Inconsistency in Classical Electrodynamics*

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I show that the standard approach to modeling phenomena involving microscopic classical electrodynamics is mathematically inconsistent. I argue that there is no conceptually unproblematic and consistent theory covering the same phenomena to which this inconsistent theory can be thought of as an approximation; and I propose a set of conditions for the acceptability of inconsistent theories.

1. Introduction. Among the different criteria for evaluating scientific theories proposed by philosophers, internal consistency appears to be privileged. A highly successful theory may be more or less accurate or may be more or less simple, but according to what appears to be a widely held view, internal consistency is not similarly up for negotiation: Internal consistency is a necessary condition for a theory to be even minimally successful, for an inconsistent set of principles threatens to be trivial in that any sentence whatever can be derived from it. In this paper I want to discuss some central features of classical electrodynamics with the aim of raising some doubts about this view. The main approach to modeling classical electromagnetic phenomena involving charged particles relies on a set of internally inconsistent assumptions. Yet despite the inconsistency this theory is strikingly successful. What is more, there does not appear to be an otherwise at least equally as successful yet fully consistent alternative theory that the standard approach could be taken to approximate.

There are a number of conclusions that I wish to draw from my case study. First and foremost, the traditional view according to which con-

*Received June 2000; revised December 2003.

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‡I would like to thank Arthur Fine, David Malament, Paul Teller, and Eric Winsberg, as well as the anonymous referees for this journal, for extremely helpful discussions, comments and criticisms.

Philosophy of Science, 71 (October 2004) pp. 525–549. 0031-8248/2004/7104-0008$10.00

sistency is a necessary condition that all successful theories have to satisfy is mistaken. In evaluating the success of a theoretical scheme in modeling the phenomena in a certain domain, consistency appears to be just one criterion of theory assessment among many, and where this criterion conflicts with other demands we might want to place on such a scheme there may be good reasons for giving up on full consistency. Second, if indeed inconsistent theories can play a legitimate role in science, then accounts of theory acceptance according to which accepting a theory implies a commitment to the truth of the theory's empirical consequences—such as Bas van Fraassen's construals of *both* empiricism *and* scientific realism (1980)—have to be rejected. Instead of appealing to the notion of truth, acceptance ought to be construed as involving a commitment to a theory's reliability. Third, in reply to a possible objection to my account, I will argue that accounts of scientific theories that strictly identify theories with the set of their fundamental equations are inadequate. Finally, I will use the theory as a guide to propose a set of criteria for the acceptability of inconsistent theories.

I will proceed as follows. In Section 2 I derive the inconsistency of standard microscopic classical electrodynamics of charged particles interacting with electromagnetic fields. An obvious objection to the philosophical significance of this inconsistency is the worry that the inconsistent set of assumptions is best thought of as an approximation to a more fundamental fully consistent theory. Against this objection I argue in Section 3 that there is no fully consistent and conceptually unproblematic theory covering the same phenomena as the standard particle-field theory from which this theory could be derived as an idealization or as an approximation. In Subsection 3.1 I discuss and criticize the view that the notion of a charged particle is an idealization and that classical electrodynamics fundamentally is a continuum theory. I also argue against the view that we ought to identify a physical theory with the set of its fundamental equations and that the claim that there are discrete charged particles ought not to be thought of as part of the content of microscopic classical electrodynamics. Subsection 3.2 is devoted to the question whether there is an alternative particle equation of motion to which the standard equation is an approximation. In Section 4 I turn to the question how it is that classical particle-field electrodynamics can be successful despite its inconsistency. According to some recent accounts in the literature, inconsistent theories can play a legitimate, yet limited role in scientific theorizing as heuristic guides in theory-development. I argue that these accounts—chiefly among them that of John Norton (1987 and 2002)—allow too limited a role for inconsistent theories. While inconsistent theories clearly have to satisfy certain constraints to be scientifically useful, they constraints proposed by Norton are too restrictive and I propose a set of weaker constraints in their stead.

2. The Inconsistency Proof. The ontology of standard microscopic classical electrodynamics consists of two basic kinds of entities—microscopic charged particles, which are usually treated as point particles, and electromagnetic fields—and the theory describes how the states of particles and fields mutually determine one another. The basic laws of the theory that govern the interaction between charged particles and fields are the Maxwell-Lorentz equations, according to which charges and electromagnetic fields interact in two distinct ways. (See, for example, Jackson 1975.) First, charged particles act as sources of fields, as determined by the four microscopic Maxwell equations, which in a standard three-vector notation can be written as:

$$\begin{aligned}\nabla \cdot \mathbf{E} &= 4\pi\rho,\\ \nabla \times \mathbf{B} - \frac{\partial \mathbf{E}}{\partial t} &= \mathbf{J},\\ \nabla \times \mathbf{E} - \frac{\partial \mathbf{B}}{\partial t} &= 0,\\ \nabla \cdot \mathbf{B} &= 0. \end{aligned} \tag{1}$$

Here bold face symbols represent vector quantities. **E** and **B** are the electric and magnetic field strengths, respectively; ρ is the charge density, which for point charges is represented mathematically by a δ-function; and **J** is the current density. Since the Maxwell equations can be formulated in a way such that they are invariant under Lorentz transformations, classical electrodynamics is a relativistic theory; it is a *classical* theory only in that it is not a *quantum* theory.[1] One component of the field associated with a charge is a radiation field, which arises when a charge is accelerated, carrying energy away from the charge. This interaction can be given an intuitively straightforward causal interpretation: The acceleration of a charge is understood to be the cause of a local excitation of the electro-

1. Since the notation will probably be more familiar to many readers, I use nonrelativistic formulations of the various equations in this paper, instead of their Lorentz-invariant analogues.

magnetic field that propagates away from the charge with the speed of light.[2]

The second way in which fields and charges interact is that external fields influence the motion of a charge in accordance with the Lorentz force law. The theory treats charged particles as Newtonian particles—that is as objects whose motion is given by Newton's second law according to which 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

$$\mathbf{F}_{\mathrm{Lorentz}} = q(\mathbf{E}_{\mathrm{ext}} + \mathbf{v} \times \mathbf{B}_{\mathrm{ext}}). \tag{2}$$

In the absence of non-electromagnetic forces, the equation of motion for a charged particle thus is

$$\frac{d\mathbf{p}}{dt} = \mathbf{F}_{\mathrm{Lorentz}}. \tag{3}$$

Again, this equation has a relativistic analogue. Thus, Maxwell-Lorentz electrodynamics, even in a fully relativistic formulation, is a Newtonian theory in one important respect: charged particles are governed by Newton's laws of motion or their relativistic analogues.[3]

