

How to Rigorously Define Fine-Tuning

By Robin Collins,

Note: This is a draft of a chapter to appear in the book on the fine-tuning argument, tentatively entitled *The Well-Tempered Universe: God, Fine-tuning, and the Laws of Nature*. This chapter should give the basic idea of my approach to defining the comparison range, and my response to the McGrew-Vestrupe objection.

1.0 Introduction:

We say a constant of physics C is fine-tuned for life if the width, W_r , of the range of values of the constant that permit, or are optimal for, the existence of intelligent life is small compared to the width, W_R , of some properly chosen comparison range R : that is, if $W_r/W_R \ll 1$.¹ [W_r could also stand for the sum of the widths of the intelligent-life-permitting regions.] The range r of intelligent life-permitting values is determined via physical calculations and thus, apart from debates about what is meant by intelligent life, is largely unproblematic from a philosophical perspective. In contrast, the choice of R cannot be decided by physical considerations alone. An outstanding issue for developing the fine-tuning argument (FTA), therefore, is to find a plausible methodology for choosing R . The proper way of choosing R will hinge on the method of

¹ Although this might seem an obvious definition of fine-tuning, there has been considerable confusion in the literature as to how to define fine-tuning. As Neil Manson has pointed out, the most common way of stating claims of fine-tuning for life is in terms of counterfactual conditionals, wherein expressions such as >slight difference=, >small change=, >delicate balance=, >precise=, >different by $n\%$ =, >different by one part in 10^n =, and >tuned to the n th decimal place= appear in the antecedent (2000, p. 342). This very simple counterfactual method is a manifestly inadequate way of defining fine-tuning, however, as can be seen from the case of the fine-tuning of the strong force for carbon/oxygen production. According to some (outdated) calculations by Oberhammer et. al. note (2000a), a 0.4% change in the strength of the strong force B that is, a relatively slight change in its value B would radically decrease either the total amount of carbon or the total amount of oxygen in the universe, thereby severely decreasing the chances of intelligent observers forming. Thus, there is a narrow island of values that are optimal for life. On the other hand, Oberhammer et. al. point out (2000b) that a change of 10% in the strength of the strong force might land one on another island that allowed for significant quantities of both carbon and oxygen to form. This illustrates that one could have fine-tuning in the sense of a narrow island of intelligent-life-permitting values (that is, fine-tuned in the >slight difference@ sense) and yet have many narrow islands right next to each other, which would render it entirely unsurprising that the constant in question had an intelligent-life-permitting value.

inference at work in the FTA. I will go along with nearly everyone in the literature on fine-tuning and frame the FTA as a quasi-Bayesian inference. *As we will see below (e.g., see end of section III and section IV), for most cases of fine-tuning, R will simply be what I call the Aepistemically illuminated region @ B that is, the range of values of C for which we can make a reasonable estimate whether or not that value of C is (intelligent) life-permitting.* This epistemically illuminated region will often be some finite local region around the actual value of the constant. It should also be noted that although this paper is part of a special issue dealing with the objection raised to the fine-tuning argument by McGrew and Vestrup (see section V), its primary purpose is not to address their objection (by, for instance, arguing that R is finite), but to develop a rigorous procedure for determining R.

1.11 The Basic Fine-tuning Argument:

Essentially, the quasi-Bayesian method of inference consists of claiming that the existence of a universe with life-permitting values for its constants is not epistemically improbable under theism, but highly improbable under the non-design, non-many-universes hypothesis B that is, under the hypothesis that only a single universe exists, and that it exists as a brute fact. Then one invokes the likelihood principle to draw the conclusion that the existence of life-permitting values for the constants confirms theism over the non-design, single universe hypothesis.²

There are several premises and steps in this argument that need to be defended that are beyond the scope of this chapter. The step relevant to this chapter is the claim that the existence of life-permitting values for the constants is very improbable under the non-design single universe hypothesis. Essentially, the argument for this claim involves two steps. First, it is argued that the range r of (intelligent) life-permitting values for the constants is very small relative to some comparison range R . That is, $W_r/W_R \ll 1$, where W_r is the width of the (intelligent) life permitting range and W_R is the width of the comparison range. Second, the claim is made that given that $W_r/W_R \ll 1$ for some constant, then it would be very improbable for that constant to have a life-permitting value under the non-design single-universe hypothesis. This argument could be rendered symbollically as follows:

(1) [premise] For some constant C , $W_r/W_R \ll 1$.

(2) [from (1) and the restricted principle of indifference] $P(L_C/AS \ \& \ k=) \ll 1$.

What (2) means is that it is epistemically very improbable for C to fall into the intelligent-life-permitting range under the atheistic single-universe hypothesis (AS). AS is the hypothesis that only one universe exists and that this universe exists as a brute fact. L_C denotes the claim that a constant C falls into the intelligent-life-permitting or intelligent-life-optimal range and $k=$ denotes some appropriately chosen background information. Of course, $k=$ must be chosen in some legitimate, non-ad-hoc way, not merely some way designed to get the result we want.

Note that the truth of (1) and (2) crucially depends on how we define R ; this project constitutes the bulk of this chapter. Put simply, we will develop a method of defining R that will

² Note that, for ease of presentation, I will assume the FTA is an argument for theism, rather than for the existence of a multitude of other universes or the existence of some less-than-divine designer(s).

result in the soundness of this inference. That is, we want to define R in such a way that $W_r/W_R \ll 1$ and thus that $P(L_c/AS \ \&k=) \ll 1$. Once we can establish the result that $P(L_c/AS \ \&k=) \ll 1$, we can then forget about how we defined R since this claim does not actually involve R and is sufficient for us to say that it is very improbable for a constant C to have life-permitting values under the non-design, single universe hypothesis. So, the sole significance of defining a comparison range R is as a means of establishing $P(L_c/AS \ \&k=)$. This is why defining R to get the result we want is in the end not question-begging. As we will see, the crucial step in determining R will be determining what background information should be included in $k=$.

As for the definition of probability at work in the quasi-Bayesian FTA, the relevant notion at work is *epistemic* probability, which is elaborated more in chapter _____. For now, we can think of the *unconditional* epistemic probability of a proposition is the degree of confidence or belief we rationally should have in the proposition; the *conditional* epistemic probability of a proposition R on another proposition S can roughly be defined as the degree to which the proposition S of itself should rationally lead us to expect that R is true. Under the epistemic conception of probability, therefore, the claim that $P(L_c/AS \ \&k=) \ll 1$ is to be understood as making a statement about the degree to which the conjunction of AS & $k=$ should, of itself, rationally lead us to expect L_c . In chapter _____, we will present a more nuanced notion of epistemic probability.

The appropriate choice of background information $k=$ is crucial here, since our total background information k includes the information that we are alive and hence by implication that the constants of physics have intelligent-life-permitting values. Accordingly $P(L_c/AS \ \&k) = 1$, and hence no confirmation argument can get off the ground if we use k as our background information. Thus we confront the much-discussed problem of old evidence. Below I will address directly this problem and propose a solution to it that will allow for a non-trivial conditional epistemic probability of a universe existing with intelligent life permitting values for its constants on AS.

It is important to stress here that, for the sake of this quasi-Bayesian FTA, there is no *statistical* probability that the universe turns out fine-tuned for life. One could only get a statistical probability if one modeled the universe as being produced by some universe generator that churns out intelligent-life-permitting universes a certain proportion of the time. The whole point of AS, however, is that there is no such universe generator. Rather, the universe exists as a brute, inexplicable fact. Thus, the probabilities in this case should not be understood as statistical probabilities, but rather as measures of rational degrees of support of one proposition for another B for instance, AS & $k=$ for L_c .

