The story of a Comment related with the non-uniqueness problem of the Dirac Hamiltonian

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Abstract

On December 13, 2013, I submitted to *Phys. Rev. D* a "Comment" on a paper published in that journal [PRD **88**, 084014 (2013), arXiv:1308.4552]. This Comment (hal-00918392 and arXiv:1312.6707) aimed at showing how the ambiguity of the curved-spacetime Dirac Hamiltonian remains true in the special gauge chosen by the authors ("Schwinger gauge"), and how that affects the physical meaning of the equations of that paper. My Comment has been rejected by PRD, based on the reports of one of the authors of the paper, plus (later) other referees. The present document contains the reports and my answers to them, in the chronological order. By thus showing the arguments and counterarguments, it may help one to make his/her opinion about the reality and importance of this non-uniqueness problem. I conclude with informations on how I finally published the content of that Comment, and on some curious consequences that followed the posting of this Comment.

1 First round

1.1 Involved referee's report [Received by me on 15 January 2014] on v1 of 'Comment on "Spin in arbitrary gravitational field" '

The author wrote a Comment on our paper [1]. In my opinion, this Comment does not contribute anything essentially new to the subject, and therefore it should probably not be published.

1. To begin with, all the material of the Comment is published elsewhere, namely, the papers of the author [2,3,7,10,13] contain the detailed discussion of the points

of this Comment. It is thus unclear why this material should be published again, this time without sufficient details.

2. The gauge freedom (3-dimensional rotations) of the Schwinger tetrad is well known. In this sense, the point 1) of the Comment is trivial. Moreover, the point 5) of the Comment is misleading. The 3-dimensional rotation acts also on the classical particle with spin in such a way that the quantum and classical equations of motion remain consistent for all choices of the Schwinger tetrad.

3. The point 3) of the Comment belongs to the discussion of the author with Gorbatenko and Neznamov [8,9], and thus probably it could be more appropriate for the author to write a different Comment on the references [8,9].

4. The points 2) and 4) are not relevant to the main results of our paper [1]. The physical dynamics of a quantum Dirac particle in an arbitrary gravitational field is correctly described by the Foldy-Wouthuysen Hamiltonian derived in our paper.

1.2 My Answer [24 January 2014] to the author's report on v1 of my Comment

a) This author wants to argue that my Comment "does not contribute anything essentially new to the subject". At least two results are in fact new, see points (d) and (e) below. However, the main aim of my Comment is to point out serious weaknesses in Ref. [1]. I note that this author does not give a technical argument against mine and that he seems to have been aware of my work — which, my Comment shows, applies to theirs.

b) The "gauge freedom (3-dimensional rotations) of the Schwinger tetrad" is not so well known. I do not see that it would have been mentioned explicitly before the 2011 papers [2] and [8]. The aim of my Point (1) is, as announced, to show that this freedom does fully appear with the authors' settings. This was not "trivial": it could have been the case that the construction of the authors restrict the choice among the possible Schwinger tetrads.

c) Classical particles with spin are discussed in two independent parts of Ref. [1]: Sects. IV A and IV B. In Sect. IV A, as I mention at Point (5), the consistency of the quantum and classical equations of motion does not appear clearly. In Sect. IV B, as I also point out, there is no dependence of either the classical spin rate (4.36) or the classical Hamiltonian (4.38) on the choice of the Schwinger tetrad. Thus, the ambiguity of the quantum-mechanical description does not survive in the classical limit.

d) Point (3) in my Comment extends the particular counterexample presented in Ref. [3] to the most general Schwinger tetrad in the most general metric, thus to the general situation considered in the paper [1] discussed by this comment. This is a new result. It shows that the mean values of the Hermitian Hamiltonian (2.15) considered by the authors depend always and strongly on the gauge choice that remains available in their approach. This is a grave non-uniqueness problem. For the counterexample to be fully convincing, I show the falseness of Gorbatenko & Neznamov's answer, at DSPIN-13 [9b], to an argument in [3]. This new argument takes the last eleven lines in Point (3).

e) Point (2) in my Comment sets the stage for Point (3), whose importance is shown right above, and for Point (4). The latter shows that the Foldy-Wouthuysen transformation (3.3) used by the authors leads, in the time-dependent case that they consider, to a Hamiltonian that is physically inequivalent to the starting Hermitian Hamiltonian (2.15). This is another grave problem. The analysis leading to Eq. (16), that confirms the time-dependence of the F-W transformation, is new.

