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Science and illusions

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Abstract It is mostly agreed that Popper's criterion of falsifiability fails to provide a useful demarcation between science and pseudo-science, because ad-hoc assumptions are always able to save any theory that conflicts with the empirical data (a.k.a. the Duhem-Quine problem), and a characterization of ad-hoc assumptions is lacking. Moreover, adding some testable predictions is not very difficult. It should be emphasized that the Duhem-Quine argument does not simply make the demarcation *approximate* (if it were so, all our problems would be solved!), but it makes it *totally useless*. Indeed, no philosophical criterion of demarcation is presently able to rule out even some of the most blatant cases of pseudo-science, not even approximatively (in any well defined sense of approximation). This is in sharp contrast with our firm belief that some theories are clearly not scientific. Where does this belief come from? In this paper I argue that it is necessary and possible to recognize the notion of syntactic simplicity that is able to tell the difference between empirically equivalent scientific and non-scientific theories, with a confidence that is adequate to many important practical purposes, and it fully agrees with the judgments generally held in the scientific community.

1 Introduction — sophisticated pseudo-science

This paper proposes an approximate solution to the classical demarcation problem, which is satisfactory to most practical purposes[1]. To explain why no present solution is satisfactory — not even approximatively — I start from an example, which bears the essential features of all today most threatening cases of pseudo-science. Imagine that Prof. Lucky Furbetto proposes a modified Newton's theory of gravity, that contains sufficient extra terms to make it as empirically accurate as Einstein's theory of gravity[2]. Moreover, Prof. Furbetto introduces a time dependence on a parameter of his theory (say $G \to G(t)$). Such dependence is totally undetectable now, but it will have dramatic effects on December 31st 2015. Finally, Prof. Furbetto also offers a prediction: by adding more assumptions that, he claims, are somehow justified by his $G(t)$, he is able to predict three numbers that are eventually extracted at the lottery of July 18th 2015[3]. A second lottery prediction is foreseen for the summer 2025, but now Prof. Furbetto claims that we should all drop what we are doing and get ready to face the disasters coming at the end of 2015.

Intuitively, we know that Prof. Furbetto's theory deserves no serious consideration. But why, *exactly*? If we can't answer this question, how can we hope to convincingly rebutting more seducing cases of pseudo-science?

Unfortunately, this question is subtle because Furbetto's theory can't be criticized on purely empirical grounds: it agrees with all known experiments as much as our best theories. It even got a very non-trivial and impressive prediction right, that no other theory could match. What could we wish more from the empirical point of view? If we argue that there is no *reason* to postulate a time dependence on G, nor to postulate a sudden change on December 31st, Furbetto can rightly request us to specify which *kind* of reason do we want: there is no *compelling* reason to assume it constant either. On the other hand, he can provide many non-compelling but very inspiring

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[1] In Scorzato (2015) the same framework is used to define the concept of scientific progress. To keep this paper self-contained, all the relevant definitions are repeated.

[2] In practice, this can be done, for example, by following the parametrized post Newtonian (ppN) formalism (Will, 2006).

[3] Other people had done similar attempts before, but Prof. Furbetto claims that they are totally unrelated to him, and indeed their theories are completely different.

narratives for his choice. What do we want exactly? If we argue that his prediction of the lottery outcome is independent from the time dependence of G, he can reply by formulating the theory in a way that they do depend on each other[4]. The fact that we *can* formulate his theory in a way that the lottery prediction is independent of $G(t)$ is a weak argument, since we *can* also modify Einstein's gravity in a way to remove some of its empirical predictions (this is indeed what Einstein did when he introduced the cosmological constant). In conclusion, also from the strictly logical point of view there is little we can do to reveal any objective weakness of Furbetto's theory.

Popper's criterion offers no help either: Furbetto's theory is as falsifiable as our best theories. This is in fact a general consequence of the Duhem-Quine argument: we can always introduce legitimate ad-hoc assumptions that remove any present conflict between our theory and the data, while we also reserve some empirically falsifiable conclusion for the future. It must be emphasized that the Duhem-Quine argument does not simply make Popper's criterion *imprecise*, or difficult to apply in *some ambiguous* circumstances. The Duhem-Quine argument reveals that Popper's criterion is completely unable to rule out even the most blatant cases of pseudo-scientific theory, as long as they are formulated somewhat carefully[5]

Even less convincing are those demarcation criteria that actually rely — more or less explicitly — on the opinion of the majority of some scientific community, or on some weird behavior of the proponents of a theory. To be clear, Prof. Furbetto is an exquisite person, with a vast culture, and he has a lot of friends who have PhDs and think he is right. So, what?