The interaction governed by the Lorentz force can be said to be 'local' in that the acceleration of a charge depends on the value of the electromagnetic field only at the location of the charge. As in the case of mechanical forces, the association between external fields and the acceleration of a charge is usually interpreted causally: External fields cause charges to accelerate. Thus, the mutual interactions between charges and fields in Maxwell-Lorentz electrodynamics satisfy several demands one might intuitively wish to place on a classical, causally well-behaved theory. Charged particles satisfy the principle of inertia and forces act as causes of accelerations. Interactions are causally local in that the influence of one charge on another is transmitted at a finite speed and that, due to the presence of the electromagnetic field, effects are not transmitted across spatio-temporal 'gaps.' In addition, effects do not precede their causes. Hence, classical electrodynamics fits extremely well into a classical, causal

2. The temporal direction of the electromagnetic radiation physically associated with a charged particle does not follow from the Maxwell equations alone, which are time-symmetric, but is usually introduced as a separate assumption in textbooks. For a detailed discussion and criticism of different accounts of the temporal asymmetry of radiation, see Frisch 2000 and Frisch forthcoming.

3. Hence, when I speak of Newton's laws in this paper, I intend this to include their relativistic generalization and do not mean to draw a contrast between non-relativistic and relativistic physics.

conception of the world and in fact is often taken to be the paradigm of a classical physical theory. Moreover, the theory is predictively extremely accurate within the domain of classical physics and the Maxwell-Lorentz equations are often taken to satisfy certain intuitive criteria of simplicity. The theory, that is, scores very high on a number of criteria of theory assessment, including accuracy, simplicity, and fit with the overall conceptual framework. There is just one problem: the theory is inconsistent.

Corresponding to the two ways in which, according to the theory, charged particles and fields interact, the Maxwell-Lorentz equations can be used to treat two types of problem. We can appeal to the Maxwell equations to determine the fields associated with a given charge and current distribution; or we can use the Lorentz force law to calculate the motion of a charged particle in a given external electromagnetic field. In problems of the first type the charges and currents are specified and, given particular initial and boundary conditions (which specify the source-free fields), the total electromagnetic field is calculated. In problems of the second type the external electromagnetic fields are specified and the motions of charged particles or currents are calculated. In these problems electric charges are treated *either* as being affected by fields *or* as sources of fields, but not both.

One can also in a stepwise treatment combine the two types of problem. An example of this are models of synchrotron radiation, which is the radiation emitted by circularly accelerated electrons in the magnetic field of a synchrotron accelerator (see Jackson 1975, section 14.6). In a first step the orbit of the electrons in the external magnetic field is calculated using the Lorentz force law. In the simplest model of a synchrotron the electrons are assumed to be injected at right angles into a constant purely magnetic field. In that case the Lorentz force equation of motion implies that the electrons move in a circular orbit. In a second step, the trajectories of the electrons are assumed to be given and are used as input in the Maxwell equation to calculate the radiation field.

We can easily see, however, that this treatment is inconsistent with the principle of energy conservation. On the one hand, since the electron orbit (as calculated in the first step) is circular, the electrons' speed and, hence, their kinetic energy is constant. (Moreover, since in this simple case the field is static, we can assign a potential energy to the electrons, which is constant as well.) On the other hand, charges moving in a circular orbit accelerate continuously (since the *direction* of their velocity changes constantly) and, thus, according to the Maxwell equations and the standard formulation for the field-energy, radiate off energy. But if energy is conserved, then the energy of the electrons has to decrease by the amount of the energy radiated and the electrons' orbit could not be the one derived from the Lorentz force in the first step, for which the electrons' energy is constant.

I now want to derive the inconsistency more generally. The following four assumptions of the Maxwell-Lorentz theory are inconsistent:

i. There are discrete, finitely charged accelerating particles;
ii. Charged particles function as sources of electromagnetic fields in accord with the Maxwell equations;
iii. Charged particles obey Newton's second law (and thus, in the absence of non-electromagnetic forces their motion is governed by (3));
iv. Energy is conserved in particle-field interactions, where the energy of the electromagnetic field is defined in the standard way.

It follows from the Maxwell equations in conjunction with the standard way of defining the energy associated with the electromagnetic field that accelerated charges radiate energy, where the instantaneous power radiated is given by $P = 2e^2a^2/3c^3$ (Jackson 1975, 659). Thus, if the acceleration of a charge is non-zero at any time t, $t_A < t < t_B$, then the energy E_{rad} radiated by the charge between times t_B and t_A is greater than zero.

Newton's second law and the definition of the external work done on a charge imply that the work done on a charge is equal to the change in the energy of the charge. That is, iii implies that

$$W = \int_A^B \mathbf{F}_{\text{ext}} \cdot d\mathbf{l} = \int_{t_A}^{t_B} \frac{d\mathbf{p}}{dt} \cdot \mathbf{v}dt = m\int_{t_A}^{t_B} \frac{d\mathbf{v}}{dt} \cdot \mathbf{v}dt$$

$$= \frac{m}{2}(v(t_B)^2 - v(t_A)^2) = E_{\text{kin}}(t_B) - E_{\text{kin}}(t_A). \tag{4}$$

But for energy to be conserved, that is for iv to hold, the energy of the charge at t_B should be less, by the amount of the energy radiated E_{rad}, than the sum of the energy at t_A and the work done on the charge. That is,

$$E_{\text{kin}}(t_B) = E_{\text{kin}}(t_A) + W - E_{\text{rad}}. \tag{5}$$

(4) and (5) are inconsistent with each other, if E_{rad} is finitely different from zero. But $E_{\text{rad}} > 0$ is implied by the conjunction of i and ii. Thus, the core assumptions of the Maxwell-Lorentz approach to microscopic particle-field interactions are inconsistent with one another.

3. Consistent (and Other) Alternatives.

3.1. Charged Particles as Idealizations. My discussion so far invites an obvious worry: Is the inconsistency of i through iv really that of a genuine

https://doi.org/10.1086/423627 Published online by Cambridge University Press

scientific theory, or should it perhaps more properly be characterized as the result of a certain idealization or approximation within the context of some other fully consistent theory?

