Remarks on the Latest Wheeler–Feynman Paper

June 27, 2014

These are my comments to the authors of the paper G. Bauer, D.-A. Deckert, D. Dürr, G. Hinrichs: *On irreversibility and radiation in classical electrodynamics of point particles*, J. Stat. Phys. **154**, 610–622 (2014); `arXiv:1306.3756 [physics.class-ph]` 17 Jun 2013. This paper looks like a clear step forward from the ones by Wheeler and Feynman (at least as far as I understand it), because it puts the cards on the table and we see what needs to be done.

I make the following remarks:

1. Equation (8) on p. 6 is exactly the way I would deal with things. α corresponds to WF's (?). This is the effective field due to the source acceleration event (SAE). The first question is: where will this be effective? Is it just 'near' the SAE, or is it everywhere? If I am not mistaken, one would like it to be effective everywhere for two reasons: firstly, one would eventually like to show that β has to be zero, and then one would have the observed result that SAEs produce retarded effects everywhere (in accordance with experience); secondly, I think it is assumed in the calculation that the relevant retarded effect of the SAE on particles in the surrounding medium (SM) is αF_-, wherever those particles may be.

2. I think this same assumption, that the effective field due to the SAE is $\alpha F_- + \beta F_+$ *everywhere*, leads to a problem when one examines the way the SM reacts by advanced effects, because one ought to assume that each particle P in the surrounding medium, when disturbed by the retarded effect of the SAE, produces an effective advanced effect βF_+^P, and not the bare $F_+^P/2$ used in the present calculation. The authors mention this later, some way down p. 9, but I am not convinced. I will return to this.

3. Talk of refractive index seems fine. As I understand this, it deals in a macroscopic way with what is described in the Feynman lectures as a whole lot of absorption and reemission. The returned advanced effect from SM is almost like a reflection, except that it comes out of the past and reaches the source at the SAE.

4. I like the way the authors then deal with the case $\alpha = 1$, $\beta = 0$, reach the key result of WF which is (14) (if that calculation is valid), then point out by the manipulation at the top of p. 9 that they have a solution. This gets

round the logical problem that WF run into over this point (I described it elsewhere).

5. Is the paragraph "At first sight" on p. 9 really necessary? It seems obvious.

6. The following paragraph "the advanced fields of the surrounding particles are not cancelled by the same reasoning as for the source" is perhaps my main problem. It is good to find the claim stated so clearly. Did WF mention that question? I don't think they noticed this difficulty at all. Here the authors say that "The acceleration of a surrounding charge, in turn, is expected to deviate little from an equilibrium value." But I am just not convinced that this will stop the surrounding medium (SM) from cancelling the advanced effect due to a given particle P in that medium. Surely, electromagnetic effects superpose even in this new theory, and when P is shaken by the retarded effects of the SAE, the extra fields it produces (above noise) will be coherent with those produced by other P' in SM (in response to the retarded effects of the SAE). On top of all the noise will be precisely what is required to cancel the advanced effects due to P (in response to the retarded effects of the SAE) and the theory breaks down, because one was using those advanced effects to cancel the advanced effects due to SAE and boost the retarded effects due to SAE to the full F_- rather than just $F_-/2$.

7. To get round this, the SM has to do something clever, perhaps on a thermodynamic level or something like that. I can't see what. Alternatively, as Deckert suggested to me, one might try to say that effective fields in this case are only effective *near* the acceleration events, not *everywhere*. This then becomes a crucial issue. The problem is that one wanted F_- to be effective *everywhere* throughout the SM in order to do the calculation.

8. It seems to me that, as things stand, one is stuck with assuming that effective fields are effective *everywhere*, and that the advanced reaction of a P in SM is therefore βF_+ rather than $F_+/2$. Then what one obtains in the general case of equation (8) is this. P is affected by αF_- and SM responds with $\alpha\beta(F_- - F_+)$, but P was also earlier affected by βF_+ and SM responds there with $\beta\alpha(F_+ - F_-)$, whence the net response of SM at the SAE is zero. Of course, this model is time-symmetric. Here we conclude that there is only one solution to the WF problem, viz., $\alpha = 1/2 = \beta$.

9. I find it striking that just assuming that SM can 'disregard' or 'stamp out' the effective field phenomenon means that one escapes, at least potentially, from having to accept the time-symmetric solution in the last point. One can at least play with a whole set of solutions $\{\alpha, \beta\}$ such that $\alpha + \beta = 1$, even though it remains to show why one might not have $\alpha = 1/2 = \beta$ in some real world situation. Put another way, without this ability of SM to stamp out the effective field phenomenon, one couldn't even begin to use this idea. But this is the stumbling block for me. I don't see why a

noisy SM would not notice the coherent effects on it from even the small acceleration of one of its members, and return them coherently to that member.

