Dirac's "fine-tuning problem": A constructive use of anachronism?

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Abstract

In order to shed light on contemporary arguments about "fine-tuning" in cosmology, I examine a possible historical precedent for fine-tuning from the early years of Quantum Electrodynamics: the divergent self-energy of the electron in Dirac's theory. I argue that viewing this problem as a fine-tuning problem involves an anachronistic reconstruction, but that such reconstructions can be philosophically useful so long as they are not confused with real historical understanding. I relate how, historically, this problem really was conceived, and show how one important step toward its solution drew upon an interpretation of Dirac's formalism in terms of "hole theory." In light of the subsequent demise of hole theory, I argue that my anachronistic reconstruction can serve as a cautionary tale that should considerably weaken the conclusions that might otherwise be drawn from attempts to give theistic or multiverse solutions to cosmological fine-tuning problems.

1 Introduction

Recent philosophical discussions of cosmology have made much of the "fine-tuning" of parameters in our currently most fundamental theories. Roughly, a parameter is fine-tuned when the range of values of that parameter that are compatible with some observed feature of the universe is quite small relative to the range of values that are, in some sense, *possible*. Some have argued that such instances of fine-tuning provide evidence for the creation of the universe by an intelligent being (Craig 2003; Swinburne 2004), claiming that no other explanations of such fine-tuning are plausible. Others think the situation calls for some variety of "anthropic" explanation (Vilenkin 2006). Such claims might be undermined should it be found that physicists have a history of solving fine-tuning problems not by invoking divine providence or the anthropic principle, but through physical theorizing and experiment.

In this paper I wish to consider a candidate for such an historical precedent drawn from the development of quantum electrodynamics (QED) in the 1930s. Closer scrutiny of that episode will reveal that, although the problem encountered can be reconstructed as a fine-tuning problem, its conceptualization by the physicists who confronted it was rather different. I will discuss the extent to which we might regard the episode as a cautionary tale that would encourage cautious skepticism about the extent to which we can draw substantive conclusions from a theory's ability to solve a fine-tuning problem.

Although such an argument requires a degree of what I shall call "constructive anachronism," I contend that the benefit of allowing the exploration of epistemological commonalities across historically distinct problem contexts justifies such anachronism, provided that the analysis is accompanied by a sufficiently diffident attitude toward the resulting historical picture.

I approach these issues as follows in this paper: In §2 I give a general characterization of fine-tuning, which I illustrate with the example of the fine-tuning of the cosmological constant. I then introduce, in §3, an analogy drawn by physicist Hitoshi Murayama between what he calls two fine-tuning problems. The first is one that concerns the mass of the electron in Dirac's Quantum Electrodynamics; the second concerns the mass of the Higgs boson. I call this "Murayama's anachronism." The reasons for considering it to be an anachronism are included in §4, which presents the historical narrative at the heart of this argument. Here I explain the problem of the divergence of the self-energy of the electron in Dirac's theory and show how Victor Weisskopf, working as assistant to Wolfgang Pauli, employed a speculative interpretation of Dirac's theory (the "hole theory") to remove much of the difficulties posed by that divergence at the same that Pauli was raising serious concerns about the hole theory. In §5 I note how this problem, contrary to Murayama's portrayal, was not conceived of as a fine-tuning problem at the time. But, I argue, just as Murayama could justifiably though anachronistically reconstruct the self-energy problem as a fine-tuning problem for scientific purposes, we can do the same for philosophical purposes. In particular, I argue, such a reconstruction can be used to highlight the potentially tenuous and instable support that solving a fine-tuning problem provides for a theory, if that theory fails to lend itself to the kind of probative testing that can generate secure support for a theory. I summarize my conclusions in §6.

2 What is fine-tuning?

The term “fine-tuning” is used in various ways by different authors, making the notion difficult to capture in a single formulation. The following, however, seems to capture central aspects of fine-tuning as it is invoked by those who wish to draw substantive conclusions from instances of fine-tuning.

Suppose X denotes some feature of the universe, and that a quantity Q is either a parameter in a physical theory or a measurement of some “initial condition,” the value of which is $Q'^{+\epsilon_1}_{-\epsilon_2}$. If there is some interval $[R_1, R_2]$, such that (i) $R_1 \leq Q' - \epsilon_2$ and $R_2 \geq Q' + \epsilon_1$, (ii) the fact that X is a feature of the universe is incompatible with either $Q \leq R_1$ or $Q \geq R_2$, and (iii) $|R_2 - R_1|$ is small relative to the range of possible values of Q,[1] then we shall say that the universe is *fine-tuned for X*.[2] Stated more intuitively, to say that the universe is fine-tuned in some way is to say that the universe exhibits some feature that would be absent were the value of some parameter in a physical theory outside of some range of values in which its actual value is found, which is quite small compared to the range of its possible values.

Authors presenting “fine-tuning arguments” for such conclusions as intelligent design or the existence of a multiverse variously contend that the existence of human life (or of intelligent creatures, or of biological organisms, or of matter organized in any very complex manner) depends on a large number of facts about the universe. Most of these facts are further dependent on a few fundamental facts of physics, viz., the masses and lifetimes of the elementary particles, and the strengths, both absolute and relative, of the four fundamental forces. A number of these lattter dependences meet the fine-tuning criteria.

2.1 The cosmological constant

An often-cited example of fine-tuning in discussions of cosmology concerns the *cosmological constant* λ in General Relativity (GR). Bypassing the subtleties of GR, we can simply note here that the cosmological constant is proportional to the energy density of the vacuum ρ_v. (Here we may safely follow the routine simplification of treating λ as if it were equal to ρ_v.) That the vacuum should have a non-zero energy density is a proposition that finds a natural motivation in Quantum Field Theory (QFT), according to which small regions of the vacuum are susceptible to fluctuations in their energy content. (One consequence of this feature of QFT is that the probability that a pair of particles — one matter and one anti-matter — will be created at any given spacetime location in the vacuum is non-zero; the origins of this idea are part of the story to be explored in this paper.)

However, if one estimates the value of ρ_v from QFT, one runs into trouble, arriving at an estimate of $\rho_v^{QFT} \sim 10^{92}\text{erg/cm}^3$.[3] This value, however, is about *120* *orders of magnitude larger* than what is allowed by observations.

The conceptualization of this disagreement as an instance of fine-tuning results from the following line of thought: Evidently, something else must be taken into account that "corrects" the QFT estimate so that almost all of it is cancelled out. We could suppose the observed vacuum energy density ρ_v to be result of the sum of two terms. The first term, ρ_v^{QFT}, represents the vacuum energy density as estimated by QFT. The second term ρ_v^{DE} expresses the density of *dark energy*. Under this representation, the question then becomes why the contribution from ρ_v^{DE} should so *very nearly but not completely* cancel the contribution from ρ_v^{QFT}. (Note that the assumption here is that this cancellation is in some sense very "surprising" and in need of explanation.)