1.12 The Restricted Principle of Indifference

The principle used to move from $W_r/W_R \ll 1$ to $P(L_c/AS \ \&k=) \ll 1$ is what I'll call the *restricted principle of indifference*. This principle states that when we have no reason to prefer any value of a parameter over another, we should assign equal probabilities to equal ranges of possible values for the parameter, given that the parameter in question directly corresponds to some physical magnitude (or occurs in the simplest way of writing the fundamental theories in the relevant domain). When conflicting parameters arise B e.g., two equally simple ways of writing a theory with different sets of parameters functionally related to each other B than one should take the probability as being given by the range of probability values formed by applying the restricted principle to each parameter separately. A full statement and defense of this

principle is presented in Chapter ____ where this issue is addressed at more length, but it should be noted here that acceptance of the restricted principle of indifference does not commit one to the general validity of the principle of indifference.

Applied to the case at hand, since R will be chosen in such a way that AS conjoined with $k=$ will give us no reason to prefer any value of C over any other within R , it will follow from the restricted principle of indifference that we should put a uniform probability distribution over region R . This is sufficient to justify the inference from $W_r/W_R \ll 1$ to $P(L_C/ AS \ \& \ k=) \ll 1$.

Any principled justification of the inference from $W_r/W_R \ll 1$ to $P(L_C/ AS \ \& \ k=) \ll 1$ must put some constraints on one's credence function over R , however the comparison range is chosen. Without some constraints, one could always choose a credence function such that $P(L_C/ AS \ \& \ k=)$ had any probability one likes. The restricted principle of indifference simply provides a particularly strong constraint, requiring that one have a uniform credence function over R . One merit of a uniform credence function is that it seems to be the least arbitrary choice. It is possible, however, that one could still justify the inference from $W_r/W_R \ll 1$ to $P(L_C/ AS \ \& \ k=) \ll 1$ by adopting some principle that yielded weaker constraints on the credence function.

2.0 What it Means to Vary A Constant of Physics

2.1 Definition of a Constant

Before presenting our procedure for determining $k=$ and R , we need to define more carefully what a constant of physics is and what it means to vary such constants. Intuitively there is a distinction between laws and constants, and physicists usually suppose such a distinction. In current physics, most laws can be thought of as mathematical descriptions of the relations between certain physical quantities. Each of these descriptions has a certain mathematical form, along with a set of numbers that are determined by experiment. So, for example, Newton's law of gravity ($F = Gm_1m_2/r^2$) has a certain mathematical form, along with a number (G) determined by experiment. We can then think of a world in which the relation of force to mass and distance has the same mathematical form (the form of being proportional to the product of the masses divided by the distance between them squared), but in which G is different. We could then say that such worlds have the same law of gravity, but a different value for G . So when we conceive of worlds in which a constant of physics is different but in which the laws are the same, we are conceiving of worlds in which the mathematical form of the laws remains the same, but in which the experimentally determined numbers are different.

2.2 More Fundamental Theory Problem:

In speaking of the laws in this way, we are already assuming a certain level of description of physical reality. The history of physics is one in which we find that a certain law is only a limiting case of some deeper law, such as Newton's law of gravity being a limiting case of Einstein's Equation of general relativity. This is especially relevant in the context of the current search for a grand unified theory. One hope is that such a grand unified theory will have no free parameters. The idea is that higher-level principles of physics B principles such as those that lie at the heart of quantum mechanics and general relativity B will uniquely determine the form of the grand unified theory along with the various fundamental constants of physics that are part of this theory. In this case, given the higher-level principles, it would be impossible for the constants to be any different than they are. Any discussion of fine-tuning must therefore be

explicated at a level of description of physical reality below that of a grand unified theory. We will return to this issue below.

This issue also arises even in the context of our current (incomplete) understanding of physics. Consider, for instance, the strength of the strong force that holds neutrons and protons together in the nucleus. This force is actually not fundamental, but is simply a product of a deeper force between the quark constituents of the protons and neutrons, much as the force of cohesion between molecules is not fundamental but rather a product of various electromagnetic (and exclusion principle) forces. This deeper force binding quarks together is given by quantum chromodynamics (QCD), which in current theory has its own set of free parameters. From the perspective of QCD, one cannot simply change the strength of the strong force while keeping everything else the same. Instead, one would have to change one or more of the parameters of QCD, which would in turn change not just the strength of the strong force, but other things such as the masses of the neutron and proton and the range of the strong force. Calculations of these effects are very difficult and hence it is difficult to develop a rigorous FTA at the level of QCD. Thus, for practical purposes, most FTAs need to be developed at a lower level of description, such as at the level of the phenomenological equation governing the strong force between nucleons.

I claim that obtaining probabilities for FTAs at this less than fundamental level is a legitimate procedure. Epistemic probabilities are only useful in conditions of ignorance; they are attempts to generate degrees of expectation, or conditional degrees of expectation, when we do not know everything about a situation.³ For example, we have an unconditional epistemic probability that a coin will land on heads of 50% (instead of 100% or 0%) because we are ignorant of the physically determined side on which it will land.

2.21 Tracking Constraint

That said, we cannot use any model of the phenomena in the domain in question, no matter how physically unrealistic. Rather, we should formulate our expectations using the best models we have for which we can make calculations. Further, when we vary a parameter in these models, we must have confidence that the variation relevantly tracks the corresponding variations of the appropriate parameters in the most fundamental theory of physics. Generally speaking, the larger the variation in our model, the less confidence we will be that it tracks variations in the deeper theory. Call this the *tracking constraint*.

As an example of the above points, suppose that although QCD is our most fundamental theory, we use our phenomenological theory, PT, to calculate what values of the strong force constant, g_s , are and are not life-permitting over some range R^* . For PT to be relevantly reliable means that if we conclude that the life-permitting range of g_s is small compared to R^* , then there

³ Though conditional epistemic probability arises in condition of ignorance, is best not understood as a measure of ignorance. Rather, it has to do with relations of support or justification between propositions. As John Maynard Keynes, the famous economist, stated in his treatise on probability, A if a knowledge of h justifies a rational belief in a of degree α we say that there is a *probability-relation* of degree α between a and h .@ (Keynes, p. 4) Although I think Keynes's account needs to be further worked out, I believe his account is on the right track. For a recent development of this notion of conditional epistemic probability, see Alvin Plantinga, *Warrant and Proper Function*. Oxford: Oxford University Press, 1993, Chapters 8 and 9.

should be some corresponding variation of the fundamental parameters of QCD that roughly correspond to values of g_s in R^* , such that using the parameters of QCD it also turns out that the life-permitting range is small. The reason for this is that calculations and arguments based on a more fundamental theory always trump calculations and arguments based on a less fundamental theory. Thus we can be confident in our calculations and arguments from the less fundamental theory only insofar as we are confident that those same calculations and arguments would hold good if we performed them using the fundamental theory. Indeed, more generally, these are the conditions of using any approximate theory in physics: we only trust the calculations based on the approximate theory insofar as we are confident they approximately reflect the results we would get if we could perform corresponding exact theory. Often, only heuristic arguments can be given for thinking that the approximate theory relevantly tracks the more exact theory. We expect nothing more in the case of the FTA.

