Comment on *Phys. Rev. D* **60** 084017 "Classical self-force" by F. Rohrlich

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Abstract

F. Rohrlich has recently published two papers, including the paper under review, advocating a particular delay-differential equation as an approximate equation of motion for classical charged particles, which he characterizes as providing a "fully acceptable classical electrodynamics". This Comment notes some mathematical and physical problems with this equation. It points out that most of the claims of these papers are unproved, while some appear to be false as stated.

1 Introduction

The correct equation to describe the motion of a charged particle in flat space-time (Minkowski space) has long been a matter of speculation and controversy. The most-mentioned candidate has been the Lorentz-Dirac equation, written here for units in which light has unit velocity and metric tensor of signature $[+,-,-,-]$:

$$m\frac{du^i}{d\tau} = qF^i{}_\alpha u^\alpha + \frac{2}{3}q^2\left[\frac{d^2u^i}{d\tau^2} + \frac{du^\alpha}{d\tau}\frac{du_\alpha}{d\tau}u^i\right] \tag{1}$$

This is written in traditional tensor notation with repeated indices summed and emphasized in Greek; $u = u^i$ denotes the particle's four-velocity, m and q its mass and charge, respectively, τ its proper time, and F_{ij} the (antisymmetric) tensor describing an external electromagnetic field driving the motion.

However, many objections have been raised to this equation. Among them are the existence of "runaway" solutions for which the acceleration increases exponentially with proper time even when the external field asymptotically vanishes. Indeed, in some physically reasonable situations, *all* solutions are runaway [1]. Even in favorable cases in which non-runaway solutions exist, they may exhibit "preacceleration", in which the particle begins to accelerate *before* the external field is applied.

F. Rohrlich [10] [9] has recently advocated a new (approximate) equation of motion which [10] says "is to replace" the Lorentz-Dirac equation. This new equation is a delay-differential equation, given below as (2), which we shall call the "DD equation".

Rohrlich claims without proof that the "great virtue" of the DD equation is that it has "no pathological solutions" [10], in particular "no runaways and no preaccelerations" [9]. We shall explain why these claims are at best optimistic and at worst false. Detailed proofs for all the statements we shall make can be found in the ArXiv paper www.arxiv.org/gr-qc/0205065, which was judged mathematically too technical for this journal.

2 Initial conditions for the DD equation

The DD equation, in the above notation, is:

$$m_1 \frac{du^i}{d\tau} = f^i(\tau) + m_2[u^i(\tau - \tau_1) - u^\alpha(\tau - \tau_1)u_\alpha(\tau)u^i(\tau)] \quad . \tag{2}$$

Here $f(\tau) = f^i(\tau)$ is a four-force orthogonal to $u(\tau)$, m_1 and m_2 are positive parameters associated with his motivation of the right side as an approximation to the self-force on a spherical surface charge, and the delay τ_1 is a positive parameter. (The sign of the second term in brackets differs from Rohrlich's because our metric is opposite in sign to his.) Below we shall omit the indices on four-vectors like u where no confusion is likely.

This is easier to discuss if we write it in simpler but more abstract form as

$$\frac{du}{d\tau} = g(u(\tau), u(\tau - \tau_1), \tau) \quad , \tag{3}$$

where g is a given function. The g corresponding to (2) is

$$g(y, w, \tau) := \frac{f(\tau)}{m_1} + \frac{m_2}{m_1}[w - (w^\alpha y_\alpha)y] \quad , \tag{4}$$

but the precise form of g will not play an important role in the discussion.

The possible dependence of $g(y, w, \tau)$ on w in (3) is what makes (3) a *delay*-differential equation rather than an ordinary differential equation. If $g(y, w, \tau)$ happens to be independent of its second argument w, then (3) becomes an ordinary differential equation whose solutions are uniquely determined by prescribing the values of $u^i(\tau)$ at some initial time τ_0, say $u^i(\tau_0) = c^i$ where c^i is given. Thus in this case, the solution space is a manifold of dimension 4, parametrized by choice of initial conditions c^i.

In the case of a hypothetical equation like $du/d\tau = g(u(\tau), \tau)$ for the four-velocity u of a particle, one has to adjoin the condition $u^\alpha u_\alpha = 1$, which must be satisfied by any four-velocity. This reduces the dimension of the solution manifold from 4 to 3; we omit the details. And this is consistent with physical expectation—there ought to be precisely one solution for each initial three-velocity. (When $u^i(\tau) := dz^i/d\tau$ is integrated to obtain the particle's wordline $z^i(\tau) \in R^4$, four more arbitrary constants appear.)

But if $g(y, w, \tau)$ is *not* independent of w, as in the DD equation (2), the nature of the solution manifold changes drastically. Barring unlikely degeneracies, it is expected to be *infinite*-dimensional, and this is indeed the case for the DD equation. This is because to obtain a unique solution to (3), we need to specify not just an initial value $u(\tau_0)$ but the values of $u(\tau)$ over an *entire interval* of length τ_1, say $[\tau_0 - \tau_1, \tau_0]$, subject to the consistency condition

$$\frac{du}{d\tau}(\tau_0) = g(u(\tau_0), u(\tau_0 - \tau_1), \tau_0) \quad . \tag{5}$$

For brevity, when we speak of a "specification of $u(\tau)$ on $[\tau_0 - \tau_1, \tau_0]$", we shall understand this to include the consistency condition. It is also understood that the specification of u is to make u a possible four-velocity, i.e., $u^\alpha(\tau)u_\alpha(\tau) = 1$ and $u^0 \geq 1$ for $\tau_0 - \tau_1 \leq \tau \leq \tau_0$).

To understand this intuitively, imagine how one could solve (3) given the values $u(\tau)$ on an interval $[\tau_0 - \tau_1, \tau_0]$. First substitute these values in (3) for $\tau_0 \leq \tau \leq \tau_0 + \tau_1$, thus obtaining an *ordinary* differential equation for $u(\tau)$ on $[\tau_0, \tau_0 + \tau_1]$. Solving this equation gives the solution $u(\tau)$, which was initially known only on $[\tau_0 - \tau_1, \tau_0]$, on the new interval $[\tau_0, \tau_0 + \tau_1]$. Repeating the process gives a unique solution on $[\tau_0, \infty)$.

Solving (3) backwards in time, i.e., for $\tau < \tau_0$, requires additional hypotheses on g. Appendix 5 of [7] shows how to solve the DD equation backward in time. The same method works for a non-relativistic approximation (7) to the DD equation to be discussed later. Henceforth, we specialize the discussion to these two equations.

In summary, to each specification of $u(\tau)$ on a given interval $[\tau_0 - \tau_1, \tau_0]$ of length τ_1, there corresponds a unique solution of the DD equation (2) on $(-\infty, \infty)$, and the same is true for its nonrelativistic approximation (7). We shall call such a specification a "generalized initial condition".