Let us briefly note two quite distinct ways of attempting to solve the problem of the fine-tuning of the cosmological constant. The first is theological. Advocates of cosmological design arguments seize upon the fine-tuning of the cosmological constant (and of a number of other parameters of physical theory) as evidence of intelligent design in the creation of the universe on the grounds that such fine-tuning is to be expected if the universe were the work of a designer intent upon creating a world inhabitable by intelligent organisms like ourselves (Craig 2003; Swinburne 2004).

The second response invokes the idea of a *multiverse* along with *anthropic constraints*.[4] On this approach, a stochastic process determines ρ_v^{DE}, with different results in different regions of the multiverse. The question we now want to ask about the cosmological constant is "why we happen to live in a region where [ρ_v^{QFT}] is nearly cancelled by [ρ_v^{DE}]" (Vilenkin 2007, 166). If we suppose the multiverse to be sufficiently (possibly infinitely) vast so as to allow every value of ρ_v^{DE} to be realized somewhere, then (by anthropic reasoning) we should not be surprised to find ourselves in a region in which the value of ρ_v^{DE} allows ρ_v to be so small. "For the Universe to expand slowly enough that galaxies can form, [ρ_v] must lie within roughly an order of magnitude of its observed value. Thus the 10^{124} orders of magnitude of fine-tuning is spurious; we would only find ourselves in one of the rare domains with a tiny value of the cosmological constant" (Donoghue 2007; also see Weinberg 1987).

3 Murayama's anachronism

Much has been written by way of criticism of both the intelligent design and multiverse responses to the cosmological fine-tuning problem. I do not propose to review those critiques here, but only to note that the resistance (thus far) of this problem to less, shall we say, *drastic* means of solution suggests that the current state of cosmology calls for a broader-than-usual mode of assessment. Here I intend to contribute a historical component to that assessment by considering a historical precedent and querying its relevance for the current plight of fundamental physics.

The prompt for this inquiry is a hint, taken from an analogy by theoretical physicist Hitoshi Murayama. The context in which Murayama presents this idea is relevant to the interpretation and evaluation of this suggestion.

Murayama draws an analogy between two problems and two theoretical approaches to solving those problems. His target is the quantum field theory known as Supersymmetry (SUSY). SUSY is a "beyond-the-Standard-Model" theory that postulates the existence of a previously unobserved *superpartner* for every particle in the Standard Model that characterizes our current understanding of the basic matter and force fields in physics. The historical precedent for SUSY, in his analogy, is the theory of the electron proposed by P.A.M. Dirac in 1928 (Dirac 1928). When first proposed, Dirac's theory faced a daunting problem commonly referred to as the "self-energy problem."

In Murayama's analogy, the self-energy problem in Dirac's theory is a "fine-tuning" problem regarding the contributions of two terms to the total rest mass of the electron, and he regards the first steps toward a solution to this problem as having been provided (in work by Victor Weisskopf) by the chiral (matter/anti-matter) symmetry of Dirac's equation. Supersymmetry (SUSY), he argues, addresses a similar fine-tuning problem regarding the mass of the Higgs boson (also known as the hierarchy problem), by employing an analogous symmetry.

As the historical discussion below will document, Murayama's argument exemplifies a phenomenon discussed some years back by Thomas Nickles: he rewrites the history of science in "Whig" fashion to contextualize and guide the interpretation of current research (Nickles 1992). Nickles points out that, though academic historians may deplore this particular misuse of history, it is a perfectly reasonable argumentative strategy for the working scientist seeking to persuade the reader of the value of her own contributions. (I thus do not use the term "anachronism" to fault Murayama.)

Murayama starts his analogy with a reconstruction of the problem of the self-energy of the electron in classical electrodynamics (Murayama 1994, 2004). Start by thinking of the electron as a tiny ball of negative electric charge with radius r_e. But remember that like charges repel one another. To keep a quantity of electric charge contained in the volume of this tiny ball requires energy. This "self-energy" of the electron can be calculated to be

$$E_{self} = \frac{3}{5}\frac{1}{4\pi\epsilon_0}\frac{e^2}{r_e}. \tag{1}$$

(Here e is the electron charge and ϵ_0 is the electric permittivity constant of free space.) Equation (1) diverges linearly as $r_e \to 0$. In other words, as we consider the electron to approximate to a point particle, E_{self} approaches an infinite value linearly. Consequently, as Landau and Lifshitz note in their influential text, "the basic principles of electrodynamics itself lead to the result that its application must be restricted to definite limits. ...it is impossible to pose the question of whether the total mass of the electron is electrodynamic" (Landau & Lifshitz 1951).

Murayama, nonetheless, asks us to consider the experimentally observed mass of the electron (m_e) to be the combination of its "bare" or "mechanical" mass ($(m_e)^0$) and its self-energy. I.e.,

$$m_e c^2 = (m_e)^0 c^2 + E_{self}. \tag{2}$$

Murayama goes on:

> As we reduce the "size" of the electron, the smaller we should take its "bare" mass $(m_e)^0$, maybe down to a negative value. It requires increasing fine-tuning to reproduce the observed electron mass.

That is, at about the size of the "classical" electron radius, on the order of a few femtometers (10^{-15}m), the observed value for $m_e c^2$ and the value of E_{self} from equation (1) agree. As r_e gets smaller than that, equation (2) requires $(m_e)^0 c^2$ to take ever larger negative values as r_e shrinks. Experimentally, the "radius" of the electron is known to be smaller than 10^{-4} fm. This means that already $(m_e)^0 c^2$ must be $\sim 10^4$ times larger than the observed value of $m_e c^2$, and negative. As the electron is treated more and more like a point particle, the value that must be assigned to $(m_e)^0 c^2$ approaches $-\infty$. In the absence of any principled reason for assigning a particular value to $(m_e)^0 c^2$, the only way to save the phenomena is to simply *give* it a quantity that very nearly *exactly* (i.e., to at least one part in 10^4) matches the value of E_{self} because otherwise one gets a value for the observed electron mass that it is wildly in disagreement with experiment.

As Murayama tells it, "The cure to this problem was supplied by the discovery of the positron," i.e., the anti-matter partner to the electron. To follow his analogy, we now enter the realm of QED. The key lies in the concept of chiral symmetry. Very roughly speaking, a chiral transformation turns an electron field into a positron field (more generally, fermionic matter to fermionic anti-matter), and QED exhibits (broken) chiral symmetry in that the theory is (in a sense) invariant under such a transformation.[5] In the context of quantum electrodynamics, the possibility arises that fluctuations in the vacuum will give rise to an electron-positron pair, subsequently annihilated. If we consider an electron in a vacuum, then, it can happen that the electron will itself annihilate along with a positron that results from such a fluctuation, leaving the electron that accompanied that positron's creation from the vacuum in the original electron's place. In Murayama's words, the new electron "remains and pretends it were the original electron coming in." The result is that the position of the electron is in principle uncertain to about the Compton length λ_C ($\equiv \hbar/m_e c \approx 4 \times 10^{-13}$ m).

