**SECOND-LEVEL EVIDENCE FOR FUTURE-PROOF SCIENCE**

Mario Alai
DiSPeA – Università di Urbino Carlo Bo

## 1. Can we identify securely true hypotheses in current science?

According to the "pessimistic meta-induction" none of our current scientific theories, hypotheses or assumptions are true and will be preserved in the future. For some hyper-optimistic outlooks (e.g., Doppelt 2007, 2011), unlike past science practically all current science is true and (save minor adjustments) it will stay forever. The latter view runs against fallibilism, and the former against the hope that through science we can reach at least some truths. However, the current debates on scientific realism abundantly shows that both extremes are wrong: most realists are nowadays selective realists, but also many antirealists are only *selectively* so. This means that we now possess at least some truths. Moreover, if science is progressive, or at least not badly regressive, it follows that many, or at least some, of our true scientific claims are future-proof, i.e., they will never be refuted.

The question, is, however: which ones? Can we identify them? Clearly, these are not all those we now believe, for that would mean that now we are infallible. Moreover, it would seem that in order to distinguish which of our claims are true and enduring, and which ones are false, we should be able to anticipate future scientific progress, which is impossible. Yet, this question is becoming crucial today, not only for philosophers or historians of science, but also for policymakers and the general public: the Covid-19 pandemic has shown how important it is for our very safety that even individual laypersons become able to distinguish between mere scientific opinions and established scientific facts.

Some, like Alberto Cordero (2017a, 2017b), maintain that for certain claims we have now overwhelming evidence, so that it would be nonsensical to imagine that they can be rejected in the future. For instance, in spite of wide disagreements on the interpretations of quantum mechanics, physicists of all leanings and allegiances agree on certain basic tenets (Cordero 2001, S307). But who is to say when evidence is enough to warrant that we are in front of an indisputable *fact*? Earlier on, Rescher (1987, ch. 5) had distinguished between "forefront science", which is precise but mostly false, and "schoolbook science", which though vague and imprecise includes the true core of the forefront science. But this distinction is vague; moreover, it doesn't seem to capture the distinction between reversible and perduring claims, because some of today's forefront science will remain in the future, while much science that was in the schoolbooks of the past has subsequently been rejected.

Deployment realists (notably, Psillos 1999) have convincingly used the "no miracle" argument from novel predictions (henceforth 'NMA') to argue that when a hypothesis was essentially deployed in a novel and risky prediction we can be practically certain of its truth; but from this it is a short step to conclude that (save unfortunate but unlikely scientific regresses) it is future-proof. There is a problem, however: to begin with, we must distinguish between claims that are *completely* true (i.e., true *tout court*) and others that are only *partly* true (i.e., false but with some true consequences). For instance,

(SW) All swans are white

is false, for some Australians swans are black. However, it entails the true statements that all European swans are white, all American swans are white, all Asian swans are white, and all African swans are white. These latter four claims are completely true, while (SW) is only partly true. This is what we mean by saying that (SW) is *approximately* true, and it explains the remarkable empirical and practical success which (SW) provides to its holders (Musgrave 2006-7). If we trust in the progressive nature of science, we can expect a completely true claim to be future-proof, while a partly true claim in the long run will (hopefully) be rejected and substituted by its completely true parts (i.e., consequences).

Now, when a hypothesis *H* is used to derive a novel risky prediction, there are two possible cases: either it has been deployed *essentially*, hence most probably it is completely true, or it was deployed *inessentially*, hence it is only partially true (Alai 2014a §7; 2021). *H* is deployed inessentially in a prediction when the latter was actually deduced from it, but it might equally well have been deduced just from a part of it. In that case, only the essential part is certainly true. For instance, (SW) may be employed inessentially in deriving the prediction

(PR) 'Any swan I'll see in Urbino will be white',

for the same prediction may also be derived from its parts, like 'All European swans are white', or 'all Italian swans are white', etc. Still even the latter statements would be inessential to that prediction, while *in practice* only something like

(UrSW) 'All swans in Urbino are white'

might count as essential.[1] Again, according to Psillos, we need not be committed to the caloric hypothesis, although it was actually deployed in Laplace's prediction of the speed of sound in air, because it was not deployed essentially, hence that prediction "did not depend on this hypothesis" (1999, 121). The same goes for the existence of ether and other false assumptions which were deployed in successful novel predictions, but inessentially (Alai 2014a §7, 2021, §§ 9.4, 9.5). Therefore, only the assumptions which have played an *essential* role in novel scientific predictions can be trusted to be *completely* (not just *partially*) true, hence destined to preservation in the long run.

