Conceptual Problems in Classical Electrodynamics[*]

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Abstract

In Frisch 2004 and 2005 I showed that the standard ways of modeling particle-field interactions in classical electrodynamics, which exclude the interactions of a particle with its own field, results in a formal inconsistency, and I argued that attempts to include the self-field lead to numerous conceptual problems. In this paper I respond to criticism of my account in Belot 2007 and Muller 2007. I concede that this inconsistency in itself is less telling than I suggested earlier but argue that existing solutions to the theory's foundational problems do not support the kind of traditional philosophical conception of scientific theorizing defended by Muller and Belot.

1. Introduction

A fundamental problem in classical electrodynamics (CED) is how to incorporate the interaction of a charged particle with its own electromagnetic field into the theory. The standard equations used to model particle-field interactions simply ignore self-interactions. This results in a formal inconsistency, as I show in Frisch 2004 and Frisch 2005. While there also exist numerous proposals for including self-interactions, these proposals either crucially rely on approximations or are otherwise conceptually problematic. I discuss several such proposals in Frisch 2005, arguing that the manner in which foundational problems are treated in CED has implications for our philosophical understanding of scientific theorizing.

My account is criticized by Gordon Belot (2007), Fred Muller (2007), and also by Peter Vickers (forthcoming). Muller argues that my argument for the inconsistency of the standard modeling assumptions is flawed and claims that any putative problems of the theory have been solved. Belot and Vickers agree with me that the assumptions at issue are indeed inconsistent but question the philosophical conclusions I want to draw from this fact. In this paper I respond to Muller's and Belot's criticisms, beginning with a few remarks concerning the issue of inconsistency.[1] Contrary to Muller's claim, the argument I presented is valid, yet I am inclined the agree with my critics that this inconsistency in itself is less telling than my previous discussions may have suggested. I then briefly rehearse some of the theory's conceptual problems and argue that while there are indeed solutions to these problems, they offer no solace to defenders of traditional philosophical conceptions of scientific theorizing.

2. Inconsistency

In Frisch 2004 and Frisch 2005 I explained that the models physicists use to represent classical interactions between discrete charged particles and electromagnetic fields fall into two classes—(1) models in which the trajectory of a charge or current configuration is assumed as given and the fields produced by the charges and currents are calculated (Muller calls these "A problems" (262)); and (2) models where external fields are given, and the motions of charges in the external fields are calculated (Muller's "B problems"). Crucially, models of the second kind treat charged particles as being influenced by *external* fields alone, even though, according to models of the first kind, each charge itself also contributes to the total field. That is to say, any effect that the field produced by a charge may have on the motion of the charge itself is ignored. I showed that ignoring the so-called 'self-fields' in the charge's equation of motion results in a formal inconsistency: the equation of motion for discrete charges *without* self-fields—what we may call 'the external Lorentz-force equation of motion'—is inconsistent with the Maxwell equations and the standard principle of energy-momentum conservation (which together imply that accelerated charges radiate energy).[2] Contrary to Muller's somewhat tortured reconstruction of my argument, the argument *begins* with the assumption that the only electromagnetic force acting on a charged particle is the force due to the *external* fields (which is the assumption made in all applications of classical electrodynamics), and under this assumption the argument I presented is valid.[3]

What ought we to conclude from the fact that the assumptions made in modeling A- and B-problems are inconsistent? Belot (2007) argues that this inconsistency is of less philosophical relevance than I have made it out to be, since it is only an instance of the widespread and well-known phenomenon of the use of idealizing assumptions that are strictly-speaking inconsistent with an underlying fully consistent theory that includes self-interactions.[4] Yet in my earlier discussions I took the inconsistency of the standard modeling assumptions paired with their empirical successfulness to be telling precisely because, as I argued, there appears to be no classical treatment of self-interactions that is both exact and conceptually entirely unproblematic. That is, approximations in CED do not appear to be approximations to an underlying 'well-behaved' and exact classical theory. My ultimate aim was to argue that a range of formal philosophical conceptions of scientific theories are misguided. Much of scientific theorizing that is interesting, I argued, does not fit well into the formal straight-jackets of the philosophers' design—be it one that construes theories syntactically as a deductively closed set of sentences in some formal language or one that reconstructs theories in terms of set-theoretic structures.