This implies that the DD equation has far more solutions than physically expected—we cannot find a unique solution satisfying an ordinary initial condition $\mathbf{u}(\tau_0) = \mathbf{u}_0$, where $\mathbf{u} := (u^1, u^2, u^3)$ denotes the space part of $u = (u^0, u^1, u^2, u^3)$. In any practical situation, we will not know which of the infinitely many solutions satisfying $\mathbf{u}(\tau_0) = \mathbf{u}_0$ will accurately describe the particle's motion.

One might counter that in principle, we could observe the particle's behavior over a time interval of length τ_1 and use that as a generalized initial condition. But in Rohrlich's motivation of the DD equation, the delay τ_1 is the particle's diameter, the time it takes for light to cross the particle. Such an observation seems hopeless in practice, and arguably impossible in principle.

To obtain a physically reasonable or useful theory, additional conditions forcing the solution manifold down to 3 dimensions (or at the very least, to a finite number of dimensions) seem necessary.[1] Such conditions should be provided before proponents of the DD equation can reasonably propose it as a sensible replacement for the Lorentz-Dirac equation.

There is one special case in which there is a unique reasonable choice of such auxiliary conditions—the case in which the force f in (2) is applied for only a finite time. This will be discussed in the next section. But no such conditions are obvious or have been proposed for general forces. A discussion of some of the mathematical considerations is given in Appendix 1 of [7].

3 The case of a force applied for only a finite time

Suppose that the force $f^i(\tau)$ in the DD equation is nonzero for only a finite time, say $\tau_0 < \tau < \tau_2$. Then a solution u of the DD equation (2) is called *preaccelerative* if $du/d\tau$ does not vanish identically on $(-\infty, \tau_0]$, and *postaccelerative* if $du/d\tau$ does not vanish identically on $[\tau_2, \infty)$. A more general definition of preacceleration applicable to forces active for all time (as are most forces in nature) is given in [7]; the simpler definition just given was introduced for expositional simplicity.

Preaccelerative solutions are generally considered physically unreasonable, and postaccelerative solutions also seem physically questionable. For example, suppose we are sitting in a room shielded from electromagnetic fields watching a beam of identical charged particles shoot in the window. It might seem strange if some of the particles speeded up, while others slowed down, for no apparent reason, according to their past histories. We shall see that this is what the DD equation predicts.

The solution u will be called "runaway in the past", or "past runaway" if the acceleration $du/d\tau$ does not approach zero as $\tau \to -\infty$, and "future runaway" if $du/d\tau$ does not approach zero as $\tau \to \infty$.

These are not quite the usual definitions of "runaway"—more typically, "runaway" is used in the sense of exponential increase. We use the weaker definition because it is only under this definition that assertions given below concerning runaway solutions have been proved; nevertheless, it seems probable that these assertions would also hold under the more usual definitions.

In the special situation under consideration (a force applied for only a finite time), past runaways are automatically preaccelerative, so a generalized initial

[1] The solution manifold is physically expected to be three-dimensional in a context in which the external force $f(\tau)$ is regarded as given, *a priori*, as a function of proper time τ. In other contexts, such as a force caused by a space-dependent electric field, one has to rewrite the equation of motion as an equation for the worldline instead of the four-velocity, in which case the solution manifold would be expected to be seven-dimensional.

We remark in passing that the solution manifold of the Lorentz-Dirac equation is also regarded as too large because it is second order in u, so that to obtain a solution, one needs to specify an initial acceleration along with the usual initial conditions of initial position and velocity. Some of the pathologies of that equation arise from this circumstance.

condition which eliminates preacceleration also guarantees that the solution cannot be runaway in the past. However, for the more general definitions of [7], this may not be true.

Rohrlich [9] claims without proof that the DD equation has

> "no unphysical solution, no runaways, and no preaccelerations."

We shall indicate why these claims are at best optimistic, and at worst false.

The claim of "no preaccelerations" is false as stated. Suppose $f^i(\tau) = 0$ for $\tau \leq 0$. Then the generalized initial condition necessary to prevent preacceleration is

$$u(\tau) \equiv \text{constant} \quad \text{for } -\tau_1 \leq \tau \leq 0. \tag{6}$$

This condition is obviously necessary (because if there is no preacceleration, $du/d\tau$ vanishes identically on $(-\infty, 0]$ by definition), and we'll see below that it is also sufficient.

Given a solution u of the DD equation on $[-\tau_1, 0]$ it is a routine exercise to solve for u on $[-2\tau_1, -\tau_1]$, and, by iteration, on any interval $[-(n+1)\tau_1, -n\tau_1]$ for any given positive integer n. The analysis is particularly easy for the case of motion in one space dimension, and is explicitly done in [7]. For simplicity, we restrict the rest of the discussion to this case.

The result when u satisfies (6) is that u is constant on $(-\infty, 0]$. When u does not satisfy (6), a closed form solution on $(-\infty, 0]$ may be difficult to obtain, but nevertheless one can show [7] that u is both preaccelerative (obviously) and runaway in the past (not obvious, but provable).

In summary, (6) is the *unique* generalized initial condition to prevent preacceleration and past runaways. This shows that the claim [9] that "[the DD equation has] no preaccelerations" is false as stated.

In this special situation, this is not so serious—we can repair the claim of no preaccelerations by simply imposing the correct generalized initial condition (6). Unfortunately, this repair is not available for forces which do not vanish for large negative times, and there seems no obvious alternative repair.

More serious, perhaps, is that it is not generally possible to eliminate preacceleration and postacceleration simultaneously. The proof, given in [7], is not difficult. The idea is that preacceleration is eliminated by solving forward in time from a condition (6) of zero acceleration in the past, while postaccelertion is eliminated by solving backward in time from a condition of zero acceleration in the future, and there is nothing in the mathematics forcing these two solution methods to produce the same solution.

For example, for the DD equation (for motion in one space dimension) with a force $f(\tau)$ which vanishes off the interval $0 \leq \tau \leq \tau_1$, one can show that the solution is *always* either preaccelerative or postaccelerative except in the special case of identically vanishing force. This example may be physically uninteresting (because the delay parameter is expected to be so small), but it well illustrates how elimination of preacceleration and postacceleration are basically independent and usually incompatible.

For forces supported on larger intervals, solutions which are not preaccelerative may happen to exhibit no postacceleration, but this requires the force f to be exquisitely "fine-tuned". For most forces, solutions which are preaccelerative will exhibit postacceleration. Thus if one considers postaccelerative solutions "unphysical", it is hard to see how the claim of "no unphysical solution" could be repaired.

The claim of no runaways turns out to be true for the special case in which

(a) The particle's motion and the applied 3-force are in one spatial dimension (e.g., the x-axis);

(b) the force is applied for only a finite time; and

(c) the correct initial condition (6) is imposed.

We have already noted the nonexistence of past runaways under these hypotheses. However, it turned out to be surprisingly difficult to prove the nonexistence of future runaways. (This is the only result of real mathematical substance in [7].)

It does not look routine to extend the proof to general three-dimensional motion. Thus the claim of "no runaways" for general three-dimensional motion remains to be proved, even for the special case of a force applied for only a finite time. And, all the above claims of [10] and [9] not covered by the above remarks remain to be proved for general forces.