However, whenever a novel prediction *np* has been derived from a hypothesis *H*, in general it is practically impossible to tell whether *H* was employed essentially or not, and if not, which part of *H* was essential. One reason is that which is the minimal assumption a scientist needs to predict *np* depends on her background knowledge. Only in hindsight, if *H* is found to be false by subsequent research, this shows that *H* had not been essential (Alai 2021, § 9.5). There follows that we cannot exactly circumscribe future-proof claims: the most we can learn from the NMA is that a hypothesis is at least partly true, and this doesn't guarantee that it will be preserved in the long run. Of course, this is to say that we do have some completely true (hence future-proof) beliefs (those that were actually essential in deriving the relevant predictions), but we don't know which ones.

Moreover, in a forthcoming book (*Identifying Future-Proof Science*, Oxford University Press) Peter Vickers maintains that the question whether a claim is actually new and risky seldom, if ever, allows for a clear-cut answer, and it can be decided only through interdisciplinary competences. In the past, I too argued that novelty, riskiness and inessentiality are gradual properties (2014b §§ 3.4, 4). Summing up, in many cases the NMA can be of little help in identifying future-proof claims.

More generally, Vickers points out that the first-level empirical evidence one would need to assess in order to decide whether a claim is future-proof is so vast and requires such a specialized competence in different disciplines that not only philosophers or laypersons, but even no individual scientist could master all of it. Even if a scientists could, over many years, study all that material, still he would see it from his individual and potentially biased perspective.

## 2. Vickers' criterion for future-proof scientific claims

Although neither the NMA nor the direct assessment of any other first-level empirical evidence allows to decide whether a claim is future-proof, Vickers argues that this can be decided by a second-level criterion:

(C) If the community of scientists competent on a claim C is sufficiently large and representative of different perspectives and sociological groups, and at least 95% of its members believe that C

[1] It may be remarked that, from a purely logical point of view only H itself is essential to deriving H. However, by hypothesis H is unknown to us, and we must derive it from some assumption A we know. Moreover, if H is to be deduced from A, a must be stronger than H. Yet, there are problems in isolating essential assumptions: see the next paragraph.

describes an established scientific fact (i.e., beyond reasonable doubt, save philosophical skepticism), then C is future-proof.

Since checking whether a given claim complies with (C) is in principle possible even for philosophers or laypersons, (C) may allow us to recognize future-proof claims even for the needs of philosophy and practical life.[2] Moreover, (C) is only a sufficient condition of truth and permanence, not a necessary one: unbeknown to us, even claims which don't command a 95% consensus, or not yet, may be future proof.

Granted, (C) runs against the current wisdom of both laypersons and philosophers: as famously argued by Kuhn (1962) scientific consensus may be achieved even for purely sociological reasons, and many theories or hypotheses that in the past were accepted as matter of course have subsequently been rejected by the "scientific revolutions" and are now considered straightforwardly false.

Yet, Vickers holds this criterion is borne out by the history of science: no claim fulfilling the requirements of (C) has ever been rejected: all the once largely accepted claims that have subsequently been rejected were still debated by at least some 6% of the specialist scientists. Moreover, Vickers lists 30 statements or whole bodies of knowledge which met criterion (C) already long time ago, (pp. 12-18, 220).

However, one might grant that (C) has been confirmed so far, but ask whether this is only a contingent fact, or we can be assured that also present and future claims fulfilling (C) will be future-proof. In other words, one may ask whether (C) is predictively reliable, and an affirmative answer may be provided only by justifying (C): *why* can we trust that claims fulfilling (C) are future-proof?

