, suppose we follow the "statement view" and formalize a theory T as a collection of sentences: then we should count the atomic sentences entailed by T. This is a problem, however, since they are in principle numberless. Further, even apart from that, things can be quite undetermined: e.g., suppose that 95% of the empirical atomic sentences entailed by a theory T are true, but only 50% of its middle-level theoretical atomic sentences are true and only 10% of its most basic atomic theoretical sentences are true. If so, it might be a matter of taste whether to call T *largely* true or *largely* false, but it would certainly be correct to call "a revolution" the substitution of T by a theory T' which preserved most of the empirical and middle-level theoretical sentences of T while substituting 90% of its most basic theoretical sentences.

Now, nothing allows to exclude that several of our best theories today are in a similar condition. This is another reason to suppose that our science, successful and largely true as it is, will undergo further revolutions in the future, as argued above.

6. Is this moderate position still a realist one?

It might be asked: does this mean that all we know about current theories is that there is some truth somewhere in them? Isn't this too little and too pessimistic? Is my moderate position still a realist one? Can one be a scientific realist without being definitely committed to the (complete) truth of any particular hypothesis? I answer "no", "no", and "yes" (in the order).

First, it doesn't follow from the above that the only judgment we can reach about our theories is that "there is some truth somewhere". Although I focused on the hypotheses about whose truth we can be practically certain, we can have a more nuanced and articulated judgment on theories as a whole. As we know, (1) some claims in our theories are admittedly strictly speaking false, since they are idealizations or acknowledged simplifications. (2) Other hypotheses are considered by scientists as purely speculative, since they have no experimental confirmation (yet), nor any compelling theoretical support in their behalf. They count just as interesting, suggestive, perhaps somewhat plausible, suggestions. (3) Still other hypotheses, instead, are (more or less) probable, since they have some empirical support, either inductive or deductive (e.g., by successful experimental control). Of course, the distinction between claims of type (2) and (3) is not neat, but gradual. Finally, (4) there are the hypotheses I have discussed so far, about which we can be practically certain that they are at least partly true, due to their role in novel risky predictions. For all we know, each of the hypotheses of kinds (2) and (3) might turn out to be completely false and be rejected in the future (although our subjective probability for their falsity may be very different for each hypothesis). Instead, those of kind (4) can be excluded to be completely false.

Second, deployment realists argue that when a theory has licensed risky novel predictions,

(I) we are *justified* in taking the theoretical claims deployed in those prediction as *true* (i.e, completely true if they are deployed essentially, and *partly* true if they are deployed inessentially),

and

(II) those claims are mostly preserved in successor theories;

therefore,

(III) we have cumulative *knowledge* of some unobservable structures of nature.

Thus, deployment realists provide a *general criterion* of realist commitment (I) and advance two *general* realist claims (II) and III): this is realism enough. On the other hand, *qua* philosophers and qua *realist*, they are not required to also *apply* criterion (I) to actual research. In fact, this would involve answering questions such as (i) has hypothesis H been deployed in a prediction? (ii) Was that prediction actually novel? (iii) How risky was it? (iv) Was H essential to NP? Perhaps question (i) may be answered even by philosophers, but (ii) and (iii) require a specific expertise in the field, they must be answered by historians of science for past theories, and by practicing scientists for current ones. In turn, (iv) is very hard to answer even for scientists, as explained above, and certainly out of reach for philosophers.[13] For this reason, even which hypotheses of current theories will be preserved in future theories and pile up in the growing accumulation of scientific knowledge cannot be securely told by scientists, let alone by philosophers.

Therefore, realists need not be personally committed to any particular theory or hypothesis, not even to the best current ones: that is not their task, they are just not equipped for it. A fortiori, it is paradoxical to ask (as Fahrbach 2017) that they teach scientists their own trade, telling them which are the working hypotheses and which are the idle parts in their theories, urging changes or suggesting directions of research.

According to Stanford (2017), realists believe in the truth of current theories, hence they are more conservative than anti-realists. But this is not the case for those who hold the moderate intermediate view I suggest: on the one hand, in fact, they only hold that current theories are more largely true than past ones, not that they are completely true. On the other hand, realists require hypotheses to satisfy a higher standard than anti-realists: truth, rather than just empirical adequacy or the like. Therefore, from their viewpoint it is even more likely that any particular hypothesis fails

[13] See Smart 1963, p. 36.

to reach that standard, hence that it must be substituted by a better one. Moderate realists are, if any, more progressivist than antirealists.

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