4 The analysis of a nonrelativistic version of the DD equation by Moniz and Sharp

The only evidence offered by [10] or [9] for its claims of no preaccelerations, runaways, or unphysical solutions is a 1977 paper of Moniz and Sharp [5] analyzing the following non-relativistic version of the DD equation:

$$\frac{d\mathbf{v}}{dt} = \mathbf{h}(t) - b[\mathbf{v}(t-\tau_1) - \mathbf{v}(t)] \quad , \tag{7}$$

Here $\mathbf{v}(t)$ represents the particle's three-dimensional velocity at time t, $\mathbf{h}$ is a force-like term (a three-dimensional force divided by certain constants), and b is a constant. This can be obtained from the DD equation by replacing the right side by an approximation which is at most of order $|\mathbf{v}|$ (i.e., expand in power series and delete terms of quadratic or higher order in the velocity), rearranging algebraically, and renaming the term involving the force.

Note that this is what one might call a "linear" delay-differential equation, and so is vastly simpler than the DD equation (2). The linearity of (7) enables Moniz and Sharp to analyze it using Fourier transforms, a technique which is difficult to adapt to nonlinear equations.

Moniz and Sharp [5] make claims similar to the above-cited claims of [9] regarding nonexistence of preaccelerations and runaways:

> "Summarizing, we have found that including the effects of radiation reaction on a charged spherical shell results neither in runaway behavior nor in preacceleration if the charge radius of the shell $L > c\tau \ldots$"

(Their condition $L > c\tau$ translates in our notation to a condition that the delay parameter τ_1 be at least as large as a certain positive constant, whose value is not relevant here.) But the truth of these claims does not imply the truth of similar claims for the more complicated, nonlinear, DD equation.

Moreover, even Moniz and Sharp's claims may not be entirely true and, and those that may be true are not entirely proved in their paper [5]. Appendix 3 of [7] analyzes precisely what their mathematics does prove.

It points out a serious flaw in their proof of the nonexistence of runaways, and it looks unlikely that this proof can be repaired. It also notes that while they do write down a formal[2] Fourier transform of a solution which eliminates preaccelerations, their proof is incomplete because the Fourier transform which they furnish for their formal solution is *unique*, leaving no room to satisfy arbitrary initial conditions $\mathbf{v}(t_0) = \mathbf{v}_0$. It seems possible that this proof could be completed, but it seems unlikely that its techniques could be extended to apply to the nonlinear DD equation (2).

5 Appendix 1: Referees' reports

The above "Comment" paper was submitted to Physical Review D (PRD). It was rejected. The chronology of the rejection is as follows.

1. PRD invited Professor Rohrlich to submit his views on my "Comment" paper. PRD communicated these to me as reprinted below.

   Note: What Professor Rohrlich calls the "Caldirola-Yaghjian equation" is the DD equation. Also, the italization is his.

   > "Subject: Your manuscript DUK893 Parrott
   >
   > From: Physical Review D
   >
   > Date: Mon, 14 Feb 2005 16:12:25 UT
   >
   > Dear Dr. Parrott:
   >
   > In accordance with our usual policy for Comments, your manuscript "Comment on "Classical self-force"" (DUK893) was sent to an author of the work being commented on. The author's reaction is enclosed for your consideration.
   >
   > If you choose to respond, your Comment will be considered further. An independent referee will be consulted if needed. Please

[2] "Formal" is used in the mathematical sense of "algebraic", with the implication that analytical subtleties are not addressed. For example, they write down an expression (containing singularities) for the Fourier transform of the solution, but do not consider the question of whether there actually is a solution with this Fourier transform.

accompany any resubmittal by a summary of the changes made, and provide a brief response to all recommendations and criticisms.

Sincerely,

D. Nordstrom

Editor

Physical Review D

Response from Dr. Rohrlich

My comments to paper DUK893 by S. Parrott, "Comments on PR 60, 08017 (1999)..." by Rohrlich are as follows.

This paper gives the results of a mathematical analysis of an equation for a classical charge of finite radius first proposed not by Rohrlich but by Caldirola in 1956 and then derived by Yaghjian in 1992 (see references in Rohrlich). The author calls it the "DD equation" since it is a differential-*difference* equation. This equation is a relativistic generalization of a non-relativistic equation for finite radius analyzed by Moniz and Sharp (ref. 1). Since in the latter paper, the un-physical preacceleration solutions are shown to be absent under suitable conditions, the same was expected for its relativistic generalization.

In the paper under review, Parrott shows that this is *not* the case for the Caldirola-Yagjian equation. This is disappointing but not entirely surprising: the Caldirola-Yagjian equation has the Lorentz-Dirac differential equation as its limit (for zero radius). The latter *does* have those defects.

While this is deplorable, *it is also physically irrelevant* for the following reason.

Since the publication of the criticized papers, *the preacceleration problem has been solved* for the point particle case. Therefore, there is no longer any need trying to repair it by using a finite radius. A physically meaningful differential equation of motion (rather than a 'DD equation') for a charged particle *now does exist* for classical physics. It has no pre- (or post-) acceleration solutions.

That result was *first justified mathematically* five years ago by H. Spohn, Europhysics Letters 50, 287-292 (2000). He showed that the Lorentz-Dirac equation has in its solution space a critical manifold to which all physical solutions must be restricted. Within the validity domain of classical physics, this restriction can be accomplished by approximating the Lorentz-Dirac equation by the Landau-Lifshitz equation of 1962. That equation can also be found in J. D. Jackson's well-known text "Classical Electrodynamics", 4th edition, p.772.

The paper under review, though correct, is therefore no longer of physical interest. My recommendation is therefore not to publish this paper.

Sincerely,

Fritz Rohrlich "

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2. I submitted a reply which is reprinted in Appendix 2.

3. PRD then sent the "Comment" paper to an anonymous referee, with the following result. Note that the anonymous referee entirely ignored Rohrlich's claims to the effect that Spohn's work solves the problems which the DD equation was supposed to solve, thereby making the DD equation "no longer of physical interest". Also, the Editor's rejection letter ignores those claims. Thus I think it fair to say that the paper was rejected solely because the editors accept the anonymous referee's opinion that the entire subject of equations of motion for classical charged particles is "not of physical interest".

   The referee's report is reprinted exactly as received. In particular, the formatting and any typos (such as the referee's misspelling of "physicist") are as in the original. (The other messages are also verbatim, but they are formatted by LaTeX. Since the referee's report may not (or may) have been intended to be all one paragraph, which is how LaTeX would have formatted it, I thought it best to print it with the original formatting.)

   "Subject: Your manuscript DUK893 Parrott

   From: Physical Review D

   Date: Thu, 24 Mar 2005 18:49:04 UT

   Dear Dr. Parrott:

   Your manuscript "Comment on 'Classical self-force"' (DUK893) has been reviewed by an independent referee. Comments from the report are enclosed.