An interesting view is the proposal to treat science as a family resemblance (Dupré, 1993). But, which traits are *relevant* to judge such resemblance (Pigliucci, 2013)? Indeed, in many respects, Furbetto's theory could be seen as a twin brother of Einstein's theory of gravity.

The reason why no serious scientist needs to wait for December 31st to declare Furbetto's theory a hoax is not based on logical and empirical considerations alone, but it is not too difficult to express either: the reason is that there are *simpler* explanations for all the phenomena described by Furbetto's theory. In fact, assuming a constant G is simpler than any time dependent $G(t)$, and assuming directly the three lottery numbers, without much ado, is certainly simpler than Furbetto's attempt to relate them to his $G(t)$, since, anyway, he is not able to deduce them from his theory of gravity *without further assumptions*. This argument to dismiss Furbetto's theory is actually fully consistent with a famous definition of science attributed to Einstein: *The grand aim of all science is to cover the greatest number of empirical facts by logical deduction from the smallest number of hypotheses or axioms*[6].

However, without a definition of *simplicity* or a rule to count the *number* of hypotheses, the previous beautiful argument is totally meaningless. In fact, Prof. Furbetto can claim that he uses a single symbol Ξ to summarize all the assumptions of his great theory[7]. If we now adopt a syntactic definition of simplicity, Furbetto can easily claim that the simplicity of his theory can't be beaten...

It is vain to blame a syntactic definition of simplicity for the failure to tell what's wrong with Furbetto's theory and argue for the need of better notions of simplicity or other cognitive values. In fact, if Furbetto claims to be able to interpret Ξ (and we have no clear argument to refute that claim), and if the theory that states just Ξ is empirically accurate, as it is, how could we regard that theory as less than optimal by *any* respect? We can't solve this problem unless we clarify — first of all — what's wrong with the Ξ formulation.

The key must lie in the plausibility of the claim that Furbetto can *interpret* Ξ. Assessing this is a delicate issue, which is neither trivial nor hopeless. But, sadly, in recent decades, both philosophers and scientists have failed to appreciate the importance of telling clearly what's wrong with a Ξ-like formulation and, at the same time, pseudo-sciences, and bad science in general, have flourished. It may be argued that the spread of pseudo-sciences relies much more on deep ignorance and dishonesty, rather than in the lack of a useful philosophical criterion of demarcation. But these claims underestimate the essential need of some kind of respectable justifications that any pseudo-science needs to show off, when necessary, in order to survive. These claims also underestimate the waste of resources on scientific programs that could be more easily recognized as ill-fated with a clearer understanding of the general goals of science. Philosophy of science isn't necessary to enable good science, but it is essential to prevent bad science.

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[4] E.g. by assuming: `(lottery-outcome =` $(7,13,42)$ `AND` $G(t) = 1 + exp[t/\sigma]$`) OR (lottery-outcome =` $(3,18,51)$ `AND` $G(t) =$ `const)`.

[5] Testability (Sober, 1999) also can't tell what's wrong with Furbetto's theory, since its statements concerning the past have been verified and those about the future are testable and different from standard Einstein's theory. Indeed, it is not difficult, in general, to elude also the criterion of testability by means of auxiliary assumptions and small, cunning modifications of a theory.

[6] Quoted in Life Magazine, January 9,1950.

[7] We can define Ξ as standing for the whole system of equations of Furbetto's theory. Alternatively Ξ may be a Gödel number that describes the whole system. I thank Elliott Sober for suggesting this latter representation.

2 A well defined framework for science — what's wrong with Lucky Furbetto's theory

In this section[8], after identifying what's wrong with the Ξ formulation of Furbetto's theory, I introduce a definition of scientific theories and their conciseness that essentially justifies the intuitive notion of simplicity commonly used by scientists — although often unconsciously. This finally provides a clear and general argument to exclude Furbetto's theory from the circle of scientific theories. In the following section I apply the same argument to other less subtle but socially more relevant cases of pseudo-scientific theories.

