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Quantum evolving constants. Reply to "Comment on 'Time in quantum gravity: An hypothesis'"

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Hajicek's interesting and valuable comments on the time hypothesis bear on the difficulties of the quantization process of a given classical theory, but they do not bear on the general issue of the viability of quantum theory without time. These comments are carefully discussed.

Hajicek [1] presents a series of accurate comments on the solution to the time issue in quantum gravity that was proposed in Refs. [2,3]. I thank him for his insights, and I take this opportunity to clarify certain aspects of the issue. In spite of the exactness of several of Hajicek's remarks, I think that the proposed solution is still viable, for the reasons explained below.

Hajicek raises two main criticisms of the ideas proposed in Refs. [2 and 3] [points (1) and (2) at the beginning of his paper], and a series of minor comments. Let me start with the two main criticisms. The first one is the fact that in order to define the "evolving constants" one has first to solve the classical equations of motion. The second is that the factor ordering of the evolving constant is problematic. Both these facts are certainly true. However, there is a difference between the issue addressed in Refs. [2,3] and the kind of difficulty raised by Hajicek.

The issue addressed in Refs. [2,3] is whether or not *a quantum theory without a well-defined Hamiltonian time evolution makes sense.* (See Ref. [3] for a precise definition of "without Hamiltonian time evolution" and for a discussion of the meaning, here, of "makes sense.") On the contrary, the problem considered by Hajicek in his comment is how difficult it is, given a *classical* theory "without time," to *actually construct* the corresponding quantum theory (namely, a quantum theory that has the given classical theory as its classical limit). The two problems are distinct. To clarify the distinction with an analogy, consider the problem: Can a quantum system have infinite degrees of freedom? It may be relatively easy to show that it is possible to define a consistent quantum theory with infinite degrees of freedom (Dirac constructed one in 1930), but it may be in practice very difficult to construct quantum theories with infinite degrees of freedom starting from an assigned classical field theory; in most cases, it may even be impossible.

The thesis presented and defended in Refs. [2,3] is *not* that it is *easy* or that it is *always possible* to quantize a system with vanishing Hamiltonian. The thesis is that, contrary to many opposite claims in the literature, a quantum system with a vanishing Hamiltonian (with "no time") may be well defined and consistent. Thus, to my understanding, Hajicek's comments are correct and relevant to the general problem, but they do not bear on the question addressed by the proposed solution of the time issue.

Let me now discuss the specific criticisms. I disagree with the statement, included in comment (2), that the problem of finding a factor ordering for the observables is not a well-defined mathematical problem. Indeed, the well-defined problem is the following: Given the self-adjoint operators $\hat{L}_i$, corresponding to the observables L_i, and a specified (classical) observable $Q(L_i)$, is there an ordering of $\hat{Q}=Q(\hat{L}_i)$ with the required properties? Here, as emphasized by Hajicek, the property required (for the projectors to be defined) is that $\hat{Q}$ be normal. Of course, in some cases the solution may be that there is no such ordering, or that there is more than one (see below).

Hajicek notes that in standard quantum mechanics the ordering is based on some choice of time. This is correct; but, again, irrelevant: As an analogy, recall that very often the quantum theory of a system, and in particular the *ordering* of the Hamiltonian operator, is defined by exploiting the symmetries of the system. This does not mean that if we do not have any symmetry, then the Hamiltonian operator does not exist. If there are no symmetries, then the Hamiltonian operator has to be found without the help given by the symmetries. Similarly, if there is no Hamiltonian time evolution, then the ordering of the observables has to be defined without the help of the unitary evolution operator. Notice that, in both cases, *if more than one ordering is available, this simply means that the quantum properties of the system are not determined by its classical limit.* If no good ordering is available, this may mean that there is no quantum theory corresponding to that particular classical system.