   We regret that in view of these comments we cannot accept the paper for publication in the Physical Review.

   Sincerely,

   Dennis Nordstrom

   Editor

   Physical Review D

Report of an Independent Referee

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This paper, though possibly correct, is not of physical interest, and
should not be published in Phys.Rev.D.  The Lorentz-Dirac equation,
although named after 2 eminent phycicists, is not god-given. On the
contrary, it has so many physical deficiencies that the question only
is by which other equation it has to be substituted under which
physical circumstances. In my judgement, the integro-differential
equation, mentioned in Jackson's Chap.17.6, and presumably first
derived independently by Haag and Sokolow in 1955, is a good
candidate. Also the Landau-Lifschitz equation, ``rederived'' in Spohn's
work may be reasonable under appropriate physical conditions. (And it
is of no interest whether this equation is a controlled or
uncontrolled approximation of the unphysical Lorentz-Dirac equation!)
As always in physics, the final decision comes from experiments.  But,
as discussed by Spohn, unfortunately no clear cut (classical!)
experiment seems to exist presently.
As long as these physical questions are not settled, it does not make
much sense to discuss mathematical subtleties of the different
equations for accelerated charges, and the interrelations between such
equations due to (more or less controlled) approximations.
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6 Appendix 2: Author's reply to Professor Rohrlich's report

The following was sent to PRD in response to Professor Rohrlich's report. Presumably, PRD sent it to the anonymous referee.

Author's reply
to
the report of F. Rohrlich

6.1 Introduction

The Lorentz-Dirac equation is an equation of motion for charged particles which was originally derived in order to ensure conservation of energy-momentum of a charged particle together with that of the electromagnetic field. Its derivation requires the controversial principle of mass renormalization, but otherwise is mathematically rigorous and free of approximations. Because of the rigor of its derivation, for about sixty years it has been the most-mentioned candidate for such an equation of motion.

The reason it is only a candidate, rather than an accepted physical principle, is that it has consequences so strange that no one will admit to believing them. Some of these are:

1. It admits solutions which are "runaway" in the sense that the particle's energy increases exponentially with proper time even when the external field driving the motion is only applied for a finite time. In favorable cases, runaway solutions may be eliminated by appropriate choice of initial conditions, but in some physically reasonable situations, *all* solutions are runaway.

   For instance, this is the case for an electron which starts at rest and moves radially in the Coulomb field of a stationary proton, a fact proved by Eliezer in 1943 [1]. He showed that, according to the Lorentz-Dirac equation, the electron is not attracted to a collision with the proton as expected. Moreover, in the asymptotic future, the electron flees outward from the proton with exponentially increasing acceleration. In particular, *this contradicts the basic notion that unlike charges attract* each other, a conclusion which no one seems to believe.

2. If a charged particle moves in an electromagnetic field applied for only a finite time, some solutions of the Lorentz-Dirac equation are "preaccelerative", meaning that the particle begins to accelerate *before* the field is turned on. This violates basic ideas of causality.

   Sometimes, it is possible to choose initial conditions such that the solution is not preaccelerative. However, it is not generally possible to simultaneously eliminate both preacceleration and runaways. In "generic" situations, eliminating one guarantees the other.

3. There are also "postaccelerative" solutions for which the particle accelerates *after* the field is turned off. Indeed, runaway solutions are automatically postaccelerative for a force applied for only a finite time.

These do not exhaust the "unphysical" consequences of the Lorentz-Dirac equation, but they are all we need to consider here. Because the Lorentz-Dirac equation has so many unbelievable consequences, many authors have proposed replacements for it.

F. Rohrlich's *Phys. Rev. D* (PRD) paper "Classical self-force" [10] proposes a particular equation of motion as a replacement for the Lorentz-Dirac equation. My paper calls this equation the "DD equation".[3] Rohrlich's PRD paper "Classical self-force" claims that the DD equation has "no pathological solutions". A similar paper of Rohrlich in another journal [9] makes clear that this means in particular "no runaways and no preaccelerations". These claims

[3] An earlier version called the DD equation "Rohrlich's equation" because he was the first to state it in the literature in full generality, but I renamed it after he objected.

were presented without proof as facts, and without any language cautioning the reader that they were merely conjectures.

My “Comment” paper points out that some of these claims are false as stated, but can be made true in special situations by adding additional hypotheses. It points out that it might be possible to reformulate some of these conjectures so that they become true, but that much nontrivial mathematical work would be necessary to put them a sound mathematical footing.

It notes that both preacceleration and postacceleration cannot, in general, be simultaneously eliminated—setting the initial conditions to eliminate one of these usually prevents eliminating the other. The situation is similar to the impossibility of eliminating, in general, both preaccelerative and runaway solutions to the Lorentz-Dirac equation.

6.2 Professor Rohrlich's latest objection

A more lengthy paper of which this “Comment” paper is a summary, an earlier version of [7], was submitted to PRD in November of 2003. Professor Rohrlich served as an identified referee and delivered a report in December, 2003. His report made the following objections to the paper, and only these:

1. The paper called the equation “Rohrlich's equation”, for reasons discussed in the introduction to that paper. He objected to this name, instead attributing the equation to Caldirola and to Yaghjian. I had sent him a copy of the paper over a year previously, but had received no comment, and in particular no objection to the equation's name.

   In deference to his wishes, I renamed the equation the “DD equation” in subsequent versions of the paper. I thought that this had settled the matter, but for reasons unclear to me, he brings it up again in his latest objection to the “Comment” paper, so perhaps I should say something about it.

   Yaghjian did propose basically the same equation in nonrelativistic notation, but he presented it only as an uncontrolled approximation to the Lorentz-Dirac equation without comment on the domain of validity of the approximation. By contrast, Rohrlich advocated it as a providing a “fully acceptable classical nonrelativistic dynamics” [9] which was “to replace” [10] the Lorentz-Dirac equation. Because of this, and because Rohrlich was the first to state it in print in full generality, I originally called it “Rohrlich's equation”.

   The attribution to Caldirola is simply wrong. The equation proposed by Caldirola was of a mathematically different kind, a difference equation involving no derivatives, whereas the DD equation is a delay-differential equation. Solutions of Caldirola's equation are not necessarily solutions of the DD equation. There is no reason to think there is any relation between solutions of Caldirola's equation and solutions of the DD equation.

2. Professor Rohrlich's report objected, incorrectly, that the paper's analysis

> was physically irrelevant because the paper replaces the DD equation as presented by Rohrlich by a superficially different, but essentially equivalent equation, in which certain redundant parameters in Rohrlich's version are collected together as one.

Objection 2 was the *only* substantive objection in that report.