Intuitively, the inconsistency appears to arise from the fact that the Lorentz-force equation of motion ignores any effect that the self-field of a charge has on its motion.[4] The standard theory treats charged particles as sources of fields and as being affected by fields—yet not by the total field, which ought to include a contribution from the charge itself, but only by the field external to the charge. This treatment is inconsistent with energy conservation, for if the charge radiates off energy, then this should have an effect on its motion, and, thus, a radiation term representing a 'force' due to the charge's self field should be part of the equation of motion. Yet an immediate problem for trying to incorporate the effects of the self-field into an equation of motion is that charged particles are usually treated as point particles in classical electrodynamics and for such a charge the field-energy stored in any volume containing the charge is infinite. This can be easily seen by considering the q/r^2-dependence of the Coulomb field associated with a charge. Since the field-energy varies as $\mathbf{E}^2$, the total energy contained in any volume containing a finite point charge q is infinite. Thus, one might think that what is ultimately responsible for the inconsistency is the idealized notion of a discrete point charge. In this subsection I want to explore this idea and ask whether we ought to think of the inconsistency as being due to an idealization. The question of alternative equations of motion that avoid the infinity in the self-fields and to which equations (2) and (3) might be an approximation will occupy us in the next sub-section.

A referee for this journal has suggested that the notion of a finitely charged point particle is problematic in a manner similar to that in which the notion of a round square is. Any theory of round squares would be inconsistent, but in this case the inconsistency would be due not to the theory's laws but to the logically prior notion of a round square. Similarly, the objection goes, since "the claim that there are finitely charged point-particles is clearly not itself a fundamental law or equation," the fact that the notion of a point-particle is problematic does not imply that the theory is problematic as well. In fact, there are several distinct possible objections here, which I will address in turn.

First, the analogy between the notion of a round square and that of a point-particle is not a good one, since the notion of a finitely charged

4. I say "most plausibly" because one could also try to avoid the inconsistency of the theory by proposing alternatives to the Maxwell equations or to the standard formulation of energy conservation. For a discussion of these options see Frisch forthcoming.

point particle on its own is not obviously incoherent. The problem with the self-field associated with a point charge is not *that* it is infinite at the location of the charge but, first, that for energy to be conserved charges ought to interact with their own fields and, second, that according to the Maxwell equations, the self-field diverges too quickly. If the field were to diverge no faster than $1/r$ then the total energy stored in a region containing a charge, which varies as $\mathbf{E}^2$, would be finite, since the volume integral contributes a factor of r^2; and this might allow for the construction of a consistent theory. Indeed, there is a point-particle versions of classical electrodynamics without fields and without self-interactions—Wheeler and Feynman's infinite absorber theory (1945)—that appear to be perfectly consistant, whatever else its problems may be. Moreover, the inconsistency proof I outlined above does not depend on the fact that the charged particles in question are *point* particles. All that is required in the proof is that the particles have finite charge and that, hence, E_{rad} is finitely greater than zero. Hence, the inconsistency of i through iv is not simply due to the role of point particles in the theory.

There are three additional objections which emphasize not the role of *point*-particles but rather the idea that the claim that there are charged particles is not a part of the theory. I have shown that the conjunction of the claim that discrete finitely charge particles exist and the Maxwell-Lorentz equations is inconsistent. This, one might object, is not an argument that can establish that the *theory* in question is inconsistent since a theory, one might hold, ought to be identified with a set of fundamental dynamical equations and the claim that there are finitely charged particles is not such an equation. The traditional philosophical view is that scientific theories have to be consistent. Thus, if the inconsistency is the result of adding a constraint extraneous to the theory to its fundamental equations, then my case study cannot undermine the traditional view. Why might claim i not be part of the theory? The worry might be (*a*) that the claim does not have the form of an *equation*, (*b*) that it is not *fundamental*, or (*c*) that it is not a *dynamical constraint*.

Now, it appears to me that we can dismiss all three of these worries rather quickly. First, contrary to what the objection might claim, a constraint doing the same work as i can be expressed in the form of an equation. The only work i does in the inconsistency proof is to ensure that a certain variable occurring in the equations—the radiation energy E_{rad}—is finitely different from zero; and this requirement can easily be expressed in the form of an equation by specifying that the charge density entering into the Maxwell equations is given by a sum over localized, discrete elementary charges of finite size. Second, restricting the charge density to a sum over discrete charges arguably *is* a *fundamental* part of the theory. For, the claim that charge is 'quantized'—that is, that there are discrete particles that carry a charge equal to multiples of a basic unit of charge identical to the charge of an electron—is one of the central tenets of microscopic classical electrodynamics. Third, the content of a theory cannot plausibly be restricted to a set *dynamical* constraints, since then even the Maxwell equations would not qualify as part of a theory in their entirety, since Coulomb's law and the prohibition against magnetic monopoles impose non-dynamical constraints on the field associated with a given charge distribution. Thus, even though the claim that charge is quantized does not have the form of a dynamical constraint on the evolution of electromagnetic systems, it seems to be a perfectly good candidate for being a lawful constraint.

At this point one might object that only the conditional 'If there is electric charge, then it is quantized' could be part of a theory but not the existence claim that there are charged particles. In fact, it appears to me that the central assumption driving the objections I have been considering is the idea that existence claims cannot be part of a theory. But this assumption is misguided. Consider, for example, an imaginary theory that postulates *both* that all massive bodies attract each other according to Newton's $1/r^2$-law of universal gravitational attraction *and* that all massive bodies exert an $1/r^3$-force on one another. On their own—that is, without the further assumption that there are at least two massive bodies—the two seemingly contradictory force laws and Newton's laws of motion form a consistent set, for the theory has models, namely universes containing at most one massive object. Yet if this theory is intended to be a theory of multi-particle interactions, then it appears to me that the correct way to describe it is as inconsistent. While the claim that there are at least two massive objects arguably is not fundamental or lawlike, it appears to be an integral part of any theory of multi-particle interactions. Scientific theories are *about* certain things, and a claim stating that what a given theory is about exists ought to be considered to be part of that theory.[5] As *a theory of multi-particle interactions* a theory with incompatible force laws is inconsistent.

Theories provide us with representations of the phenomena in their domain and a theory's representational content may go beyond what is captured in the theory's fundamental dynamical equations. For example, a theory may account for a phenomenon by positing that the world is populated by certain entities whose interactions give rise to the phenomenon in question. Maxwell-Lorentz electrodynamics, which is a direct descendent of Hendrik A. Lorentz's attempts to derive Maxwell's macroscopic particle-field equations from the interactions of microscopic 'elec-

5. This does not mean that we all have to become scientific realists. For we need not endorse the existence claims of a theory we accept.

trons' with microscopic fields, is just such a theory: Like Lorentz's theory, the modern theory treats many electromagnetic effects as ultimately due to the interaction of discrete charged particles and fields. And the claim that *there are* discrete microscopic charged particles appears to be part of the content of the theory. In fact, in light of the central importance of the concept of discrete charged particles to post-Maxwellian classical electrodynamics, the fact that a view implies that the theory denies the existence of such particles (as the theory would if it were to be identified with the set of its fundamental equations) strikes me as a *reductio* of that view.