An important comment by Hajicek is the detection of a mistake in Ref. [2]. The mistake is the introduction of spectral projection operators [Eq. (54) and the text just preceding it] for an operator which is symmetric but, as Hajicek explicitly shows, not normal. As Hajicek implicitly suggests, a better choice of ordering may correct the problem. Here, indeed, I present the ordering that makes the relevant operator diagonalizable. Therefore, the mistake can be corrected. Notice that this entire controversy illuminates how the theory puts stringent (but not necessary insoluble) constraints on the choice of the ordering of the observables.

The classical evolving constant $Q_2(t)$ is (for every t) a function on the phase space Γ. For any fixed t, this function becomes imaginary on certain regions of the phase space. The observable is real only on the region $\Gamma(t)$ of the phase space defined by

$$L_z > \tfrac{1}{2}(t^2 - M) \tag{1}$$

(see Eq. (25) in Ref. [2]). Physically, this means that the motions described by this region of phase space never reach a "time" q_1 larger than t, because the system "comes back in q_1" before $q_1 = t$ (see the figure of Ref. [2]). In Ref. [2] it was claimed that this fact has the consequence that the quantum operator $\hat{Q}_2(t)$ "develops imaginary eigenvalues," and the idea was to consider only the self-adjoint projectors on the eigenspaces with real eigenvalues. Hajicek notes that the consequences are more serious than that, because $\hat{Q}_2(t)$ is not normal and, therefore, nondiagonalizable. Since at the classical level $Q_2(t)$ is only meaningful on $\Gamma(t)$, the problem can be circumvented by considering the "restriction" $\tilde{Q}_2(t)$ of $Q_2(t)$ on $\Gamma(t)$, rather than $Q_2(t)$ itself. If $P(t)$ is the characteristic function of $\Gamma(t)$,

$$P(t) = \theta(\tfrac{1}{2}(M - t^2) + L_z) \tag{2}$$

[$\theta(x) = 1$ if $x > 0$, zero otherwise], then I consider the classical observable

$$\tilde{Q}_2(t) = Q_2(t) P(t) , \tag{3}$$

which satisfies

$$\tilde{Q}_2(t) = \begin{cases} Q_2(t) & \text{on } \Gamma(t), \\ 0 & \text{otherwise} . \end{cases} \tag{4}$$

The quantum operator corresponding to the classical observable $\tilde{Q}_2(t)$ is defined by

$$\hat{\tilde{Q}}_2(t) = \hat{P}(t) \hat{Q}_2(t) \hat{P}(t) \tag{5}$$

(note the symmetric ordering chosen). Up to now, I have only chosen a slightly different observable than the one used in Ref. [2]. Now I introduce the key correction on the ordering by choosing for $\hat{P}(t)$ the ordering

$$\hat{P}(t) = \theta(\tfrac{1}{2}(M - t^2) + \hat{L}_z - \hbar) . \tag{6}$$

Note the crucial $-\hbar$ term, which of course disappears in the classical limit. It is straightforward to show that $\hat{\tilde{Q}}_2(t)$ is diagonalizable. The spectral projection operators used in Ref. [2] must be considered as the spectral projection operators defined by $\hat{\tilde{Q}}_2(t)$. Note that $\hat{P}(t)$ can be written as

$$P(t) = \sum_{m > (1/2\hbar)(t^2 - M) + 1} |m\rangle\langle m| . \tag{7}$$

$P(t)$ is the projector on the subspace $\mathcal{H}(t)$ of the Hilbert space of the theory spanned by the eigenstates $|m\rangle$ of $\hat{L}_z$ with eigenvalue larger than $\frac{1}{2}(t^2 - M) + \hbar$. Clearly,

$$\langle \psi \hat{\tilde{Q}}_2(t) \phi \rangle = \begin{cases} \langle \psi \hat{Q}_2(t) \phi \rangle & \text{if } |\psi\rangle \text{ and } |\phi\rangle \text{ are in } \mathcal{H}(t), \\ 0 & \text{otherwise} , \end{cases} \tag{8}$$

to be compared with Eq. (4). Note that in order to have a sensible observable in the quantum theory, the "time" t must satisfy

$$t^2 < 2L_z + M - \hbar , \tag{9}$$

while in the classical theory it has to satisfy

$$t^2 < 2L_z + M . \tag{10}$$

It is essentially this small discrepancy that leads to the difficulty with the diagonalization of $\hat{Q}(t)$ discovered by Hajicek. Physically, this means that in the quantum theory the measurement of $Q_2(t)$ loses sense slightly earlier than in the classical theory. I think that this aspect of the Heisenberg principle, pointed out by Hajicek's comment, may deserve to be studied.

