“When you follow any of our physics too far
you find that you always get into
some kind of trouble.”
Richard Feynman

Discussion Note: Conceptual Problems in Classical Electrodynamics

In (Frisch 2004) I argued that the main approach to modeling classical particle field interactions is formally inconsistent and maintained that attempts to construct a fully consistent particle-field theory face a host of serious and not fully resolved conceptual problems. Muller criticizes my paper, arguing that classical electrodynamics (CED) is consistent and that it is a mistake to think of the theory as being conceptually problematic (Muller forthcoming). Any putative problems of the theory, Muller maintains, have been successfully handled. In this discussion note I want to advance the debate, first, by pointing out that on both issues there is considerably more agreement between Muller’s position and mine than is apparent from his criticism, and, second, by arguing that both Muller’s and my own earlier characterization of the issues are misleading in important ways and by suggesting what hopefully is a more fruitful way of thinking about these issues. I will begin with a few remarks concerning the question of inconsistency and will then turn to a discussion of some of the theory’s other conceptual problems.

1. Inconsistency

Is classical electrodynamics inconsistent? If this question is equivalent to the question whether the Maxwell-Lorentz equations have models, then the answer is clearly and unambiguously ‘*No.*’ As I pointed out in (Frisch 2004) and discuss in more detail in (Frisch 2005), the Maxwell equations and the Lorentz force equation of motion have models—models for continuous charge distributions.[1] Moreover, this theory is fully relativistic and one can prove that for continuous charge distributions CED satisfies energy and momentum conservation, if and only if one adopts the standard expression for the energy-momentum tensor of the electromagnetic field (see section 2.1., *The Consistency Proof*, in Frisch 2005).

But have I not claimed to have derived the inconsistency of CED? Not quite. What I have argued is that the standard way of modeling phenomena involving the interaction between discrete charged particles and electromagnetic fields relies on inconsistent assumptions. I explained (and Muller reiterates this point in his paper) that the models physicists use to represent phenomena involving discrete charged particles, such as electrons, fall into two classes—(1) models in which the trajectory of a charge is assumed as given and the fields produced by the charges are calculated; and (2) models where external fields are specified, and the motions of the charges in the external are calculated. (Both Muller and I got this point from Jackson's well-known textbook (Jackson 1975)). Since models of the second kind ignore the effect of the 'self-fields' of a charge on the motion of that charge—fields that ought to be present according to the first kind of models—the two kinds of model rely on what strictly speaking are inconsistent assumptions. In my inconsistency proof I showed that the equation of motion for discrete charges that is used in all applications of classical electrodynamics, which ignores the self-fields of the charge, is inconsistent with the Maxwell equations and the standard principle of energy momentum conservation. Thus, contrary to Muller's reconstruction, my proof ***begins*** with the assumption that the only force acting on a charged particle is the force due to the ***external*** field and under this assumption the argument I presented is valid.

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[1] Even a theory of continuous charge distribution is not problem-free: Standard existence and uniqueness proofs guarantee only local solutions to the field equations, and there are intuitively plausible initial charge distributions for which the Maxwell-Lorentz equations have no global solutions (Frisch 2005).

The only real disagreement between Muller and me, as far as the inconsistency argument is concerned, turns out to be simply this. I assert that the equation of motion that states that momentum change is equal to the ***external*** force acting on the charge is a ***Newtonian*** equation of motion. Muller claims that this understanding of Newton's law "is simply in error" and that Newton's law equates momentum change to the ***total*** force acting on an object.

Now, there are reasons for thinking that my understanding of the content of the Second Law is closer to Newton's own. In the *Principia* Newton distinguishes "inherent" forces from "impressed" forces. The former are passive: inherent forces are responsible for a body's satisfying the principle of inertia but do not result in changes to a body's acceleration. Only the latter, external forces, are associated with momentum change. That is, Newton did not allow for internal forces that could alter an object's momentum and equates momentum change with external forces. Contemporary usage, however, does not appear to be uniform and physicists sometimes appear to take Newton's Second Law to state that momentum change is equal to the total force. At this point, however, our dispute strikes me as not much more than a terminological quibble. The real issue is that the equation of motion that is generally used in modeling the motion of a charged particle in an external field ignores self-interactions, and not whether the equation is an instance of the Second Law.