2.22 No Free Parameters Problem

What if our most fundamental theory does not have any free parameters, but rather the structure of the theory itself entails the values of the constants? To begin, I think this is unlikely: the grand unified theories developed so far have not greatly reduced the number of free parameters (for example, the standard model of particle physics has twenty four) and although it was hoped that the various physical principles underlying string theory would dictate only one viable set of constants for physics, this hope has largely faded (See Chapter _____). Suppose, however, that our most fundamental theory has no free parameters and yet dictates the exact values of the free parameters of some lower level theory, such as that of the standard model in particle physics. In that case, variation of some parameter p in the standard model will correspond to a variation in the fundamental laws of physics themselves. Thus the values for which p is life-permitting will correspond to a certain set of life-permitting law structures, and values for which p is not life-permitting will correspond to another set of law structures. My suggestion is that we then take the parameter p as providing the relevant epistemic measure over these law structures, as long as p is part of the most fundamental theory with free parameters. Consequently, if the life-permitting range of p is small compared to the comparison range R , we could also say that the relevant class of life-permitting law structures are small compared to the relevant class of non-life-permitting law structures.

In this case, therefore, the fine-tuning is not so much of a fundamental parameter of physics as of a set of law structures generated by variations in p . But I do not see that this presents a problem. There is nothing more or less fundamental about varying a law than a parameter. In the strict sense, when we vary the fundamental parameters, we also vary the laws since laws and parameters come as single packages in nature. It is only humans that make the distinction. What matters is being able to calculate what happens

for nearby law structures, and being able to provide a non-arbitrary epistemic measure over the reference class consisting of our law structure and these nearby possible law structures. Of course, our choice of nearby law structures must not be biased to yield the results we want. Certainly, however, the above method of choosing to vary the parameters in the most fundamental theory for which we can make calculations is not biased in favor of fine-tuning.

An analogy might help see the above point. Suppose that superdeterminism were true: that is, that everything about the universe, including its initial conditions, was determined by some set of laws, though we did not know the details of those laws. It would still be the case that we would have an equal reason to believe that the laws would turn out to cause the particular coin that I am now flipping to land heads as to land on tails. Thus, the probability is still 50%. The fact that the laws of nature determine the initial conditions, instead of the initial conditions not being determined by any law, should have no influence on the probability. Yet, when Laplacian determinism was thought to be true, everyone gave a fair coin a 50% chance of coming up heads.

2.3 Concluding Thoughts: The Weight Problem

Finally, one must keep this whole enterprise in perspective. The ultimate goal is not to provide exact numerical values for the degree of belief one should have that a certain constant falls into the intelligent-life-permitting range. Rather, the goal is to provide as much objective support as possible for the surprise felt at some parameter=s falling into the intelligent-life-permitting range. The goal is to show that this intuitive sense of surprise is not based on some mistake in thinking or perception or on some merely subjective interpretation of the data, but rather can be grounded in a justified, non-arbitrary procedure. Put another way, because we are only considering one reference class of possible law structures (the one we can perform calculations for), it is unclear how much weight to attach to the values of epistemic probabilities one obtains using this reference class. Hence one cannot simply map the epistemic probabilities obtained in this way onto the degrees of belief we should have at the end of the day. What we can do, however, is say that given that we choose our reference class in the way suggested, and assuming our other principles (such as the restricted principle of indifference), we obtain a certain epistemic probability, or range of probabilities. Then, as a second order concern, we must assess the confidence we have in the various components that went into the calculation, such as the representative nature of the reference class. As Peter Achinstein notes for more mundane cases, such as concerns about the quality of the reference class in drug testing, there is yet no way of integrating these second order concerns into the first order proposals of probability theorists@ (1994, P. 349).

One can think of what is going on here as analogous to the use of approximation in physics. Virtually all calculations in physics are made based on idealized circumstances. Then, as a second order concern, one attempts to assess the degree to which one=s calculations apply to the actual circumstances being considered. Similarly, our calculations of epistemic probability only strictly apply to idealized conditions, such as when there is absolutely no concern about possible competing reference classes. Given that there is no algorithm that can get us from these calculations to the actual degree of confidence we actually should have in the theistic hypothesis,

or the degree to which the evidence supports the theistic hypothesis over the atheistic single-universe hypothesis, I suggest that the probability calculations should be thought of as simply providing an objective confirmation of the common intuitive sense that the fine-tuning provides evidence for theism over the atheistic single-universe hypothesis.⁴

3.0 Procedure for Determining R

Above, we said that determining k^* was the critical step in determining R. Determining k^* will involve two components: Addressing the issue will first involve confronting the problem of old evidence, and then looking at the fact that we can only determine whether a parameter is life-permitting for a local range B what I call the epistemically illuminated (EI) range.

3.1 The Problem of Old Evidence

As mentioned above, the problem of old evidence is that if we include known evidence e in our background information k , then even if an hypothesis h entails e , it cannot confirm H under a Bayesian or quasi-Bayesian methodology since $P(e/k \ \& \ h) = P(e/k \ \& \ -h)$. But this seems incorrect: general relativity's prediction of the correct degree of the precession of the perihelion of Mercury (which was a major anomaly under Newton's theory of gravity) has been taken to confirm general relativity even though it was known for over fifty years prior to the development of general relativity and thus entailed by k . This issue has been extensively discussed in the literature on applying Bayesian methods to scientific inference. This issue confronts the fine-tuning argument since the fact that the constants of physics have life-permitting values follows from our own existence and hence is old evidence.

An attractive solution is to subtract out our knowledge of old evidence e from the background information k and then relativize confirmation to this new body of information $k^* = k - B\{e\}$. As Colin Howson explains, "When you ask yourself how much support e gives [hypothesis] h , you are plausibly asking how much knowledge of e *would* increase the credibility of h , but this is the same thing as asking how much e boosts h relative to what else we know" (1991, p. 548). This "what else" is just our background knowledge k minus e . As appealing as this method seems, it faces a major problem: there is no unambiguous way of subtracting e from k . Consider the case of fine-tuning. Let e be the claim that C falls within some range of experimentally determined values x . Now, the fact that C falls into x , along with the laws of physics, the initial conditions of the universe, and the other values for the constants of physics, entails certain facts F_1 about the large-scale structure of the universe, and further renders other such facts F_2 highly probable. So, our question is, when subtracting e from k , do we also

⁴ By objective confirmation, I mean a confirmation based on a plausible, non-arbitrary procedure. Given that the multiplicity of possible reference classes, one could simply decide not assign any epistemic probability to the fine-tuning. As I explicate in more detail in my (unpublished) manuscript on the restricted principle of indifference, part of the justification for the overall method discussed above for determining epistemic probabilities is based on the choice to eschew agnosticism and to seek the least arbitrary assignment of epistemic probabilities compatible with our knowledge and abilities to perform calculations. I claim that, since agnosticism with regard to epistemic probabilities is almost always an option given the lack of a complete fundamental justification of epistemic probabilities assignments, generally eschewing such agnosticism in the way suggested is at the heart of inference in science or any other inferential enterprise.

subtract F_1 and F_2 ? If yes, then things get very murky: among other problems, what level of probability counts as probabilistically dependent? That is, to what degree must e render F_2 probable in order to say that we should subtract F_2 out? If no, then k will effectively contain e , because elements of F_1 will entail e , and elements of F_2 will make e likely.