To begin with, Vickers remarks that in some cases almost unanimous consensus is reached when, thanks to technical progress, entities, structures or behaviors which were originally unobservable become directly observable by means of appropriate instruments, as it happened with continental drift and SARS-CoV-2 virus. But, he adds, scientific debates on the reliability of instruments, like the question whether we see through optical microscopes, were closed many years ago (pp. 198-200, 221): therefore, claims that are unanimously accepted on such basis are certainly true and destined to be preserved in the future.

As to the claims that are not even instrumentally observable, it must be considered that science is essentially a critical activity, with strong epistemic and sociological premiums on criticism and nonconformism; therefore, if any doubts on a given claim were still possible, at least a substantial minority of scientists (well over 5%) would have raised them. "Any solid international scientific consensus is so hard-won that the evidence base has to be truly enormous to achieve it" (p. 112). As Kuhn himself once remarked, "History suggests that the road to a firm research consensus is extraordinarily arduous". This is why if at least 95% of scientists has no doubts on a claim, it must be future-proof.

Still, it may be objected that various sociological and epistemic forces in scientific practice drive to conformity, and Vickers is ready to grant that these forces may be even stronger today than in Kuhn's times. Therefore, when a claim gains acceptance, the "band-wagon" effect is a possible threat (p. 220). Thus, in the end, Vickers gives up providing a full principled explanation of *how* scientists may reach such a 95% consensus, and *why* it should be such a reliable indicator of truth and stability; instead, he settles for just maintaining *that* it is, by induction from its confirmation by the history of science so far.

## 3. *Why* is Vickers' criterion reliable?

[2] In practice this may be more difficult: as Vickers remarked in discussion, some scientists are not convinced that global warming is anthropogenic, but they are very few. More or less than 5%? Of course, there aren't exact figures, moreover, it depends on which scientists we count as competent on this matter. Therefore, some have objected that in practice checking whether a claim satisfies (C) can be so hard, that it isn't actually helpful. Yet, while in some cases it may be difficult, in other cases is certainly easier. Besides, even if it may be difficult to give an *exact* answer, in many cases it will be possible to give a more or less approximate one, and for practical purposes that may certainly be enough.

However, if a 95% consensus could be reached for sociological reasons (or for other epistemically irrelevant reasons), the fact that claims passing the (C) standard have been preserved *so far* wouldn't be a sufficient reason to assume that they are true and that they will always be accepted in the future, i.e., the inductive support for (C) would become weaker, or even irrelevant. This is especially so if we consider the empirical underdetermination of theories, Stanford's (2006) claim that at any time, hence even now, there are alternatives to accepted theories which escape us, but which might be the true answer to our questions, and the abovementioned difficulties in identifying certainly true claims by the NMA. Therefore, the questions of how the 95% consensus is reached and why it is reliable remain compelling.

Therefore, in order the reliability of criterion (C) one should show

(A) which *epistemically* relevant reasons (i.e., relevant to truth of a claim) may produce a 95% consensus,

and

(B) why epistemically irrelevant reasons could not produce such a consensus.

One answer to question (A) is provided by Vickers

for the claims which have become observable due to new sophisticated instruments.

Here I will try to suggest some further answers for non-observable claims.

While I take it that in the case of (instrumentally) observable claims Vickers' answer is clear and satisfactory, here I shall try to explore some further steps toward answering this question in the general case.

It is usually assumed that the probability conferred by some empirical evidence *e* to a hypothesis *H* is given by Bayes' theorem

(i) $p(H/e) = [p(H) \cdot p(e/H)] : p(e)$,

or, in its extended formulation,

(ii) $p(H/e) = [p(H) \cdot p(e/H)] : \{[p(H) \cdot p(e/H)] + [p(\neg H) \cdot p(e/\neg H)]\}$.