As argued in the Introduction, we must first understand what's wrong with the Ξ formulation of Furbetto's theory, which must be related to the fact that Ξ can't plausibly be interpreted or *measured directly*. Defining in general what can or cannot be measured directly is perhaps impossible. But, as first observed in (Scorzato, 2013), we can at least identify *one necessary property* of any directly measurable quantity, which is a sufficient characterization for our goals:

Postulate 1. (Directly measurable properties). *The result of a valid* direct measurement *of a property Q is expressed as: $Q = Q_0 \pm \Delta$, where Q_0 is the (unique) central value of the measurement and Δ the sensitivity of the experimental device (i.e. the minimal detectable difference between two nearby outcomes). This expression implies a probability distribution for the result of the measurement that is centered in Q_0 and systematically decreases with the distance from Q_0 (other features of the distribution are theory dependent).*

Postulate 1 might seem obvious and easily fulfilled, but it is not always so. In particular it is not fulfilled by the variable Ξ. In fact, Prof. Furbetto is certainly able to describe entirely the status of the system with just one variable Ξ. But he is in trouble when he has to assign an errorbar Δ to his measurement of Ξ. In fact, if the mass of one planet were slightly different — a difference that he can't exclude, because it is at the limit of the experimental sensitivity of any possible measurement of that mass — the value of Ξ should be still within its errorbar Δ. However, no conceivable representation of that system with a single variable Ξ can ensure that. For example, if we represent the system with a Gödel number, a slightly different status of the systems will almost always correspond to a completely different number (see Scorzato, 2013, for a more general discussion). In conclusion, although we can represent every system with a single variable, we have no guarantee that we can (and, indeed, we mostly can't) use that variable to represent the interval of confidence in which the system might be, as far as we can tell.

This observation identifies a well defined and testable limitation of the Ξ formulation of Furbetto's theory. This is very important, because it can be used to correct the only serious difficulty of a classic view of scientific theories (Feigl, 1970). In fact, we can now describe a scientific theory as a logical-mathematical system whose empirical interpretation does not rely on mysterious *correspondence rules*, but it simply requires that some concepts are *directly measurable*, at least in the testable sense expressed by Postulate 1. More precisely:

Definition 1. (Scientific theories). *A scientific theory is a quadruple $T = \{P, R, B, L\}$, where*
- *P is a set of principles*[9],
- *R is a set of results deduced from P (according to the logic rules included in P),*
- *B is a set of properties that appear in P and are* directly measurable *in the sense of Postulate 1 (we call them Basic Measurable Properties, or BMPs, of T),*
- *L is the language in which all the previous elements are formulated.*

Note that the BMPs do not constitute a *universal* basis: there is no such thing. Note also that Postulate 1 and Def. 1 cannot *fix* the interpretations of the BMPs. Nothing can do that: neither correspondence rules, nor bridge laws, nor models. Theories face the tribunal of experience as a whole (Quine, 1950), and the *assumptions* that their BMPs are sufficiently unambiguous are necessarily part of the overall theoretical assumptions. The aim of Postulate 1 is not to fix the interpretation of any expression, its aim is rather to exclude a class of interpretations that we deem certainly implausible.

Not all measurable properties need to be included in the principles P of the theory. The other (possibly unlimited) measurable properties can be characterized as follows:

Definition 2. (Measurable properties). *The measurable properties (MPs) of a theory T are all those properties that can be determined through observations of the BMPs B of T, by possibly employing some results R of T. Their precision is also determined by T.*

[8] This section is a brief review of the framework introduced in (Scorzato, 2015, 2013), applied to the case of Furbetto's theory.

[9] The principles contain *all* the assumptions needed to derive the results of the theory, from the logical rules of deduction to the modeling of the experimental devices and of the process of human perception, so that no further *background science* is needed. Note that also the *domain of applicability* of the theory can and must be defined by specifying suitable restrictions on the principles themselves. Any needed auxiliary assumption should also be included.