Indeed, Muller's own account, in which he endorses a reconstruction of classical electrodynamics as a set of set-theoretic models that obey the postulates of CED, provides evidence for the dangers associated with such philosophical reconstructions. For when Muller explains how solutions to 'A'-and 'B'-problems are meant to fit into his formal framework, he invites the very confusion which my discussion was meant to help avoid. He says: "Let *AB* be called the class of CED-models that solve A- or B-Problems […]. Then models in *AB* neglect self-effects." (Muller 2007, 263) Yet structures that *ignore* self-effects are in general not members of the class of structures that obey the postulates of CED, as Muller presents them, since one of the postulates of his reconstruction of the theory is a particle equation of motion that *includes* the self-force acting on each charge. The structures that "solve A- or B-problems" are models in some sense—they are the structures physicists use to *represent* certain physical systems and hence are what I call "representational models"—but they are not members of the class of *set-theoretic* CED-models satisfying the Lorentz-force equation of motion for the total fields.[5]

While I still take my ultimate conclusion to be correct that CED fits ill with traditional philosophical accounts of theorizing, I am now inclined to agree with my critics that it may have been a mistake to place the inconsistency of the standard modeling assumption at the center of my discussion.[6] This way of framing the discussion directed attention away from what is arguably the philosophically more interesting issue: the fact that a host of conceptual problems arises when one tries to develop a classical theory of charged particles interacting with electromagnetic fields in a way that includes self-interaction effects; and it is this issue to which I want to turn next.

3. Conceptual Problems

The aim of "theories of the electron''—i.e., theories of microscopic charged particles with self-interactions—as Arthur Yaghjian puts it in his monograph, is "to determine an equation of motion for […] the electron that is consistent with causal solutions to the Maxwell-Lorentz equations, the relativistic generalization of Newton's second law of motion, and Einstein's mass-energy relation." (Yaghjian 2006, 1) That is, we begin with an assumption about the background spacetime in which particles and fields live—the relativistic assumption that Muller calls the "Space Time Postulate"—dynamical laws governing the propagation of particles and fields, and conceptual constraints on acceptable solutions, such as causal assumptions or the principle of energy-momentum conservation. We then try to find a model of a discrete charged particle that results in an equation of motion for the particle satisfying these assumptions as much as possible.

In Frisch 2005 I survey a range of such theories of self-interactions. Muller 2007 covers much the same ground, adding some additional details.[7] Muller concludes that the theory's conceptual problems "have been solved at various levels of sophistication and rigor" (275) but emphasizes that in all these solutions "approximations and idealizations are mandatory" (263) and continues: "Small wonder there is not a *single* account of self-effects available but there is a multitude of accounts, each of which rely on different approximations and different idealizations." (263, italics in original) I fully agree with this characterization. "A majority of the exact equality signs (=) in most physics papers, articles and books," Muller stresses, "means approximate equality (≈)." (261) One of the aims of my own discussion of different accounts of self-energy effects was to argue that existing solutions are at best that—approximations. But approximations to what? *Pace* Belot, it is unclear that there is an exact, fully satisfactory classical theory—a "full theory of classical electrodynamics"—lurking in the background to which the many ingenious solutions can be considered as approximations.