However, according to the NMA the success of novel predictions provides also some second-level evidence for *H*, and this is why: all true hypotheses have only true consequences, while not all the consequences of false hypotheses are true: in fact, most false hypotheses have mostly false consequences. For instance, if it is true that

**H** Tomorrow is Tuesday,

all the consequences of H are also true:

$\mathbf{C_1}$ Today is Monday
$\mathbf{C_2}$ Yesterday was Sunday
$\mathbf{C_3}$ The day after tomorrow is Wednesday
$\mathbf{C_n}$ Etc.
$\mathbf{C_{n+1}}$ Today is a weekday
$\mathbf{C_{n+2}}$ Tomorrow's name's initial is T
$\mathbf{C_{n+3}}$ Etc.

Instead, consider the six false hypotheses on which day is tomorrow: none have the true consequences $C_1$-$C_n$; besides, only five of them have $C_{n+1}$, only one has $C_{n+2}$, etc.

Now, theoreticians look for true hypotheses, so when they pick a false hypothesis with a true consequence, this is just by chance. Therefore, we may ask which is the probability that by chance one picks a false hypothesis *H* with a true consequence *ne*. Clearly, this is equal to the ratio of false hypotheses which entail *ne* to all the false hypotheses on that subject.

In turn, that ratio is inversely proportional to the logical content of *ne*: if *ne* is tautological all hypotheses entail it, while if it is contradictory none does; in between, the greater the logical content of *ne*, the fewer the hypotheses which entail it. We may measure the logical content of *ne* as the ratio of the number of equiprobable cases it excludes to the number of all equiprobable cases (where equiprobable are the cases which we have no reason to believe are differently probable).

Conversely, we may call “logical probability” of *ne* the ratio of the number of equiprobable cases it allows to the number of equiprobable cases: if *ne* is tautological (allowing all the cases) its logical probability is 1, if it is contradictory (excluding all the cases) it is 0.

Hence, the probability that by chance one picks a false hypothesis H entailing *ne* is equal to the logical probability of *ne*.

Determining what are the equiprobable cases is not always possible, and it involves a principle of indifference which depends on our background assumptions. However, in several actual scientifically relevant cases one can figure out in a substantially clear way what the possible alternative cases are, hence which is logical probability of *ne*.

For instance, when Adams and Leverrier predicted the mass and position of a new planet (later called Neptune), , the equiprobable cases where the other positions and masses a new planet might possibly have had. As concerns the mass, it is rather vague which the possible equiprobable alternatives were (e.g., those not obviously too large or too small). However, their prediction of the position missed the mark by just 1° over 360°, so their true prediction was n°±1°, hence its logical probability was 2/360, or 1/180. This is to say that only one in 180 logically possible (groups of) hypotheses entailing predictions on a new planet entailed the right one. Therefore, also the probability that *by chance* a theoretician picked a hypothesis entailing the right prediction was 1/180.

But, on the one hand, all false hypotheses are picked by chance, since theoreticians look for true hypotheses. On the other hand, there are countless false hypotheses for any true one, hence, even if by chance one might have picked also the *true* hypothesis entailing this prediction, that chance is negligible. Therefore, in practice, all the (groups of) hypotheses entailing a true consequence are false if and only if they are picked by chance.

Hence, the probability that Adams and Leverrier predicted Neptune’s position using false hypotheses (i.e. that the hypotheses of Newton’s gravitation theory essentially used by them were false) is 1/180. Conversely, the probability that they made this prediction using true hypotheses (i.e., that the hypotheses of Newton’s gravitation theory essentially used by them were true) was 1-1/180.

In general, the probability that one makes a true prediction *ne* using a false hypothesis *H* equals *lp(ne)*. Hence, the probability that one has used a true hypothesis, i.e., that *H* is true, is the complement of *lp(ne)*:

(iii) p(H/ne) = 1-lp(ne).

Therefore, when *ne* is very risky, i.e., *p(ne)* is very low, *p(H)* gets close to 1: it would be a “miracle” if *H* were not true.

Besides, many contemporary novel predictions are much riskier and much more approximated than the prediction of Neptune’s position, hence the probability that the hypotheses used to derived them is proportionally much larger: for instance, quantum electrodynamics predicted the magnetic moment of the electron to be $1159652359\times10^{-12}$, while experiments found $1159652410\times10^{-12}$: hence John Wright (2002, 143-144) figured that the probability to get such an accuracy *by chance*, i.e., through a false theory, is as low as $5\times10^{-8}$.