As Victor Weisskopf (with a helfpul correction from Wendell Furry) showed in 1934 (Weisskopf 1934b, 1934a, 1983), the result of this cancellation between re-absorption and fluctuation processes was not quite to eliminate the divergence of the electron self-energy as $r \to 0$, but to reduce it to a mere logarithmic divergence:

$$E_{self} = \frac{3}{4\pi}\frac{1}{4\pi\epsilon_0}\frac{e^2}{\lambda_C}\ln\frac{m_e c r_e}{\hbar}. \tag{3}$$

Granted, this equation still yields $E_{self} \to \infty$ as $r \to 0$, but only very slowly. In fact, as Muryama points out, "Even for a size equal to the Planck length [$\sim 10^{-35}$m], the self-energy is only about 10% correction to the 'bare' mass."

In Murayama's advertisement for supersymmetry, the point of this description of the chiral symmetry solution to electron-mass fine-tuning is to show how supersymmetry yields a similar solution to a similar problem for the mass of the Higgs boson.

The Higgs is a scalar (spin-zero) boson, postulated as part of the Higgs mechanism invoked to explain why the elementary particles have non-zero mass. Although the Higgs remains to be experimentally confirmed, it has become such a fixture in elementary particle physics that it is often treated as part of the Standard Model.

The problem of the self-energy of the Higgs boson is even worse than for the electron. If we consider the (still-to-be-) observed mass of the Higgs boson (m_H) to be the sum of the mass due to the self-energy of the Higgs field (E_{self}/c^2) and the Higgs "bare" mass (δm_H),

$$m_H = E_{self}/c^2 + \delta m_H, \tag{4}$$

then it turns out that $E_{self} \propto r_H^{-2}$. In other words, as the "size" of the Higgs boson shrinks, its self-energy diverges quadratically, and the degree of fine-tuning of δm_H required to get a realistic value for m_H grows even more quickly than in the case of the electron. Supersymmetry removes this difficulty by introducing a symmetry beyond those of the standard model.

Murayama argues that SUSY, by introducing a new symmetry that entails the existence of a new class of elementary particles, deploys a strategy to solving the Higgs mass fine-tuning problem (sometimes also called "the hierarchy problem") that has been used quite successfully before, in QED's solution to the electron mass fine-tuning problem. Thus, although alternative approaches also merit attention, the many years of effort that have gone into the development of SUSY and the current experimental searches for evidence of SUSY are well-motivated. Murayama's verdict is that "supersymmetry is one good candidate which is known to be consistent with the present phenomenology" (Murayama 1994, 4).

4 Confronting the infinite self-energy of Dirac's electron

Let us now consider the historical episode invoked by Murayama. In his telling of the story, Dirac's theory faced a fine-tuning problem regarding the mass of the electron, and Weisskopf's work showed how that problem could be, if not entirely solved, at least greatly mitigated.

As it turns out, however, in the 1930s neither Weisskopf nor anyone else among the physicists here discussed regarded the problem of the divergence of the electron's self-energy as a fine-tuning problem. This is not to say that the problem could not be reconstructed in this manner, but in doing so we are imposing an anachronistic conceptualization. In this section I will give a brief historical sketch of the context of Weisskopf's work to vindicate the charge of anachronism and to shed light on how Weisskopf and others did think of the problem and their attempts to solve it. In §5 I will argue that, just as scientific uses of anachronism need not be vicious, so might we make good philosophical use of this particular anachronism to shed light on current fine-tuning problems and the inferences that have sometimes been drawn from them.

4.1 Dirac's hole theory

Paul Dirac's "The Quantum Theory of the Electron" was received by *Proceedings of the Royal Society* on January 2, 1928 (Dirac 1928). In this first fully relativistic quantum theory of the electron, Dirac gave an equation that had as solutions both postive- and negative-energy states. The negative-energy states posed a conundrum, as they seemed to correspond to phenomena that had never been observed and that defied physical common sense. A negative energy electron would lose energy as it gained momentum and would have to absorb energy to come to rest. A force exerted on it in one direction would produce an acceleration in the opposite direction. Moreover, the density of negative energy electrons turned out to be infinite.

Dirac well knew that the problem of negative energy electrons is "a general one appearing in all relativity theories, also in the classical theory" (Dirac 1930, 360). But he argued that, unlike in the classical case, where the variation of dynamical variables is continuous and the negative energy solutions can simply be ignored as unphysical, quantum theory allows that perturbations might induce transitions between positive and negative energy states. In addition, spontaneous emission of radiation might cause such a transition. "Thus we cannot ignore the negative-energy states without giving rise to ambiguity in the interpretation of the theory" (Dirac 1930, 361).

Dirac went on to show that if we consider space to be filled with an infinitely dense “sea” of negative-energy electrons, then any “holes” in this sea would behave very much like positive-energy states with positive electrical charge.

> These holes will be things of positive energy and will therefore be in this respect like ordinary particles. Further, the motion of one of these holes in an external electromagnetic field will be the same as that of the negative-energy electron that would fill it, and will thus correspond to its possessing a charge $+e$. We are therefore led to the assumption that *the holes in the distribution of negative-energy electrons are the protons.* (Dirac 1930, 361; emphasis in original)

This was a mistake. In an article published in *Physical Review* in March 1930, Robert Oppenheimer calculated the time it would take for electrons to annihilate with holes in Dirac’s theory, and found a mean lifetime of 10^{-10} seconds, a result obviously incompatible with the observed stability of atoms (Oppenheimer 1930).

Dirac was not easily deterred from following out what he regarded as the clear implications of his equation, however. Rather than seeing the problems as limitations on the validity of the theory, as Bohr had urged, Dirac sought to resolve them by giving a different interpretation to the holes. Indeed, as argued by Moyer (1981), the “main business of Drac’s paper” was to implement his proposed methodology for advancing physical theory by first “perfect[ing] and generalis[ing] the mathematical formalism that forms the existing basis of theoretical physics” and then attempting to “interpret the new mathematical features in terms of physical entities" (Dirac 1931, 60). Dirac thus sets forth the proposal of anti-electrons with such diffidence that one hesitates to call it a prediction:

> Following Oppenheimer, we can assume that in the world as we know it, *all*, and not merely nearly all, of the negative-energy states for electrons are occupied. A hole, if there were one, would be a new kind of particle, unknown to experimental physics, having the same mass and opposite charge to an electron. We may call such a particle an anti-electron. We should not expect to find any of them in nature, on account of their rapid rate of recombination with electrons, but if they could be produced experimentally in high vacuum they would be quite stable and amenable to observation. (Dirac 1931, 61)

It must, therefore, have been a surprise even to Dirac when Carl Anderson, pursuing an investigation into the energies of cosmic gamma rays by studying the tracks of electrons accelerated by Compton scattering (the scattering of X-rays or γ-rays by matter) in a cloud chamber with a magnetic field, found evidence that came in time to be seen as a clear signature of the passing of an anti-electron. Anderson himself, who had not set out to test Dirac's hole theory, did not draw a connection to Dirac's work initially, claiming only to have found evidence of "a particle carrying a positive charge but having a mass of the same order of magnitude as that normally possessed by a free negative electron" (Anderson 1933). Additional and more detailed evidence was quickly found by P.M.S.