His latest report on the "Comment" paper indicates that he has abandoned Objection 2, does not question the conclusions of the paper, and has given up on the DD equation as a likely replacement for the Lorentz-Dirac equation. He has made this the basis of the following new objection, in which the notes in brackets are mine and the italics his:

> "Since the publication of the criticized papers [presumably he means his papers [9] and [10]], *the preacceleration problem has been solved* for the point particle case. Therefor, there is no longer any need trying to repair it by using a finite radius."
>
> > [Note: The Lorentz-Dirac equation and DD equations are both mathematically equations of motion for a moving point. The moving point could be visualized a a point particle, or as the "center" of an extended particle. Rohrlich [10] [9] motivated the DD equation by considering the particle as a charged sphere with a nonzero radius.]
>
> "A physically meaningful differential equation of motion ... for a charged particle *now does exist* for classical physics. It has no pre- (or post-) acceleration solutions."
>
> That result was *first justified mathematically* five years ago by H. Spohn [[12]].... He showed that the Lorentz-Dirac equation has in its solution space a critical manifold to which all physical solutions must be restricted. Within the validity domain of classical physics, this restriction can be accomplished by approximating the Lorentz-Dirac equation by the Landau-Lifshitz equation of 1962. ..."
>
> "The paper under review, though correct, is therefore no longer of physical interest. My recommendation is therefore not to publish this paper."

I don't agree that the cited work of Spohn [12] solves *any* of the problems with the Lorentz-Dirac equation mentioned in the introduction. Rohrlich's summary of its results is inaccurate and highly misleading.[4]

[4]Spohn's paper [12] does *not* "show" that physical solutions lie on the so-called "critical manifold—this is merely an unproved claim in a sketchily written work which contains significant errors. Even if this claim turns out to be true, it would imply nothing about situations in which there are *no* "physical" solutions (e.g., when all solutions are runaway, as in the conclusion to Eliezer's theorem).

Spohn [12] does *not* show that "within the domain of classical physics", the Landau-Lifshitz equation adequately approximates the Lorentz-Dirac equation on the critical manifold. Spohn's approximation is uncontrolled—he gives no rigorous estimates of the degree of approximation.

Spohn's paper [12] does not even mention preacceleration. It might give a casual reader the impression that it somehow solves the problem of runaway solutions, but a careful mathematical reading does not support any such conclusion. And it certainly doesn't negate Eliezer's Theorem stating physically reasonable circumstances in which the Lorentz-Dirac equation implies that unlike charges repel each other.

What it does do is give a mathematically questionable motivation for an approximate equation of motion proposed over forty years ago by Landau and Lifshitz (and appearing in one of their texts). This equation does admit neither preaccelerative nor postaccelerative solutions, in common with many other replacements for the Lorentz-Dirac equation proposed in the literature over the past sixty years. Also in common with most of these proposed replacements is the fact that the Landau-Lifschitz proposal is an uncontrolled approximation to the Lorentz-Dirac equation.

A survey of such proposed equations (which unfortunately does not include the proposal of Landau and Lifshitz) is given in [6], Section 5.7. Three of them are very similar in structure to the Landau-Lifshitz equation. Their common features are that they are second order in the particle's worldline (instead of third order as is the Lorentz-Dirac equation) and their solutions reduce to inertial (i.e., zero acceleration) motion when the external electromagnetic field vanishes. They are all obtained by approximating the acceleration in certain terms of the Lorentz-Dirac equation by the Lorentz force. *Any* equation with these features automatically eliminates preacceleration, postacceleration and runaway solutions for electromagnetic fields applied for only a finite time.

These equations have been around for decades without achieving substantial acceptance. Most been criticized in the literature for predicting unphysical effects different from those noted above for the Lorentz-Dirac equation.

The Landau-Lifshitz equation has not escaped criticism. For example, it predicts that a charged particle moving in a straight line in a uniform electric field in the direction of that line experiences *no* radiation reaction (and hence presumably doesn't radiate), a conclusion which some physicists regard as contradicting physical observations of *bremsstralung*. (*Bremsstralung*, "braking radiation", is radiation observed when energetic charged particles are slowed by collision with a target.)

If Professor Rohrlich finds the Landau-Lifshitz equation such a compelling resolution of the logical problems of classical electrodynamics, why didn't he mention this in his December, 2003 report on my original paper [7]? And why did he publish [10] and [9] advocating the DD equation?

Surely there must have been some aspect of the Landau-Lifschitz equation which he found less than satisfactory. Although Spohn's work was not published until 2000, after Rohrlich's DD equation proposals [10] and [9], the Landau-Lifschitz equation has been around for over forty years. It is hard to imagine that Professor Rohrlich was unaware of it when he submitted his PRD paper [10], and he was certainly aware both of it and of Spohn's work when he submitted his 2003 report on the earlier version of my paper [7]. Indeed, Spohn's paper [12], published in 2000, specifically thanks him for calling the Landau-Lifschitz equation to his attention:

> "I am most grateful to Fritz Rohrlich for instructive discussions and for the hint that the independently derived Eq. (8) [the Landau-Lifschitz equation, which Spohn had obtained independently as an uncontrolled approximation to the Lorentz-Dirac equation] appeared in Landau and Lifschitz already a long time ago."

Why did Rohrlich's 2003 report on my paper [7] contain no mention of Spohn's 2000 "solution" (as claimed by Rohrlich, not by Spohn) of the "preacceleration problem"? Why did it give no hint that he considered the DD equation "no longer of physical interest"?

This question is not presented in a sarcastic or derisive way. Professor Rohrlich's latest report on my "Comment" paper asks us to accept, *solely* on the strength of his reputation, that Spohn's work solves the problems associated with the Lorentz-Dirac equation (a claim which Spohn himself does not make), thereby making further research in this area "no longer of physical interest". In assessing the validity of this claim, Rohrlich's recent related statements, such as his 2003 report on my paper [7], should be taken into consideration.

Any advocacy of the Landau-Lifshitz equation ought to be based on a detailed analysis of the predictions of that equation, not on the fact that it is obtained as an uncontrolled approximation to the Lorentz-Dirac equation. Given the problems with the Lorentz-Dirac equation noted above, the fact that some equation may approximate it seems a questionable recommendation for that equation.

Spohn's work neither solves any problem with the Lorentz-Dirac equation, nor, in my view, provides convincing motivation for the Landau-Lifschitz equation. An appendix analyzes in some detail what Spohn's mathematics actually establishes, and it it is quite far from what Professor Rohrlich seems to believe. I don't want to become diverted here into an analysis of Spohn's work because it is essentially irrelevant to whether a paper commenting on Rohrlich's PRD [10] proposal of the DD equation is justified.

Rohrlich's PRD paper [10], makes claims for which there was never any real evidence. If these claims remain uncorrected, many readers will uncritically accept them on the basis of his considerable reputation as an expert on the foundations of classical electrodynamics, or on the basis of a common perception that PRD is a reliable journal. Others with an interest in these problems may, like me, invest months of hard mathematical work trying to resolve the validity of these claims, work which is likely to be ultimately wasted. Scientific integrity demands that PRD open its pages to correction of these misleading claims.

6.3 Appendix on the work of Spohn cited by Rohrlich

Professor Rohrlich's report on my "Comment" paper claims that the paper is "no longer of physical interest" because the problem which the DD equation was intended to solve has since been solved by Spohn [12], and therefore renders the

Comment paper (and, presumably, the DD equation) “no longer of physical interest”.

