List of Corrections to Ph.D. Thesis
Second Revision

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Abstract

This document describes the small corrections and amendments made to the printed copies of my Ph.D. thesis, required by the two Examiner's Reports.

The amendments described in the first revision of this report have been slightly enhanced, according to comments from Prof. B. H. J. McKellar.

1. Examiner's Report: Gerard J. Stephenson Jr.

This report does not recommend amendment of the thesis, but, given the necessity to do so anyway (due to the second Examiner's Report), and due to the invaluable proof provided by Prof. Stephenson of an important verification result, I have incorporated his suggestions into the corrections and amendments.

He refers to "an exercise for the reader" (e.g., page 140, page 171) for the proof of propositions that he admits are not yet proved. That phrase is usually reserved for points which the author knows to be true, could prove but a) does not wish to devote time and space and/or b) would, in fact, be a useful exercise to assure that the reader has absorbed the material. If I read those portions correctly, I believe that he really means that those propositions are left to future work, most probably not by him.

A valid point. I was using this phrase somewhat tongue-in-cheek, and intended it to be followed by an exclamation mark to convey this meaning. This oversight led to the sentence being interpreted literally. I have corrected the punctuation. **Corrected: page 140, line 8; page 171, line 19.** □

Typo: Abstract, 1st para: "the Dirac equation <u>is</u> also ..."

Corrected: page 1, line 6. □

Typo: page 58: (2.43) has mixed type: LHS is a component, RHS full vector"

Actually, I explicitly defined this form of mixed notation in Sec. A.9.6 (see especially Eq. (A.71)). **No correction required.** □

Typo: page 134, following (4.34): is the phrase "untuitively obvious" correct? I must confess to not understanding it.

This should of course be "intuitively ...". It should also have a colon following it (the following sentence explains *why* it should be obvious). **Corrected: page 134, line 19 (two corrections).** □

Typo: page 166, (4.77): since there are retained terms of order $1/m^2$, *should not the exclusion be* $O(1/m^3)$*?*

Yes. **Corrected: page 166, line 11.** □

Typo: page 225, line 19: "... is not as trivial as ..."

Corrected: page 225, line 19. □

Typo: page 302, 3rd from end: "They obtained ..."

Corrected: page 302, line 27. □

(6.124) is, I believe, all right in the contexts in which you use it. However, the product of a delta function and a Heaviside function is ill-defined and can, in certain situations, lead into difficulty. A case in point was discussed by McKellar and me in Phys. Rev. C35, 2262 (1987), see section IV C.

I am intimately familiar with the subtleties of such products, and have discussed them, and the mentioned paper, with Prof. Stephenson and Prof. McKellar in 1992, and again with Prof. McKellar just before completing the thesis.

I disagree that such products are ill-defined; rather, they are sometimes *used* incorrectly. If one considers the limiting process described in my *definitions* of the Dirac and Heaviside functions in Sections A.5.2 and A.5.3, one sees that there is nothing wrong with having such a product—it is simply the product

$$g(t,\varepsilon)h(t,\varepsilon)$$

[where $h(t,\varepsilon) \equiv \int_{-\infty}^{t} dt'\, g(t',\varepsilon)$] in my "Dirac delta ensemble", *before* the limit $\varepsilon \to 0$ is taken. My apparently flippant prescription that this limit is taken "on the first blank page after the end of the thesis" was actually carefully thought-out, with precisely the abuses pointed out by Profs. Stephenson and McKellar in mind.

Of course, as noted by Prof. Stephenson, the identity in question,

$$\int dt\, \delta(t)\theta(t)f(t) \equiv \frac{1}{2}f(0),$$

being an *integral* equation, over a *smooth* function $f(t)$ (implied by the context of the sentence), is undeniably correct. **No correction required.** □

The integral in (6.37) can be done analytically and does, indeed, give 4. This gives added weight to both your analysis and your programming skill. A sketch of the calculation is *appended on a hand-written sheet.*

I thank Prof. Stephenson greatly for taking the time to derive, and describe, the proof of this result. Although it would be neither appropriate nor permissible for me to add this material to the body of my thesis, I have produced an insert for page 250 containing the details (verbatim) contained in Prof. Stephenson's sheet, and have obtained his permission to attribute the proof to him. **Insert added to page facing page 250.** □

2. Examiner's Report: R. Delbourgo

This report suggests a number of changes, mainly typographical, and also makes several comments about the physics involved.

Page 58: The last paragraph in 2.4.5 didn't make much sense to me.

Prof. Delbourgo does not seem to have grasped the difference between the *canonical* and *mechanical* stress–energy tensors; this is reflected in one of his later (incorrect) comments. This is probably why the given paragraph did not make sense to him. I have, however, added a reference to *Noether's theorem*, to make the statement about conservation theorems more precise. **Note added to page 57, line 26.** □

Page 64–5: The order of presentation is queer. The 2.6.2 stuff should have preceded (2.19).