Blackett and G.P.S. Occhialini, using a triggered cloud chamber yielding more accurate measurements of the tracks recorded. Blackett and Occhialini discuss at some length their significance of their evidence of positive electrons, in light of Dirac's hole theory, including a discussion of Dirac's calculations of the mean free path of positrons in water. From this discussion they conclude that, while their own experiment does not include sufficiently detailed data to *test* Dirac's calculations, his theory "predicts a time of life for the positive electron that is long enough for it to be observed in the cloud chamber but short enough to explain why it had not been discovered by other methods" (Blackett & Occhialini 1933, 716; see Roqué 1997 for an illuminating discussion).

These developments led Oppenheimer toward a positive assessment of Dirac's "hole theory" (as Dirac's interpretation had come to be called). In a letter to the editor of *Physical Review* dated June 9, 1933 and published in July 1933, Oppenheimer and his assistant Milton S. Plesset wrote that "The experimental discovery of the positive electron gives us a striking confirmation of Dirac's theory of the electron, *and of his most recent attempts to give a consistent interpretation of the formalism of that theory.* ... Dirac has pointed out that we might obtain a consistent theory by assuming that it is only the absence of electrons of negative kinetic energy that has a physical meaning" (Oppenheimer & Plesset 1933, 53; emphasis added).

4.2 Pauli's skepticism

When it reached him in Zurich, Oppenheimer and Plesset's published letter prompted a skeptical Wolfgang Pauli to express worries about Niels Bohr. Bohr had initially been skeptical of Blackett and Occhialini's claims (Roqué 1997, 76). In a letter to Peierls (22 May 1933), Pauli noted that Bohr had said in March that he thought Blackett had just taken "pathological photographs" (Pauli 1985, 164). Pauli asked Peierls to share that letter with Victor Weisskopf, since the letter also dealt with his selection of Weisskopf for an assistantship scheduled to begin in the Fall.

By August, Pauli was writing directly to Weisskopf about administrative issues and the most recent developments in physics. In a letter dated August 2, 1933, Pauli worried that, due to "a certain tendency to swing from one extreme to another," Bohr might enter a "period of extreme gullibility [Leichtgläubigkeit] in the face of hole ideas."[6] Pauli's use of the expression "hole ideas" (*Löcheridee*) is here calculated to express his skepticism toward Dirac's theory. The "hole theory" is not really a theory, according to Pauli, unless one can use it to make quantitative predictions, and Pauli expresses in this letter that he does not know "whether one can calculate any quantitative effect at all" with Dirac's theory because infinities in the theory, including the infinite self-energy, "ruin" the calculation.

Apparently, Weisskopf in his response asked about the basis for Pauli's "prophesies" about Bohr.[7] In a letter dated August 29, 1933, Pauli explains that, aside from his "general knowledge of [Bohr's] psychology," his only "clue" is that the note published by Oppenheimer in *Physical Review* includes a note of thanks to Bohr. (The acknowledgment in question thanks Bohr, "who has helped us to understand the essential consistency of the theory which we have here applied" (Oppenheimer & Plesset 1933, 55).) Pauli concludes that "everything that seemed alarming and dubious in the Note has its origin in Bohr."

In the same letter, Pauli goes on to explain his disagreement with the optimistic assessment of Dirac's theory expressed in the Oppenheimer note:

> *Above all I believe him neither now nor before, that in the present state of the theory lengths of the order h/mc and those of the order of the classical electron radius e^2/m^2c^2 can be sharply divided, and moreover that one can claim in regions of the order h/mc that everything is now in beautiful order.* (That was more or less stated in the cited note; what Bohr thinks *now* about it, I naturally do not know.) . . . The state of affairs is simply that a separation of the difficulties of the theory between those coming from questions of the stability of the electron and the atomism of the electric charge and those connected with the states of negative energy or rather with the creation and entanglement [Verwicklung][8] of particle pairs[9] is *not at all possible* (emphases in original).

Pauli goes on to temper his negative assessment by acknowledging the success of Dirac's "Löcheridee" at leading to the prediction of the positive electron, "so there must be something to it" ("Dann muß also auch etwas daran sein!"). But "the problem of the quantitative elaboration of this idea into a theory seems insolubly tied up with the theoretical understanding of the atomism of electrical charge and the stability of the electron."

It was not only to Weisskopf that Pauli was expressing his skepticism about the hole theory. Skepticism about hole theory in the face of the findings of Anderson and of Blackett and Occhialini is a recurring theme in his 1933 correspondence, as evidenced in letters to Blackett (19 April 1933), Dirac (1 May 1933),[10] and Peierls (22 May 1933),[11] among others (Pauli 1985, 158, 159, 164) (See also Miller 1994, 47–51).

At this time Pauli and Heisenberg were corresponding intensively on the problems in Dirac's quantum electrodynamics and the prospects for fixing them (Darrigol 1986; Miller 1994; Rueger 1992). In a letter of 17 February 1934, Pauli wrote to Heisenberg about his recent work with Weisskopf on the polarization of the vacuum in Dirac's theory, noting that "the question of the infinite charge density [in Dirac's theory] is so closely tied to the self-energy, that one can hardly hope to solve one problem without the other." Pauli then goes on to propose that Heisenberg join Weisskopf and himself in authoring a new "Dreimänner-Arbeit"[12] and attaches an outline of the proposed paper.

4.3 Weisskopf calculates the electron self-energy in hole theory

The new Dreimänner-Arbeit never materialized, but Weisskopf continued to focus his work on the divergences in Dirac's theory, both in collaboration with Pauli and on his own with Pauli's encouragement. In an interview he recollected that

> Pauli told me that we must re-calculate the self energy of the electron because so far it had only been calculated by Ivar Waller for a single particle, that is, not with the Dirac theory of the vacuum. "The vacuum will probably change it; try to calculate it," Pauli said. So I sat down to calculate it and made a calculation mistake, a very primitive, bad mistake, which made me terrifically unhappy at the time, and got the result that it diverges as badly as Waller's diverges (Interview of V. F. Weisskopf, Archive for History of Quantum Physics (henceforth, AHQP)).

The paper to which Weisskopf refers, "On the self-energy of the electron" was received by *Zeitschrift für Physik* on 13 March 1934 (Weisskopf 1934b, translated in Miller 1994). In it Weisskopf makes the first attempt to calculate the self-energy of a free electron in a manner that takes into account the occupied negative energy states postulated by Dirac's hole theory. Previous efforts had used Dirac's wave equation of the electron and his theory of interaction between light and matter, but without consideration of the "sea" of occupied negative energy states (Oppenheimer 1930; Rosenfeld 1931; Waller 1930).