According to the traditional philosophers' view, a successful theory, which can be formalized either syntactically or set-theoretically, provides us, at least in principles, with models or exact solutions for all the phenomena in its domain. Hence, a successful classical electrodynamics ought to be able to tell us a consistent, complete, and exact 'in principle' story of the detailed mutual interactions between charges and fields. However, CED does not appear to provide us with such a story.[8]

Here is another way to express the point I tried to make. In his recent book Mark Wilson argues for the prevalence of what he calls "theory facades" in classical physics, which contain "weak spots". Classical physical theories, Wilson maintains, do not easily submit to formal axiomatization and instead should be thought of as "sets of linked, but nonetheless disjoint, patches" which he calls *facades* (2006, 179). The theoretical treatments of A- and B-problems, I submit, constitute two such patches of a theory facade, while the interaction of a charge with its own field constitutes a weak spot of the theory that cannot readily be covered by extensions of the two patches. That there exist solutions to the self-energy problem in various approximation regimes attests to the fact that the patches are linked or 'stitched together' and, indeed, that formal mathematical rationales can be given for how the patches are tied together, but despite their undeniable mathematical rigor these solutions do not support the axiomatizers' dream of a theory that unproblematically and exactly covers the entire domain of classical electromagnetic interactions.

Now, one way in which one might try to defend a traditional account of theories in light of the phenomenon that Wilson describes is to 'balkanize' the theoretical domain at issue. Thus, Belot argues that if the equations used in modeling A- and B- problems could not be understood as approximations to a fully consistent underlying classical theory, then we should think of the equations governing the two kinds of problems—the Maxwell equations on the one hand, and the external Lorentz force equation of motion, on the other—as part of two distinct well-behaved and axiomatizable classical theories. Each theory, on this proposal, governs a distinct 'patch' of phenomena. Belot concedes that we need to add principles to our two theories that tell us only to look for solutions to each theory on its own (to prevent logical mayhem), but he maintains that the addition of such principles constitutes nothing novel, since they merely serve to restrict the domain of the theories' applicability:

> We can get away with thinking of each of these theories as determining its class of models in the usual way—i.e., as generating the set of solutions to the equation of the theory. We will of course need further principles that demarcate the domain of applicability of each theory—but *every* non-fundamental theory involves such principles. (Belot 2007, 279-80, italics in original)

I have three worries about this defense of a traditional account of theories. First, we need to be careful about what we mean by a theory's *domain*. According to the traditional picture, specifying the domain of a theory is a fairly innocuous addition to the formalism: a domain simply constitutes the class of objects over which the theory ranges. In the case of non-fundamental theories, domain restrictions may also include restrictions to certain length- or energy-scales. Understood in that way, however, the domains of Belot's two theories are identical: both theories have classical microscopic charged particles and electromagnetic fields as their objects. The only difference between the two theories' 'domains' of application consists in the different *aspects* or *contexts* of the interactions between particles and fields with which the two theories are concerned. Hence, the further principles needed are more 'weighty' than a traditional philosophical accounts suggest.

Second, when we try to save the traditional conception by carving up recalcitrant theories into distinct formalizable sub-theories, we lose any account of possible mathematical and conceptual relations between the sub-theories. In the present case the two sub-theories are 'stitched together,' for example, via the consistent theory for continuous charge distributions and appeals to the principle of energy-momentum conservation (see Frisch 2005, 50-1). Thus, we can learn from the Maxwell equations and the principle standard of energy conservation that using the external field formulation of the Lorentz law should be empirically acceptable in the domain of classical particle-field interactions, since the error made in ignoring self-effects is very small. This line of reasoning requires that we apply Maxwell's theory to phenomena that according to the present proposal are part of the domain of a different theory.

Third, not in all applications are the two 'sub-theories' applied to distinct sets of phenomena. There are contexts in which we are interested *both* in the effect of the fields on the motion of a charge *and* in the fields produced by that charge and in these contexts we need to appeal to the full resources of both sub-theories. Models of synchrotron radiation are one example where we feed the results obtained from one sub-theory as input into the other (and hence need to be careful about the relations between the different steps in our calculation). Other examples are models of self-interactions. All three worries can presumably be met by invoking substantive interpretive principles governing the applications of the two theories, but the more we need to rely on such additional assumptions, the less our account will still resemble the traditional philosopher's axiomatic reconstruction.