For example, consider the case of gravity. The fact, L_g , that the strength of gravity falls into the intelligent-life-permitting range entails the existence of stable, long-lived stars. On the other hand, given our knowledge of the laws of physics, the initial conditions of the universe, and the value of the other constants, the existence of stable, long-lived stars entails L_g . Thus, if we were to obtain $k=$ by subtracting L_g from our total background information k without also subtracting our knowledge of the existence of long-lived stable stars from k , then $P(L_g/k=) = 1$.

To solve such problems, Howson says, we should regard k as A in effect, an independent axiomatization of background information and $k - \{e\}$ as the simple set-theoretic subtraction of e from k (1991, p. 549). That is, Howson proposes that we axiomatize our background information k by a set of sentences A in such a way that e is logically independent of the other sentences in A . Then $k=$ would simply consist of the set of sentences $A - \{e\}$. One serious problem with this method is that there are different ways of axiomatizing our background information. Thus, as Howson recognizes, the degree to which e confirms h becomes relative to our axiomatization scheme (1991, p. 550). In practice, however, this is not as serious a problem as one might expect, since in many cases our background information k is already represented to us in a partially axiomatized way in which e is logically isolated from other components of k . As Howson notes, A the sorts of cases which are brought up in the literature tend to be those in which the evidence, like the statements describing the magnitude of the observed annual advance of Mercury's perihelion, is a logically isolated component of the background information (1991, p. 549). In such cases, when we ask ourselves how much e boosts the credibility of h with respect to A what else we know, $@$ this what else we know is well defined by how we represent our background knowledge. Of course, in those cases in which there are alternative ways of axiomatizing k that are consistent with the way our background knowledge is represented to us, there will be corresponding ambiguities in the degree to which e confirms h . I agree with Howson that this is not necessarily a problem unless one thinks that the degree of confirmation e provides h must be independent of the way we represent our background knowledge. Like Howson, I see no reason to make this assumption: confirmation is an epistemic notion and thus is relative to our epistemic situation, which will include the way we represent our background information.

In the case of fine-tuning, our knowledge of the universe is already presented to us in a partially axiomatized way. Assuming a deterministic universe, the laws and constants of physics, along with the initial conditions of the universe, supposedly determine everything else about the universe. Thus the set of propositions expressing these laws, constants, and initial conditions constitutes an axiomatization of our knowledge. Further, in scientific contexts, this represents the *natural* axiomatization. Indeed, I would argue, the fact that this is the natural axiomatization of our knowledge is part of our background knowledge, at least for scientific realists who want

scientific theories to Acute reality at its seams.⁵ Furthermore, we have a particularly powerful reason for adopting this axiomatization in this case. The very meaning of a constant of physics is only defined in terms of a particular framework of physics. Saying that the strong force constant has a certain value, for instance, would be meaningless in Aristotelian physics. Accordingly, the very idea of subtracting out the value of such a constant only has meaning relative to our knowledge of the current set of laws and constants, and hence this constitutes the appropriate axiomatization of our relevant background information k with respect to which we should perform our subtraction.

Using Howson's method, therefore, we have a straightforward way of determining $k - \{e\}$ for the case of the constants of physics: we let k be axiomatized by the set of propositions expressing the initial conditions of the universe and the laws and fundamental constants of physics within our currently most fundamental theory which we can do calculations. *Since the constants of physics can be considered as given by a list of numbers in a table, we simply subtract the proposition expressing the value of C from that table to obtain k' . Thus, k' can be thought of as including the initial conditions of the universe, the laws of physics, and the values of all the other constants except C .* Further, if we want to obtain the appropriate k' for these existing intelligent-life-permitting values for a set of constants $\{C_1, C_2, C_3 \dots C_n\}$, we would simply subtract the knowledge that these values fall into the intelligent-life-permitting range from our background information k .⁶

⁵ One might object that this procedure is only justified under the assumption that we live in a deterministic universe, since otherwise the k we have chosen is not a true axiomatization of our knowledge. This is true, but it is difficult to see how the thesis that the world is indeterministic could be relevant to the legitimacy of the fine-tuning argument.

⁶ In general, a more accurate assessment of fine-tuning would consider all the n purportedly fine-tuned constants together, thus resulting in fine-tuning being defined in terms of the volume of a life-permitting region being small compared to a comparison volume in a n -dimensional space. The reason for doing it this way is that the range of values of the life-permitting range for a given constant will often depend on the values of the other constants. For practical purposes of making calculations, however, we are often restricted to considering each constant separately.

Finally, as mentioned above, we will not include in our original axiomatized background information k the exact value of the constant C that we are considering, but only that it falls into the known intelligent-life-permitting range. The reason is that it makes the argument conceptually simpler. It is legitimate to make this exclusion because all we need to include in k is all information relevant to the quasi-Bayesian FTA we are interested in B that is, all information that makes a difference in the ratio $P(L_C/T \ \& \ k=)/P(L_C/AS \ \& \ k=)$. Additional information regarding the experimental value of C or more precisely, the range of values for C within experimental error B presumably will not affect our judgment of this ratio.⁷ Thus, for the purposes of this argument, it can be excluded from our background information, just as other irrelevant information, such as the location in which I live, can be excluded. On the other hand, even if it were relevant, we would have arrived at an identical k and hence an identical comparison range by initially including the experimentally determined value (within experimental error) of C in k , and then subtracting it out to determine k . If we followed this procedure, we would have had to consider the experimental value of C as our relevant evidence, instead of the mere fact that it fell into the intelligent-life-permitting range. Although one could run quasi-Bayesian FTA this way, it seems less natural to do so.

3.2 The Epistemically Illuminated Region

The second step in determining k involves what I call the epistemically illuminated (EI) range. This range is defined above as the range of values for a constant C such that, given our current physical theories, we can make a reasonable judgement as to whether a particular value of the constant in that range is (intelligent) life-permitting. In some sense, it is obvious why we must limit our range R to the EI range, but we will offer some further justification for it by looking at a mundane example involving a dart-board. Suppose we had a dart board that extended far into the distance, but the entire dart board was not illuminated: only some finite region around the bull's eye was illuminated, with the rest of the dart board in darkness. We thus neither know how far the dart board extends nor whether there are other bull's eyes on it. If we saw a dart hit the bull's eye in the illuminated (IL) region, and the bull's eye was very, very small compared to the IL region, we would take that as evidence that the dart was aimed, even though we cannot say anything about the density of bull's eyes on other regions of the board. One way of providing a quasi-Bayesian reconstruction of the inference to include the fact that the dart fell into the IL region as part of the information being conditioned on: that is, include it into the background information k . The confirmation theory argument therefore would proceed as follows: given that as part of our background information k we know that the dart has fallen into the illuminated region, it is very unlikely for it to have hit the bull's eye by chance but not if it was aimed, and thus its falling in the bull's eye confirms the aiming hypothesis. That is, BI

⁷ To see this more clearly, consider the theistic hypothesis T . It is reasonable to suppose that T gives us no reason to favor one value for a constant of physics within the intelligent-life-permitting range r over any other, thus giving rise to a uniform probability distribution over r . Similarly, the atheistic single-universe hypothesis AS would also give rise to a uniform probability distribution over r (along with a uniform probability distribution over the total comparison range R). Thus, since the probability distributions over r are uniform in both cases, any further knowledge about exactly what sub-region of r the value falls into will be irrelevant for purposes of the argument for T .

strongly confirms A over C because $P(BI/C \ \& \ k=) \ll P(BI/A \ \& \ k=)$, where BI is the hypothesis that the dart hit the bull=s eye, C is the chance hypothesis, A is the aiming hypothesis, and k= includes the information that it landed in the illuminated region. Similarly, for the case of fine-tuning, we should include the fact that the value of a constant is within the EI region as part of our background information k= being conditioned on.