This appendix summarizes the part of the 2000 paper of Spohn [12] relevant to the claim that Spohn has somehow solved “the preacceleration problem”. Rohrlich doesn’t mention the numerous other problems with the Lorentz-Dirac equation, but his report gives the impression that he thinks Spohn has solved those as well.

First of all, Spohn’s paper [12] does not claim to solve any “preacceleration problem”. Preacceleration is not even mentioned in this paper, and nothing in the paper has anything to do with preacceleration. Indeed, Spohn [12] doesn’t directly claim to solve *any* of the generally accepted logical problems with the Lorentz-Dirac equation.

However, it is not hard to imagine that a casual reader might get the incorrect impression that he somehow disposes of the problem of runaway solutions for the Lorentz-Dirac equation. I shall now attempt to explain, without going into technical details, what Spohn’s mathematics actually does show.

The mathematics of his paper is based on 1971 work of Fenichel on singular systems of ordinary differential equations [2] [3] as expounded in the monograph of C. Jones [4]. Spohn’s paper is sketchily written, and had I not been previously acquainted with Fenichel’s nontrivial work, Spohn’s account would have been virtually incomprehensible to me. I would be surprised if many physicists are aquainted with Fenichel’s highly technical mathematics—Spohn’s paper is the first time I have seen it mentioned in the physics literature. I think that anyone *not* previously acquainted with Fenichel’s work would be unlikely to be able to read Spohn’s paper with understanding. The issue here is whether Professor Rohrlich has the mathematical background necessary to fully understand what Spohn has done, and whether he has carefully checked Spohn’s mathematics.

I hope the independent referees will make an effort to determine this. If he continues to maintain that Spohn’s work solves some fundamental problem in electrodynamics, he should be asked to respond to the objections to follow. I would be happy to write them out in greater detail to facilitate such a response.

Now we begin the explanation of Spohn’s application of Fenichel’s results to the Lorentz-Dirac equation. The Lorentz-Dirac equation for a particle of charge q and mass m can be written, in the notation of my paper:

$$\frac{du^i}{d\tau} = \frac{q}{m} F^i{}_\alpha u^\alpha + \epsilon \left[\frac{d^2 u^i}{d\tau^2} + \frac{du^\alpha}{d\tau}\frac{du_\alpha}{d\tau} u^i \right]$$

where $\epsilon := 2q^2/3m$ is a dimensionless parameter which is very small for real particles. Note that for $\epsilon = 0$, the Lorentz-Dirac equation reduces to the Lorentz equation, which describes charged particles neglecting effects of radiation. Note also that ϵ multiplies the term of highest differential order, namely $d^2u^i/d\tau^2$ of order 2, and that setting $\epsilon = 0$, reduces the differential order of the equation from two to one. A differential equation containing a parameter ϵ, such that setting $\epsilon = 0$ changes the fundamental nature of the equation (e.g., from order 2 to order 1), is called a “singular” equation.

For simplicity, we shall regard $F = F(\tau)$ as given, *a priori*, as a function of proper time τ. For $\epsilon \neq 0$, the above form of the Lorentz-Dirac equation has a solution manifold of dimension eight, which is larger than the physically expected dimension of four (which reduces to three after taking into account the fact that a four-velocity u has to satisfy $u^\alpha u_\alpha = 1$). Thus physically relevant solutions are expected to lie on an invariant submanifold of dimension three within the eight-dimensional manifold of all solutions; here "invariant" means invariant under the solution flow.

Fenichel established (under technical hypotheses) the existence of an ϵ-indexed family of invariant submanifolds M_ϵ, defined for sufficiently small $\epsilon \geq 0$, which vary smoothly with ϵ. It is trivial to obtain such a family for $\epsilon > 0$ (just choose a fixed initial acceleration $du/d\tau(0) = a_0$, and parametrize the manifold by the initial velocities), but if we insist on including $\epsilon = 0$, where the order of the equation abruptly changes, the problem becomes nontrivial.

However, the mathematically nontrivial problem of including $\epsilon = 0$ is of questionable relevance for physics. In the real world, a particular particle of charge q and mass m is associated with a definite "physical" $\epsilon = 2q^2/3m$. This physical ϵ can't be varied.

The invariant submanifolds M_ϵ which Fenichel obtained are called "critical manifolds" by Spohn. Spohn explains the terminology by a claim that all solutions off the critical manifold must be runaway, so that all physically possible solutions must lie on the critical manifold. This claim is unproved in [12] for the Lorentz-Dirac equation, though a "toy" model is presented to make it plausible by analogy.

Next, Spohn presents an argument which claims to prove that (under very special hypotheses given below, which Spohn does not state) solutions of the Lorentz-Dirac equation which *do* lie on the critical submanifold are *never* runaway, but instead have acceleration tending asymptotically to zero in the infinite future (and past). I believe this argument to be fundamentally incorrect.

For one thing, it contradicts Eliezer's Theorem stating that in certain situations (which satisfy Spohn's hypotheses), *all* solutions are runaway. So, either Eliezer's theorem (whose proof has survived careful checks over more than 60 years) must have an erroneous proof, or Spohn's argument must be incorrect.

I am sure that it is Spohn who is incorrect because

1. I can point out the precise places where his argument becomes invalid, and

2. I have carefully checked the proof of Eliezer's Theorem.

Spohn's argument appears to be based on the *assumption*, that the particle's acceleration is bounded on the critical manifold. If so, what his argument actually shows (under his other restrictive hypotheses) is that *if* the particle's acceleration is bounded on the critical manifold, then the acceleration tends asymptotically to zero. But this assumption rules out runaway solutions by hypothesis.

Another problem with the argument is that his "energy balance" equation (equation (6) of his paper), on which his argument is based, seems to require that the external field F be of a very special form. It seems to require the unstated assumption that there exists a fixed Lorentz frame in which F is derivable from a time-independent scalar potential with zero vector potential. This implies that F is a static, pure elecric field in that frame.

For most electromagnetic fields (including all so-called "radiation fields"), it is impossible to choose a fixed Lorentz frame in which F is a pure electric field. A quick way to see this is to recall that the quantities $\mathbf{E}^2 - \mathbf{B}^2$ and $\mathbf{E} \cdot \mathbf{B}$, with $\mathbf{E}$ the electric field and $\mathbf{B}$ the magnetic field, are "invariants" in the sense that they are independent of the Lorentz frame in which they are computed. For "radiation" fields (examples of which are easy to write down explicitly), both of these quantities vanish identically, so such a frame is impossible for nonzero radiation fields. Thus even if the other objections to Spohn's argument could be overcome, the resulting conclusion (that solutions on the critical manifold vanish asymptotically) would be too special to be considered a solution of any fundamental problem with the logical foundations of electrodynamics.

The only other result in Spohn [12] of interest here is that he calculates an approximate equation for the critical manifolds, valid to first order in ϵ. This is then used to obtain the Landau-Lifschitz equation as an uncontrolled approximation to the Lorentz-Dirac equation.