While the standard way of modeling particle field interactions does indeed rely on an inconsistent set of assumptions, I believe that it was a mistake for me to place this point at the center of my discussion. By framing the discussion in terms of the inconsistency of the standard modeling assumptions, I directed attention away from what is arguably the much more interesting issue: the fact that there are a host of conceptual problems that arise when one tries to develop a fully coherent and complete classical theory of charged particles interacting with electromagnetic fields—a theory that does not simply ignore self-interaction effects—and it is this issue to which I want to turn next.

2. Conceptual Problems

In the second half of his paper Muller presents what he calls a "birds eye view" of the history of classical particle-field theories in support of his thesis that "physicists have handled [various conceptual problems] and are handling them *wholly within* CED." (Muller forthcoming) The list of theories or theoretical proposals Muller discusses to a large extent mirrors the list of proposals that I discuss in my book. How is it that the lessons we draw from the same theoretical developments appear to be so much at odds with each other? As I see it, there are two sources of disagreement. First, we disagree on how to interpret some of the technical details at issue. Some of the things Muller says overstates how successful putative solutions to theoretical problems in CED in fact are. Second, and more importantly, there is a difference in philosophical perspective. Once this difference in perspective is put into focus, it becomes evident that there is considerably less disagreement between Muller and me than it may initially appear.

My discussions in (Frisch 2004, 2005) had as their target a certain philosopher's conception of scientific theorizing—the conception that Mark Wilson has derided as the "theory T approach"—according to which any successful theory provides us with a complete account of every aspect of the phenomena in its domain. My aim was to show that classical electrodynamics does not fit this conception, since the resources of the theory alone are not sufficient to tell a fully plausible, consistent, and complete story of the detailed mutual interactions between charges and fields. But these problems do not stand in the way of the theory's being extremely successful. We can successfully represent particle-field interactions in ways that gloss over certain aspects of the complicated physics governing these interactions, even though the problem of characterizing these interactions in the vicinity of a charge present what we may call a “weak spot” of CED.

The aim in classical electrodynamics has been to arrive at a conceptually coherent classical theory describing the interaction between localized charged particles and the electromagnetic field. Since the Maxwell equations can be written in Lorentz-invariant form, the aim is to arrive at a fully relativistic theory—a theory that is classical only in that it is a non-quantum theory. That is, relativistic constraints are among the theory’s conditions of adequacy, along with other constraints such as the principle of energy-momentum conservation and a prohibition against backward causation. Yet attempts to arrive at a fully satisfactory classical theory satisfying all our conditions of adequacy have encountered several problems, among them the three problems on which Muller focuses: (i) the electro-magnetic field of a finitely-charged point particle is infinite at the location of the charge; (ii) certain particle equations of motion predict that a free charge can accelerate without experiencing any external force (“self-acceleration”); (iii) these equations also predict that a charge accelerates before the onset of an external force (“pre-acceleration”).

On the philosophers’ conception of theories, a theory that can successfully solve these problems would have to delineate coherent possible worlds that not only are free of self- and pre-acceleration but also would have to satisfy all other conditions of adequacy, such as energy-momentum conservation exactly. While it may be the case that a theory’s models represent the phenomena only approximately—that is, that the theory is only approximately true—a successful theory has to present us with complete possible worlds in which the theory’s laws and conditions of adequacy are satisfied exactly. (On that conception, the particle-equation of motion without self-fields, which I examined in my 'inconsistency proof,' founders precisely because it does not allow for exact energy-momentum conservation.) Now, Muller stresses that physicists frequently take recourse to various approximating assumptions in treating physical problems. In fact, "a majority of the exact equality signs in most physics papers, articles, and books," Muller says, "means approximate equality." It should be obvious that I fully agree with the sentiment expressed in this claim. But the cluster of philosophical views that was the target of my discussion requires more than however well-motivated approximations.