As Muller characterizes the situation, I have claimed that the self-energy problem "[defies] resolution," (253) while he has argued that the problem has in fact been solved. But it is important to realize that the question whether or not the self-energy problem has been solved has no unequivocal answer. Whether we take a solution to be successful depends crucially on the purpose it is meant to serve and on what we take the conditions of adequacy for solutions to be. The philosophers' view of theories requires that a solution be part of an axiomatizable account covering the phenomenon at issue and that there be an in principle exact solution backing up any approximation we make. On a view such as Wilson's or my own, a solution can be successful, even if it relies essentially on approximations. But not only do the traditional philosophers' conception of scientific theories and conceptions that are arguably closer to scientific practice disagree on the appropriate conditions of adequacy, physicists also disagree amongst themselves on the merits of certain solutions—disagreements that appear to be due to differences in how much weight is assigned to various conceptual constraints on classical theories.

Thus, on the one hand some physicists think that ultimately none of the solutions to the self-energy problem are fully satisfactory. Richard Feynman, for example, said:

> This tremendous edifice, which is such a beautiful success in explaining so many phenomena, ultimately falls on its face. When you follow any of our physics too far, you find that you always get into some kind of trouble. Now we want to discuss a serious trouble—the failure of classical electromagnetic

> theory. You can appreciate that there is a failure of all classical theory because of the quantum-mechanical effects. Classical mechanics is a mathematically consistent theory, it just doesn't agree with experience. It is interesting, though, that the classical theory of electromagnetism is an unsatisfactory theory all by itself. (Feynman 1964, 28-1)

And in discussing alternative force laws of the kind Muller cites as solutions to the theory's conceptual problems Feynman says "many attempts have been made, […] but all these theories have died. It is still interesting to discuss some of the possibilities that have been suggested—to see the struggles of the human mind." (*Ibid.*, 28-6)[9] This skeptical view is echoed by J. D. Jackson, who in the most recent edition of his well-known textbook says that 'although partial solutions [to the problem of self-energy effects in CED], workable within limited areas, can be given, the basic problem remains unsolved." (Jackson 1999, 745)[10] On the other hand there are those who believe that, from the physicists' perspective, the theory's problems have been solved. In the most recent edition of his classic text Fritz Rohrlich claims that the remaining difficulties for solutions to the self-energy problem have finally been overcome and that we now know the "physically correct dynamics" of a classical charged particle (Rohrlich 2007, vii).

Here is not the place for another extended survey of treatments of self-energy effects, nevertheless I want to make some brief remarks to support the view I sketched above. Broadly, models of particle-field interactions fall into two classes, depending on whether the theories treat charged particles as point particles or as extended particles. A first problem for point-particle theories is that the electromagnetic field of a finitely-charged point particle is infinite at the location of the charge. As I discuss in Frisch 2005 (59-63), there is a well-known strategy for handling this particular problem even within the point-particle framework. The core idea is to 'renormalize' the mass of the charged particle—that is, to treat part of the diverging self-field of the charge as contributing to the mass of the particle, which nevertheless can be made finite overall by positing a negative and equally diverging non-electromagnetic 'bare mass.'

The procedure of mass renormalization provides us with a nice example of how different standards of success can come apart. While the procedure is mathematically well-defined and arguably even provides us with an exact solution to the self-energy problem, solutions to the resulting equation of motion are generally viewed by physicists to be conceptually deeply problematic and in addition do not fit into formal philosophical reconstructions of theories, such as the set-theoretic account favored by Muller.

