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Q&A on Naturalness and Extra-Empirical Theory Assessments

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I have had numerous questions and comments of my recent paper "Naturalness, Extra-Empirical Theory Assessments, and the Implications of Skepticism", which can be found at arXiv:1806.07289 [1]. Due to the many inquiries and the overlapping questions, I thought that for some efficiency I'd collect some of the most often heard "soft" questions into a Q&A format below.

The unnaturalness of high finetuning has been compared to pornography — it's hard to define, and the edges might be fuzzy, but away from the edges you know it when you see it. Aren't you just messing around at the edges, and then using that to unfairly throw out naturalness and finetuning as required elements of a theory?

I have heard that analogy many times, with the implication that when you're deep into very high finetuning then nature just will not tolerate it, like polite society will not tolerate pornography. Well, if you want to go with that analogy the cosmological constant appears to be a Caligularian Debauchery of Bestiality. Yet nature tolerates it. The Higgs boson appears to be at least as bad as Nymphomaniac, which some people say is a pornographic film while others might disagree. Nature appears perhaps to be tolerating this pornographic finetuning at levels that most did not anticipate.

So, you believe that naturalness has no value at all, and nature has no worries about finetuning?

I answer that question by saying that I am officially neutral. The reason is that I believe that theories *generally* should be natural and not finetuned, but that there are cases where it may not be. The analogy is that the B meson usually decays into a charm meson but on rare occasions it decays into a strange meson. Thus, I think it has value in general and is worth considering, but one should not consider it a primary required consideration in theory model building. In my heart of hearts I think it is quite possible that what we see today as a large finetuning (e.g., cosmological constant and Higgs boson mass) will one day appear more natural through more enlightened eyes. Maybe it is landscape physics that will do it. Maybe it is extreme fixed point behavior of a deeper theory. Whatever the case may be, I certainly do not rule that out and even expect it in most cases. However, forbidding theories out of hand because our limited brains think they look finetuned or unnatural is short sighted and counter-productive to science progress. Likewise, requiring that we only pursue and investigate natural, un-finetuned theories from our immediate limited perspective is short-sighted.

You have put forward SEETA (skepticism of extra-empirical theory assess-

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ments) as a guiding principle. But how can science progress if there is no judgments between competing theories?

First, the premise of your question is a misunderstanding. I strongly advocate the use of extra-empirical assessments. It's what makes human understanding increase, and helps make science so powerful. SEETA, as I describe in the paper, is skepticism that extra-empirical theory assessments can state which speculative theory among competing theories is "more correct" or "more likely".

Furthermore, I have not argued that one is *required* to accept SEETA. SEETA is a credible approach, however. In my view, there is no incontrovertible argument in the literature that would compel one to think the SEETA approach is wrong, just as there is no compelling argument that SEETA must be accepted. Thus, one should contemplate its consequences, which may reveal some of its weaknesses and strengths, and spend some time forgetting about trying to divine which theories are "more correct" or "more likely" and focus on developing theories that are "better" and more empirically driven. Now, what's "better"? There are a thousand answers to that, and letting a thousand flowers bloom is good for science. Let people hash it out. Among many qualities I like are theories that unify what we know — less parameters than others, more symmetries, etc. They are often falsifiable in their predictions because they are more constrained, and if they turn out to be confirmed they are more economical descriptions to base further investigation upon.

Wait, just to be sure I get what you're saying, how can you make a distinction between evolutionary and geological science versus creationism if one adheres to SEETA?

SEETA certainly does not allow creationism to be on equal footing as our standard scientific explanations for geology and biology. There are no extra-empirical attributes to creationism that anybody would agree are superior to normal science except possibly one: God played a frequent and decisive interventionist role in the unfolding of creation as is claimed in the bible. That is not my religion, but it is for some. Creationism as we call it has been an evolving theory, as science has pushed them about in many directions. It looks to me very similar in spirit to what I call a "tautological theory" [2], which merely states that any observation is true (and God made it that way, in this case). In the marketplace of ideas, this has not been judged very useful, and falls far short of many other desirable extra-empirical attributes when compared to ordinary science understanding.

So, you are saying creationism is just wrong. But doesn't that contradict the SEETA approach that we should not think about wrong and right theories, or "more correct" theories?