Is it, then, true that there is a classical theory which enables us to "handle" the list of putative conceptual problems "wholly within CED," as Muller claims (Muller forthcoming)? Contrary to what Muller suggests, this question has no unequivocal answer. Whether a solution is successful depends crucially on the purpose it is meant to serve and on what we take the conditions of adequacy on a solution to be. While the philosophers' conception of theories that I criticized requires *exact* solutions, Muller's remark emphasizes that physicists often are content with only *approximate* solutions. But even among physicists we find widespread disagreement over whether certain solutions do in fact successfully handle the conceptual problems of CED—disagreement that may be due to differences in how much weight different physicists assign to various conceptual constraints on classical theories. Indeed, there are experts who take CED ultimately to be deeply problematic despite the many interesting attempts to overcome the theory's problems. Thus, Richard Feynman said of the theory:

> 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. […] 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—the kind of proposals Muller cites as solutions of 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) This pessimistic view is echoed by Jackson in his well-known textbook, where he says that "a completely satisfactory treatment of the reactive effects of radiation does not exist." (Jackson 1975, 781)

Let us now take a brief look at some of the details of the solutions surveyed by Muller and ask whether and in what sense they might solve the theory's conceptual problems. Broadly, attempts at constructing relativistic particle-field theories fall into two classes, depending on whether the theories treat charged particles as point particles or as extended charges. A first problem with point-particle theories is that the electro-magnetic field of a finitely-charged point particle is infinite at the location of the charge—problem (i) above. As I discuss and Muller reiterates, there is a well-known strategy for handling this particular problem even within a point-particle framework. The core idea is to 'renormalize' the mass of the charged particle—that is to treat part of the infinite self-field of the charge as contributing to the mass of the particle, which is nevertheless taken to be finite overall. Does this result in a successful solution to the problem of infinities? Muller believes the answer is 'yes' and claims that the discovery of the neutron—an electrically neutral massive particle—makes it plausible to assume that electrons, too, have a non-electromagnetic bare mass. But there are good reasons to be less optimistic than Muller is, since the existence of neutrons raises another conceptual problem for the renormalization program: if there exist both charged and neutral point particles, we need to postulate two radically different kinds of bare, non-electromagnetic mass: the *positive* and *finite* bare mass of neutrons and the (rather conveniently) *negative* and *infinite* bare mass of electrons and other charged particles.

There are additional problems. The resulting renormalized equation of motion, the so-called *Lorentz-Dirac equation*, permits solutions according to which a free charge accelerates even though it never experiences any external force and solutions according to which a charge begins to accelerate even before the onset of an external force. These are the self-acceleration problem and the pre-acceleration problems mentioned above. Muller claims that *both* these problems can be solved by imposing certain boundary conditions at infinity and rewriting the differential Lorentz-Dirac equation in the form of an integro-differential equation. This claim is false. It is true that once one demands that the acceleration of a charge vanish asymptotically at infinity, there is no *self-acceleration*, but the problem of *pre-acceleration* remains.

(Curiously, Muller cites (Rohrlich 1990) in support of his claim, even though Rohrlich explicitly discusses how the integro-differential equation of motion violates causality, in the sense that it allows charged particles to accelerate before the onset of any external force. “The most interesting feature” of this equation, according to Rohrlich, “is its nonlocality in time” (Rohrlich 1990, 150), which consists in the fact that the acceleration at a time *t* is determined by the effective force at *later* times: “Thus, it appears as though the force at time [*t*’] had sent a signal along the world line (backward in time), telling the particle that it will act on it at a later time” (*Ibid.*, 151).)[2]

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[2] Muller also refers to the discussion in (Yaghjian 1992), who does indeed claim to have solved the problem of pre-acceleration. But as I show in (Frisch 2005), Yaghjian’s argument is not valid. Yaghjian shows that it follows from the assumption that all functions at issue are *analytic*, that one can *predict* the future motion of a charge from the local value of the forces and accelerations and all their derivatives. But that analyticity gives us determinism on the cheep is a point that is well-familiar to philosophers since (Earman 1986). Yaghjian’s argument that analyticity ensures *future determinism* 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 caual*.