Yet for my purposes here I do not even need to convince those who want to deny that scientific theories can include existence claims of the 'folly of their ways.' For even if we were to accept this rather unnatural view of theories, it does not solve the substantive problem at issue. The puzzle presented by classical electrodynamics is how it can be that a theory or a set of equations can be used to represent the phenomena in a certain domain, despite the fact that the theory's basic equations have no models in that domain. Those who would want to insist that consistency is a privileged criterion of theory choice would equally want to insist, I take it, that for a theory successfully to represent a certain range of phenomena the theory would have to have models (in the logician's sense of structures in which the equations are jointly true) that can function as representations of these phenomena. Now, equations (1) through (3) together with the principle of energy conservation imply that there are no charged particles; that is, the set of equations has no models involving charged particles. How is it, then, that we nevertheless can successfully represent electromagnetic phenomena in terms of charged particles that are governed by just this set of equations? This puzzle remains the same, independently of whether we want to say that the theory in question (taken to include the claim that there *are* charged particles) is inconsistent or whether we want to insist that the theory construed without the existence claim is consistent, yet has no models involving charged particles.

There is one further issue to consider in this context, however. Unlike in the case of the imaginary theory with inconsistent force laws the fundamental equations of classical electrodynamics have a physically interesting class of models of which they are jointly true: systems of continuous charge distributions (or *charged dusts*) interacting with electromagnetic fields. In this case the self-fields do not contribute to the equation of motion, because the field, and hence the radiation energy E_{rad}, associated with each infinitesimal 'particle' of the distribution is likewise infinitesimal. Thus, (4) is compatible with (5). In other words, there is no need to distinguish between *external* fields and *total* fields for charged dusts, since the work done by the external fields on an infinitesimal charge is equal to the work done by the total fields. What, then, is the relation between the theory of charged dusts and that of discrete particles? Since the former theory is consistent, should we perhaps conclude that the intended models of the Maxwell-Lorentz equations are continuous, and not discrete structures?[6]

This suggestion permits of two readings. On one reading it is reminiscent of the proposal Hilary Putnam famously discussed in connection with a view he called "metaphysical realism." Putnam's suggestion was, roughly, that we intend our theories to be about whatever it is that makes them come out true. But whatever the merit of Putnam's proposal is in general, in the context of a theory with limited scope the suggestion can be dismissed rather quickly. When physicists intend to model phenomena such as synchrotron radiation as involving discrete charged particles, their intentions are not somehow thwarted and redirected toward models involving continuous charge distributions simply in virtue of the fact that the equations they use in representing discrete charges are inconsistent with the very existence of discrete charges. The representational models physicists construct using the Maxwell-Lorentz laws might not be fully successful in that they fail to represent their intended targets truthfully. Yet they do succeed in being the kind of models they are intended to be. The vast majority of applications of microscopic classical electrodynamics appeal to discrete charged particles and not continuous distributions. This is a phenomenon that needs to be 'saved' by any philosophical reconstruction of theorizing in classical electrodynamics.

Alternatively, and more plausibly, the suggestion might be that the phenomena that physicists represent as involving charged particles could at least in principle be represented in terms of continuous charge distributions. According to this suggestion, the notion of discrete charged particle might be a convenient idealization but one with which we could in principle dispense. That is, the suggestion is that what I am calling "the *theory* of charged particles" is properly speaking not a theory at all, not because claims about ontology are not part of the theory proper, but because discrete charged particles are only an *idealization* within a consistent theory of continuous charge distributions. Physicists, according to this suggestion, model electromagnetic phenomena in terms of discrete particles but this is only a matter of convenience, since the phenomena in question are all in principle governed by the consistent continuum theory as well. And it is in this sense that the 'intended domain' of the theory has continuous distributions of charge as its objects.

The problem with this suggestion is that the pure continuum theory

6. This was suggested by the referee who pressed the objections concerning point particles.

cannot even in principle account for many of the phenomena that on the face of it appear to involve discrete charged particles. One might think that we should be able to 'de-idealize' discrete particles by representing them by differences in the charge density of a continuous charge distribution and then apply the ordinary Lorentz law to the infinitesimal 'point charges' of the distribution. But the existence of maxima in the charge density which retain their shape through time (corresponding to stable particles) is inconsistent with a *pure* electrodynamics in which the *only* force acting on the charge distribution is the Lorentz force. For due to Coulomb repulsions among the different parts of the distribution local regions of higher charge density are in general not stable and flow apart.

Experimental evidence suggests, however, that we cannot do without discrete localizations of charges. A historically particularly interesting example of the failure of continuum electrodynamics to represent 'particle phenomena' adequately is the case of synchrotron radiation, which I briefly discussed above. Originally physicists did in fact represent the stream of electrons orbiting in a synchrotron accelerator as continuous constant current. Since it follows from the Maxwell equations that constant currents do not emit radiation, physicists did not expect any radiation to be associated with synchrotron charges and it came as a surprise when the radiation (which happens to occur in the visible part of the electromagnetic spectrum) was discovered purely by accident. The discovery was then taken to show that electrons in a synchrotron have to be modeled as circularly accelerating *discrete* particles rather than as a continuous distribution.

We tend to think of idealizations as computationally useful yet as 'leading us away from the truth.' Yet if the present suggestion is right, classical electrodynamics presents us with a case where introducing an idealizing assumption inconsistent with the fundamental equations of the theory dramatically improves the theory's predictive power and accuracy. The laws of the continuum theory have no (model-theoretic) models that can *even in principle* adequately represent the behavior of compact localizations of charge, since they do not on their own allow for localized electromagnetic objects that retain their integrity through time. Thus, the theory fails empirically quite dramatically, while introducing the particle-idealization leads to an empirically quite successful scheme. Thus, if we assume that the continuum theory is the basic or fundamental theory, from which the particle approach is obtained by introducing the concept of discrete particles as idealization, then we are forced to conclude that there are idealizations—and even idealizations inconsistent with the theory's fundamental dynamics—that are absolutely essential to the a theory's empirical adequacy.