Of course, figures like these cannot be considered as literally and exactly true, since besides these probabilistic relations there may be a number of confirming factors which resist quantification. However, they are vivid illustrations of how epistemically relevant considerations may warrant a 95% consensus.

Thus, in the ideal case of NMA we might recognize that *H* is (at least partly) true just by one piece of evidence. Something similar, perhaps, happened with Eddington’s confirmation of Einstein’s prediction of the bending of light in the 1921 solar eclipse.

It might be objected that, just because there are infinitely many false hypotheses for any true one, the probability of finding a true hypothesis entailing *ne* is still lower than that of finding a false one entailing *ne*. Indeed, this would be the case if all hypotheses were picked by chance. But in that case even the probability of picking a false hypothesis entailing a true risky prediction would be minimal, definitely too low to explain our many extraordinary successful predictions. Therefore we must conclude that, scientists do not pick hypotheses randomly, but seek *true* hypotheses (which necessarily entail true consequences), and sometimes find them, since they employ reliable heuristics. This is why hypotheses which licensed novel risky predictions are most probably true (White 2003, Alai 2014c § 6).

Bayes' theorem establishes the probability of *H* by assessing the first-level evidence we have for *H* itself, both the earlier evidence accounting for H's antecedent probability, and the additional evidence *e*, conferring H its conditional probability (a first-level assessment). Instead, the NMA assesses the probability that *H* is true given that it has licensed a novel risky prediction *ne*, irrespectively of what *H* and *ne* say and of the first-level evidence we have for them. In fact, '*p(H)*' and '*p(e)*' do not appear on the right-hand side of equation (iii). Thus, the NMA bypasses the thorny problem of the prior probabilities of *H* and *e*.

Of course, in the overall assessment of *H* one should take into some consideration also the logical probability of H and its conditional probability based on first-level evidence; but the logical probability of relevant scientific hypotheses is typically very low, and as pointed out by Vickers it is practically impossible to establish that a claim is future-proof by assessing first-level evidence; instead, in ideal cases the NMA by itself may produce a reliable 95% consensus that a hypothesis describes a scientific *fact*.

There are problems, however. First, as explained above, in this way we cannot establish whether *H* was used essentially, hence whether it is completely true or not, so whether it is actually future-proof. Alternatively, we might say that we can establish that it is future-proof *in the weaker sense* that *a part of it* is completely true, hence properly speaking future-proof.

Second, the novel prediction *ne* may be not very risky, and the probability of *H* decreases as the logical probability of *ne* increases. Third, a NMA may be less than ideal also because it is unclear whether the prediction or predictions involved are actually novel, and even more because (as suggested by the literature on novel predictions), we should probably consider novelty as coming in degrees.

Often, however, a hypothesis *H* is employed in more than one (more or less risky) novel predictions $ne_1 \ldots ne_j$. For instance, Adams and Leverrier predicted both the position and the mass of Neptune; Mendeleev predicted various properties of different new elements; etc. Thus, we get a new measure

(iv) $p(H/NE) = 1\text{-}lp(NE)$,

where *lp(NE)* is the conjunctive logical probability of $ne_1 \ldots ne_j$. Of course, *lp(NE)* (the conjunctive probability of many predictions) is smaller than *lp(ne)* (the logical probability of just one prediction) and it gets smaller and smaller as the number *j* of predictions raises. Therefore, it can be very low, hence the probability of the hypothesis *p(H/NE)* can approach 1, even if each if those predictions are not very risky.

Instead, if it is unclear whether the predictions are actually novel, or if they are only partially novel, the probability of *H* will be lower by a factor *k* (where the less novel the prediction is, or the less clear that it is novel, the greater is *k*): if *n?e* is a *possibly* novel prediction, we have

(v) $P(H/n?e) = 1\text{-}lp(n?e)\text{-}n$,

or in case of joint predictions

(vi) $P(H/N?E) = 1\text{-}lp(N?E)\text{-}n$.

Even this smaller probability, however, may raise the degree of confirmation conferred to *H* by first-level evidence.