Muller argues that while it may initially appear that "*point-particles fall outside the domain of* CED" (267, italics in original), the renormalization program shows that "models of point particles fit within CED," (268) but this claim, once more, trades on an ambiguity in the notion of *model*. Since the bare mass is divergent, solutions to the renormalized equation of motion—the so-called *Lorentz-Dirac equation*—are *not* members of the set of set-theoretic structures that, according to Muller's reconstruction, constitute CED. And this is so, even though the Lorentz-Dirac equation can be reasonably rigorously derived from basic principles of CED—the Maxwell-Lorentz equations and the principle of energy-momentum conservation. According to Muller's reconstruction, the set-theoretic models of CED are tuples which have as a member the mass m of the charged particle, such that $m \in \Re$ and m is constrained to satisfy the relativistic generalization of Newton's second law (Muller 2007, 255). But there is no mass m in the renormalization program that satisfies these constraints. For the empirically observable renormalized mass m_{ren} it is the case that $m_{ren} \in \Re$, but m_{ren} satisfies the Lorentz-Dirac equation and not Newton's second law; and even though the bare mass m_0 may be thought to satisfy Newton's law by stipulation, there is no $m_0 \in \Re$ such that $m_{ren} = m_0 + m_{em}$ is finite (where m_{em} is the divergent electromagnetic mass of the point charge). Thus, while the renormalization program shows how a well-defined point-particle equation of motion can be derived from the Maxwell equations and energy-momentum conservation through an extension of the Newtonian concept of mass, the program does not show that "models of point particles fit within CED" in the sense required by the Muller's set-theoretic reconstruction.

Physicists have worries about the renormalized equation of motion different from the preceding one. Herbert Spohn, for example, says that "while the mere mathematical operation [of mass renormalization] is admissible, it would result in a theory with very little physical content." (Spohn 2004, 146) By far the most frequently expressed worry about the Lorentz-Dirac equation is that it permits acausal solutions which either allow a free charge to accelerate, even though it never experiences any external force, or predict that a charge's acceleration at t depends on the external forces applied to the charge at all times later than t (see Frisch 2005, 59-63). The first problem can be solved, but the second problem—the problem of *pre-acceleration*—remains.[11] A further worry about the Lorentz-Dirac equation is that there are no general existence and uniqueness proofs for systems consisting of two or more charged particles, and the two-particle systems that have been studied exhibit rather problematic behavior (see Parrott 1987, sec. 5.5).

Rohrlich, as we have seen, argues that, from a physicist's perspective, the conceptual problems besetting the renormalization program have been solved. He reports that if we introduce a cutoff into the theory for small length scales, we can replace the problematic Lorentz-Dirac equation with a 'regularized' equation of motion that exhibit none of the conceptually problematic behavior of the Lorentz-Dirac equation itself.[12] The procedure is justified empirically by arguing that the classical theory becomes empirically inadequate at very small distances anyway. This is an instance where approximations play a crucial role. Even though the acausal and temporally non-local behavior of the *exact* point particle equation is viewed as problematic, the renormalization program's procedure of extending the resources of classical electrodynamics to account for the charge's self-interactions is ultimately judged to be successful, since there exists a formally and empirically well-motivated *approximation* to the exact equation that is well-behaved in ways in which the exact equation is not. While this verdict makes little sense on the traditional philosophers' conception of theories, it fits well with a view that allows for physical theories to have weak spots that can be treated rigorously yet not exactly with the theory's resources.

The second approach to the self-energy problem models charged particles as extended particles. This was the dominant approach in the early twentieth century and recently once again appears to have become the main focus of research on classical theories of the electron. Extended-particle models provide an example of a phenomenon that Wilson calls "foundational looping" (Wilson 2006, 195) and which occurs when a putatively more fundamental, lower-level theory or model is in turn accounted for with the help of the resources of the higher-level theory. Microscopic electrodynamics allows us to explain the bulk properties of a macroscopic medium—such as the electric permittivity ε and the magnetic permeability μ—in terms of the interaction between microscopic elementary charged particles and microscopic fields propagating in vacuum. The macro-properties are derived from the microscopic theory by modeling charges as structureless point particles and averaging over the charge density (see Jackson 1999, 6.6). Extended-particle models, however, treat the 'elementary' electron much like a miniature version of the macroscopic objects whose behavior it is meant to explain. Yaghjian, for example, models the electron as a spherical insulator with a uniformly distributed surface charge *e* (2006, 2) and, thus, needs to address the question as to what the permittivity and permeability of the insulator are. That is, the putatively reducible macro-properties have reappeared on the micro-level.[13]