Continuum electrodynamics does not allow for compact localizations of charges, since the theory on its own provides no mechanism, as it were, that could prevent regions of higher charge density from flowing apart. What if we postulated additional, perhaps not further specified non-electromagnetic forces to ensure the stability of charged particles? Once we introduce such cohesive forces, however, we are led to a picture with discrete finitely charged particles with finite radiation effects affecting the motion of the center of mass of such a particle in conflict with the Lorentz force equation of motion. Thus, we need to distinguish carefully between a theory of continuous charge distributions and one of extended, yet discrete charged particles. While in the former case the Lorentz force equation of motion describes the motion of the infinitesimal 'particles' consistent with energy conservation, in the latter case taking the center of mass motion for a charged particle to be given by the Lorentz force law is inconsistent with energy conservation and the Maxwell equations. The question whether the Maxwell-Lorentz scheme ought to be understood as an approximation derivable from a more fundamental theory with a different force law is what I will address next.

3.2. Alternative Particle Equations of Motion. The suggestion we discussed in the previous section concerned the ontology of the theory: Is it perhaps mistaken to think of the Newtonian Maxwell-Lorentz equations (1) through (3) as part of a theory of charged particles rather than of continuous charge distributions? As I have argued the problem with a Newtonian continuum theory is that it is empirically grossly inadequate. It simply does not allow us to represent the vast majority of phenomena that are taken to be in the purview of classical microscopic electrodynamics. The suggestion to which I want to turn now concerns the status of Newton's second law together with the Lorentz force law. As I said earlier, the most plausible account of the inconsistency is that the equation of motion for charged particles (3) ignores the effect of self-fields. Thus, can we perhaps understand the Newtonian Lorentz force law as an approximation and if so, can that perhaps explain the inconsistency of the scheme we have been discussing?

A theory can be said to be an approximation in two quite distinct senses. In one sense, when we speak of a theory's being only approximate, what we mean is that the theory is only nearly but not completely correct; that is, the theory is not entirely true of the phenomena it is intended to represent. In this sense classical particle electrodynamics clearly is only an approximation. The theory is not literally true; in fact, due to its inconsistency the theory could not possibly be true of the world. But this is not the sense of "approximation" that is at issue here. In a second sense, when we speak of a scheme of equations or a certain set of modeling assumptions as being approximate, we have in mind not its relation to the world, but its relation to some other, usually more fundamental theory. In this second sense, to say that the Lorentz force equation of motion is only approximate is to say that there exists some other, more fundamental particle equation of motion that could in principle represent the phenomena in question, and that the Lorentz force law allows us to make very nearly the same predictions as this other equation.

One might suggest, then, that the role of the Lorentz law is analogous to that of the pendulum equation of motion in Newtonian mechanics that is derived with the help of the small-angle approximation. The small-angle approximation (which replaces sin α, the sine of the angle of displacement, with the angle α) leads to an equation of motion that strictly speaking is inconsistent with Newton's laws, since the latter tell us that the force on the bob of the pendulum is proportional to sin α and not to α. Yet this inconsistency clearly does not show that Newtonian mechanics is in any way conceptually problematic.

Is there, then, a consistent microscopic particle electrodynamics to which the Maxwell-Lorentz scheme is an approximation?[7] In trying to answer this question we need to distinguish between theories that treat charged particles as point particles and those that treat extended particles. As far as point particle theories are concerned, there actually is an equation of motion incorporating a 'radiation reaction term' in a manner that is arguably consistent with the Maxwell equations and energy conservation. This is the so-called *Lorentz-Dirac equation* (which is derived from the Maxwell equations and energy conservation by 'renormalizing' the mass of the electron—a procedure for sweeping the infinity in the self-fields under the rug; see Rohrlich 1990). However, this equation faces several serious conceptual problems. The equation arguably is backward causal, allows for forces to act where they are not, and allows for causal interactions between spacelike separated events (despite the fact that the theory is Lorentz-invariant). Most troublesome, perhaps, is the fact that there are no general existence and uniqueness proofs for systems consisting of more than one charge and that the two particle systems that have been studied exhibit what is known as *runaway behavior*—that is, the charges' accelerations grow unbounded as they move off toward infinity, in violation if not of the letter so at least of the spirit of energy conservation (see Parrott 1987). In all these respects the Lorentz-Dirac theory compares unfavorably to the Maxwell-Lorentz framework. Thus the price for con-

7. For a detailed examination of alternative equations of motion, see Frisch forthcoming.

sistency is rather high—too high, in fact, in the eyes of most physicists who reject the Lorentz-Dirac theory due to its many conceptual problems.[8]

Of course one might nevertheless insist that due to the very fact that the Lorentz-Dirac equation is consistent with the Maxwell equations this equation ought to be thought of as the fundamental equation of motion. I have no knockdown objection against this position. It is enough for my purposes here to show that scientific practice suggests that in comparing different theoretical schemes there can be a trade-off between different criteria of adequacy and, importantly, that consistency is one of the criteria that can be given up in favor of other conditions. While the Lorentz-Dirac equation might be consistent, it scores rather poorly, compared to the Maxwell-Lorentz theory, as far as other criteria of theory assessment are concerned. Sometimes, as in the case of quantum theories, there may be good empirical reasons to give up a conceptually 'well-behaved' theory in exchange for a much stranger beast. But in the case of classical electrodynamics only very few physicists seem to feel compelled to make such a move. And those who prefer the inconsistent Maxwell-Lorentz theory over the Lorentz-Dirac theory do not appear to be violating any standards of scientific rationality in doing so.

There are also various proposals for equations of motion of extended charged particles. Extended charges can be thought of as arising in a theory of continuous charge distributions by imposing additional constraints that ensure that charged particles do not 'explode.' One problem for any such theory is that it has to make assumptions about the internal structure of charged particles—assumptions which can only be motivated by appealing to the relative simplicity of the resulting equation. In modeling extended charged particles one has the choice of treating the particles either as rigid or as possessing internal degrees of freedom. Treating charged particles as rigid is inconsistent with special relativity. But the assumption that charged particles have internal degrees of freedom is problematic as well. The most promising candidate for an extended particle equation of motion incorporating the self-fields is what is known as a *differential-difference* equation of motion, which is incompatible with what appears to be a central assumption of classical determinism. This assumption is that the state of a system at one time determines the state of the system at all other times, where the state of a system at a time is given by the values of a *finite* set of variables at each point in space. Differential-difference equations require the values of an infinite number of variables at each point as input. Most importantly for present purposes,

8. The Wheeler-Feynman theory that I mentioned above leads to the same particle equation of motion as the Lorentz-Dirac theory. Thus, the two theories share many of the same problems.

however, is that a differential difference equation of motion is itself not fully consistent with the Maxwell equations and energy conservation.