I agree. It is difficult, in a large work, to ensure that all precedents appear in the correct order. I have added a note to Eq. (2.19) to direct the reader's attention to the later sections. **Note added to page 47, line 5.** □

Page 83: The last part was quite confusing.

It is a confusing issue. I would welcome a more transparent explanation. **No feasible correction known.** □

Page 106: Near the top the author gives details about LaTeX *which are scientifically totally irrelevant.*

But important to any reader. Prof. Delbourgo would have been the first to point out my lack of boldface as "typos" had I not explained its absence. **No correction required.** □

Page 108: The last part is again confusing.

It is, again, a subtle and confusing issue. My explanation is the best that my understanding would allow. **No feasible correction known.** □

Page 124: Thank goodness $\dot{s} = 0$ *since the magnitude is fixed at* $\hbar/2$.

Good to know that Prof. Delbourgo has grasped this point. **No correction required.** □

Page 126: The middle (historical) paragraph detracts from the continuity of the reading.

I felt it was important to point out, clearly and explicitly, that my paper published on the topic, included in Appendix F, contained results found to be incorrect, by the reasoning of the chapter in question.

I consider scientific honesty more important than continuity.

Page 132: What on earth is the difference between an Engineer's current and a Physicist's? Another affectation.

In Physics, the term "current" generally refers to a current *density*—especially in the realm of electrodynamics and field theory. On the other hand, in Engineering, the term "current" refers to the total charge per unit time passing through a given two-dimensional surface.

In the Physics literature, one generally finds that the qualified terms "current *density*" and "*total* current" are used to disambiguate these two notions. However, because I only needed to use the latter concept on one occasion (the section in question), I instead chose to add the adjective "Engineer's" in a commonsense way.

I have added a note ("total current") to page 132 to clarify this definition. **Note added to page 132, line 6.** $\square$

Page 140: It is presumptuous of the author here (and in other places) to leave loose ends, as an "exercise for the reader".

Point taken. See Sec. 1. **Corrected in Sec. 1.** $\square$

Page 150: The bracketed sentence at paragraph end is misleading: in the true dipole limit ($\varepsilon \to 0$), the electric field lines for an electric dipole become closed too.

I disagree. Just because the distance between the poles shrinks to infinitesimal size does not in any way mean that the lines of electric field are "closed"; indeed, the divergence of the electric field never vanishes. **No correction required.** $\square$

Page 153: Bottom. I believe that one can readily modify the Lagrangian description to include magnetic charges and currents. See the work by Zwanziger.

I have. I maintain that, to describe both electric *and* magnetic charge in a Lagrangian framework, in agreement with the Maxwell equations (extended to the case of both types of charge), you must introduce a non-trivial structure for spacetime (equivalently, you must give up the concept of a unique potential at any point in spacetime).

I would willingly discuss the pros and cons of such a topic with Prof. Delbourgo in more detail, in a more appropriate place. **No correction required.** $\square$

Page 161: The particle–antiparticle number is not the same as β.

Agreed. This had already been pointed out to me by Prof. McKellar. I had in mind the Newton–Wigner representation, in which β does fulfil this role. I have crossed out the incorrect parts of this sentence. **Corrected: page 161, line 19.** $\square$

Page 230: Last para onwards. If the author feels that strongly about the Barut–Unal claims, he should forward his complaints to them and ask them to defend themselves.

I have previously attempted to initiate a meaningful correspondence with Barut. I am not sure that Prof. Delbourgo's suggestion would lead to any useful outcome.

I felt it appropriate to raise these issues in my Ph.D. thesis, as the Barut–Unal paper is the only major contemporary work on the topic. Had I been in academia when the Barut–Unal paper were published, and working on this topic, I would have no doubt submitted a Comment to the journal at the time. **No correction required.** □

Page 289: It's rather nice how $\mu^2 + d^2$ come together.

Yes, isn't it. **No correction required.** □

Page 296: The author should have suggested reasons for the factor of 2 and not requested them from the reader. A phrase such as appears at the end of the second paragraph of p. 297 is more acceptable.

Clearly, I exhausted all of my own ideas. I do not think that seeking suggestions from the reader is a bad thing. **No correction required.** □

Pages 348–352: Nice notations, but setting $v^i = v_i$ then becomes confusing.

No it isn't. $\boldsymbol{v}$ is a non-covariant three-vector, and hence clearly cannot be used in any of the notations mentioned; the definitions in question apply only to covariant quantities. **No correction required.** □

Page 46: It is not *true that $T^{\mu\nu}$ is* always *symmetric from (2.39). But it can indeed be so constructed by Belinfante's method or if it is derived from a general relativistic theory by differentiation w.r.t. the metric.*

Prof. Delbourgo has clearly confused the *mechanical* stress–energy tensor (page 46) with the *canonical* stress–energy tensor of (2.39). The former is of course always symmetric, but the latter need not be, as he points out. Belinfante's method is in fact a way to proceed from the latter to the former.