Is including EI in k= an adequate procedure? The case of the dartboard, I believe, shows that it is not only a natural procedure to adopt, but arguably the only way of providing a quasi-Bayesian reconstruction of the inference in this sort of mundane case. First, it is clearly the ratio of the area taken up by the bull=s eye to the illuminated region around the bull=s eye that leads us to conclude that it was aimed. Second, one *must* restrict the comparison range to the illuminated range (that is, include IL in k=) since one does not know how many bull=s eyes are in the unilluminated portion. Thus, if one expanded the comparison range outside the illuminated range, one could make no estimate as to ratio the area of the bull=s-eye regions to the non-bull=s-eye regions, and thus could not get a confirmation theory argument off the ground. Yet, it seems intuitively clear that the dart=s hitting the bull=s eye in this case does confirm the Aaimed@ hypothesis over the chance hypothesis.

In general, as applied to the fine-tuning, one can also think of k= as determining the reference class of possible law structures to be used for purposes of estimating the epistemic probability of Lc under As: the probability of Lc given k= & As is the relative proportion of law structures that are life-permitting in the class of all law structures that are consistent with k= and As. (The measure over this reference class is then given by the restricted principle of indifference.) Thinking in terms of reference classes, the justification for restricting our reference class to EI region is similar to that used in science: When testing an hypothesis, we always restrict our reference classes to those for which we can make the observations and calculations of the frequencies or proportions of interestB what in statistics is called the sample class. This is legitimate as long as we have no reason to think that such a restriction produces a relevantly biased reference class. Tests of the long-term efficacy of certain vitamins, for instance, are often restricted to a reference class of randomly selected doctors and nurses in certain participating hospitals, since these are the only individuals that they can reliably trace for extended periods of time. The assumption of such tests is that we have no reason to think that the doctors and nurses are relevantly the same as those who are neither doctors nor nurses. As discussed in section II, the justification for varying a constant, instead of varying the mathematical form of a law, in the fine-tuning argument is that in the reference class of law structures picked out by varying a constant we can make some estimate of the proportion of life-permitting law structures, something we probably could not do if our reference class involved variations of mathematical form. The same sort of justification underlies restricting the class to the EI range.

Including EI in k= provides a quasi-Bayesian reconstruction of John Leslie=s Afly on the wall@ analogy, which he offers in response to the claim that there could be other distant values for the constants of physics, or unknown laws, that allow for life:

If a tiny group of flies is surrounded by a largish fly-free wall area then whether a bullet hits a fly in the group will be very sensitive to the direction in which the firer=s rifle points, even if other very different areas of the wall are thick with flies. So it is sufficient to consider *a local area of possible universes*, e.g., those produced by slight changes in gravity=s strength, It certainly needn=t be claimed that Life and Intelligence could

exist *only if* certain force strengths, particle masses, etc. fell within certain narrow ranges All that need be claimed is that a lifeless universe would have resulted from *fairly minor changes* in the forces etc. with which we are familiar. (1989, pp. 138-9).

As Leslie stresses, what matters for the FTA is the comparison between the life-permitting range and the local region around it. Including EI into $k=$ is just a way of casting Leslie=s response in terms of conditional epistemic probabilities.

3.3 Defining R

We are now ready to define R. We define R as the region of possible values for our constant C allowed by our background information $k=$. Typically, within this region, $k=$ & AS will give us no reason to prefer one value of C over any others. Given this, the restricted principle of indifference implies that under AS and $k=$, we should assign equal probabilities to equal ranges within R. This implies that $P(L_C/AS \ \& \ k=) = W_r/W_R$, and hence that if $W_r/W_R \ll 1$ then $P(L_C/AS \ \& \ k=) \ll 1$. Since in almost all, if not all, cases the EI range will be the major delimiting factor in determining the values allowed by $k=$, not the background theories and values of the other constants included in $k=$, the comparison range R will simply be the range EI.⁸

3B.0 Second Procedure: Reference Class Method

4.0 Examples of Determining R: Force Strengths and Other Constants

⁸ Imagined variations in the masses of elementary particles might offer an exception to this claim because in some cases the fact that the mass of a particular elementary particle falls into a certain range should be included in the definition of that particle. This means that by definition one cannot vary the mass of such particles beyond their defining range. For example, as far as physicists can determine there are two other particles, the muon and the tau particle that are identical to the electron except that they have different masses (electron = 0.511 Mev, muon = 105.7 MeV, tau = 1,780 MeV). If these particles are otherwise identical, then it seems that falling into a certain mass range must be part of the definition of an electron. Quarks might also have this same redundancy beyond current mass ranges that have been explored, which are at least 180,000 MeV, the mass of the top quark. If so, this would force the inclusion of a mass range in the definition of the masses of the neutron and proton. This would need to be taken into account in a more sophisticated estimate of R in these cases.

Let us now see how the procedure outlined above delimits R in the case of force strengths and other constants. The EI range effectively determines the range R for the force strengths. The EI range is determined by the fact that our current physical theories have known limitations of applicability with regard to energy levels. In the past, we have found that physical theories are limited in their range of applicability B for example, Newtonian mechanics was limited to medium size objects moving at speeds relatively slow compared to the speed of light. For fast objects, we require special relativity; for massive objections, general relativity; for very small objects, quantum theory. When the Newtonian limits are violated, these theories predict completely unexpected and seemingly bizarre effects, such as time dilation in special relativity or the phenomenon of tunneling in quantum mechanics. There are good reasons to believe that current physics is limited in its domain of applicability. The most discussed of these limits is that of energy scale. The current orthodoxy in high-energy physics and cosmology is that our current physics is either only a low-energy approximation to the true physics that applies at all energies or the low-energy end of a hierarchy of physics, with each member of the hierarchy operating at its own range of energies.⁹ The energy at which any particular current theory is no longer to be considered approximately accurate for the purposes under consideration is called the *cutoff* energy, though despite its name we would typically expect a continuous decrease in applicability, not simply a sudden change from applicability to non-applicability.

⁹ See, for instance, Zee (2003, pp. 437-438), Cao (1997, pp. 349-353), and Teller (1988, p. 87). For example, Zee says that he espouses the philosophy that a quantum field theory provides an effective description of physics up to a certain energy scale Λ , a threshold of ignorance beyond which physics not included in the theory comes into play. (P. 438). The need for a cutoff energy comes from the renormalization procedure used in quantum field theory, in which quantities are calculated by taking the limit as the energies of the allowed types of interactions goes to infinity, resulting in various terms such as the bare mass becoming infinite. Although the renormalization procedure is generally accepted as a mathematically legitimate, conceptually it is highly problematic: e.g., no mass can be actually infinite. Thus, the current orthodoxy resolves this by holding that quantum field theories are approximate theories that only apply to relatively low energies.