Weisskopf begins his calculation by noting that the electron self-energy is given by the expectation value of

$$E = -\int (\mathbf{iA}) d\mathbf{r} + \frac{1}{8\pi}\int (\mathbf{E}^2 + \mathbf{H}^2)\mathrm{d}\mathbf{r}, \tag{5}$$

where $\mathbf{E}$ and $\mathbf{H}$ denote the electric and magnetic fields respectively, $\mathbf{A}$ represents the vector potential and $\mathbf{i}$ is the current-density operator. He then proceeds to separate this quantity into an "electrostatic" contribution E^S and an "electrodynamic" contribution E^D. Making use of Heisenberg's 1931 radiation theory[13], Weisskopf's approach is to calculate the radiation field generated by the electron classically from the current and charge densities, but modified so as to subtract out those due to the occupied negative energy states. He then treats the amplitudes of the calculated potentials as non-commuting in the final result.

Calculating first the electrostatic self-energy, Weisskopf arrives at a logarithmic divergence ($E^S \propto \int_0^\infty \frac{\mathrm{d}p}{p}$), whereas in a "single electron" theory (calculating from Dirac's theory on the basis of the single occupied positive energy state of a free electron without considering the negative energy states) the divergence is linear ($E^S \propto \int \mathrm{d}p$). However, when he turns to the calculation of the electrodynamic contribution, he obtains the very same quadratic divergence ($E^D \propto \int k \cdot \mathrm{d}k$, where $k = |\mathbf{p} - \mathbf{p}'|$) that one gets from the single-electron theory. Thus, when both contributions to the self-energy are considered, Weisskopf's paper concludes that the self-energy divergence is just as bad in the hole-theory approach as in the approach that disregards negative energy states.

Shortly after the publication of Weisskopf's paper, he received a letter from Oppenheimer's assistant Wendell Furry that revealed to him the "very primitive, bad mistake" previously mentioned. Furry noted that he and Oppenheimer had read the paper with interest and approved of the method followed, but "We are, however, not able to agree with your result for the magnetic proper energy. About a year ago at Prof. Bohr's suggestion Dr. Carlson and I made this calculation. Our result was

$$E^D = \frac{mc}{(m^2c^2 + p^2)^{1/2}} \left[1 - \frac{4}{3}\frac{p^2}{m^2c^2}\right] \cdot \frac{e^2}{hc} \int \frac{\mathrm{d}k}{k} + \text{finite terms} \tag{6}$$

and is of same order as your result for the electrostatic proper energy."[14]

Furry not only provided the correct result, but went on to explain the physical reasoning behind the difference from the single electron result:

> ...the contribution to E^D from the negative K.E. [Kinetic Energy] states is much *smaller* than in the calculation with empty negative K.E. states, instead of merely being opposite in sign. This is physically evident when we use the light quantum picture and treat the energy displacement as due to the transitions of the system from a given state to other states and back, with corresponding emission and reabsorption of light quanta. For in the case of transition to a state of large negative K.E. and simultaneous emission of a quantum there is a high degree of resonance, and in the case of simultaneous pair production and light quantum emission such a resonance is lacking.

Furry's letter marks an interesting contrast with Weisskopf's paper, which presented calculations without entering into any kind of physical reasoning to explain the results obtained.

Although Weisskopf encouraged Furry to publish his correction, Furry declined, insisting that Weisskopf should publish the correction with just an acknowledgement of his letter. Weisskopf later recalled that Pauli reacted with his characteristic bluntness: "[Pauli's] attitude was, 'Well, I didn't expect much more from you; I'm not surprised. I never make mistakes, but of course you people do. That happens when one has to work with people like this.' And, 'I *said* that it must be different' " (Interview of V. F. Weisskopf, AHQP).

In any case, Weisskopf's use of Dirac's "Löchertheorie" to mitigate the divergence of the electron self-energy did little to mitigate Pauli's skeptical attitude. To the contrary, just a few months after Weisskopf's paper was received at *Zeitschrift für Physik* a paper that Pauli called the "anti-Dirac paper" (Weisskopf 1991, 83), co-authored by Pauli and Weisskopf, was received at *Helvetica Physica Acta* (Pauli & Weisskopf 1934, translated in Miller (1994)). The anti-Dirac paper set forth a relativistic quantum theory for spin-zero particles leading to the prediction of pairs of oppositely charged particles with identical masses. Pauli noted in a letter to Heisenberg (14 June 1934) that they obtained this result "*without further hypotheses* (without "hole-ideas," without "limit acrobatics," without subtraction physics!)." Because the theory would not work for spin-1/2 particles like electrons, but only for spin-zero particles (none of which had yet been discovered), the theory "has little to do with reality," but Pauli noted that "it gave me pleasure that I was able again to stick one to my old enemy — Dirac's theory of the spinning electron" (Pauli 1985, 329, translated in Enz 2002, 295).

(A historical aside is necessary here: This same letter to Heisenberg introduced Pauli's term "subtraction physics" as a pejorative term for a procedure, the idea of which Dirac introduced in an address at the 1933 Solvay conference (Dirac 1934, translated in Miller 1994, 136–144). Dirac, in the face of the fact that his postulated sea of negative energy electrons would have an infinite density, proposes that, provided that one identifies which distribution of electrons corresponds to *no* electromagnetic field, one could proceed to determine the field due to any other distribution by determining a rule for "subtracting that distribution from the one which exists effectively in each particular problem, in such a way as to obtain a finite difference . . . since, in general, the mathematical operation of subtraction between two infinities is ambiguous" (Miller 1994, 138). Heisenberg attempted a gauge- and relativistically-invariant formulation of Dirac's approach that would express in its "basic equations" the symmetry between negative and positive charges without introducing any new divergences beyond those due to "the well-known difficulties of quantum electrodynamics" (but he wound up deriving an infinite self-energy for light quanta) (Heisenberg 1934, translated in Miller 1994). Weisskopf himself then applied these ideas in 1936 (though without the density matrix approach employed by both Dirac and Heisenberg) to the calculation of the polarization of the vacuum. Weisskopf justified the subtraction procedure on the grounds that the divergent properties of the *vacuum electrons* that cannot be measured are "physically meaningless" (Weisskopf 1936 translated in Miller 1994).[15] So even as Pauli railed against subtraction physics, his closest collaborators were in fact developing and rationalizing it.)

