



**SDI Review Form 1.6**

Journal Name:	<a href="#">Physical Review &amp; Research International</a>
Manuscript Number:	2013_PRRI_6994
Title of the Manuscript:	Two-Body Dirac Theory
Type of the Article	

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**PART 1: Review Comments**

	Reviewer's comment	Author's comment (if agreed with reviewer, correct the manuscript and highlight that part in the manuscript. It is mandatory that authors should write his/her feedback here)
<b>Compulsory</b> REVISION comments	<p>The author's two posited equations (line 383 and 468) are quite interesting particularly in light of the previously published results on the Lamb shift and anomalous magnetic moment. However, I do not recommend publication of this paper in its present form. There are numerous technical problems I ...nd with the paper.</p> <p>1) In Eq. (II-5) the notation <math>E; H</math> is not clearly de...ned. Is it a cross product? If so, how does its dot product with <math>S</math> vanish? (His notation throughout is quite irregular, making it difficult to read and understand).</p> <p>2) His statement in line 546 is not clear and needs justification or elaboration.</p> <p>3) 692 he means II-4</p> <p>4) His statement that the Coulomb problem for the two body Dirac system is not Lorentz invariant (he quotes Ref [23] but there is no mention of a two body Dirac equation there) overlooks treatment of positronium using Dirac's constraint dynamics, in which a Lorentz invariant Coulomb interaction is used [P. Van Alstine and H. W. Crater, Phys. Rev. D 34, 1932 (1986).] and [H. W. Crater, R. Becker, C. Y. Wong, and P. Van Alstine, Phys. Rev. 46, 5117 (1992)]. In that approach the c.m. motion and relative motion can be separated covariantly</p> <p>5) He proposes two simultaneous Dirac equations, one for the electron and one for the positron, with the coupling taking place with a smeared Coulomb potential via Hartree Fock. He does not separate out center of mass motion and relative motion. Presumably this is because he believes it cannot be done covariantly in the two-body problem. His</p>	<p>I thank the referee for his careful reading of the manuscript and his apposite comments. My response follows.</p> <p>(1) Clarified in the text</p> <p>(2) Done</p> <p>(3) Done</p> <p>(4), (7) Added discussion and references in the second and third paragraphs Section III.</p> <p>(5) On the question of center-of-mass motion added sentence page 18, "It is unlikely..."</p> <p>(6) The state is already observed in the observation of the emission of two gamma photons, interpreted here as a bound-bound transition rather than an annihilation reaction. New experiments should look for a Ps atom bound in the negative-energy state.</p> <p>(8) I am not concerned with the Bethe cut off in this paper but with the Bethe</p>



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	<p>Hartree Fock approach will suffer from the well known problem among nuclear physicists of the spurious center of mass motion in the formalism. That problem becomes less and less important the more the number of nucleons involved. Conversely it becomes more and more an issue, the fewer the number of particles involved. He has just two! Whereas the Hartree - Fock does not have this problem in atomic physics (where there is a heavy central nucleus) it will in his case which has no heavy center. Perhaps related to this is his Eq. (III-1). If he tries to work around this problem by working in the center of mass frame, then the coordinate of the electron and that of the positron are oppositely directed and of equal magnitude. In that case his potential should be <math>e^2/(2r)</math>, where <math>r</math> is the electron coordinate relative to the c.m. frame, not Eq. (III-1). Beyond this, in my opinion I do not see how it is possible that he could reproduce the observed 6.8 eV binding energy with relativistic corrections regardless of the number of iterations performed. One reason (besides that of the problem with the Hartree Fock) is that the two one body Dirac equations do not include recoil effects. They play an important role in the spectral results at higher order (for example, there are no three vector potentials).</p> <p>6) His second (negative energy solution ) is intriguing. Is he claiming that state is stable? How could it be observed? Note that a similar extra (tightly) bound state for positronium has been proposed by H. Crater and C. Y. Wong, Magnetic States at Short Distances, Phys. Rev. D 85, 116005 (2012) (arxiv: 1203.0687).</p> <p>7) He states that "the Bethe-Salpeter equation is of questionable usefulness for the present bound-state problem" What justification does he have for this claim. It (in the form of the Salpeter approximation) has been known since the</p>	<p>counter term which is added to the calculation in order to cancel the divergent term linear in the photon frequency and which is present in all QED calculations of the Lamb shift. This renormalization procedure essentially corrects the divergent result generated by the use of Dirac's quantized radiation field, which is physically correct for the calculation of the Einstein A and B coefficients as Dirac showed in his 1927 paper, but physically incorrect for the Lamb shift due to the boundary artifice of emission and reabsorption by the same quantum state. Lamb's experiment shows us that radiation-free matter does not exist in nature. Once this notion is incorporated into the calculation using the renormalization scheme to account for the radiative shift of a free electron such that one has a radiation-dressed rather than a radiation-bare electron, then the divergence is removed. Theorists of the day glossed over the fundamental lesson of Lamb's experiments and came up with a mathematical fix. The lesson of Lamb's experiments is: the quantization of the radiation field and the second quantization of matter fields which follow it, which are quite neat and pleasing mathematically, describe matter-free photons and photon-free matter which does not exist in nature, hence the divergences requiring renormalization schemes to achieve a sensible result. This a</p>
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	<p><b>1950s to produce the correct positronium bound states. Is he just stating that because of the complexity of that equation, it is impractical to solve for negative energy solutions?</b></p> <p><b>8) He should be more precise about his critique of the standard renormalization approach to QED. Bethe introduced the cutoff. No such cutoff appears (except as a regularization parameter) in the later QED theory developed by Schwinger and others. Renormalization is essential regardless of the nature of the divergences. It just demands that the observed values of the electron's charge and mass coincide with the computed perturbative ones and along the way removes the appearance of the regularization parameters. It would be necessary even if there are no divergences. Nevertheless this referee appreciates the desire of a number of physicists (including the late Mendel Sachs) to propose divergence free theories that account for the standard QED predictions and would be willing to reconsider this paper once the above items are addressed and the connection of his two-body approach to the earlier ones mentioned above are examined and discussed.</b></p>	<p>further example in physics of the emphasis on mathematical skill at the expense of empirical knowledge and natural philosophy, which is a conflict not found in the other experimental sciences.</p>
<b><u>Minor</u></b> REVISION comments		
<b><u>Optional/General</u></b> comments		