I want to summarize what I have argued so far. I have shown that the standard approach to particle electrodynamics is inconsistent. I then discussed a number of considerations that might be taken to show that the inconsistency is simply due to the fact that this approach relies on idealizations or approximations of one form or other. The first suggestion is that classical electrodynamics ought to be understood as a theory of continuous charge distributions and that introducing discrete particles into the theory constitutes an idealization—an idealization that might be mathematically useful but inevitably results in an inconsistent scheme. The problem with this suggestion is that the pure continuum theory does not cover *even in principle* many of the phenomena that the particle enables us to model, since the continuum theory is incompatible with the existence of compact localizations of charges that persist through time.

The second suggestion is that the Newtonian Lorentz force equation of motion is an approximation to a more fundamental equation of motion consistent with the Maxwell equation. Possible contenders are the Lorentz-Dirac equation or a number of different equations of motions for extended charged particles. The first is arguably consistent with the Maxwell equations. Yet due to its many conceptual problems only few seem to be willing to accept it as the fundamental equation of motion of classical charged particles. On the other hand, none of the candidate equations of motions proposed for extended charged particles is in fact fully consistent with the Maxwell equations. The upshot of this brief survey of possible alternatives is that the Maxwell-Lorentz theory is not an approximation to any actual consistent and conceptually unproblematic more fundamental classical theory.[9]

One final suggestion might be that the theoretical scheme is an approximation to some unknown, yet to be developed consistent theory. Yet after a century of ultimately unsuccessful attempts of developing a fully consistent classical theory, one may have legitimate doubts about the prospects of finding such a theory. Clearly, believing now in the existence of such a theory can be nothing more than an act of faith. I will discuss this suggestion in more detail in the next section.

4. Inconsistency and Theory Acceptance. I have argued that the standard approach to microscopic particle-field electrodynamics is inconsistent and

9. What about quantum theory? An appeal to quantum theory would be of no help here for several reasons, perhaps the least controversial being that similar problems to those of the classical theory reemerge on the quantum level and it is not clear that there is a consistent quantum theory either.

https://doi.org/10.1086/423627 Published online by Cambridge University Press

that this approach cannot be derived from any fully consistent and conceptually unproblematic covering theory. What philosophical lessons can we draw from this? The existence of inconsistent theories or inconsistent theoretical schemes is incompatible with several traditional accounts of scientific theories, such as syntactic views that identify a theories with deductively closed sets of sentences or versions of the semantic view, such as van Fraassen's (1980), that identify theories with sets of models in which the theory's laws are jointly true. But there also is a small but growing literature realizing that inconsistent sets of hypothesis can be a legitimate part of the process of scientific theorizing. This literature presents a welcome departure from philosophical orthodoxy. But I believe that the case of classical electrodynamics shows that it has not gone nearly far enough in recognizing the kind of role inconsistent theoretical schemes can play.

Inconsistent theories raise the following obvious puzzle: If drawing inferences from the theory's fundamental equations is anything like standard deductive inference, then we should be able to derive any arbitrary sentence from an inconsistent set of assumptions. How then can an inconsistent theory have genuine empirical content? To be sure, derivations in mathematical physics are not always easily reconstructed as logical deductions in a formal language. But clearly they proceed 'quasi-formally' within some appropriate mathematical language and consistency appears to be an important constraint on such derivations.

One strategy for allowing for the possibility of inconsistent theories is to try to reconstruct scientific inferences in a non-classical, paraconsistent logic. But I do not think we have to resort to such perhaps somewhat extravagant formal solutions to the problem of inconsistencies. The rules of classical logic license us to make certain inferences but do not require us to do so. Thus, one way to safeguard against the possibility of deriving arbitrary conclusions is to ensure that there are additional constraints, depending on a theory's content, that restrict the types of permissible derivations. That there might in fact be such additional constraints on theorizing, either implicit or explicit, is suggested by the way in which physics students learn a new theory. As Kuhn famously argued, instead of merely being given a set of equations students learn a theory by learning how to use the basic equations in modeling paradigmatic phenomena within a theory's domain. Part of a student's task is to learn *which* members of a set of equations to apply in a given context and *how* to use them. And quite plausibly it is also part of this process to learn which equations *not* to use in a given context and how *not* to derive inferences from the theory's laws.

John Norton has recently argued for what he calls a "content driven control of anarchy" (2002, 192). Norton maintains that Newtonian cosmology (2002)[10] and the old quantum theory of black body radiation (1987) are inconsistent. His diagnosis in both cases is that anarchy is avoided, since scientists employ constraints that selectively licenses certain inferences but not others based on reflections on the specific content of the theory at issue. I believe that such a content driven approach to reconstructing theorizing in the presence of inconsistencies points in the right direction, yet Norton's proposal as to what kind of role inconsistent theories can play in science provides a relatively conservative revision of the traditional wholesale prohibition against inconsistencies, and here I disagree with him.

According to Norton, there are two fairly restrictive constraints on the permissibility of inconsistencies in theorizing. First, he holds that when physicists use an inconsistent theory, then their commitment can always be construed as extending only to a *consistent subset* of the theory's consequences. That is, according to Norton, it always will be possible to reconstruct a permissible inconsistent theory by 'surgically excising' the inconsistency and replacing the theory with a single consistent subset of its consequences in all its applications. And second, for Norton an inconsistent theory is only permissible as a *preliminary stage* in theorizing that eventually is replaced by a fully consistent theory.[11] Classical electrodynamics, however, does not fit either of these constraints. Thus, I want to propose a set of alternative constraints weaker than Norton's that nevertheless are strong enough to safeguard against logical anarchy and that can help us account for theorizing in electrodynamics.

As far as the first constraint is concerned, Norton holds that a theory's inconsistency is no threat to the theory's empirical applicability only if there is a consistent subset of the theory's consequences that alone is ultimately used to make empirical predictions. Take the old quantum theory of radiation, which appears to be *prima facie* inconsistent because it involves principles from classical electrodynamics and a quantum postulate. According to Norton we can give a consistent reconstruction of the theory which consists of only a subset of classical electrodynamics together with the quantum postulate. In generating predictions only this consistent sub-theory is involved. In the case of Newtonian cosmology the entire set of premises is used in derivations, yet, Norton maintains, scientists avoid being committed to inconsistent predictions by accepting only one of the infinitely many inconsistent force distributions derivable from the theory (2002, 191–192).