In contemporary terminology, our current physical theories are to be considered *effective field theories*. The limitation of our current physics directly affects thought experiments involving changing force strengths. Although in everyday life we conceive of forces anthropomorphically as pushes or pulls, in current physics forces are conceived of as interactions involving exchanges of quanta of energy and momentum.¹⁰ This idea has developed naturally from the definition in classical mechanics of *work* B that is, the amount of energy expended B as being proportional to the *force* applied in the direction of motion. Further, the change in momentum is proportional to the *force* applied times the amount of time over which it is applied, by $F = ma$. The strength of a particular force, therefore, can be thought of as proportional to rate of exchange of energy-momentum, expressed quantum mechanically in terms of probability cross sections.¹¹ Drastically increasing the force strengths, therefore, would drastically increase the energy- momentum being exchanged in any given interaction. Put another way, increasing the strength of a force will involve increasing the energy at which the relevant physics takes place. So, for instance, if one were to increase the strength of electromagnetism, the binding energy of electrons in the atom would increase; similarly, an increase in the strength of the strong nuclear force would correspond to an increase in the binding energy in the nucleus.¹²

The limits of the applicability our current physical theories to below a certain energy scales, therefore, translates to a limit on our ability to determine the effects of drastically increasing a value of a given force strength B for example, increasing the strong nuclear force by a factor of 10^{1000} . If we naively applied current physics to that situation, we would conclude that no complex life would be possible because atomic nuclei would be crushed. If a new physics applies, however, entirely new, and almost inconceivable effects could occur that make complex life possible, much as quantum effects make the existence of stable atomic orbits possible whereas such orbits were inconceivable under classical mechanics.

Further, we have no guarantee that the concept of a force strength itself remains applicable from within the perspective of the new physics at such energy scales, much as the concept of a particle having a definite position and momentum, or being infinitely divisible, lost applicability in quantum mechanics, or the notion of absolute time lost validity in special

¹⁰ Speaking of gravitational force as involving energy exchange is highly problematic, though speaking of gravitational binding energy is not nearly as problematic. One problem is that in general relativity, gravity is not conceived of as a force but as curvature of space-time. Another problem is that there is no theoretically adequate definition for the local energy of a gravitational field or wave. (See, for instance, Wald, 1984, p. 70, note 6 and p. 286.) Finally, although physicists often speak of gravitons as the carriers as the carrier of the gravitational force, the quantum theory of gravity out of which gravitons arise is notoriously non-renormalizable, meaning that infinities arise that cannot be eliminated. Nonetheless, since gravitational waves cause changes in energy of material objects at a certain rate, we can still meaningfully speak of energy scales at which a particular gravitational $A_{\text{force@}}$ is operating, which is all that is needed for this argument.

¹² The weak force does not involve binding energies, but is an interaction governing the transmutation of particles from one form to another and so this last argument would not apply to it.

relativity, or gravitational force (versus curvature of space-time) in general relativity.¹³ Thus, by inductive reasoning from the past, we should expect not only entirely unforeseen phenomena at energies far exceeding the cutoff, but we should even expect the loss of the applicability of many of our ordinary concepts, such as that of force strength.

The Planck scale is one often assumed cutoff for the applicability of the strong, weak, and electromagnetic forces. This is the scale at which unknown quantum gravity effects are suspected to take place thus invalidating certain foundational assumptions current quantum field theories are based on, such as a continuous space-time. (For example, see Sahni and Starobinsky, 1999, p. 44; Peacock, p. 275). The Planck scale occurs at the energy of 10^{19} GeV (billion electron volts), which is roughly 10^{21} higher than the binding energies of protons and neutrons in a nucleus, which are at the most around 10 MeV per nucleon. This means that we could expect a new physics to begin to come into play if the strength of the strong force were increased by more than a factor of 10^{19} . Another commonly considered cutoff is the GUT (Grand Unified Theory) scale, which occurs around 10^{15} GeV. (Peacock, 1999, pp. 249, 267). The GUT scale is the scale at which physicists expect the strong, weak, and electromagnetic forces to be united. From the perspective of GUT, these forces are seen as a result of symmetry breaking of the united force that is unbroken above 10^{15} GeV, where a new physics would then come into play. Effective field theory approaches to gravity also involve general relativity being a low energy approximation to the true theory. One common proposed cutoff is the Planck scale, though this is not the only one (See for example, Burgess, 2004, p. 6).

Where these cutoffs lie, and the fundamental justification for them, are controversial issues. The point of the above discussion is that most likely the limits of our current theories are finite, but very large since we know that our physics does work for an enormously wide range of energies. Accordingly, if the life-permitting range for a constant is very small in comparison, then $W_r/W_R \ll 1$, and then there will be fine-tuning. Rigorously determining the degree of fine-tuning, however, is beyond the scope of this paper.

Almost all other purportedly fine-tuned constants also involve energy considerations: for example, the masses of particles, such as the electron, neutron, proton, and electron correspond to energies ranging from around 1 MeV to a 1 GeV and the cosmological constant is now thought of as corresponding to the energy density of empty space. Thus, the considerations of energy cutoff mentioned will play a fundamental role in defining the epistemically illuminated region, and hence of our comparison range R.

5.0 The Purported Problem of Infinite Ranges

5.1 Introduction

¹³ More generally, since constants are only defined with respect to the theories of physics in which they occur, their range of applicability is restricted to the range of applicability of those theories. Of course, one could simply plug these enormous values into our current physical models and obtain a result, but we know that such large variations in the constants will not relevantly *track* anything in the true underlying physics, as required by the methodology established in section II.

Timothy McGrew, Lydia McGrew, and Eric Vestrup (2000) have recently argued that the comparison ranges for the various cases of fine-tuning are all infinite, and that such infinite ranges pose two fatal problems for the FTA: first, it implies that the probabilities in the FTA are not normalizable, and second it implies the validity of what they call the coarse-tuning argument. In developing their argument they assume that the comparison range just is the range of values that the parameters conceivably could have, and since we can conceive of the values being anything, this range is infinite.

As argued above, the comparison range should not be taken as the conceivable range, but rather will be effectively delimited by what I called the epistemically illuminated region. For the sake of the argument and in case a future physics develops a theory in which EI is infinite, let us suppose that for some of the constants the comparison ranges are infinite. I will argue, however, that even if the proper comparison range is infinite, the McGrew/Vestrup objection to the FTA fails.

5.2 The Normalizability Problem

The McGrews and Vestrup argue that if the comparison range is infinite, and if we consider all sub-ranges of equal width equally probable B which is, they claim, the only possible non-arbitrary assignment of probability B then the probability assignments are not normalizable:

Probabilities make sense only if the sum of the logically possible disjoint alternatives adds up to one. If there is, to put the point more colloquially, some sense that attaches to the idea that the various possibilities can be put together to make up "one hundred percent" of the probability space. But if we carve an infinite space up into equal finite-sized regions, we have infinitely many of them; and if we try to assign them each some fixed positive probability, however small, the sum of these is infinite. (2003, p. 203).

An immediate response to this objection is that we can assign each finite sub-range a zero probability. To see this, note that any infinite range can be divided into a countably infinite set of finite sub-ranges of equal length that cover the entire range. If we apply the restricted principle of indifference, we must assign a probability of zero to the parameter landing in any given sub-range.