I don't see why obtaining Landau-Lifshitz as an uncontrolled approximation to Lorentz-Dirac using all this fancy mathematics is any better than obtaining it as an uncontrolled approximation in simpler ways. Moreover, since nobody believes the consequences of the Lorentz-Dirac equation listed in the Introduction (e.g., that in some circumstances, unlike charges repel instead of attract), why should one have any confidence in an equation obtained as an approximation to the Lorentz-Dirac equation? And why should it be considered a definitive solution to any of the logical problems of classical electrodynamics?

7 Appendix 3: Remarks and corrections concerning the reply to Rohrlich's report

For historical accuracy, I included the reply to Professor Rohrlich's report verbatim as it was submitted to *Phys. Rev. D* (PRD), including some insignificant typos (e.g., "happly" for "happy"). This section corrects some inadvertent misstatements (luckily minor) in that reply and adds some comments.

After PRD refused to publish the "Comment" paper, I wrote Dr. Spohn to let him know that I intended to include the reply as an appendix to the copy in the ArXiv. The message offered to correct any inaccuracies and to include a reply from him should he care to furnish one. I thank him for subsequent correspondence which deepened my understanding of the intent, assumptions, and conclusions of his approach to the Lorentz-Dirac (LD) equation. I also thank him for sending me a reply in LaTeX format, which comprises Appendix 4.

1. The constant $\epsilon := 2q^2/3m$ in the Lorentz-Dirac equation on page 7 is not dimensionless as stated—it has the dimensions of time. This does not affect any of the conclusions.

2. I objected to Rohrlich's statement that Spohn's paper [12] "showed" (i.e., proved) that "the Lorentz-Dirac equation has in its solution space a "critical manifold" to which all physical solutions must be restricted". Although [12] does give the impression of claiming this (it certainly isn't proved there), Dr. Spohn informs me that this is a conjecture rather than a theorem.

3. Dr. Spohn confirms that his "energy balance" equation (which is the basis of his argument) requires the assumption (unstated in [12]) that there exists a Lorentz frame in which the potentials are static (i.e., time -independent), and hence the field F is static.

   In guessing what additional conditions might be assumed by [12] to justify his energy balance equation, I guessed a condition which was too strong—I guessed that he was assuming that F was static with vanishing magnetic field.

   Assuming only his weaker condition (static F), my previous argument that it is not generally possible to find such a Lorentz frame no longer suffices. However, it is still true that such a Lorentz frame need not exist. One class of fields for which such a Lorentz frame cannot exist consists of fields which in three-dimensional notation (relative to some given Lorentz frame, with three-vectors in boldface) have electric and magnetic fields $\mathbf{E}$ and $\mathbf{B}$ of the form

   $$\mathbf{E}(x) = \mathbf{P}e^{ikx}, \quad \mathbf{B}(x) = \mathbf{Q}e^{ikx} \quad .$$

   Here $\mathbf{P}$ and $\mathbf{Q}$ are constant three-vectors which are orthogonal to each other and to the space part $\mathbf{k}$ of the four-vector $k = (k^0, \mathbf{k})$, and for $x = (x^0, \mathbf{x})$, kx denotes Lorentz inner product $kx := x^0k^0 - \mathbf{x} \cdot \mathbf{x}$. These will satisfy the free-space Maxwell equations if $k^0 = |\mathbf{k}|$ and $\mathbf{k} \times \mathbf{P} = -k^0\mathbf{Q}$. Although these are written as complex fields for algebraic simplicity, similar real fields for which the argument to follow is valid can be obtained by taking real or imaginary parts.

   For $k \neq 0$, these fields are obviously not static (relative to the given Lorentz frame). Moreover, the standard formulas for transforming them to a new Lorentz frame yield fields of the same form (with generally different $\mathbf{P}$ and $\mathbf{Q}$), so they cannot be static in the new frame, either.

4. Dr. Spohn has confirmed that, as I had guessed, the argument of [12] does assume that the acceleration is bounded on the critical manifold. He says that this follows from something in Sakamoto's paper [11]. (Neither the assumption nor its justification by Sakamoto are mentioned in [12].) Since the nearest copy of that paper is probably 200 miles away, I am not in a

position to confirm this.

This argument also requires the unstated assumption that the velocity on the critical manifold is bounded away from the speed of light—I do not know if this also derives from Sakamoto.

5. The argument of [12] requires that the potentials be bounded on the particle's worldline, and Eliezer's theorem does guarantee this for a Coulomb field. This is why I thought that Spohn's results must conflict with Eliezer's theorem for a Coulomb field. However, Dr. Spohn informs me that another unstated assumption of [12] is that the potentials be globally bounded (not just bounded on the worldline). This added assumption removes the formal contradiction between Eliezer's theorem and Spohn's work.

   After learning this, I was still skeptical because there are forms of Eliezer's theorem which apply to the potential of a Coulomb field which is cut off at both large and small distances from its source (e.g., the scalar electric field $E = 1/r^2$ for $r_1 \leq r \leq r_0$ and vanishing elsewhere). (The cutoff can be "smoothed" near $r = r_1$ and $r = r_0$, if desired, to obtain a C^∞ field.) It seemed to me that the conclusion of [12] that solutions on the critical manifold tended asymptotically to zero might be in conflict with such variants of Eliezer's Theorem.

   One of these variants concludes that for a charged particle which enters such a cutoff Coulomb field with sufficiently small velocity, all solutions of the Lorentz-Dirac equation are runaway. However, it requires the additional hypothesis (not required by Eliezer's original theorems) that the initial acceleration (the acceleration when the particle enters the field) must vanish. This hypothesis is equivalent to assuming that there is no preacceleration.

   Under the hypothesis that preacceleration is impossible, the results of [12] seem in direct logical contradiction to this variant form of Eliezer's theorem. Put differently, *all* solutions of the Lorentz-Dirac equation (for a cutoff Coulomb field and sufficiently small initial velocity) are either runaway or preaccelerative. In particular, assuming Spohn's conclusion [12] that solutions on his critical manifold are asymptotic to zero in the future (hence not runaway), these solutions must be preaccelerative.

   Thus it is hard to see what could justify Professor Rohrlich's claim that [12] solves "the preacceleration problem". (It should be emphasized that Dr. Spohn has never made any such claim, to my knowledge, either in print or in correspondence with me.)

   The precise statements and proofs of these variant forms of Eliezer's theorem can be found in www.arxiv.org/math-ph/0505042.

8 Appendix 4: Dr. Spohn's reply

Comment by

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Dr. Parrott kindly asked me to add a comment to the exchange of opinions. My full view on radiative friction is explained in my recent book *Dynamics of Charged Particles and Their Radiation Field, Cambridge University Press, 2004.* Since the exchange of opinions is focused on my 2000 Europhysics Letter, I briefly explain what I did and did not achieve in this contribution.

(i) I point out that the Lorentz-Dirac equation is a singularly perturbed differential equation, a topic on which considerable mathematical knowledge has accumulated. The same observation was stressed by Galgani and coworkers already in 1995. His and more recent studies from this point of view are cited in my book.