2 Second round

2.1 Report [14 April 2014] of the involved referee on v2 of my Comment

I still maintain my opinion that the Comment does not contribute anything essentially new to the subject. In his reply, the author avoided giving an answer to my previous statement that all the material of the Comment was published elsewhere, see the papers of the author [2,3,7,10,13].

Moreover, this Comment in fact appears to be very loosely related to the article [1], despite the claims of the author.

It is well known that the Hamilton operator changes according to the eq. (12) under the time-dependent unitary transformation. However, this (let me stress once again) well-known fact is not a specific feature of the relativistic covariant Dirac theory. The same applies to the non-relativistic quantum mechanics, and even to classical mechanics. I am surprised that the author interprets (12) as a "grave problem" of the classical and quantum mechanics.

What kind of "physical inequivalence" arises from (12)?

Let me remind, that in classical mechanics there exists a canonical transformation that brings the Hamilton function to zero, $H' = 0$. This is the Hamilton-Jacobi theory. Following author's line of thought, should one then consider the Hamilton-Jacobi picture (with $H' = 0$) "physically inequivalent" to the original description of the same system by the non-transformed (nonzero) Hamiltonian H? Why?

Furthermore, in quantum mechanics there exists analogous time-dependent canonical transformation that brings the transformed quantum Hamilton operator to zero. See M. Roncadelli and L.S. Schulman, Phys. Rev. Lett. 99 (2007) 170406, for example. Is the quantum Hamilton-Jacobi theory physically inequivalent to the original Schroedinger quantum theory? I am curious to learn the answer.

The author should study the relevant literature, and if he afterwards feels that there is a "grave problem" with the time-dependent canonical transformations in classical and quantum mechanics, he is encouraged to come up with its solution and to write a corresponding comprehensive paper.

But the current Comment is really inappropriate in view of the two points: (i) the material was already published elsewhere, and (ii) its contents is very loosely related to [1].

2.2 Report [14 April 2014] of external referee A on v2 of my Comment

The Comment contains several statements, but only one is closely related to the paper in question. The main objection which the author of the Comment raises is the ambiguity of the Hamiltonian in the Schwinger gauge. However, the main reason for the ambiguity is actually the time dependent unitary transformation. This ambiguity is indeed generic and not specific just to the gauge in question.

The author of the comment concentrates on the specific choice of the tetrad - the Schwinger gauge, but the same arguments apply to any other tetrad. In fact, for the purpose of the studies presented in the original paper, Schwinger gauge is the most convenient since the dynamics of spin is not distorted by purely inertial effects, and only gravity is essential.

Since the Comment points out a generic and well known issue, I would not recommend it for publication as a comment on the paper in question.

2.3 My answer [18 April 2014] to the report of the involved referee (author) on v2 of my Comment

1. The author's report does not address the real content of my Comment. Nor does it address my answer to his first report on that Comment. The author/referee states:

i) that I "avoided giving an answer to [his] previous statement that all the material of the Comment was published elsewhere". On the contrary, in my answer to his first report, I pointed out several new results present in my Comment. I then wrote: "However, the main aim of my Comment is to point out serious weaknesses in Ref. [1]" (the paper discussed in my Comment);

ii) that I << interpret (12) as a "grave problem" of the classical and quantum mechanics. >> Let me recall that equation (12) of my Comment:

$$'\mathcal{H} = U^{-1}\mathcal{H}U - \mathrm{i}\hbar U^{-1}\partial_t U. \qquad (12)$$

First of all, there is no question about classical mechanics in my Comment, and it is obvious that Eq. (12) does not apply in classical mechanics. Secondly, I do not and did not interpret Eq. (12) itself as a problem of any kind. On the contrary, I mentioned in the Comment right after that equation: << This is actually the relation between two Hamiltonians exchanging by the most general "operator gauge transformation" [Foldy-Wouthuysen 1950, Goldman 1977]. >> Indeed (12) can be derived easily from the transformation (11) of the wave function:

$$\psi \mapsto \psi' = U^{-1}\psi, \qquad (11)$$

on the condition that the Schrödinger equations before and after the transformation (11) be equivalent. See e.g. my previous papers Refs. [2,3]. This derivation is of course independent of the particular wave equation. So there is no point in the "stress" put by the author/referee on that fact [that (12) follows from (11) for a general Hamiltonian] and its well-known character.