Thus, despite the fact that the renormalization procedure is mathematically well-defined one may with some justification worry about its conceptual and physical plausibility. And physicists have worried. For example, Herbert Spohn says:

> While the mere mathematical operation [of mass renormalization] is admissible, it would result in a theory with very little physical content. *The attempt to compensate through a proper adjustment of the bare mass fails*, since the electromagnetic mass merely adds to the bare mass. Thus, the bare mass necessarily becomes negative, which results in an unstable Hamiltonian. (Spohn 2004, 146, my emphasis)

Thus, some purported solutions to CED's conceptual problems are less successful than Muller's survey suggests, partly because the 'solutions' do not in fact solve the problems and partly because the solutions raise serious conceptual problems of their own.

But it is also evident from Muller's survey that the notion of "handling" or "solving" a conceptual problem that he has in mind is a rather weak notion—too weak to support the philosophers' conception of theories as delineating complete and coherent possible worlds. For example, Muller says that "a completely different way to solve" the problems of preacceleration and self-acceleration is to replace the radiation reaction term in the Lorentz-Dirac equation with a different term that does not give rise to the unwanted behavior. As an example of this strategy he cites the Mo-Papas particle equation of motion, which does not satisfy energy-momentum conservation, if the energy-momentum of the field is given by the standard expression. At most the Mo-Papas framework gives us approximate energy-momentum conservation. Evidently, then, on Muller's view a theoretical proposal can handle a problem successfully, even if it satisfies some of the theoretical conditions of adequacy only approximately. Therefore Muller's claim that the Mo-Papas equation solves (*in a weak sense*) the problems of pre- and self-acceleration does not conflict with my thesis that CED faces conceptual problems that appear to resist solution (*in a strong sense*). While the equation may be successful within some approximation regime, it has no non-trivial models—there are no ‘Mo-Papas possible worlds’—in which charged particles satisfy energy-momentum conservation and other theoretical conditions of adequacy exactly.[3]

The second class of approaches to particle-field interactions that Muller discusses treats charged particles as extended objects. A first problem faced by these approaches is that classical electrodynamics is incompatible with the existence of purely electromagnetic stable extended charges, since a purely electromagnetic extended charge would blow apart due to the repulsive forces among the different parts of the charge. In order for extended charges to be stable, there have to be other, non-electromagnetic cohesive forces. Thus, whatever else the virtues of a program postulating extended charges may be, any such program violates from the very outset a constraint on successful solutions endorsed by Muller himself, namely that it handle any conceptual problems “*wholly within* CED” (italics in original). For not only do charges on this picture have non-electromagnetic bare inertial mass (one sense of not remaining ‘wholly within CED’) but the theory also has no models that do not involve non-electromagnetic forces (a second, stronger sense of not solving the problems ‘wholly within CED’)

This, however, is a point at which I am inclined to be more permissive than Muller himself is. It seems to me that the very fact that extended particle models need to postulate non-electromagnetic forces does not in itself count against the model as providing a successful solution. For many physicists, at least, the worry appears to be less that an extended particle theory cannot treat particle-field interactions wholly within CED but rather that the electron posited by extended-particle models is a structurally rather complex object. Feynman, for

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[3] For a discussion of the Mo-Papas equation and other alternative equations of motion see (Parrott 1987). As Parrott shows, one can use the Mo-Papas equation to derive an alternative energy-momentum tensor for which energy-momentum is conserved. The problem with this non-standard tensor, however, is that—unlike the standard energy-momentum tensor—it apparently does not allow us to explain radiation effects successfully.

example, thought that “the complex structure implied by this theory is undesirable” (Feynman 1964, 28-5) One way in which the lack of simplicity manifests itself is that there exists no exact closed-form particle equation of motion within the ‘extension program,’ as Muller calls it, and many physicists seem to see it as an important desideratum that electrons are governed by some reasonably simply closed-form equation of motion.