(ii) Locally in space-time a singularly perturbed differential equation has a center (or critical) manifold of "slow" motion. Globally this manifold may have a complicated structure. I argue that the physical solutions of the Lorentz-Dirac equation must be on its center manifold, thereby improving on Dirac's asymptotic condition.

(iii) If one expands the center manifold in the small parameter, then to first order the motion on the center manifold is governed by the Landau-Lifshitz equation for radiative friction, which provides a more systematic understanding for a otherwise seemingly *ad hoc* procedure.

(iv) One point in the discussion with Dr. Parrott, also in relation to Eliezer's theorem, is the structure of the center manifold in concrete cases like the motion in an attractive Coulomb potential. This issue leads to the question under what conditions the Landau-Lifshitz equation is a good approximation to the true center manifold motion. In my opinion such points can be clarified only through more detailed investigations. Considered as a low dimensional dynamical system the Lorentz-Dirac equation has many still unexplored features.

Herbert Spohn, May 20, 2005

9 Final thoughts

The mathematical analysis of this paper and its precursor [7] took a few months. The time from submission to final rejection was about three years. In the process, several hundred pages of correspondence were generated, probably requiring more time than did the initial mathematical analysis. It has been a frustrating and exhausting process, and I am relieved that it is finally over.

Before putting this work to final rest in the ArXiv, I have been asking myself what can be learned from this experience. The subsections below summarize my conclusions and raise some questions which I hope others in this field will consider.

9.1 Editorial standards of *Phys. Rev. D* (PRD)

This "Comment" paper was apparently rejected solely because an anonymous referee deems its subject (proposed equations of motion for classical charged particles) as "not of physical interest". Since its subject is identical to the subject of Rohrlich's recent PRD paper [10] on which it comments, this seems to put PRD in the strange position of affirming that Rohrlich's paper [10] which they recently published was so uninteresting that comments on it are superfluous. I wouldn't have believed that the editors of PRD could maintain this with a straight face.

The anonymous referee's characterization may be a defensible personal opinion, but it is certainly not universally accepted. After PRD published Rohrlich's [10] in 1999, it has continued publishing papers on proposed equations of motion for classical charged particles, about one paper a year by my count.

Less than a year ago (Fall, 2004), Cambridge University Press published a book by H. Spohn [13] which prominently emphasizes this subject. It presents his method of obtaining the Landau-Lifshitz equation from the Lorentz-Dirac equation, and it applies the Landau-Lifshitz equation to draw conclusions about the behavior of an electron in a Penning trap.

While it is surely true that not all physicists are interested in this subject, it is also unquestionable that does exist a substantial community which is interested. Some in this community hope that progress in constructing a consistent classical electrodynamics might lead to a consistent quantum electrodynamics. Others hope that new technologies such as Penning traps might make it possible to extract useful predictions from classical equations of motion.

It would be a legitimate editorial decision to refuse to accept all papers dealing with equations of motion for classical charged particles. However, that decision should be made on the editorial level, not arbitrarily by referees. And, if such a policy is adopted, it should be made public and uniformly enforced.

Before I prepared the "Comment" paper, I wrote the Editor of PRD, D. Nordstrom, asking specifically if PRD would consider a "Comment" paper based on the rejected [7]. He did not reply. If the subject of the paper were, *a priori*, unacceptable to PRD, the editors should have informed me of that when I asked.

9.2 Unreliability of the physics literature

The process of researching my book [6] made me painfully aware of the general unreliability of the literature on classical relativistic dynamics. My experience has been that something like a third to a half of all published papers in this field contain errors sufficiently serious to potentially invalidate some of their main conclusions.

The likely explanation is that few papers are carefully read by their referees. The reasons are easy to guess—refereeing is tedious volunteer work which confers inadequate professional rewards. Nevertheless, this is not a necessary state of affairs. For example, the mathematical literature is much more reliable than the physics literature despite a similar refereeing system.

The unreliablity of the physics literature forces serious readers to check all calculations in extraordinary detail. Since many of the calculations in this field are very tedious, this creates an enormous waste of time. Every serious reader has to duplicate this work, to be reasonably sure of the correctness of a paper.

To make matters worse, discovered errors are seldom corrected. The experience of this "Comment" paper clearly shows how difficult it can be to get correction of errors into the literature.

For example, the elementary mathematical errors of Moniz and Sharp's PRD analysis [5] of the nonrelativistic DD equation have gone uncorrected for almost 30 years. Rohrlich's recent PRD article [10] relied on Moniz and Sharp's incorrect analysis to justify, by analogy, its questionable claims about the relativistic DD equation. Given that PRD is evidently unwilling to correct such misleading claims and outright errors, it is easy to imagine that other work based on these errors may continue to appear.

The standards of PRD are, unfortunately, not atypical among physics journals. Given this, little can be done about the unreliability of the literature within the present system.

However, the ArXiv could be a powerful tool for reform because articles posted there are permanently accesible and subject to peer review. Though the peer review is informal, it could easily become more reliable than the current refereeing system for journals. I would place more weight on a citation of an ArXiv paper by someone whom I knew to be competent than on acceptance in a journal like PRD which publishes much erroneous work.

9.3 The role of the ArXiv

Given this experience, I will never submit another paper to PRD. Indeed, I doubt that I shall submit any of my future work for publication to physics journals. As a retired person, publication gives no professional benefit, and it is highly unpleasant to have to deal with incompetent referees and journals which lack scientific integrity. Instead, I plan to post my future work in the ArXiv, and this raises another issue.

I am uneasy about recent changes in ArXiv policies which make it more difficult to place papers there. Some people are required to find an "endorser" for any paper which they wish to post. I am not so required because I was grandfathered in. But if I did need to find an endorser, it might not be easy, and I might not want to try. For example, I am not sure that I could find an endorser for this submission, and I probably wouldn't try.

The endorsement system is apparently meant to eliminate "crackpot" papers. Unfortunately, a likely side effect will be to effectively bar useful work from the ArXiv.

It is not clear to me that significant harm is done by letting a few misguided individuals post papers which some might regard as "crackpot". Also, the line between "crackpot" and "brilliant but inarticulate" is not always clear. Cases of obvious abuse (e.g., if someone attempts to "publish" a novel in gr-qc) could be handled by other means.[5] It does seem clear that if policies result in potentially useful work not being posted, the entire community loses.

Scientific history is full of instances of work which could not find proper publication in its time but which later became generally recognized as valuable. So long as the ArXiv is partially supported by public funds, the public should require that its policies enhance, rather than limit, free scientific expression.

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[5] For example, users of the ArXiv could be asked to inform its administration of any obviously inappropriate submissions, which could be removed by hand. Repeat abusers could be barred from posting anything. There will always be a few problems which cannot be mechanically weeded out, but a few simple rules such as those suggested should cover the vast majority of abuses. And, one would not expect a great number of abuses anyway, given the nature of the ArXiv.

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