2. The involved referee further asks: << What kind of "physical inequivalence" arises from (12)? >> So he insistently does as if I were seeing a problem in (12) itself. My reply is inserted in the Comment as the new Footnote 4 on P. 5:

"Of course, a transformation of the general form (11)–(12) can be applied to any quantum system. Yet in the time-dependent case, at most one among the two

Hamiltonians $\mathcal{H}$ and $'\mathcal{H}$ can be the relevant *energy operator* of the system described by the Schrödinger equation $\mathrm{i}\hbar\frac{\partial\psi}{\partial t} = \mathcal{H}\psi$ [which is equivalent to $\mathrm{i}\hbar\frac{\partial\psi'}{\partial t} = {}'\mathcal{H}\,\psi'$ with the transformation (11)–(12)]. The problem, for the covariant Dirac equation in the Schwinger gauge, is that both $\mathcal{H}$ and $'\mathcal{H}$ are Hermitian Hamiltonians, each of which is deduced from choosing one Schwinger tetrad. Thus, each of them is an equally good candidate for being the energy operator — but, as discussed below [in the Comment], the energy mean values of $\mathcal{H}$ and $'\mathcal{H}$ differ and this is not by just a constant. Hence, two different choices of the Schwinger tetrad, related by a time-dependent rotation, lead to physically inequivalent energy operators."

As recalled right above, and as it has been obvious in my Comment since its first version, the problem which I point out is the non-uniqueness of the energy operator and its mean values in the covariant Dirac theory. This problem can be shown even more explicitly when the Schwinger gauge is chosen, as is done by the authors of Ref. [1]. The energy mean values are the most important measurable quantities: they include the energy eigenvalues, i.e. here the energy levels of a Dirac particle.

3. The long digression made by the author/referee about the Hamilton-Jacobi equation has simply no relation to the content of my Comment. That equation is not one for the physical observable "energy". Thus, in classical mechanics, the Hamilton-Jacobi method of integration of the Hamiltonian equations of motion uses a canonical transformation of a general kind, after which the Hamiltonian function can not be interpreted any more as the energy of the particle.

4. The author/referee states about my Comment that "its contents is very loosely related to [1]." However, the Hermitian Hamiltonian $\mathcal{H}$, got by choosing a Schwinger tetrad and then applying the non-unitary transformation (2.14) of Ref. [1] [Eq. (8) in my Comment], plays a central role in Ref. [1]. As discussed in my Comment, $\mathcal{H}$ is the input for the subsequent calculation of the Foldy-Wouthuysen Hamiltonian $\mathcal{H}_{\mathrm{FW}}$, Eq. (3.2), on which are based the equation of motion of spin (3.15), the semi-classical limit, and the quantum-mechanical equations of particle dynamics.

Therefore, the non-uniqueness of $\mathcal{H}$, including the dependence of the energy mean values on the choice of the Schwinger tetrad, is indeed a grave problem since all important quantities like the various Hamiltonians ($\mathcal{H}$, $\mathcal{H}_{\mathrm{FW}}$, and its semi-classical limit), the spin operators, the force operator, etc., *a priori* depend on that choice (certainly, for $\mathcal{H}$ and $\mathcal{H}_{\mathrm{FW}}$) — thereby strongly questioning their very physical meaning. It is significant that the author/referee did not even try to address these arguments.

An additional point was shown in detail in my Comment: due to its time-dependence in the general case, the Foldy-Wouthuysen transformation used by the authors of Ref. [1] leads to a Hamiltonian $\mathcal{H}_{\mathrm{FW}}$ which is physically inequivalent to the starting one $\mathcal{H}$. The involved referee does not say a single word about this equally grave problem.