Differentiation with respect to the metric, in a generally relativistic theory, as Prof. Delbourgo suggests, *is* in fact the most fundamental definition of the mechanical stress–energy tensor. I have therefore noted this in the text of page 46. **Note added to page 46, line 4.** □

Page 114: I see no reason why the constituent spins should be aligned in the rest frame.

They are aligned *by construction*. This is made clear by the word "deem".

Of course, in an *arbitrary* extended body, spins of constituents need not (and, in general, will not) be aligned. If Prof. Delbourgo mistook my construction for a *general* description, he should have objected to many other statements in the chapter. **No correction required.** □

Page 178: Here and in other places (pp. 179, 187, (5.47), (5.49)), the author has used *a wrong sign for the Lagrangian of the em field, at least if I use his metric convention. It should be $-F^2/4$ in order to guarantee that the spatial part of the potential A comes with the right sign;* i.e., *we want $L = (\partial A)\cdot(\partial A)/2$ for the kinetic energy. I suggest that a simple remedy is to put a minus sign on the left of such relations.*

In full generality, the Lagrangian of a physical system is arbitrary up to a constant factor—*including* an arbitrary sign. There is no need to "guarantee" any overall sign or scale factor; all that is important is that the various terms appearing therein have correct *relative* factors (which I was very careful to check).

Of course, any such overall scale factor must be similarly considered in the fundamental definition of the stress–energy tensor. This definition would *automatically* be invariant, if we were to include the generally-relativistic expression for the stress–energy coupling to the spacetime metric in our Lagrangian.

It *is* true that, in the "standard" definition of the stress–energy tensor, the sign convention is opposite to that used in the mentioned sections. The reason can be traced back to my desire to make my manifestly-covariant Lagrangian treatments—in large part new physics—intuitively understandable, yet mathematically rigorous. In such treatments, it is convenient to define the free-particle relativistic Lagrangian, (2.77), with a *positive* sign:

$$L = +\frac{1}{2}mU^2;$$

this ensures that the relativistic application of (2.24),

$$b_\alpha \equiv \partial_{U^\alpha}L = mU_\alpha,$$

comes out with the correct sign. This choice, however, means that the manifestly-covariant interaction Lagrangian *must have the opposite sign to the corresponding nonrelativistic interaction Lagrangian* (because the leading-order zero-component U^0 has no nonrelativistic counterpart, functionally speaking).

To make this sign change explicit to the reader, I have added a note to the definition (2.77) of the free-particle manifestly-covariant Lagrangian. **Note added to page 81, line 27.** □

Page 358: It was not obvious to me why $\tau \to -\tau$ is the classical version of charge conjugation. The only way this makes sense to me is if I adopt Feynman's interpretation that backward motion along a worldline is equivalent to C.

That's right. I don't know of any other interpretation. I have added the missing attribution to Feynman. **Note added to page 358, line 10.** □

Page 392: With the author's γ_5 convention as $i\gamma_0\gamma_1\gamma_2\gamma_3$, there is an error at the top of p. 392: I think factors of i should appear. Also $k \to i\partial$, not ∂.

Corrected: page 392, line 1; page 392, line 2 (two corrections); page 392, line 3; page 392, line 8. □

Page 73: Typographical error: in<u>s</u>tructive

Corrected: page 73, line 18. □

Page 75: Typographical error: brackets

Corrected: page 75, line 10. $\square$

Page 112: Typographical error: constant

Corrected: page 112, line 6. $\square$

Page 116: Typographical error: inadvertently

Corrected: page 116, line 17. $\square$

Page 134: Typographical error: intuitively

Corrected: page 134, line 19. $\square$

Page 173: Typographical error: electromagnetic

Corrected: page 173, line 5. $\square$

Page 204: Typographical error: $\delta(z \mp \varepsilon)$

Corrected: page 204, line 19. $\square$

Page 209: Typographical error: contribution

Corrected: page 209, line 14. $\square$

Page 214: Typographical error: represents

Corrected: page 214, line 18. $\square$

Page 216: Typographical error: centre

Corrected: page 216, line 22. $\square$

Page 225: Typographical error: as

Corrected: page 225, line 19. $\square$

Page 227: Typographical error: is

Corrected: page 227, line 28. $\square$

Page 228: Typographical error: algebraic

Corrected: page 228, line 4. $\square$

Page 268: Typographical error: $\tilde{r}$ *not* $\boldsymbol{n}_{\tilde{r}}$

Corrected: page 268, line 7. $\square$

Page 285: Typographical error: con$\underline{s}$*tant*

Corrected: page 285, line 13. $\square$

Page 302: Typographical error: The$\underline{y}$

Corrected: page 302, line 27. $\square$

Page 337: Typographical error: $F \to \tilde{F}$

(This page number should read 377.) **Corrected: page 377, line 12; page 377, line 13; page 377, line 14.** $\square$