4.4 Weisskopf revisits the problem

Weisskopf's second attack on the self-energy of the electron was written in 1939, after he had moved to the United States, where he had obtained a position at the University of Rochester. As he later recalled in his AHQP interview, "I felt this guilt that we do not understand the self energy and this logarithmic self energy. I have to really investigate thoroughly where this self energy comes from. Then I wrote this paper in the *Physical Review* of '39 ... It investigated all the details of why the self energy is logarithmic, how it comes about that the influence of the vacuum on the electron makes the electron broader and therefore increases the Coulomb energy, and all these detailed analyses of what's going on."'[16]

Describing the problem of the self-energy of the electron as being in a "critical state," Weisskopf writes in this paper that its main purpose "is to show the physical significance of the logarithmic divergence [of the electron self-energy] and to demonstrate the reasons of its occurrence" according to positron theory (Weisskopf 1939, 73). The key to understanding the "new situation" that Dirac's positron theory creates for the self-energy problem is to note that for a single free electron — in which situation all of the negative energy states are filled and only the *difference* between the "actual" and the unperturbed charge density of the vacuum is observable — the effect of the Pauli principle is "similar to a repulsive force" between particles with equal spin at distances $\sim h/p$. Thus, at the position of the electron there is a "'hole' in the distribution of the vacuum electrons which completely compensates its charge." This displacement of vacuum electrons in turn results in a "cloud of higher charge density" found one wave-length from the electron, so the the electron's charge is effectively "broadened" over $\sim h/mc$.

Weisskopf goes on to show that this broadening reduces the electrostatic self-energy from a quadratic to a logarithmic divergence. It also has an effect on the distribution of the of the magnetic field energy so that the equation for the magnetic potential now has terms with both quadratic and logarithmic divergences: $U_{mag} = \lim_{(a=0)} \left[\frac{e^2 h}{2\pi mca^2} - \frac{e^2 mc}{4\pi h} \cdot \log \frac{h}{mca}\right]$. Interference between the phase of the original electron wave and the phase of the fluctuation of vacuum electrons reduces the total electric field energy (in effect, then, the electron's contribution is negative: $U_{el} = -U_{mag}$). As a consequence, the total electric field energy due to spin is given by:

$$W_{sp} = -2U_{mag} = -\lim_{(a=0)} \left[\frac{e^2 h}{\pi mca^2} - \frac{e^2 mc}{2\pi h} \cdot \log \frac{h}{mca}\right] \tag{7}$$

Finally, there is "the energy of the action of the electromagnetic field fluctuations upon the electron," W_{fluct}. Weisskopf shows that the effect of the field on displaced vacuum electrons is negligible, so to first approximation, W_{fluct} is the

Figure 1: Schematic rendering of the charge distribution due to a single free electron (1a) and the vacuum electrons in the vicinity of the electron (1b) from Weisskopf 1939, 74.

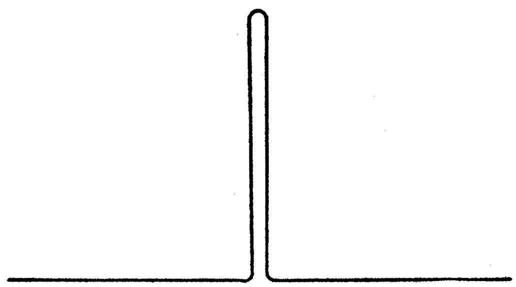

FIG. 1a. Schematic charge distribution of the electron.

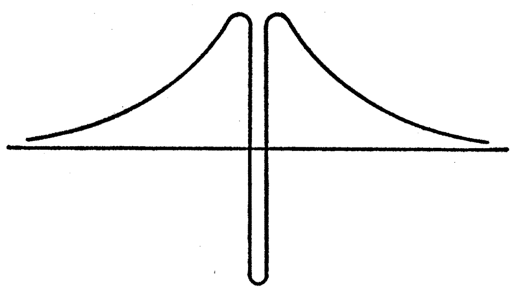

FIG. 1b. Schematic charge distribution of the vacuum electrons in the neighborhood of an electron.

same in positron theory as in single electron theory, and thus diverges quadratically. Weisskopf thus identifies three contributions to the electron self-energy in Dirac's positron theory: (a) W_{st}, the energy of the Coulomb field, which diverges logarithmically (b) W_{sp}, the spin contribution, which has terms exhibiting both logarithmic and quadratic divergence 7, and (c) W_{fluct}, the energy due to "forced vibrations under the influence of the zero-point fluctuations of the radiation field", which diverges quadratically. But because contributions (b) and (c) are *opposite in sign*, the quadratic terms cancel and only the logarithmic divergence is left.

This is, of course, the same result that Weisskopf reached in 1934, after Furry corrected his calculation error. What has changed is that Weisskopf now emphasizes, on the basis of the subtraction physics rationale, that although one still must treat the vacuum in terms of the sea of negative-energy electrons, it is only the *differences* between that state and the vacuum + 1 electron state that matter for the self-energy calculation. The divergent properties of the vacuum electrons are unobservable and thus do not matter. Indeed, all references to the hole theory have disappeared in his 1939 paper and have been replaced by references to "Dirac's positron theory" in contrast to the "single electron theory," which describes the electron using the Dirac wave equation without filling the negative energy states.

5 Discussion: A cautionary tale about fine-tuning?

Although the anti-Dirac theory that Weisskopf and Pauli published in 1934 could not be used as a theory of electrons with spin, it did in a sense contain the seeds of the destruction of hole theory (Pauli & Weisskopf 1934). Dirac's wave equation has both positive- and negative-energy solutions, but yields a spatial particle density function that, once integrated, yields an observable with the eigenvalues 0 and +1. Weisskopf had noticed that the Klein-Gordon scalar relativistic wave equation could be used to describe annihilation processes involving oppositely-charged particles if one assumed them already to exist. He raised this point to Pauli, who was initially uninterested. Through persistence he managed to engage Pauli's attention. (Pauli eventually said, "Oh, is that what you mean; why didn't you tell me right away?") Then Pauli pointed out that by quantizing the matter-wave (which Weisskopf had not done), one would get not only annihilation but also pair production, so that one would not have to assume the oppositely-charged particles at the outset (Weisskopf, AHQP interview). Moreover, the solutions to the Klein-Gordon equation are always positive in energy. The particle density, however, is not always positive. Pauli and Weisskopf argued that the particle density "no longer has a direct physical meaning" in Dirac's theory in any case, so negative particle densities are not a great price to pay in order to avoid negative energy states and the holes that are needed to make physical sense of positrons in light of the infinitely dense sea of negative-energy electrons. Moreover, the Klein-Gordon equation is both gauge- and relativistically-invariant.

Thus Pauli and Weisskopf succeeded in formulating a theory that described the processes of pair production and annhilation for oppositely charged particles, without recourse to negative energies or holes. The hole-theoretic understanding of quantum electrodynamics would in time give way to the more field-theoretic approach of which the Pauli-Weisskopf theory was a precursor (see Schweber 1994 for an erudite historical perspective and for a late defense of hole-theory from Peierls, 128–129).