2.4 My answer [18 April 2014] to the report of external referee A on v2 of my Comment

1. The other referee states that "The Comment contains several statements, but only one is closely related to the paper in question," namely "the ambiguity of the Hamiltonian in the Schwinger gauge."

This is not true. In the Comment, I also show in detail that, due to its time-dependence in the general case, the Foldy-Wouthuysen transformation used by the authors of Ref. [1] leads to a Hamiltonian $\mathcal{H}_{\mathrm{FW}}$ which is physically inequivalent to the starting one $\mathcal{H}$. The referee does not say a single word about this, which is equally grave for the work [1].

2. The referee acknowledges explicitly the correctness of my statement about the ambiguity of the Hamiltonian (in the Schwinger gauge as well as in general), when he/she writes right after the foregoing quote:

"However, the main reason for the ambiguity is actually the time dependent unitary transformation. This ambiguity is indeed generic and not specific just to the gauge in question."

3. The referee then insists on that genericity of the ambiguity: "The author of the comment concentrates on the specific choice of the tetrad — the Schwinger gauge, but the same arguments apply to any other tetrad."

This is almost true. (In the Schwinger gauge and with the transformation (2.14) used in Ref. [1], the ambiguity shows up more easily and explicitly [3], e.g. due to the fact that the scalar product is the "flat" one.) However, the fact that the ambiguity applies also without the Schwinger gauge used by the authors, does not diminish in any way the seriousness of its consequences for the approach used by the authors of Ref. [1], which is shown in my Comment. See the summary at Point 4 in my answer to the involved referee.

The referee finds that "Schwinger gauge is the most convenient since the dynamics of spin is not distorted by purely inertial effects, and only gravity is essential". But what conclusion can one draw about the dynamics of spin, when the Hamiltonian operator and hence *a priori* also (and most likely, see P. 8 in the Comment) the very spin operators are ambiguous?

4. The referee concludes thus: "Since the Comment points out a generic and well known issue, I would not recommend it for publication as a comment on the paper in question." Recall that this issue is the ambiguity of the Hamiltonian and the energy operator of the covariant Dirac theory. The genericity of the issue does not diminish its seriousness: the opposite is true. As to its "well known" character: to the best of my knowledge, the non-uniqueness problem of the Hamiltonian and energy operators of the covariant Dirac theory has been pointed out only in papers by me, with F. Reifler or alone, published — after hard battles — since 2011. If this is a well known issue, I believe it is only due to those papers, and I suppose that the other theoreticians in the field will eventually take our results into account.

3 Third round

3.1 Report [20 May 20 2014] of external Referee B on v3 of my Comment

I have read the comment and responds of the referees. Mayeul Arminjon (MA)(author of the comment) comments on 5 points: 1. Non-Uniquenss of the tetrad in the Schwinger gauge. 2. Non-Uniqueness of the Hermitian Hamiltonian operator. 3. Physical inequivalence of $\mathcal{H}$ and $'\mathcal{H}$. 4. Inequivalence of $\mathcal{H}$ and $\mathcal{H}_{FW}$ in the time-dependent case. 5. Semi-Classical limit and comparison with spinning particles.

Both the author of the original paper (spin in an arbitrary gravitational field, PRD **88**, 084014 (2013)) and the independent referee, to me, do not give enough convincing arguments not to publish the comment of MA in PRD.

In their reports:

Author of the original paper points out that: *i) all the material of the comment is published elsewhere ii) The gauge freedom of the Schwinger tetrad is well known , in this sense the point 1) of the comment is trivial. ii) The point 3) of the comment belongs to the discussion of the author with Gorbatenko and Neznamov.* These points can not be used as weakness of the "comment".

Independent referee points out that *only one of the comments is closely related to the original paper, ambiguity of the Hamiltonian in the Schwinger gauge. This ambiguity is generic and not specific just to the gauge in question.*. That is the report.

Both the original paper and the "comment" are correct in the sense of calculations and formulas. The essential points raised in the comment of MA are "non-uniqueness of the tetrad in Schwinger gauge", "Non-uniqueness of the Hamiltonian operator and physical inequivalence of the Hamiltonian operators" need further explanation in the original paper. To me , the reader of the original paper ought to share the critical points discussed in the comment of MA. For this purpose I am in favor of publishing the comment of MA.