Hence, the BMPs must be sufficient to enable the measurements of all the MPs that the theory needs to describe. In other words, the BMPs provide — together with the principles to which they belong — the basis on which the whole interpretation of the theory is grounded. Thanks to the identification of the BMPs, the principles truly encode *all the assumptions* of the theory, in a sense that goes beyond the logical structure of the theory, and it includes the assumptions that *we know how to measure some quantities*. These assumptions might be wrong, like any other.

For example, both Einstein's theory and Furbetto's theory need to assume standard logical rules, standard mathematics and physics principles. They also need to assume a model for each experimental device that they use (we can assume they use the same devices). They need to assume that the values displayed by the readers of those devices are unambiguous (these are the BMPs). The two theories differ because Furbetto's theory assumes a ppN framework, as opposed to the classic Lagrangian for Einstein's gravity; moreover, Furbetto's principles include the assumption of a time dependent $G(t)$, instead of the usual constant G; finally, Furbetto's principles include some assumption that implies (by using also some properties of $G(t)$ that we do not need to detail) the lottery outcome: $(7, 13, 42)$, for July 18th. The theory that we will compare to Furbetto's theory includes, besides the standard assumptions of Einstein's theory, also the straightforward assumption that the lottery outcome for July 18th is $(7, 13, 42)$, with no attempt to establish deeper connections. In the following, I use the symbol E when referring to the theory that includes Einstein's assumptions and the straightforward assumption of the correct lottery result for July18th, while I use the symbol F for Furbetto's theory. Hence F and E are fully equivalent, from the empirical point of view, until next December 31st 2015.

Before telling in which sense F is less valuable than E, we need one last definition. In fact, according to Def. 1, two theories expressed in different languages count as different theories. This is not satisfactory, because we cannot criticize only some formulation of F: we need to target any of its legitimate reformulations. In general, we should then identify those theories that are equivalent both from the logical and the empirical point of view (including future predictions). More precisely:

Definition 3. (Equivalent formulations for T). *We say that T and T' are equivalent formulations iff:*
*(i) there is a translation $\mathcal{I}$ between T and T' that preserves the logical structure and the theorems (*logical equivalence*);*
*(ii) and for each MP c of T (resp. c' of T'), $\mathcal{I}(c)$ (resp. $\mathcal{I}^{-1}(c')$) is also measurable with the same precision and the same interpretation (i.e., an experiment that measures c within T also measures c' within T') (*empirical equivalence*).*
$\mathcal{L}_T$ denotes the set of all pairs (L, B) of available languages and BMPs in which we can reformulate T and obtain a new theory T' that is equivalent to T. In the following, the symbol T refers to a scientific theory up to equivalent formulations, while $T^{(L[,B])}$ refers to its formulation in the language L [and basis B].

Def. 3 implies, in particular, that the Ξ formulation suggested by Furbetto is not equivalent to F. In fact, the translation that makes them logically equivalent (which does exist!) cannot realize also an empirical equivalence, because if Ξ stands for all of Furbetto's assumptions, Ξ is not an acceptable BMP and — being the only formal property of the theory — the theory is left with no MP at all. So, we have been finally able to draw precisely a useful distinction between valid and non-valid formulations of F.

Can we now also tell what's wrong with the scientific value of F? We have already noticed that E offers a simpler set of assumptions to describe all the phenomena described by F. If we could rely on their respective formulations in ordinary language, we could clarify what we mean by *simpler* merely in terms *word counting*. In fact, E is more concise than F, as already noticed. Now that we have justified new rules for legitimate reformulations, that, in particular, rule out the Ξ formulation, there is no obvious reason why word counting should not be an adequate measure. So, can Prof. Furbetto still reformulate his theory as concisely as E? No.

In fact, Furbetto's task has become suddenly awfully hard, because adding new symbols won't help him. To match or surpass the conciseness of E he should compensate the complexity of his $G(t)$ by relating it more directly to some measurable quantity. This is impossible without recognizing a deep empirical meaning for $G(t)$, which is exactly what his pseudo-scientific theory cannot offer.

Hence, a formulation independent measure of conciseness that emphasizes and exploits these limitations of F can be defined as the minimal word counting over all legitimate formulations. More precisely:

Definition 4. (Complexity of the assumptions; conciseness). *Let $P^{(L,B)}$ be the principles of T, when expressed in language L and with BMPs B. Let the* complexity of the assumptions *of T be:*

$$\mathcal{C}(T) = \min_{(L,B)\in\mathcal{L}_T} \text{length}[P^{(L,B)}] \tag{1}$$

Let the conciseness *of T be the inverse of $\mathcal{C}(T)$:* $\mathrm{Conc}(T) = 1/\mathcal{C}(T)$.