Finally, also the fact that *H* accounts for *old* evidence may constitute additional second-level evidence for it. In fact, we may ask what the probability was to find a hypothesis which (no matter whether true or false) accommodated a known datum *e*. Apparently, the answer is that if the theoretician was minimally skillful, the probability was 1, for this was just a puzzle-solving exercise. But since there are infinite false hypotheses for each true one, the probability that *H* is false is practically 1.

However, things are seldom so simple. To begin with, usually *H* must account for *many* data $e_1 \dots e_m$, and the more they are, the harder the task becomes. Moreover, the data are practically never entailed *just* by *H*, but by *H* in conjunction with several collateral assumptions $A_1 \dots A_n$, which in turn must be derived from, or at least be consistent with, a number of independently accepted theories $T_1 \dots T_n$. So, one must also find the right $A_1 \dots A_n$, and *H* must not only entail $e_1 \dots e_m$, but be compatible with $T_1 \dots T_n$.

Therefore, finding a hypothesis entailing $e_1 \dots e_n$ (no matter whether true or false) may become impossible for a minimally skilled theoretician, and very difficult also for a truly gifted one: the probability that *H* is false becomes *1-n*, where *n* increases with the number of the accommodated data and of the collateral assumptions needed. In fact, if *n* becomes quite high, it is no longer plausible that *H* has been found just by pure puzzle-solving skill, and another hypothesis becomes more plausible: that the theoretician was not just trying to accommodate $e_1 \dots e_m$, but, more importantly, looking for a true hypothesis by a reliable heuristic, so that she actually found one.

This is why once I suggested that certain confirming instances apparently different from the confirmation provided by novel predictions are actually of a similar nature: for instance, the convergence of independent theories, the convergence of measurements by different experimental procedures based on independent theories, Keynes' distinction of confirming instances, and non-*ad hoc* explanations (Alai 2014b). In other words, this argument from the complexity of the theoretician's task can be turned into an argument from the improbability that just by chance independent theories converge in accounting for a large number of disparate data. Even this argument, however, may show at most that *H* is partly true, for even a partly false hypothesis may entail $e_1 \dots e_m$, and so be employed (inessentially) to account for them.

## 4. Conclusion

According to Vickers a certain kind of second-level evidence (the 95% consensus) may show *that* certain claims are future-proof. Here I have suggested that some different kinds of second-level evidence (roughly sharing the NMA structure) may motivate the achievement of such a consensus, and so explain *how* and *why* it can be reached for epistemic and not (just) sociological reasons, hence why it can be a reliable indicator of future-proof statements. That is, when at least 95% of specialists in the field take a claim to describe a scientific *fact*, they are probably right on this, at least in the *weak* sense that at least certain (unidentified) parts of those hypotheses are future-proof. This happens especially when hypotheses yield various novel and risky predictions, but possibly also when they display great systematicity and unifying power (by accounting for a very large number of known data $e_1 \dots e_m$) and great plausibility and coherence with accepted theories and assumptions (by accounting for $e_1 \dots e_m$ in full consistence with assumptions $A_1 \dots A_n$ and theories $T_1 \dots T_n$).

Vickers grants that a claim fulfilling criterion (C) may be only *approximately* true, hence it may be future-proof *modulo-minor adjustments*. This however is a potential threat for his enterprise, for approximation is a vague concept, thus there is a continuum ranging from being true, to being approximately true, or half-way between true and false, or approximately false, or finally purely false. So, if a 95% consensus can be achieved by claims that are just approximately true, why it couldn't it be achieved by claims which are even more "distant" from truth, and down to very close to falsity? He notices that in practice we can clearly distinguish when a claim is substantially true and when it is not. Perhaps we can, but only retrospectively: *ideally*, and very roughly, we might suppose that 100% consensus shows that *H* is 100% true, 95% consensus shows that it is 95% true, and so on. But things may be far from ideal, there can be a very wide consensus on hypotheses which are far from essential, hence far from completely true. Again, therefore, it is safer to assume simply that Vickers' criterion warrants that *H* is at least partly true, hence weakly future-proof.

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