3.2 First report [20 May 20 2014] of external Referee C on v3 of my Comment

In brief, I almost completely share the point of view of the authors of the critiqued paper: "*this Comment does not contribute anything essentially new to the subject, and therefore it should probably not be published*" . Actually, my opinion is the same as the one of the independent referee: "*the Comment has not to be published*". And I add: because it is conceptually wrong.

As far I understand, the author starts from a specific slicing of the space-time, on which he write the Dirac equation in the form of an explicit first order evolution one : $i\partial_t\Psi = \mathcal{H}\Psi$. Of course this is perfectly legitimate. Where I cannot follow the author is when he wants to give a meaning to what he call the mean value of $\langle\mathcal{H}\rangle$, that, as he notices, is gauge (and time) dependent[1]. In the framework of a first quantised approach followed in the work discussed, only operators/quantum numbers defined by Killing vectors and their generalisations (Penrose-Floyd, etc) [2] really make sense. Anyway, comparing the mean values of gauge dependent operators, with respect to a fixed state (i.e. without making the appropriate gauge transformation on the state) is incomprehensible for me.

[1]By the way, when the author answers that the gauge freedom is not so well known, because it is not explicitly mentioned in the literature, here again I share the point of view of Obukhov and *al.* . It is so obvious, that usually it is not mentioned.

[2]See for instance : Spindel, "Gravity Before Supergravity", Published in In Bonn 1984, Proceedings, Supersymmetry*, 455-533.

In conclusion, in my opinion, there is no physical inequivalence between $\mathcal{H}$ and $'\mathcal{H}$, on the contrary both are equivalent time evolution operators expressed in different gauges.

3.3 Rejection email from the Journal (20 May 2014)

Dear Dr. Arminjon:

The manuscript "Comment on "Spin in an arbitrary gravitational field"" (DMK1017)
by Arminjon,M
has been reviewed by two of our referees. Comments from the reports are enclosed.
We find the report of referee C more persuasive.

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

This concludes the review of your manuscript.

Sincerely,
(Name and title omitted for the sake of discretion)
Physical Review D

4 Appeal

4.1 My reply (22 May 2014) to the Report of Referee C

The arguments of Referee C do not justify his severe judgment that my Comment "is conceptually wrong". It is incorrect to speak of "what [I] call the mean value" of an operator such as the Hamiltonian $\mathcal{H}$, because the definition that I use for $\langle \mathcal{H} \rangle$ [Eq. (13) in my Comment] is fully standard. There is simply no quantum mechanics if one cannot "give a meaning to the mean value" of the energy operator E. [3] (E coincides with the Hamiltonian operator H when H is Hermitian. This is the case for the Hamiltonians considered in the commented paper: $\mathcal{H}$, $\mathcal{H}_{\mathrm{FW}}$, etc.) For example, there are then no energy levels for the electron in the hydrogen atom. Thus, if the referee's criticism were correct, there would be no quantum mechanics in a curved spacetime. In that case, it is in the first place the very paper which I

[3] Why did the referee write this: "Anyway, comparing the mean values of gauge dependent operators, with respect to a fixed state (i.e. without making the appropriate gauge transformation on the state) is incomprehensible for me"? It is clear in my Comment that I do transform also the state after the gauge transformation: Eq. (11), see also the sentence on pp. 5 (bottom) and 6 (top).

am commenting which should not have been published.

It is not true [and not stated in Spindel's paper (1984), quoted by the referee] that in "a first quantized approach", "only operators/quantum numbers defined by Killing vectors and their generalizations (Penrose-Floyd, etc) really make sense". If that were true, the commented paper again should not have been published. The notion of a reference frame, as a congruence of time-like world lines (ideal observers), is a physical one, and can be formulated in precise mathematical terms for a general spacetime. [4] The energy of already a classical particle, either in Newtonian mechanics or in a Minkowski spacetime, as also in a curved spacetime, does depend on the reference frame. This is also true for the energy mean values of a quantum-mechanical particle. So the energy operator in a given reference frame, as well as its mean values, should be well defined. In the commented paper, the reference frame is fixed due to the use of a given coordinate system.