Note that the measure in Eq. (1) — being the minimum over all available equivalent formulations, in the sense defined previously — is as language independent as we could possibly wish. Note, however, that for theories that include many assumptions from a variety of background theories — as it is common for any realistic theories — it is practically impossible to achieve greater conciseness by employing a radically new language, without making the connection to measurable quantities even more cumbersome. This is so, for the same reasons noted in the case of F. *This consideration justifies, for realistic theories, an estimate of conciseness based on their formulation in ordinary language*.

In conclusion, theory F should be dismissed because — even in its most concise formulation available — it is unambiguously less concise than another theory (E) which is empirically as accurate as F. To be clear, I state the general criterion as follows:

Definition 5. (theories with no scientific value). *If two theories T and T' are empirically equivalent, but theory T' is more concise than T, then T has no scientific value*[10].

Note that a theory T may have no scientific value, according to Def. 5, for very different reasons: it may be the result of a completely ill-fated strategy that will never lead to valuable theories, or it might be simply a failed attempt within a good strategy. The goal of this criterion is not to give a simple rule to decide which directions of research are worth pursuing (although it might help also to that purpose), but to give a simple rule to decide which theories are not worth consideration when we need to relay on their predictions.

Note, finally, that Def. 5 does not rely on any trade-off between conciseness and empirical accuracy: conciseness rules out a theory only when it has no empirical advantage.

3 Other (pseudo-)scientific theories

In the previous section I have introduced a general and well defined criterion to tell when a theory has no scientific value, and I have shown that it leads to the expected conclusions for Furbetto's theory. Does my criterion work for other (pseudo-)scientific theories? Since the previous discussion essentially justifies the use of ordinary language to estimate the complexity of the assumptions of realistic theories, it is not difficult to apply the same criterion to many other cases. In this section I consider some notable cases of pseudo-science and unsuccessful science. These accounts are necessarily brief, but I think they convey all the important ideas.

It is also natural to ask whether this criterion might also rule out any valuable scientific theory. I am not aware of any such case (some examples of valuable theories that are recognized as such are discussed in Scorzato, 2015). Finding a counter-example is my challenge to the readers.

It is important to note that the above criterion only tells whether a theory is better than another available one. It does not judge entire research programs. However, this criterion may certainly help to evaluate the chances of progress that a research program might have.

3.1 Solipsism

A paradigmatic case that we should regard as pseudo-science is solipsism. This is actually a very valuable philosophical idea, because it provides a very good test for any theory of science. In fact, any good theory of science should be able to tell why solipsism is not a valuable scientific option. Solipsism cannot be excluded neither on logical nor on empirical grounds. It is sometimes excluded because it is declared *weird*, which is hardly a fruitful argument, although I do not want to contest the judgment itself.

But the framework of the previous section does give a clear verdict in this case: solipsism requires an unnecessary amount of assumptions to explain the experience. In fact, the experiences *reported* to the subject by other people require different explanations — and hence additional assumptions — besides those explaining the *direct* experiences of the subject. What the subject sees and what she hears from the reports of the other people can be explained much more *concisely* by assuming an underlying reality, independent of her mind. Hence, our rule of conciseness does the job of declaring solipsism a non scientific option.

[10] Note that the determination of conciseness is affected by many sources of uncertainty and can only be estimated approximatively (Scorzato, 2015). Hence, we can say that a theory T has no scientific value only within the confidence that we can associate to the determination of its lower conciseness with respect to T'. For this reason, any conclusion about the lack of scientific value of a theory is necessarily approximate (just like any scientific conclusion), but it is so in a well defined sense.

3.2 Intelligent Design

The theory of Intelligent Design (ID) (Dembski and McDowell, 2008) has been expressed in many forms, and criticized with many arguments (see, e.g., Sober, 2007; Lutz, 2013, and references therein, for an introduction to a larger literature). But, unfortunately, these criticisms do not address the most careful versions of ID, and they are therefore too weak.