10. See, however, Malament 1995.

11. While I am here focusing on Norton's account in particular, these two constraints are fairly standard in the literature. For similar views, see Smith 1988 and the papers by Arthur Miller and Nancy Nersessian in Meheus 2002.

A similar reconstruction is not possible in the case of classical electrodynamics, since scientists endorse consequences of the theory which are mutually inconsistent given the basic postulates of the theory while also accepting these postulates in their entirety. As we have seen, scientists use the theory to make predictions based on the Lorentz force law, on the one hand, and predictions based on the Maxwell equations, on the other, without abandoning their commitment to the principle of energy conservation (which itself is invoked in certain derivations). Thus, there is no single consistent subset from which all the theory's acceptable empirical consequences can be derived. Rather, in different applications scientists appeal to different internally consistent yet mutually inconsistent subsets of the theory's postulates. Unlike in the examples Norton discusses a consistent reconstruction of the theory's entire predictive content is impossible. That means that one obvious route for 'sanitizing' inconsistent theories is blocked. This, however, raises the following problem: How can scientists be committed to incompatible predictions derivable from the theory, given that knowingly accepting inconsistent empirical consequences seems to be prohibited by standards of rationality?

One response to this problem would be to argue that we should revise our standards of rationality in a way that allows for knowingly accepting inconsistent claims. But I think nothing that radical is needed in the present case. Instead I want to suggest that the source of the problem is a certain picture of *theory acceptance* that we should give up. The problem arises, if we assume that accepting a theory entails being committed either to the literal truth of the theory or at least to the theory's empirical adequacy in van Fraassen's sense (1980)—that is, to the theory's being true about what is observable. If accepting a theory entails being committed to the literal truth of the theory's empirical consequences, then accepting an inconsistent theory entails being committed to inconsistent sets of consequences. Thus, if it is irrational to knowingly accept a set of inconsistent sentences as true, then it is irrational to accept the Maxwell-Lorentz scheme, if one is aware of its logical structure. Yet this problem disappears if in accepting a theory we are committed to something weaker than the truth of the theory's empirical consequences. I want to suggest that in accepting a theory our commitment is only that this theory allows us to construct successful models of the phenomena in its domain, where part of what it is for a model to be successful is that it represents the phenomenon at issue to whatever degree of accuracy is appropriate in the case at issue. That is, in accepting a theory we are committed to the claim that the theory is *reliable* but we are not committed to the literal *truth* of the theory or even just of its empirical consequences. This does not mean that we have to be instrumentalists. Our commitment might also extend to the ontology or the 'mechanisms' postulated by the theory. Thus, a scientific realist might be committed to the reality of electrons and of the electromagnetic field yet demand only that electromagnetic models need to represent the behavior of these 'unobservables' reliably, while an empiricist could be content with the fact that the of the models are reliable as far as the theory's observable consequences are concerned.

If acceptance only involves a commitment to the reliability of a theory, then accepting an inconsistent theory can be compatible with our standards of rationality, as long as inconsistent consequences of the theory agree approximately and to the appropriate degree of accuracy. Thus instead of Norton's condition that inconsistent theories have to have consistent subsets which capture all the theory's acceptable consequences, I want to propose that our commitment can extend to mutually inconsistent subsets of a theory. If our commitment is to a theory's reliability, then the only constraint on the permissibility of inconsistent theories is that predictions based on mutually inconsistent subsets agree approximately.

This constraint is in fact satisfied by classical electrodynamics. Given energy conservation, the Lorentz force will do a good job at representing the motion of a charged particle only if the energy of the charge is very large compared to the energy radiated. In that case the error we make in ignoring the radiation losses implied by energy conservation is negligible. If one plugs in the numbers, it turns out that for an electron radiative effects would only influence the motion of the particle appreciably for phenomena characterized by times of the order of 10^{-24} s (such as that of a force that is applied to an electron only for a period of 10^{-24} s) or by distances of the order of 10^{-13} cm (Jackson 1975, 781–782). These times and lengths lie well outside the theory's empirical limit of validity and within a domain where quantum mechanical effects become important. Within its domain of validity, the theory is approximately consistent: Predictions based on the Maxwell equations and the Lorentz force law, although strictly speaking inconsistent given energy conservation, agree within any reasonable limit of accuracy.

The flip side of the point that the theory is approximately consistent within a certain domain is that the theory puts limits on its domain of applicability *from within*, as it were. Independently of any empirical considerations, we can know that the theory would not be applicable to phenomena involving very short distances and time scales. For in that case the energy characteristic of the phenomenon is comparable in magnitude to that of the radiation loss and predictions based on the Lorentz law would appreciably disagree with the requirement of energy conservation. We can contrast this with the case of Newtonian classical mechanics. Today we believe that the (non-relativistic) theory does not apply to phenomena involving very high speeds or very short distances. But these limits to the theory's domain of applicability had to be discovered empirically, and were not dictated by the theory itself. Unlike standard classical electrodynamics, Newtonian mechanics has no internal limits of reliability. Now, as it turns out classical electrodynamics becomes empirically inapplicable several orders of magnitudes before its internal limit of application is reached. But this does not conflict with the claim that there is such an in-principle limit.

One may also wish to put the point differently: It is precisely because classical electrodynamics is taken to have a limited domain of application that the theory's inconsistency is acceptable. As a candidate for a universal physics an inconsistent theory would be unacceptable. Yet a theory with a limited domain of validity may be inconsistent as long as the inconsistency does not notably infect predictions within its domain. This is how the physics community by and large appears to view the situation. Before the development of quantum theories the question of the consistency of a classical particle-field theory was of central concern to research in theoretical physics. But with the advent of quantum physics interest in developing a coherent classical theory seems to have rapidly declined.[12] Thus, an additional constraint on the permissibility of inconsistent theories is that they cannot be candidates for a universal physics. Yet again this constraint is significantly weaker than Norton's constraint according to which inconsistent theories are only permissible as guides to consistent theories.