The problem is that, for a particle obeying the covariant Dirac equation, the energy operator and its mean values have an additional dependence, contested in none of the referee reports: they depend on the gauge choice — the choice of the tetrad field. It means that, in the commented paper, the essential objects like $\mathcal{H}$, $\mathcal{H}_{\mathrm{FW}}$, etc. (e.g. most likely the spin operators), depend on that gauge choice. In addition, due to its time-dependence in the general case, the Foldy-Wouthuysen transformation used in the commented paper leads to a Hamiltonian $\mathcal{H}_{\mathrm{FW}}$ which is physically inequivalent to the starting one $\mathcal{H}$.

4.2 Second Report of Referee C (3 June 2014)

The following considerations are written in response to the "Reply to the Report of Referee C".

There is a profound difference between the Hamiltonian of a dynamical system: the time evolution operator of the system, and the energy: a conserved quantity that only is defined if a non space-like Killing vector exists on the space-time region where the physical system under consideration is located. On the contrary to what the author claims, there is no energy level for the electron of a hydrogen atom on a general gravitational background. In general there is no bound state; all normalizable configurations correspond to wave packets.

According to the author point of view, the energy concept he want to consider is defined by a congruence of time-like world lines. From a strict mathematical point

[4] C. Cattaneo, *Nuov. Cim.* **10**, 318 (1958). M. Arminjon and F. Reifler, *Int. J. Geom. Meth. Mod. Phys.* **8**, 155 (2011).

this is not enough, and a slicing of the space-time (a definition of time) has also to be provided. Even if we suppose that time is defined by the time-like congruence itself, i.e. that the velocity field of the observers is integrable, and that these observers have synchronized their clocks (what constitute both assumptions on the set of observers and on the space-time global topology - an implicit assumption in the paper), it is (physically) unacceptable that the value of the energy that an observer attributes to a quantum system depends (in principle) on the choice of other observers located arbitrary far from him or her. Or in others words, that what the author calls the energy is a non-conserved quantity that requires a collection of tuned apparatus filling the whole Universe.

But of course all these considerations are in some sense purely academic, and only relevant in presence of very strong gravitational fields, whose typical radius of curvature is of the order of the dimensions of the system, in which case quantum field theory has to be used, instead of (one particle) quantum mechanics.

Accordingly I maintain my opinion about the work, it is not acceptable for publication in the Physical Review, being in my opinion conceptually incorrect.

4.3 Definitive rejection by PRD (3 June 2014)

Dear Dr. Arminjon:

The manuscript “Comment on “Spin in an arbitrary gravitational field”” (DMK1017) by Arminjon,M has been reviewed by one of our referees. 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.

We will not consider this manuscript any further.

Sincerely,
(Name and title omitted for the sake of discretion)
Physical Review D

5 My specific answer to two remarks of Referee C

5.1 Answer to the remark about transforming the state

Referee C's first report on my Comment contains the following terrible sentence:

"Anyway, comparing the mean values of gauge dependent operators, with respect to a fixed state (i.e. without making the appropriate gauge transformation on the state) is incomprehensible for me."

This remark shows a serious lack of study of my paper. It is clear in my Comment that I do transform also the state after the gauge transformation. (I wrote this in my answer to the journal, see Note 3 above.) To be sure, see Eq. (11) in the Comment for the definition of the new state ψ' after the transformation and look the following sentence after Eq. (13):

"The mean value $\langle '\mathcal{H} \rangle$ for the corresponding state ψ' after the transformation (11) is given by the same Eq. (13), with primes. "

Equations (11) and (13) of the comment are respectively Eqs. (27) and (30) in the Int. J. Theor. Phys. 2015 paper referenced below.

5.2 Answer to the remark on the need for a time map

In his second report, Referee C stated that "the energy concept [I] want to consider is defined by a congruence of time-like world lines. From a strict mathematical point this is not enough, and a slicing of the space-time (a definition of time) has also to be provided."