To appreciate the subsequently eroding epistemic standing of the hole theory, it may be useful to draw a contrast with the positron. This spin-off of the hole theory turned out to be much more lasting than the hole-theoretic framework from which it sprang, surviving the transition away from hole-theory under a new interpretation (Roqué 1997).

To put it another way, the evidence for the positron was *secure* in a manner that the support (one hesitates to use the term 'evidence') for hole theory was not. Here I use the term 'secure' to refer to a kind of stability of support or evidence under the accumulation of additional information and theoretical change. I have elsewhere elaborated what I call the *secure evidence* framework for thinking about this kind of stability (Staley 2004; Staley & Cobb 2011; Staley 2011). The details of that framework need not detain us here. It will suffice to rely on the following rough negative characterization: An inference to a given conclusion (whether theoretical or experimental), is insecure for a given epistemic agent to the extent that there are ways that the world might be for all that agent knows, such that the inference to that conclusion would not be well-supported.

To say that support for the hole theory was less secure than the evidence for the positron is thus to say that whatever reasons might have supported reliance on the hole theory in the early 1930s were highly vulnerable to what were then, for the physics community, *epistemically possible* future discoveries, whereas the conclusions drawn by experimentalists regarding the positron from this era were much less vulnerable. (In addition, we can use this framework to read Alexander Rueger's 1990 as arguing that the basic principles of QED were similarly secure.)

To be sure, the decline of hole theory was not inevitable, but there is a certain logic to it if we consider the epistemic credentials of hole theory as opposed to the positron. Although a complete discussion of the reasons for hole theory's success in the 1930s exceeds the scope of this paper, suffice it here to note that the factors that various authors have cited include Dirac's authority, the influence of Heitler's 1936 book *The Quantum Theory of Radiation* (Heitler 1936), which made prominent use of the hole theory, and the heuristic value of hole theory (Darrigol 1986; Roqué 1997; Rueger 1992; Schweber 1994). The present paper has focused on one aspect of the last kind of consideration, exemplified in Weisskopf's use of hole theory to understand the divergence of the electron self-energy. By deploying the formal apparatus and physical interpretation of hole theory, Weisskopf was able both to show (with Furry's help) that the processes of positron production and annihilation mitigated the self-energy problem and to explain physically why that result obtains.

If the self-energy problem that Weisskopf tackles can be regarded as a fine-tuning problem, what should we make of the role of hole theory in Weisskopf's work on that problem? Hole theory's subsequent demise notwithstanding, is it reasonable to take its heuristic role in these efforts as providing support for the theory? And if so, under what interpretation?

As mentioned at the outset, nowhere in Weisskopf's work do we see the self-energy problem presented as a fine-tuning problem.[17] Neither have I found any such formulation of the problem in any of the other papers on the self-energy problem that are here cited, in Dirac's papers, or in Pauli's correspondence. So any discussion of the self-energy problem as a fine-tuning problem will require a reconstruction of the problem that will reek of anachronism. That is precisely what Murayama's analogy does, but this does not make the reconstruction useless, either for his purposes in theoretical physics or for ours in philosophical critique.

As the example of supersymmetry that motivates Murayama's analogy shows, physicists have certainly not forgotten that symmetry principles provide a sometimes fruitful source of solutions to fine-tuning problems, which can then be subjected to detailed experimental probing. But these symmetry principles enter into theory not in bare form, but clothed in concepts that call for physical interpretation. The chiral symmetry of quantum electrodynamics originally came in the guise of hole theory. The demand for experimental testing of the commitments of the theory lies at the heart of Pauli's skeptical questions. He introduced his distinction between "hole ideas" and "hole theory" as a means of challenging defenders of Dirac's apparatus to show that it could be used to calculate quantitative results that could be compared with experiment. But the very same developments in "subtraction physics" that served to rationalize the use of Dirac's theory also pushed the hole-theoretic interpretation towards functioning as a kind of irrealistic flourish on the theory: as the properties of the "sea" of negative energy electrons come to be seen as physically meaningless, it becomes less clear just what the hole theory describes, if anything.

Now let us consider how these considerations bear, if only indirectly, on current attempts to solve fine tuning problems. Here I will confine my comments to the cosmological constant problem. Consider first theistic or "intelligent designer" solutions (the comments that follow apply equally to both and I will not continue to mark the distinction). Although there have been attempts to give the fine-tuning argument for God's existence a Bayesian probabilistic formulation, these inevitably face the problem that the alternative "theories" against which the theistic explanation contends (often just stated as "atheistic single universe" or "atheistic multiple universe") are themselves not real theories that lead to definite predictions regarding the cosmological constant, nor do we really have much idea of what such theories would look like – we haven't got any good examples to draw upon. Thus, not only do such arguments depend strongly on subjective prior probabilities, their invocation of "ideas" rather than "theories" (in Pauli's sense) means that the likelihoods (the probabilities that they confer on particular observations) are not well-defined. Moreover, they are highly susceptible to the kind of "new-theory" problem that calls for a complete scrapping of the posteriors and a redefinition of the priors (Earman 1992). In light of these problems we would have to say that any support for theism that might be thought to derive from fine-tuning is very insecure. Moreover, because the resources of experiment can not be brought to bear on any aspect of the theistic explanation of fine-tuning, such support is likely to remain highly insecure, or else to disappear entirely upon the arrival of a more adequate physical explanation.

Defenders of a multiverse explanation might claim that such a more adequate physical explanation is already in hand. For example, they might claim that the use of anthropic reasoning in an inflationary multiverse context has led to a successful prediction for the cosmological constant λ (Martel, Shapiro, & Weinberg 1998; Weinberg 1987). Some skepticism about this claim seems appropriate, however. As Lee Smolin has pointed out (Smolin 2007), the part of Weinberg's argument that draws explicitly on anthropic reasoning applied to a multiverse is not robust under the choice of the initial ensemble of universes to which it is applied (Graesser, Hsu, Jenkins, & Wise 2004).[18] A weaker constraint on λ that Weinberg derives does not in fact require anthropic selection as applied to a multiverse but is simply a deduction from the observed fact that there are galaxies. But even should we grant that Weinberg has in some sense successfully used anthropic selection applied in a multiverse context to predict the value of λ, thus vindicating the claim that such an approach solves the fine-tuning problem of the cosmological constant, this would seem to be support for a multiverse approach that is no more secure than was the support for hole theory generated by Weisskopf's treatment of the self-energy problem (see also Norton 2010 for a trenchant critique of some prominent arguments relying on multiverse ensembles).

At most, this prediction might give us a reason to look for ways that the inflationary multiverse theory might be made susceptible to more probative tests. As Pauli might (again) put it, we might call for investigations into how such multiverse-based thinking might be turned from an *idea* into a *theory*.

6 Conclusion

The appeal of Supersymmetry draws upon the demonstrated power of new symmetry principles to solve that class of problems that we have come to think of as "fine-tuning" problems. Thus far, that kind of approach has failed to yield promising results when applied to the fine-tuning of the cosmological constant. It would thus be naive to propose that one could simply model a solution to that problem on the precedent of Dirac's positron theory as deployed by Weisskopf.