The careful version of ID that I want to consider here (I call it ID*) is constructed as follows. ID* accepts all modern best scientific theories, except that, occasionally — and precisely in front of complex biological structures for which there is currently no convincing evolutionary explanation (i.e. those structures for which the estimated probability of an evolutionary development seems puzzlingly low, given the present evidence) — ID* postulates the intervention of a *designer*[11].

There is no way to rule out ID* in terms of empirical accuracy, predictivity, falsifiability, testability or similar criteria, for the same reasons why we could not rule out Furbetto's theory. The sound reason why ID* is not scientific is that, when we clear ID* from all the rhetoric and leave only the bare minimum which has some deductive value, we find that the most concise assumption consists in just postulating the appearance of those structure[12]. This reduces ID* to something that would not appeal its proponents anymore and it is clearly no better than the standard theory.

In other words, the fundamental problem of ID is very simple: ID does not even attempt to do what science always must do: deducing as many phenomena as possible from as few assumptions as possible. On the contrary, ID de facto claims that the goal of science is, sometimes, not attainable. Denying the possibility of scientific progress is not a crime (and might even prove true!), but it has nothing to do with science either.

3.3 Climate change denials

Denialism is sometimes defined as the *rejection of basic concepts that are undisputed and well-supported parts of the scientific consensus*[13]. Indeed, many philosophers who analyze single cases of denialism concentrate on assessing the present level of consensus among the scientific community. Of course, this strategy is inconclusive, at best. Instead, it is essential to discern good from bad reasons of dissent, regardless of their popularity.

For example, until about 15 years ago, climate models typically accepted that the effects of anthropogenic CO_2 were negligible. This was justified because the amount of anthropogenic CO_2 was much smaller than the amount routinely exchanged by the oceans. However, that conclusion could be derived only under further assumptions. Hence, the fact that anthropogenic CO_2 could be neglected was, essentially, a reasonable *assumption*.

Around the year 2000 the International Panel for Climate Change (IPCC) started to recognize that those climate models that *calculated* (through computer simulations) the anthropogenic interference on climate were in better agreement with the data than the models *assuming* no interference. This was a turning point, because it showed that a more concise model (in which the assumption of negligible anthropogenic effects was removed[14]) was also in better agreement with the data. At that point — with a confidence determined by the associated imprecision — it became less and less scientific to assign any value to the old models.

Of course this does not mean that *any denial* of significant anthropogenic effect is not scientific. But if one does deny it, then he has to indicate *which alternative model* he is considering. Because what is certainly not scientific (but, sadly, fairly common) is to deny anthropogenic effects while still referring to the old models, or to no model at all. A serious scientific statement about some phenomena should always refer to the model (or set of assumptions) that it is adopting to deduce those phenomena. It is not the denial itself of a phenomenon that can be not scientific, but the choice of the theory to support the claim.

Unfortunately, much rhetoric claiming that some scientific results be *incontrovertible scientific facts* greatly undermines the possibility to draw a robust distinction between scientific and non-scientific claims.

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[11] This is also nicely described as the *God of the gaps* (Coulson, 1955).

[12] Alternatively, one could postulate a *designer* with sufficient details to logically deduce the probability that the designer might be able, and willing, to build those complex structures. Without a concrete proposal in this sense, there is no point to discuss this possibility further.

[13] See en.wikipedia.org/wiki/Denialism and references therein.

[14] I assume, as it seems legitimate, that the assumptions needed to use suitable Monte Carlo simulations were already needed anyway also in the older models.

3.4 Some versions of multiverses

Poor science is not reserved to unscrupulous groups determined to fool their audience. It is a serious risk also among the best families of scientists.

A typical illusion is that of trading some postulates that we do not like with others that have no scientific advantage but sound more appealing. As long as this exercise is recognized as a simple rephrasing, there is nothing wrong. The problem comes when we fall into the illusion of having achieved a much deeper understanding, we establish research programs and invest resources to elaborate on it.

For example, it is a great temptation to try to *explain* the puzzling fundamental constants of the Standard Model of particle physics and cosmology by saying that, perhaps, there are actually many universes with different fundamental constants and the one in which we live is simply one of the few that are compatible with life[15].