Assuming SEETA, I would not say that anything is wrong or right there. Theories just need to compete and society declares winners and losers on equally empirically adequate theories based on reasons that have very little to do with guessing which is "more correct". It comes down to what is more predictive, more useful, more consilient, and a myriad of other extra-empirical reasons that jumble about in the competition. Creationism has failed in this competition for almost all of us, but it has succeeded in some small pockets because it is more

"useful" (in different, non-scientific ways) to some to hold onto those beliefs.

You seem to have implied that all empirically adequate theories are equally likely, but how can that be true when only one theory and only one theory point, to use your language, can be correct? The others are mutually exclusive to the right choice. Isn't it untenable to advocate equal likelihood of all empirically adequate theories?

The point is not that all theories and all theory points are equally likely. Rather, it is that without knowing beforehand you have no choice but to be agnostic about what theories will survive the next steps of empirical investigation, and therefore you must focus on other traits besides "likely" to decide your preferred theory. Of course, it should go without saying that preferred theories must be compatible with data. Identifying empirically adequate theories is step zero in deciding preferred theories. The worst mistake a scientist can make is to subconsciously and sloppily assume that because not every theory can be correct therefore his/her theory must be the correct one.

But doesn't that mean that when you give up on trying to assess what is the more likely next theory that you are abandoning the search for truth?

Again, channeling the SEETA approach, I don't know what is meant by "search for truth" here, but let's take it to mean "the search for a new more encompassing theory that will have confirmable new phenomena." In that case, no, I am not giving up on that. On the contrary, I am advocating a plethora of approaches to extra-empirical theory assessments that can open up the search for new theories and new phenomena. A community tends to turn inward, especially when it comes to declarations by the powerful of "more correct" theories. It can stifle growth and creativity. Take the Lamarckian biology nonsense of the former USSR as an example. A thousand flowers looking for new empirically adequate testable theories that solve problems is much more likely to advance the cause of science than ideologies burdened by a single theory "most likely to be correct" pronounced by elder statesmen of science.

You wrote a paper five years ago on the "Utility of Naturalness" [3], and claimed that it could predict the Standard Model? Isn't that an example of the power of naturalness, and why it's a useful extra-empirical theory assessment?

That paper was about the implication of animating research through naturalness pursuits. As I claim in the paper, I did not totally succeed in deriving the Standard Model — only showing that it was a possibility among many, and that although one naturalness problem was solved (lightness of the electron) new ones were introduced (the weak scale). Nevertheless, the focus on naturalness could be interpreted as useful in developing new theories. I should say that it was controversial, with I'd say roughly half the people agreeing with me and half not among those I think have background to weigh in expertly. With respect to this context, most would agree that you could not start with QED and converge on the Standard Model from naturalness pursuits alone. Nevertheless, naturalness and the search for non-finetuned theories is, in my view, a *generally* useful procedure since nature *generally* prefers such theories in my view. But there is no guarantee of that for every theory from every (limited) perspective. For convergence, experiment must play a mutually key role with theory in this evolution, and

that's exactly how the real history played out.

You have given some prominence to the notion of "diversity" in theory development. Are you telling me that theorists should stop worrying about what is correct and just diversify itself blindly into a myriad of stupid theories whose only value is that they are different than other theories?

Presumably you have some criteria for a "stupid" theory, an extra-empirical assessment, which should go into the marketplace of ideas and compete. Your appellation suggests that those theories would not survive long. So, no, stupid theories are never good. However, when I'm reading the literature and I see two new theories and they are equally empirically adequate, and both attempting to solve outstanding problems in physics, but one suggests new phenomena never before suggested and which would require deliberate development and work by experiment to see it, then I am much more interested in that. Hochberg, Zhao, and Zurek's work [4] on super light dark matter, which I cite in my article, is a particularly brilliant example of that. This is top-flight theory that is significantly more valuable than the latest "natural" theory brought forward with no new phenomena anywhere to be seen.

Works Cited

[1] J.D. Wells. "Naturalness, Extra-Empirical Theory Assessments, and the Implications of Skepticism." arXiv:1806.07289.

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[3] J.D. Wells. *Stud.Hist.Phil.Sci.* B49, 102 (2015). arXiv:1305.3434.

[4] Y. Hochberg, Y. Zhao, K.M. Zurek. *PRL* 116, 011301 (2016). arXiv:1504.07237.