To this, the McGrews and Vestrup could argue that such an assignment of probability violates the axiom of countable additivity, an issue they unfortunately do not address. The axiom of countable additivity says that the countable sum of mutually exclusive classes of events must be equal to the probability of an event occurring in the union of the classes. That is, $\sum_{i=1}^{\infty} P(x_i) = P(x_1 \text{ or } x_2 \text{ or } x_3 \dots)$, where the x_i are mutually exclusive events. In the case of fine-tuning, if the ranges are mutually exclusive and exhaustive, then the probability of a parameter falling somewhere within the entire range should be the sum of the respective probabilities for each of the infinite number of sub-ranges. Since the parameter must have some value, however, the probability for the entire infinite range is one, whereas the sum of the probabilities of each range is zero. One response here is to save countable additivity simply by assigning each region an infinitesimal probability such that the countable sum of these infinitesimals adds up to one; this issue is explored in detail in [author unnamed]'s paper in this symposium. This technical

solution aside, the Anormalizability problem[@] rests on confusing the mathematical definition of probability with the sort of probability used in science and everyday life, particularly *epistemic* probability, which is the type of probability I am claiming occurs in the fine-tuning argument.¹⁴ The McGrews and Vestrup never tell us what type of probability they are working with. Is it relative frequency? Logical probability? Epistemic probability? The answer to this question is crucial to determining whether countable additivity applies. As mentioned above, epistemic probability has to do with rational degrees of belief. It seems to be rational to be certain that any given member of an infinite class of events will not occur, and at the same time believe that one of the events will nonetheless occur. Or, at the very least, such a set of beliefs is not obviously irrational.

Consider a situation in which there is an infinite number of discrete alternatives a_i that are mutually exclusive and exhaustive, and for which we are epistemically indifferent between the alternatives. Under this circumstance, it certainly seems rational to be certain, for each alternative a_i , that it will not be the case: that is, to assign each a_i zero epistemic probability. Indeed, in such a situation, this is the only non-arbitrary degree of belief one could have in any given a_i . Any uniform *finite* degree of belief over the a_i would violate the axiom of finite additivity. Countable additivity, on the other hand, would demand that we either assign some a_i different probabilities than other ones, or claim that there is no probability in this case. Clearly, however, it cannot be rationally obligatory to assign different a_i different rational degrees of belief when there is no epistemic distinction between them. As Williamson notes (1999, p. 408), de Finetti objected that such a principle would require one to treat as epistemically unequal what one regarded as epistemically equal: Aeven if I think they are equally probable, [...] I am obliged to pick a convergent series which, however I choose it, is in absolute contrast to what I think.[@] Worse, picking such a convergent distribution would force one to be practically certain (\square 99.9999% epistemic probability) of the occurrence of a member of some finite set of alternatives when one believed that the set was epistemically on a par with each of an infinite class of sets of other alternatives. As de Finetti remarked, AShould we force him, against his own judgement, to assign practically the entire probability to some finite set of events, perhaps chosen arbitrarily?[@] (quoted in Williamson, 1999, p. 407). It is difficult to see how such a principle could be rationally obligatory!

One might respond that when the only non-arbitrary distribution of degrees of belief violates the axiom of countable additivity, the most rational alternative is to remain agnostic.

¹⁴ Mathematical probability is defined simply by a certain set of axioms, typically the Kolmogorov axioms. Any class of entities and relations that meets those axioms (e.g. the class of all volume elements in some enclosed three dimensional space) can be considered a probability in the mathematical sense. The various types of probability used in science and everyday life, however, refer to some particular relation, such as the relation of support between propositions in the case of epistemic probability, or relative frequency in some class in the case of the relative frequency interpretation. It is controversial to what extent these sorts of probability obey the Kolmogorov axioms. For extensive discussion of whether the axiom of countable additivity applies in these cases, see von Plato (1994, pp. 276-278), Williamson (1999), and Gillies (2000, pp. 65-69).

After all, one need not assign epistemic probabilities to all propositions. I do not believe this is an adequate response, since I think in some cases it would be irrational to remain agnostic. To illustrate, suppose that God creates an infinite lottery by creating a truly random number generator that randomly picks the winning number from the integers. (Never mind for now whether you think the creation of such a generator is possible.) Further suppose that you have no reason to believe that this number falls into one range instead of any other and that each ticket sells for \$20, with a \$100,000 jackpot. Finally, suppose that your sister decides to buy lottery tickets, spending \$20 a month on them. I am sure you would try to persuade her not to. You would not merely be agnostic about her winning; you would be sure that she would lose. And this certainty could be substantiated with significant arguments. For example, you might reason that if the lottery had a trillion, trillion, trillion tickets, it would be a waste of money to buy tickets, since the chance of winning would be so small you could say with confidence that she could be certain of not winning. Moreover, the larger the number of tickets, the more you would be certain that she won't win. Certainly, in the limit as the number of tickets becomes infinite, you should not merely become agnostic about whether she will win. You would be absolutely certain she would lose. Compare this with the case in which there is a lottery, but you are given no idea of how many tickets there are. In such a case, you would be truly agnostic about your sister's winning. You might simply hope she would win, without any idea of what her chances are. In the infinite lottery case, it seems clear that you should have no hope whatsoever of her winning, which is not the attitude of agnosticism.

Here is another argument against requiring agnosticism in these cases. Suppose you adopted the agnostic alternative. Then for any finite range $[M, M + N]$, you should be agnostic about whether the winning number is in that range, where M and N are positive integers. Even though agnosticism does not commit you to a degree of belief, to be agnostic is to be less than certain. Now consider some given number k that is in the range $[0, M]$, and let k^* be the proposition that k is the winning number. If you were certain that the number was in the range $[0, M]$, but did not have any information about which member of the range it was, then you would assign every member of this range an epistemic probability of $1/M$ for being the winning number. If you were agnostic about whether it fell into that range, then your probability would obviously be less. Thus, you could reason as follows: A If I were sure k was in that range, then k^* would have an epistemic probability of $1/M$. But, since I don't even know that the winning number is in the range, I have even less reason to believe the winning number is k B that is, my epistemic probability $P(k^*)$ should be less than $1/M$. Now, for any positive integer N , k will be a member of some range $[0, M + N]$. Thus, for every N , the probability of k being the winning number will have to be less than $1/(M + N)$. The only consistent value I can assign to the probability of k^* , therefore, is zero or an infinitesimal, since $P(k^*)$ is less than $1/(M + N)$ for all N .@

Of course, one might object that no such random number generator could ever be constructed, even by God. As Williamson notes, however, this objection is irrelevant [1999, p. 407]. The issue is not whether some objective random number generator could be made, but whether the agent in question believes that such a random number generator exists, since epistemic probability is a relation between propositions held by some agent in a set of specified epistemic circumstances. Further, many of the arguments would go through even without having a random number generator: one could simply believe that God picked some finite number, and that God

picked the number independently from any foreknowledge of what number one would guess, and that one's choice was causally independent of what God chose.

Given the formidable set of arguments above for rejecting countable additivity for epistemic probability, those who (like the McGrews and Vestrup) insist on countable additivity need to show that distributions that violate this axiom are rationally incoherent.