This brings me to the second disagreement I have with Norton's account. Echoing the traditional worry that inconsistent theories allow us to derive arbitrary conclusions, Norton holds that the consequences of inconsistent theories can be of no interest to us, unless the approximately the same conclusions can also be derived from a consistent theory. Thus, Norton concludes his discussion of Newtonian cosmology by saying:

> In sum, my proposal is that the content driven control of anarchy can be justified as meta-level arguments designed to arrive at results of an unknown, consistent correction to the inconsistent theory. The preferred conclusions that are picked out are not interesting as inferences within an inconsistent theory, since everything can be inferred there. Rather *they interest us solely in so far as they match or approximate results of the corrected, consistent theory*. (Norton 2002, 194, my emphasis)

12. As Philip Pearle puts it in his review of classical electron models: "The state of the classical theory of the electron theory reminds one of a house under construction that was abandoned by its workmen upon receiving news of an approaching plague. The plague in this case, of course, was quantum theory" (Pearle 1982, 213).

Inconsistent theories, according to Norton's view, can play a certain heuristic role, but cannot on their own provide us with reasons for accepting any of their consequences. Thus, the inconsistency of Newtonian cosmology, according to Norton, eventually served as a guide to the discovery of a consistent relativistic theory of gravitation, just as the old quantum theory of black body radiation served as a heuristic guide in the development of quantum mechanics. Even though Norton allows for inconsistent theories to play an important role in the process of scientific theorizing, he seems to agree with traditional worries about inconsistency in one important respect. Like the traditional view, Norton does not believe that the *best* theory in a certain domain and an *end-product* of scientific theorizing could turn out to be inconsistent.

Yet classical electrodynamics is not a preliminary theory in the way in which the old quantum theory of black body radiation might be thought to be. Classical electrodynamics has reached a certain stage of completion and appears to be, in some sense, an end product of physical theorizing. But, one might object, has classical electrodynamics not been replaced by quantum electrodynamics? Thus, has classical electrodynamics not been a stepping stone in the history of physics, analogous to Norton's examples? This objection, however, glosses over an important distinction. Classical electrodynamics is no longer regarded as the most 'fundamental' theory governing the interaction of charged particles with electromagnetic fields. In this sense, one might say, it has been 'replaced.' Yet it remains the most successful and most appropriate theory for modeling phenomena in its domain. Trying to use quantum electrodynamics to model classical phenomena—that is, phenomena characterized by classical length and energy scales—would be grossly inadequate, if it were possible at all. As far as the modeling of classical phenomena is concerned, quantum electrodynamics has not replaced the classical theory; rather it has helped to establish limits to the theory's domain of validity and, insofar as the classical theory can be shown to be a limit of the quantum theory, the quantum theory allows us to explain certain salient features of the classical theory. Nevertheless, in justifying the use of the classical theory in its domain scientists do not appeal to a quantum theory. By contrast, the old quantum theory of black body radiation is no longer regarded as the best theory for modeling atomic phenomena and has been replaced *in its domain of application* by quantum mechanics. Similarly, Newtonian *cosmology* has been replaced by Einstein's general theory of relativity, despite the fact that Newtonian physics remains the most appropriate theory for the mechanics of medium-sized objects. Classical electrodynamics, unlike the theories usually discussed by philosophers interested in inconsistency, is what Fritz Rohrlich calls an *established theory*—that is, a theory with known validity limits that coheres well with other theories and is empirically well supported within its domain (Rohrlich and Hardin 1983; Rohrlich 1988).

Moreover, unlike in the case of Newtonian cosmology, the considerations that block the derivation of arbitrary conclusions in classical electrodynamics cannot be construed as a guide to a potentially consistent theory—in this case, quantum electrodynamics. If anything the relationship has been the reverse historically: One of the main motivations for attempts at arriving at a satisfactory and consistent classical theory of point charges was that some of the same problems faced by the classical theory reemerge for quantum electrodynamics. The hope was that a consistent classical theory could then function as a guide for constructing a consistent quantum theory. This hope has not been fulfilled.

There is no conceptually unproblematic classical particle equation of motion fully consistent with the Maxwell equations and energy conservation from which the Lorentz force equation could be derived as an approximation. But can we not think of the Maxwell-Lorentz theory as an approximation to an as yet undiscovered future theory? That is, could we not, in keeping with Norton's suggestion, assume that the consequences of the inconsistent theory are only accepted provisionally, recognizing that if they cannot ultimately be backed up by a "corrected, consistent theory," then they can be of no interest to us and should be discarded?

This final suggestion cannot, however, account for the important role of the Maxwell-Lorentz theory (and of particle-field theories more generally) in modern physics. The theory has in the last century become a central part of modern physics, and one or more courses on some amalgam of the particle theory and the continuum theory form an integral part of any physics student's education. The particle theory is predictively extremely successful, even if the goal of developing a fully consistent and conceptually unproblematic theory has proved elusive so far. Thus it seems that the results of classical electrodynamics within its domain of application, similar to those of classical mechanics, are here to stay quite independently of whether or not physics will ever be able to solve the foundational problems posed by particle-field theories. The results of the existing classical theory are of interest to us and, I submit, the classical theory has explanatory power, even if physicists never develop a corrected, consistent theory.

5. Conclusion. Despite the fact that classical electrodynamics of charged particles interacting with electromagnetic fields is inconsistent, it offer us a picture of the world that goes beyond merely providing a disunified set of instrumentally successful models of individual phenomena. According to the theory there are electromagnetic fields, which causally interact with charged particles. Fields carry energy and momentum and the interactions between electric charges and fields satisfies various locality principles. The theory provides us with a contentful account of 'what the world is like' without, however, delineating a coherent class of physically possible worlds, since the mathematical tools available for making this account precise do not allow us to construct consistent models of charged particles interacting with electromagnetic fields. And just as physicists can learn what the theory says about the world without worrying (too much) about the consistency of the theory, it is possible to investigate many aspects of the conceptual structure of the theory philosophically in interesting and fruitful ways, as for example Lange has done (2002), without ever even mentioning problems concerning the theory's consistency.

Contrary then to what many philosophers still seem to consider 'philosophical common sense,' a theoretical scheme can be inconsistent and yet be successful. What is more, inconsistencies can play a role far beyond that of being a provisional guide to the development of consistent successors. Even without the certain prospect of a 'correct' theory waiting in the wings, very good, and interesting physics can be done with an inconsistent theory. Finally, the commitment of physicists to such theories need not be restricted to a single consistent subset of the theories consequences. How is this possible? I have argued that the worry about inconsistent theories can at least in part be attributed to what I take to be a mistaken view on theory acceptance. If we replace a commitment to the literal truth of a theory's empirical consequences with a commitment to a theory's reliability, then content-driven constraints on permissible derivations can ensure that accepting an inconsistent theory need not violate our standards of rationality.

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