What that precedent does illustrate is the caution that is appropriate when evaluating a theory on the basis of its success in resolving a fine-tuning problem. The ability of a theory (or an "idea") to solve a problem, even to yield a prediction, might be reason to think "there must be something to it." But we should be careful in our consideration of what that something has been shown to be in a given case. Specifically, what we can learn from the example of hole theory/positron theory is that it is important to ask of the support offered for a theoretical claim the extent to which it is secure against changes in what we regard as the available alternatives.[19]

Although the answers to these kinds of questions are far from straightforward, we can at least identify some relevant considerations. How remote from experimental probing are the suppositions of the theory? What is the scope of the theory, relative to what is empirically accessible to us? How well understood and how well-established are the facts that the theory has been introduced to explain? How reasonable is it to assume that those facts are the sort of facts for which there must be some explanation?

We have already seen how Pauli raised concerns about Dirac's hole theory with regard to some of these questions. In particular, he was concerned about the extent to which the theory could be made to yield quantitative predictions such as would be needed to subject the theory to probative testing. In this respect, the ideas that have been put forward as solutions to fine-tuning of the cosmological constant seem in much *worse* off than the hole theory. Neither theistic nor multiverse solutions show much promise of probative testing. They both have implications that far exceed our empirical reach. Finally, although we may certainly take current limits on the value of the cosmological constant to be reasonably well-established, it is not entirely obvious that the fine-tuning of that value must *have* an explanation, however much we might wish for one. All of these considerations should incline us to place rather little weight on arguments that purport to show that these theories 'solve' the problem of the fine-tuning of the cosmological constant.

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Notes

[1]That "small" is vague is a minor worry here. Much more significant is the worry that there may be no meaningful sense of "possible" for use in the final condition that is applicable and that does not render the conditions trivially satisfied by too many cases. It is far from obvious, for example, that physical possibility can be applied to many of the cases of interest. And logical possibility seems make the condition of relative smallness too easily satisfied. I shall not press this point, but will assume for the sake of argument that there is some way address these worries.

[2]This account is a modified version of that given by Sherry Roush (2003).

[3]This estimate is based on an integration over wavelengths that is cut off at the Planck length.

[4]The idea that the "laws" of physics that we take to govern all physical processes should vary from one region of spacetime to another, with radically different physical processes as a consequence, has received much attention lately, with the publication of a number of popular books on the topic. This idea has a number of distinct theoretical motivations, and it is not my intention to comment here on the idea of the multiverse as such, but only to discuss it as motivated by such fine-tuning problems as

that involving the cosmological constant.

[5]More accurately, a chiral transformation turns a positive-energy solution to the Dirac equation for a spin-1/2 particle into a negative energy solution, and the symmetry is exact only for a massless electron. Although a broken symmetry of quantum electrodynamics, chiral symmetry is "softly" broken, in the sense that it is explicitly broken in such a way as to leave the short-range behavior of the theory unaltered from the symmetric case.

[6]Unless otherwise specified, the letters from Pauli cited here are from the Victor Frederick Weisskopf Papers, MC 572, Series 3, box 1. Massachusetts Institute of Technology, Institute Archives and Special Collections, Cambridge, Massachusetts. The letters dated August 2 and August 29 1933 are not published in (Pauli 1985). Translations of these letters are my own.

[7]I have not located Weisskopf's letter to Pauli, but I infer this from Pauli's response.

[8]Note that the term used by Pauli here is distinct from that used by Schrödinger (Verschränkung) in his discussion of entanglement beginning in 1935 (Schrödinger 1935a, 1935b, 1936).

[9]This passage in Pauli's letter includes a word that is not clearly legible, and which I have rendered as "bzw." (abbreviation for *beziehungsweise*),

and translated as "or rather." As Don Howard has pointed out to me (personal correspondence), Pauli here appears to be referring to a connection between the production and entanglement of *virtual* electron-positron pairs as a result of vacuum fluctuations and the problems of the infinite self-energy and vacuum polarization in Dirac's theory. This is puzzling, because although the calculations of these divergences (particularly Weisskopf's calculations discussed below), do involve the occupied negative energy states of the hole theory, they do *not* involve entanglement of virtual electron/positron pairs and it is unclear just what importance Pauli attributes to this.

[10]Written in English: "I do not believe on [sic] your perception of "holes", even if the existence of the "antielectron" is proved."

[11]In this letter Pauli proposes that the positive electron might in fact carry integer spin and obey Bose statistics, and presents the incompatibility of this proposal with hole theory as a virtue: "Daß dieser Gesichtspunkt der Diracschen Löchertheorie entgegengesetzt ist, spricht nur *für* ihn."

[12]Pauli uses the moniker given to the work carried out by Born, Heisenberg, and Jordan that elaborated Heisenberg's first matrix mechanics paper into a full-blown theory (Born & Jordan 1925; Born, Heisenberg, & Jordan 1926).

[13]Heisenberg formulated a semiclassical approach, and claimed that it connected "*intuitive* conceptions of classical theory with those of wave mechanics" and yielded "in most cases, and without detours . . . the result expected in terms of the correspondence principle" (Heisenberg 1931, translated in Miller 1994).

[14]Ms. letter in Weisskopf collection, MIT Institute Archives, printed in full in Schweber 1994, 124–125.

[15]Even more than the subtraction methods of Dirac and Heisenberg, Weisskopf's discussion takes an important step in the direction of the renormalization techniques eventually adopted for ridding quantum electrodynamics of its divergent quantities. Weisskopf himself came eventually to view this paper as "really the beginning of renormalization" (Interview of V. F. Weisskopf, AHQP; see Miller 1994).

[16]That he should feel *guilt* about this seems to be connected to the fact that he considered his credit for having discovered the logarithmic divergence undeserved in light of Furry's correction of his original calculation. He also says in the interview that he wrote the 1939 paper "to make up for" his earlier error.

[17]Weisskopf does use a "sum of two terms" representation in a later review article, though for the self-energy problem in classical electrodynamics.

Even here, though, one does not find the language of fine-tuning or adjusting or choosing parameter values (Weisskopf 1949, 306).

[18]Smolin offers an alternative to standard inflationary multiverse cosmology in the form of his own multiverse theory, Cosmological Natural Selection (Smolin 1992, 1999), which I do not here undertake to discuss.

[19]Kyle Stanford has argued explanatory theories are generally subject to a problem of unconsidered alternatives because of our cognitive limitations that prevent us from being able to conceive of relevant alternatives, and offers this as an argument against scientific realism (Stanford 2006). My point here is more limited. I claim only that there are occasions when we have good reason to think that alternatives to a given theory are possible that we have not yet been able to think of, and that in those situations, we should not consider the success of that theory in solving problems (such as fine-tuning problems) to constitute significant support for it.