Another criticism raised by Hajicek is a consequence of a misunderstanding of Eq. (17) of Ref. [3], due to the very poor notation used. The equation is

$$\frac{\partial Q(T)}{\partial T} \{q_T, K\} = \{q, K\} . \tag{11}$$

From the way it is obtained, this equation only holds in the points where $q_T = T$. A more accurate notation is

$$\left. \frac{\partial Q(T; q_n, p_n)}{\partial T} \right|_{T = q_T(q_n, p_n)} \{q_T(q_n, p_n), K(q_n, p_n)\} = \{q(q_n, p_n), K(q_n, p_n)\} . \tag{12}$$

Therefore the equation cannot be integrated in the way suggested by Hajicek. Certainly, as Hajicek correctly concludes, no rearrangement of Eqs. (15) and (16) in Ref. [3] may change the fact that the complete construction of the evolving constants amounts to solving the classical equations of motions. The reason I presented Eqs. (15) and (16) in Ref. [3] is to provide a general intrinsic definition of the evolving constants.

The difficulty of giving sense to Eq. (30) of Ref. [3], emphasized by Hajicek, is certainly real. The comment in Ref. [3] (one may try to give meaning to this equation in a representation in which q_T is diagonal) refers of course to a standard constraint quantization scheme in which operators are defined also on the unconstrained state space. There is no reason for which a quantum system without time should necessarily be defined by group representation theory rather than standard Dirac constraint theory.

Finally, as far as Eq. (32) in Ref. [3] is concerned, I agree with Hajicek that this equation is wrong. In general, it does not follow from Eqs. (29) and (30) of Ref. [3] because of ordering difficulties in taking the derivative. Thus, as Hajicek points out, if we want to define intrinsically the evolving constants in the quantum theory, we must use Eqs. (29) and (30) of Ref. [3]. The reason I tentatively introduced this intrinsic definition is the hope that some approximation scheme may be developed in order to construct the quantum evolving observables order by order, without solving the classical theory first.

The problems with Eq. (32) of Ref. [3] and the bad notation of Eq. (17) in Ref. [3] were earlier pointed out to me also by Jacobson [4]. The fact that the presence of a time and a Hamiltonian provide the factor ordering *for every t*, while in the formalism without time the factor ordering problem is more serious (one has to find a good ordering for each *t*), was repeatedly pointed out also by Kuchař [5].

In conclusion, it is important to make a distinction between two questions: (a) Does a quantum theory without Hamiltonian time evolution make sense as a physical theory? (b) How difficult is it to find such a theory if we only know its classical limit? To my understanding, Hajicek's comments do not invalidate the main claim of Refs. [2,3], namely, that question (a) has a positive answer: A quantum theory without time can be well defined, consistent, have a sensible classical limit and a well-defined probabilistic interpretation. On the other hand, Hajicek's comments bear on question (b) and on the difficulty of actually *constructing* the quantum theory starting from a given classical theory. Hajicek's comments are certainly helpful in reaching a deeper insight into this conceptually difficult subject.

[1] P. Hajicek, preceding Comment, Phys. Rev. D **44**, 1337 (1991).
[2] C. Rovelli, Phys. Rev. D **42**, 2638 (1990).
[3] C. Rovelli, Phys. Rev. D **43**, 442 (1991).
[4] T. Jacobson (private communication).
[5] K. Kuchař (private communcations).