10. Apart from this difficulty, which I hope one can explain away, I like the clean description on p. 9 (end of Sect. 2) of what one hopes will come from equation (8) in the general case. One would have a whole set of solutions to choose from and the only remaining question is: Why should our world generate $\beta = 0$ as the solution we actually see?

11. As an aside, perhaps ridiculous (sic), has anyone ever looked to see if there are advanced effects, i.e., look at a test charge, then shake the source at a suitable later time to see ... ? Would we recognise an advanced effect if we saw one?

12. In Sect. 4, p. 10, we have: "Wheeler and Feynman's computation of radiation damping is completely independent of the precise arrangement of the surrounding particles as well as their physical properties like their particular masses or charges." Equation (14) cries out for explanation. I thought Fink's time-reversal mirror might be somehow related. He mentions WF in his papers and it seems to me that this theory inspired his work. The mirror is like an absorber surrounding the source. The retarded field from the SAE is detected and reversed by computers, returning to the position of the source as another retarded signal, of course, hence arriving after the SAE. It seems like the surrounding medium SM in WF does something like this, but as an advanced signal which can therefore arrive at the source at the SAE. But what you get from WF in response to F_- looks like some F_- and some F_+, so that's different.

13. Why is $\beta = 0$? We have equation (16) on p. 11, a kind of initial condition which holds because "motion of the $j \neq i$ other particles is equilibrium-like before particle i is accelerated". Why does this single out the solution $\beta = 0$? What I understand here is this: If β were not zero, then there would be a coherent advanced effect of the SAE on SM and this would lead to a coherent retarded reaction of SM at the SAE and (16) is there precisely to rule this out. So we select the solution with $\alpha = 1$ and $\beta = 0$. But that leaves open the question as to why that should happen. I have the same problem as in my point (6). Even if SM is randomised, if there were some βF_+ from the SAE, that would have a coherent effect on SM and, amongst the noise, SM would return a coherent reaction to SAE. So I don't feel that the authors' equilibrium-like SM explains why I should accept (16). After all, if SM is equilibrium-like before, why shouldn't it be so after, when αF_- reaches it from the SAE? And that doesn't appear to stop SM reacting in a coherent way with (14) on the SAE.

Those are all my remarks about the paper. I like the way they discuss and dismiss the 'absorber condition'. That's very clear.

Here is a different kind of question. At the beginning of their paper, the authors say: "[WF] is the only theory of classical electrodynamics that has been shown to be capable of predicting radiation phenomena". But is that true? We don't need WF to see why antennas radiate, do we? Surely they should add the words "... predicting radiation phenomena *by point particles*"? I really need someone to spell out to me why the explanation for radiation by considering self-forces in spatially extended charge distributions is not valid.

In response, one of the authors replied like this:

> I think [all these questions] are all but one big question: Why is the treatment of WF consistent and believable, since it seems to break symmetry from the very beginning? What WF do and we repeat in the paper is a consistency check for a particular solution. The action of the atypical SAE on the SM is described effectively by retarded action and then one checks whether this is consistent with the dynamics. The SM's action will be a radiation friction. It is very important and may not be stressed enough in the paper that what is computed (i.e., the back reaction of the SM) is only statistical, and fluctuations are not spelled out or further scrutinized. Therefore the natural question as to why not every SM particle which gets a a kick from the SAE should obey the same effective description is indeed a question of scale. The SAE is clearly out of 'equilibrium' while the SM particles stay 'more or less' in equilibrium concerning their action on the SM/SAE. Their effect is lost within fluctuations. It is as in the Brownian motion of a fast particle which gets some friction (Stokes' law, let's say) from the gas, and although every gas molecule also suffers friction, that 'systematic' part of the motion is hidden by the fluctuations and not considered in the heuristic arguments. I do find it hard at this moment to say things more explicitly, which I guess would actually mean that we would be able to give a microscopic derivation of the radiation friction. We are a long way from that. At the moment we can only ask whether the effect of radiation friction is consistent with WF, where the environment is a bath at equilibrium (at least for the interaction with the SAE). I concede that the situation of the Brownian particle seems more easily accepted, but I feel that in the end the argument in WF cuts the same way. The SM particles do not in total conspire in a way in which the damping of the SAE is cancelled. Perhaps one should start looking first for a radiation friction equation incorporating fluctuations. That could be helpful.

Of course, it is the statement that "the SM particles stay more or less in equilibrium concerning their action on the SM/SAE" that bothers me, because it seems to me that they only need to be somehow coherently slightly off equilibrium, and it seems to me that they would indeed be coherently slightly off equilibrium if stimulated by the acceleration of any particle whatever, no matter how insignificant that acceleration might seem.

It is the nonlinearity that I have not sufficiently understood. In the WF computation the equations of motion of the SM is truncated to just $F \sim a$, which is justified by the acceleration of the SAE being very large. What 'very large' means is not specified at this stage and needs much more scrutiny.