This kind of Multiverse is certainly a possibility. What is wrong, however, is to sell it as an *explanation*. Note that we can't naively blame Multiverse for the lack of novel predictions that distinguish it from the standard theory[16], more than we can blame the standard theory itself. The important point is that these Multiverses also lack of any *cognitive* advantage. But we need a cognitive value like conciseness to understand it. In fact, we should ask ourselves: is it simpler to assume some specific constants as fundamental or is it simpler to assume a more fundamental probability distribution of possible fundamental constants? There seems to be little hope that the latter could be more concise, in any realizable formulation. Hence, we should conclude that, unless we are able to deduce something more, the advantages of Multiverse are mere illusions.

Once again, it is important to consider only those formulations that don't loose contact with measurable quantities, and to disregard all the narrative that has no deductive power.

3.5 The general risk of bad science

Good science is hard: it requires formulating and testing theories on relevant topics that are more concise and/or more predictive than the available ones. This is the only way we have to understand and control better the natural phenomena. Often there is no clear cut between bad and good scientific disciplines, but rather a continuum connecting unsuccessful theories to unsuccessful research programs, hopeless research programs, ill-conceived research programs, and finally fully fledged pseudo-science.

The temptation to address questions that are meaningful only within a narrow community, because of the loss of contact with more general goals, is always strong. This temptation can take many forms: regarding new formulations as progressive, although they only change the narrative; working for too long on theories that can't even reproduce our current best theories, working on theories where it is always acceptable to add a new term when we see a disagreement with the experiments; performing experiments that, if they fail, we can only blame the experiments, and if they succeed, we won't be able to find a useful application... It would not be fair to name topics, because the relevant boundaries are often not between topics, but have a much more complex structure.

The only way to avoid all these problems is to be regularly evaluated by referees who are not field experts, and hence are less likely to share the same prejudices. But to do that seriously, it is necessary to enable them to assess what has been achieved and what we want to achieve. To this end, it is absolutely necessary to recognize all the assumptions that we need and express them with the lowest possible degree of decorations.

There was a time, not too long ago, when scientists seemed to enjoyed the mystery that surrounded their work. These times are gone. Now, if we believe that what we do is important, we need to convince someone to invest on it (be private investors or taxpayers). Showing better applications is not the only way: also showing a better understanding is a strong and convincing argument, but we must also show that we understand what *understanding* means.

[15] Actually, as far as I know, the only case in which we can convincingly conclude incompatibility with life is for a larger cosmological constants. Most other claims are based on the observation of how one particular phenomenon would change, while ignoring all other possible effects.

[16] Assuming that there aren't, of course.

4 Conclusions

It is often very difficult to tell whether a new theory is valuable: we need to recognize all its explicit and implicit assumptions, draw its obvious and less obvious consequences, estimate what we might be able to deduce with more calculations. We also need to judge whether it represents a fruitful direction of research, beyond its present, perhaps limited, results. However, once we have recognized all its assumptions and put together all its present results, it is usually not difficult at all to determine whether that theory is better, in some respects, than some currently accepted theory or not. And it is also not difficult to explain to non-experts the precise reasons of our conclusion. Philosophy of science does not need to solve the problems that no scientist can solve, but it does need to identify clearly the general rules behind those important decision that the scientists regard as obvious.

Although the scientists often feel that they do not need philosophical help for this task, they should recognize that spelling clearly the detailed rules for their *obvious* decision of theory selection is essential to limit the spread of pseudo-science, to prevent the waste of too many resources on bad science and also the offer better tools to the society who need to know what the scientists are doing.

In this paper I have argued that it is possible to identify rules that are sufficiently general and precise to tell what's wrong in many important cases of theories with no scientific value. But to do this, it was essential to drop the rhetoric of science as something that *proves facts*, and recognize science as the art of finding the simplest description of what we see. Moreover, it was essential to recognize that the notion of *simplicity* does have a well defined and rather precise meaning in science, if we use a formulation that refers to measurable quantities.

This leads to a view of science which is just a small, but crucial, clarification of the famous Einstein's view already mentioned: *The grand aim of all science is to cover the greatest amount of empirical data by logical deduction from the smallest amount of hypotheses, quantified as the length of the most concise formulation available, that refers to measurable quantities*. The narrative surrounding some theories is valuable when we construct them, but it is helpless on judgment day.

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