Perhaps the biggest problem for the extension program is that there is no fully relativistic model of an extended charge. The best understood model, the Abraham model, treats charges as rigid and spherically symmetric in a preferred reference frame. In this model there are general existence and uniqueness proofs for the coupled system of the Maxwell-Lorentz equations (see Spohn 2004). Alternatively, charged particles have been modeled as so-called ‘relativistically rigid’ objects—that is objects, that always have the same shape in their instantaneous rest frames. Even this model, however, is not *fully* relativistic in that it allows for superluminal propagation. (A fully relativistic extended particle would have to allow for the particle to be dented when it experiences an external forces.) Moreover, there are to date no global existence and uniqueness proofs for solutions to the particle-field equations for relativistically rigid charges and there are some considerations that suggest that such a proof may not be possible. (See Spohn, 2004, ch. 5, especially p. 30) Thus, like other solutions surveyed by Muller, the extension program offers at best approximate solutions to the problem of arriving at a fully relativistic particle-field theory. The solutions solve the problems only in a weak sense, but not in the strong sense of delineating coherent and complete classical relativistic worlds.

Conclusion

Muller's overview of problems in classical electrodynamics and potential solutions provides valuable details of a kind that unfortunately are all too often missing in philosophical accounts of scientific theorizing. One interesting lesson we can learn from such surveys of the history of classical particle-field theories is that there appears to be a tension between the notion of a discrete particle and the theory of special relativity. No extended particle model that has been explored in any detail is fully relativistic, while point-particle models result in infinities which can be 'tamed' only with difficulty and only in some approximation regime. Nevertheless CED is an extremely successful theory within a certain domain. Thus, the theory provides us with an illuminating case study of how a theory can successfully represent phenomena despite having 'weak spots.' The theory is successful, despite the fact that it does not seem to have the resources on its own to model the details of relativistic particle-field interactions exactly and in complete generality. (This issue, I want to emphasize, is completely independent of the empirical problems the theory faces at very small length scales.)

My earlier focus on the inconsistency in the standard modeling assumptions, I now believe, detracted from this main point. By the same token, Muller's unqualified claim that the theory's conceptual problems have been solved suggests a 'sanitized' view of CED that is belied by the details of his own discussion. Not all the proposals Muller surveys have been as successful as he himself suggests. But more importantly, if we do not distinguish carefully between different senses of solving a theoretical problem, it is easy to draw the wrong conclusions from the survey, despite his insistence of the widespread use of approximations in physics. Rather than asking simply whether a given conceptual problem has been solved or not, we oughth to ask how successful a solution is *given its intended purpose* and *in light of our* *theoretical conditions of adequacy*. The latter include not only general criteria for theory-choice, such as accuracy and simplicity, but also conditions more specific to classical physics, such as the condition of energy-momentum conservation and relativistic constraints.

Some of the theory's problems have been successfully treated in the sense that there are ways of rendering the theory's weak spots unthreatening to the empirical success of the theory, while approximately satisfying our theoretical conditions of adequacy. But solving the theory's problems in this sense is not the same as being able to *remove* the theory's weak spots. To the extent that they are solved at all, the theory's problems are successfully handled 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 interactions between charges and fields. But this kind of solution offers no support for the traditional philosophers' conception of scientific theories as providing us with internally coherent and complete world pictures, no matter how successful the solutions may be from a physicists' perspective. This, I want to insist—and I think Muller would agree—presents a problem *not* for the various attempts to solve the theory's problems, but for the traditional philosophers' conception of scientific theorizing.

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