The extended-particle model that is best understood—the Abraham model—treats charges as rigid and spherically symmetric in a preferred reference frame—the laboratory frame. The repulsive forces between different charge elements are taken to be counteracted by not-further-specified cohesive forces. For this model there are general existence and uniqueness proofs for the coupled system of the Maxwell-Lorentz equations (see Spohn 2004) and it is this model that Belot cites to support his claim that there is a consistent classical theory of discrete charged particles. Solutions to the equations have been explored in detail in the approximation regime of slowly varying fields and in this regime the model exhibits none of the troubling behavior of the Lorentz-Dirac particle. A problem for all extended-particle models, however, is that extended particles do not sit well with the special theory of relativity. The Abraham model requires a privileged reference frame and is inconsistent with what Muller calls the relativistic "Space-Time Postulate" of CED, according to which the only structure of spacetime is that of Minkowski space (Muller 2007, 254; see also Frisch 2005, 55-7).

Even models that treat charged particles as so-called *relativistically rigid* objects—that is objects, that always have the same shape in their instantaneous rest frames—are not *fully* relativistic since they allow for superluminal propagation. (A fully relativistic extended particle would be dented when it experiences an external forces and would have to have vibrational degrees of freedom.) Also, there are to date no global existence and uniqueness proofs for solutions to the particle-field equations for relativistically rigid charges and there are considerations that suggest that such a proof may not be possible. (See Spohn 2004, ch. 5, especially p. 30) Thus, the extension program, too, offers at best approximate solutions to the problem of arriving at a fully relativistic classical particle-field theory.[14]

4. Conclusion

One lesson we can learn from the history of classical particle-field theories is that there is a tension between the notion of discrete particle and relativistic field theories. On the one hand, structureless point particles fit more naturally into a relativistic theory than extended objects, but they result in infinities, which can be 'tamed' only with difficulty, and lead to other conceptual problems that can be resolved at most in some approximation regime. On the other hand, no extended-particle model that has been explored in any detail is fully relativistic. To the extent, then, that the self-field problem has been solved, it has been solved with the help of approximating assumptions—by telling what are, by the theory's own lights, only 'sort of' or 'as if' accounts of what goes on in the detailed interaction between charges and fields.

I am not sure that either Muller or Belot would disagree with this characterization. Belot concedes that the fact that we do not know how to reconcile extended particles with Lorentz-invariance provides good reasons for preferring point-particle models (2007, 276), yet he also says that the point-particle model "leads to various well-known difficulties" (2007, 266; see also 267). While Muller insists that despite these difficulties the self-energy problem has been solved, he emphasizes that "approximations are mandatory" in these solutions. But then Belot and Muller ought also to agree with the following conclusion: No matter how successful the solutions "at various levels of sophistication and rigor" may be from a physicists' perspective, they offer no support to the traditional philosophers' conception of successful scientific theories as axiomatizable accounts that provide us with fully coherent and complete world pictures.

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FOOTNOTES

[1] Vickers forthcoming is rather more sympathetic to my original account.

[2] In contrast to what Muller claims in a note added at the end of his paper, it is not the case that "the self-field [is] ignored always and everywhere" (276) in standard treatments of particle-field interactions. The inconsistency arises precisely because the field of a charge is ignored when its motion in an external field is calculated, but the field of the charge is *not* ignored, when we calculate the field produced by a given charge and current configuration.