5.3 The Coarse-tuning Argument

The second argument presented by the McGrews and Vestrup goes back to a similar argument presented by theoretical physicist Paul Davies (1992), and also repeated by Manson (2000). According to this argument, which McGrew and Vestrup call the Coarse-tuning Argument (CTA), if the comparison range is infinite, then no matter how large the range r of intelligent-life-permitting values, as long as it is finite, the ratio of it to the conceivable range will be zero. This means that the narrowness of the range becomes irrelevant to our assessment of degree of fine-tuning. The mere finitude of the intelligent-life-permitting range would confirm design or many-universes. The McGrews and Vestrup conclude that if we are determined to invoke the Principle of Indifference regarding possible universes, we are confronted with an unhappy conditional: if the FTA is a good argument, so is the CTA. And conversely, if the CTA is not a good argument, neither is the FTA (2003, p. 204). Davies presents a similar worry:

From what range might the value of, say, the strength of the nuclear force... be selected? If the range is infinite, then any finite range of values might be considered to have zero probability of being selected. But then we should be equally surprised however weakly the requirements for life constrain those values. This is surely a *reductio ad absurdum* of the whole argument. [1992, pp. 204-205]

One response is to agree that we should be equally surprised however weakly the requirements for life constrain those values. If we truly found for some constant C that (i) the comparison range R as determined by our method R is infinite, and (ii) the range of intelligent-life-permitting values is finite, and (iii) we should be epistemically indifferent over the range of possible values, then we should think the fact that C falls into the intelligent-life-permitting range does strongly confirm design (or multiple universes) over the atheistic single-universe hypothesis. That is, we should conclude that the CTA is a sound argument. I see nothing odd or counterintuitive about this. The CTA is certainly not obviously absurd. Physics would still be required in constructing the CTA. For example, to show that the intelligent-life-permitting range was indeed finite and that the total theoretically possible range was infinite, and to show that we have no physical reason for preferring one value of a parameter over any other in the range.

The main reason Davies has trouble with the CTA is that he claims that we are more impressed when the requirements of life are more restrictive. [Davies, p.] According to Davies, it is the smallness of the range of intelligent-life-permitting values that appears to ground our temptation to infer design or many universes. If, however, we had good reason to believe that the comparison range for C is truly infinite, and that we should be epistemically indifferent over that range, then we would just have to conclude that our initial impressions were wrong that it was the smallness of the range, not its finiteness, that gave the FTA its force. Although there is a slight presumption in favor of such initial impressions, they are by no means indefeasible.

That said, we can rationally reconstruct why we are impressed with the relative smallness of the intelligent-life-permitting region instead of merely its finiteness. The reason we are impressed with the smallness, I suggest, is that we actually do have some vague finite range to which we are comparing the intelligent-life-permitting range. We do not, as a matter of fact, think that all values for a parameter are equally likely, just those in some limited range around the actual value of the parameter in question. This vague sense of a comparison range is given a rigorous foundation when we actually apply my method above. If we ever developed a theory with free parameters which we are confident has no limit of applicability, and the sum of the life-permitting ranges were known to only take up a finite width, then the CTA would be the correct argument to apply, not the FTA. Since in today's physics EI is finite, the FTA is the correct argument and thus the stress in the literature on the relative smallness, versus simply finiteness, of the life-permitting range is appropriate.

Finally, rejecting the CTA for the reasons the McGrews and Vestrup give is counterintuitive. Assume that the FTA would have probative force if the comparison range were finite; although they might not agree with this assumption, making it will allow us to consider whether having an infinite instead of finite comparison range is relevant to the cogency of the FTA. Now imagine increasing the width of this comparison range while keeping it finite. It seems that the more W_R increases, the stronger the FTA gets. Indeed, if we accept the restricted principle of indifference, as W_R approaches infinity, $P(L_C/AS)$ will converge to zero, and thus $P(L_C/AS) = 0$ in the limit as W_R approaches infinity. Accordingly, if we deny the CTA has probative force *because* W_R is purportedly infinite, we must draw the counterintuitive consequence that although the FTA gets stronger and stronger the larger W_R gets, magically when W_R becomes actually infinite, the FTA loses all probative force.¹⁵

6.0 Concluding Thoughts:

Suppose that my argument for a finite range fails....the Vestrup and the McGrews are right.

References

- Achinstein, Peter. A Stronger Evidence, @ *Philosophy of Science*, 61, September 1994., pp. 329 - 350.)
- Burgess, Cliff P. (April, 2004). *Quantum Gravity in Everyday Life: General Relativity as an Effective Field Theory*. *Living Reviews in Relativity*, Published by the Max Planck Institute for Gravitational Physics, Albert Einstein Institute, Germany (<http://www.livingreviews.org/lrr-2004-5>, pdf format).
- Cao, Tian Yu. (1997). *Conceptual Developments of 20th Century Field Theories*, Cambridge: Cambridge University Press.
- Collins, Robin. (2003). A Evidence for Fine-tuning. @ In Neil Manson, ed., *God and Design: The Teleological Argument and Modern Science*, London: Routledge, pp. 178-199.

¹⁵ For an argument showing that various inferences in contemporary cosmological speculation use infinite ranges, and for some mathematical justification of these, see Jeffrey Koperski, A Should We Care About Fine-Tuning? @ (Put reference in Bibliography)

- Davies, Paul (1992) *The Mind of God : The Scientific Basis for a Rational World* (New York: Simon and Schuster).
- Gillies, Donald (2000) *Philosophical Theories of Probability* (London and New York: Routledge).
- Hacking, Ian (1987) *Coincidences: Mundane and Cosmological*, @ reprinted in *Origin and Evolution of the Universe: Evidence for Design*, ed. John M. Robson (Montreal: McGill-Queen's University Press), pp. 119-138.
- Howson, Colin (1991) *A Discussion: The Old Evidence Problem*, @ *The British Journal for the Philosophy of Science* 42, pp. 547 - 555.
- Leslie, John. *Universes*. New York, Routledge, 1989.
- Keynes, J. M. *A Treatise on Probability*. London: Macmillan, 1921.
- Manson, Neil A. (2000) *There is No Adequate Definition of >Fine-tuned for Life=*, @ *Inquiry* 43, pp. 341-52.
- McGrew, Timothy, McGrew, Lydia, and Vestrup, Eric (2001) *Probabilities and the Fine-tuning Argument: A Skeptical View*, @ *Mind* 110, pp. 1027-38. Reprinted in *God and Design: The Teleological Argument and Modern Science*, ed. Neil A. Manson (London: Routledge, 2003), pp. 200-208.
- Oberhummer, H., Csótó, A. and Schlattl, H. (2000a) *Fine-Tuning of Carbon Based Life in the Universe by Triple-Alpha Process in Red Giants*, @ *Science* 289, no. 5476 (7 July 2000), pp. 88-90.
- BBB (2000b) *Bridging the Mass gaps at A = 5 and A = 8 in nucleosynthesis*, @ <http://xxx.lanl.gov/abs/nucl-th/0009046> (18 September 2000).
- Peacock, John. (1999). *Cosmological Physics*. Cambridge: Cambridge University Press, 1999.
- Plantinga, Alvin. (1993). *Warrant and Proper Function*. Oxford: Oxford University Press.
- Sahni, Varun and Starobinsky, Alexei. (1999) *The Case for a Positive Cosmological Λ -term*, @ at <http://xxx.lanl.gov/abs/astro-ph/9904398>, (28 April 1999).
- Teller, Paul. (1988). *Three Problems of Renormalization*. @ In Rom Harré and Harvey Brown, eds., *Philosophical Foundations of Quantum Field Theory*, Oxford: Clarendon Press, pp. 73-89.
- von Plato, Jan (1994) *Creating Modern Probability: Its Mathematics, Physics and Philosophy in Historical Perspective* (Cambridge, U.K.: Cambridge University Press).
- Wald, Robert. (1984). *General Relativity*, Chicago, IL: University of Chicago Press.
- Williamson, J.O.D. (1999) *Countable Additivity and Subjective Probability*, @ *British Journal for the Philosophy of Science* 50, pp. 401-416.
- Zee, A. (2003). *Quantum Field Theory in a Nutshell*. Princeton, NJ: Princeton University Press.