The second sentence is partly correct, see the paragraph below this one for a correction. However, it is not in my papers but merely in my answer to his first report, which I sent two days after having received it, that I omitted to mention the need for a time map. I omitted that for the sake of conciseness. In my papers on the covariant Dirac theory, I have emphasized many times that the Hamiltonian and energy operators depend on the reference frame, and that indeed the relevant notion of a reference frame includes the data of a time coordinate map $X \mapsto t(X)$. See for instance Section 3 in the paper *Int. J. Theor. Phys.* **53**, 1993-2013 (2014) (arXiv:1211.1855). In my Comment, I mention that the coordinate system is an arbitrary one but is fixed, as it is in the commented paper, and so in particular a given time coordinate map is being considered.

Moreover, Referee C is overlooking an essential point. The notion of reference frame that I am using, and which I believe is the correct one for the definition of the quantum-mechanical energy, is a *local* one. Thus, the time coordinate map $X \mapsto t(X)$ (as well as the spatial coordinate maps) is defined only over a *subdomain* U of the spacetime (i.e., U is an open subset of the spacetime manifold V). More exactly, I define a reference frame as an equivalence class of charts which have a common domain U of definition and which exchange by a purely spatial coordinate change. In practice, U will easily be taken large enough so that the wave function could safely be assumed to vanish when the spatial position is at the boundary of the spatial subdomain Ω, in a relevant time interval I. In order to be thus large enough, U does not actually need to be very large: remind that we are speaking of a quantum particle such as e.g. a neutron. Therefore, it is not true "that the value of the energy that an observer attributes to a quantum system depends (in principle) on the choice of other observers located arbitrary far from him or her".

In addition, there does not need to be effective "observers" following the world lines attached with the reference frame, since in practice one considers schematic reference frames assumed to correctly model the relevant physical situation: e.g. an inertial frame or a uniformly rotating frame in a Minkowski spacetime, or the static reference frame in a Schwarzschild spacetime, or still a frame undergoing a uniform rotation (say, in terms of standard coordinates) in that spacetime, etc..

Anyway, this is the way energy can be precisely defined in quantum mechanics in a curved spacetime. The existing literature on that field is much less precise on that point, as one can easily check. Once again, if the critique of Referee C were justified, it is that whole literature which should be rejected. As to the appeal to quantum field theory (in a curved spacetime): I have explained elsewhere in some detail why, at least in its current state, that theory does not allow one to make unambiguous predictions about the only available or prospective laboratory experiments relative to the gravity-quantum coupling. See the Appendix in *Int. J. Geom. Meth. Mod. Phys.* **10**, No. 7, 1350027 (2013) (arXiv:1205.3386).

6 Concluding remarks

In the mean time, I have succeeded at publishing my Comment (with some minor improvements), in the form of Section 3 in the following paper: M. Arminjon, "Some remarks on quantum mechanics in a curved spacetime, especially for a Dirac particle", *Int. J. Theor. Phys.* **54**, No. 7, 2218-2235 (2015). The reasons why my concept of energy does not suffer the main objections of Referee C have been explained in the introduction of that latter paper (IJTP 2015) and, in more detail, in its Section 4. Therefore, the last report of Referee C is not answered here, except for two additional explanations given in Section 5 above.

Submitting this Comment to arXiv has cost to me, on December 25, 2013 (the same Christmas day as the effective posting of the Comment by arXiv), the loss of my endorsement capacity for the gr-qc section of arXiv.

On February 13, 2015, I posted on HAL (hal-01116603) the text of a talk at the DICE2014 Conference, and that posting was forwarded by HAL to arXiv (arXiv:1502.04085). That text [appeared since: *J. Phys. Conf. Ser.* **626**, 012030 (2015)] is essentially a summary of the IJTP 2015 paper. This posting has cost to me my endorsement capacity on any section of arXiv.

Post-Scriptum: This document was put online on July 17, 2015. In the mean time, it seems that arXiv has restored my endorsement capacity for the physics.gen-ph and quant-ph sections. At least this is the situation today – August 17, 2015. Thus, I currently can't endorse in the gr-qc and math-ph sections. Note that I have 47 papers in the gr-qc section (including 40 with gr-qc as the primary category) and 10 papers in the math-ph section (including 1 with math-ph as the primary category).