[3] Muller's reconstruction of my argument has as a premise that momentum change is equal to the *total* field-force acting on a charge, *including* the self-force, that is: $d\mathbf{p}/dt=q(\mathbf{E}_{TOTAL} + \mathbf{v} \times \mathbf{B}_{TOTAL})$. But here is what I assume in Frisch 2004 (italics in original): "the momentum change of the charge is equal to the total *external* force acting on it. According to the Lorentz force law, the electromagnetic force on a charged particle is given by $\mathbf{F}_{Lorentz}=q(\mathbf{E}_{ext} + \mathbf{v} \times \mathbf{B}_{ext})$. In the absence of non-electromagnetic forces, the equation of motion for a charged particle thus is $d\mathbf{p}/dt=\mathbf{F}_{Lorentz}$"

[4] Vickers forthcoming also questions the conclusions I wish to draw from the inconsistency, arguing for what he takes to be a middle ground between Belot's and my views.

[5] I discuss different notions of *model* in philosophical accounts of scientific theorizing in 2005, chapter 1.

[6] This point has also been pressed on me by Mark Wilson and Bob Batterman in comments on my book in a symposium at the 2006 Pacific APA. I want to thank both of them for their insightful comments and criticisms.

[7] In light of the fact that I discuss in some detail most of the solutions mentioned by him it is rather surprising that Muller suggests that I have "ignored" them. At the end of his survey he says: "Frisch *also* ignores A, Grünbaum's penetrating analyses of [the problem of backward causation in the Lorentz-Dirac theory]." (275, my italics) But I ignore neither Grünbaum's analyses (see Frisch 2005, 85-88) nor most of the other accounts surveyed. To be sure, I disagree with Grünbaum's conclusions. But, to quote Muller, offering a detailed account of one's disagreement with a certain view *"is not quite the same thing as there being no account at all.*" (263, italics in original)

[8] What is at issue here is not whether the theory is empirically exact, i.e. true. Rather, the issue is whether the theory can provide an account, at least in principle, that does not contain any approximations to the theory's basic principles—that is, an account that is exact as judged by the theory's own lights.

[9] Feynman also says of his own contribution to the problem—the Wheeler-Feynman theory—"You see what tight knots people have gotten into in trying to get a theory of the electron" and says his survey is meant "to show the kind of things people think of when they are stuck." (28-8) I agree with Muller that the problems Feynman is concerned with are not all best thought of as inconsistencies, but, contrary to Muller's claim (see 2007, 267), it is

obvious from Feynman's discussion that he has worries both about point-particle theories and about theories of extended charges.

[10] Contrary to Muller's reading of this passage (2007, 262), Jackson is not here talking about the *empirical* problems any classical theory faces in the domain of the very small. Like Feynman, Jackson maintains that there is no theoretically or conceptually fully satisfying classical solution to the problem of radiation reaction.

[11] Muller claims that Yaghjian 1992 has solved the problem of pre-acceleration. But as I show in Frisch 2005, Yaghjian's argument is not valid. Yaghjian argues that analyticity ensures *future determinism* but this does nothing to address the worry that the present acceleration of a point-particle appears to be *caused by* later forces—that is, that the theory is *backward causal*.

[12] See Rohrlich 2007, 257-262 and references therein. I discuss this approach in Frisch 2005 (63-6).

[13] Feynman's complaint that once we model electrons as extended objects "the beauty of the whole idea begins to disappear" (1964, 28-5) may well reflect his unease with this kind of foundational looping. As he points out, models of electrons as extended particles held together by internal forces raise the same kind of questions arising for macroscopic objects: "How strong are the stresses [holding it together]? How does the electron shake? Does it oscillate? What are all its internal properties?"

[14] What, then, are the members of the set of set-theoretic structures that obey the postulates of CED as specified by Muller? Clearly, the theory has models for continuous charge distributions. It is less clear, whether there are any non-trivial set-theoretic models for discrete finitely charged particles: The class of structures that solve 'A'- and 'B'- problems ignore self-interactions and, hence solutions to B-problems satisfy the external-field equation of motion and not the total-field equation; point-particle models do not involve a finite mass *m* that also satisfies Newton's laws; and extended-particle models for which there exist existence proofs violate the Space-Time postulate.