Von: "AJP Editorial Office" <ajp@dickinson.edu> Donnerstag, 12. Januar 2012 22.39 Uhr
An: ajp@dickinson.edu; Ulf.Klein@jku.at
Betreff: MS 24950 received by AJP

Dear Professor Klein,

We acknowledge the submission of your manuscript entitled, "What is the limit $\hbar \to 0$ of quantum theory ?," which we have assigned the manuscript # 24950. Please refer prominently to this number in all correspondence.

Please do not send any revisions before receiving further correspondence from us. Revisions sent in the middle of the review process will delay the review process.

We try to provide a report to authors within three months of the date of receipt, but delays are sometimes unavoidable. Authors can check the status of their manuscript by going to https://editorialexpress.com/s.cgi?i=acb2a4c7facd55afb31cbac21c24b3ce

Thank you for your submission to AJP.

Sincerely yours,

David Jackson

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David P. Jackson, Editor
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Von: AJP Editorial Office <ajp@dickinson.edu> Donnerstag, 1. März 2012 16.35 Uhr
An: ajp@dickinson.edu; Ulf.Klein@jku.at
Betreff: AJP: decision on your manuscript #24950
Anlagen: 2 Anlagen
- AJP MS24950-1 R1's report.pdf (63 KB) Anzeigen
- AJP MS24950-1 R2's report.pdf (59 KB) Anzeigen

Download

Dear Professor Klein,

Attached you will find copies of the reviewers' reports on your manuscript "What is the limit $\hbar \to 0$ of quantum theory ?," MS 24950. As you can see, these reviewers favor publication, but raise some important points for you to consider and address. Any revised manuscript should NOT just make the specific changes suggested by the reviewers. Instead carefully consider the recommendations in context, and revise the manuscript to make sure that there exists a coherent and logical story line for the manuscript.

If you wish to revise your manuscript along the lines indicated, we would continue its editorial consideration once it has been resubmitted using the procedure indicated on the AJP website <ajp.dickinson.edu>. If you do resubmit, please indicate in a single cover letter how you have responded to the various comments of the reviewers. DO NOT send separate replies for each reviewer.

Thank you for your interest in the American Journal of Physics.

Sincerely,

David Jackson

AMERICAN JOURNAL of PHYSICS
David P. Jackson, Editor
Daniel V. Schroeder, Associate Editor
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Review of AJP MS 24590, “What is the limit $\hbar \to 0$ of quantum theory”, by U. Klein

This paper address an interesting, and now almost ninety-year old, question: in what sense can classical mechanics be understood as the limit of quantum mechanics? The author takes care to define the “limit” here in a precise manner, discussing two different ways in which this limit can be taken. The paper is well-organized, and the English is very readable. I noted only a few minor errors, which I have listed at the end of this report. The manuscript appears technically correct, although I have to say that I found some of the epistemological discussion in Section VIII a little unnecessary and slightly grating. For example, I am not sure that I agree with the author’s definition of a “complete” theory. Furthermore, I disagree that the correspondence principle is “dogma”. It seems more an inspired guess, justified *a postieri* by the path-integral formulation of quantum theory, on which I will have more to say below. I am also not sure that all physicists would agree with the final statement in the paper. Indeed, a more Bayesian (or perhaps Pascalesque) understanding of probability could be invoked in order to avoid the need to introduce hypothetical ensembles in order to describe a single particle.

Leaving these (somewhat personal and metaphysical) complaints aside, the clarification regarding the classical limit of Schrödinger equation here could be very useful for students of physics, since this subject is one that is often glossed over in quantum-mechanics courses. The presentation is at a level that an advanced undergraduate or beginning graduate student could mostly understand. It should be noted, though, that there is some knowledge of, e.g. kinetic theory and statistical mechanics assumed, in that the Fokker-Planck equation is employed without very much discussion. Regardless, the material could easily be mediated by a motivated instructor who wished to discuss what, beyond Ehrenfest’s theorem, was needed in order to guarantee that classical mechanics emerges in an appropriate limit from quantum mechanics.

A small point in this regard is that, given the central role given to that theorem in both past discussions of the problem, and the presentation here, it seems to me that it might be worthwhile to include the Ehrenfest relations, (15) & (16), earlier than is the case in the present manuscript.

The only significant problem with this paper is that everything Klein presents is based on the Schrödinger equation, and hence founded in the Schrödinger picture of quantum mechanics. It would be useful if he/she added some discussion about how these arguments manifest themselves in the Heisenberg picture. There the commutator in the Heisenberg equation of motion would seem to go straightforwardly over to the corresponding Poisson bracket, thereby yielding the classical equation of motion for the quantity of interest, if the limit $\hbar \to 0$ is taken.

Perhaps more significantly, there is no discussion of the construction of the classical limit in the path-integral approach to quantum mechanics. As many textbooks emphasize, in the limit $\hbar \to 0$ the classical path becomes the dominant contribution to the path integral. Indeed, the recovery of the classical result in that limit is rigorous and straightforward if one is prepared to accept to an analytic continuation to imaginary time. And even if one is not, the result can still be obtained by appeal to Lebesgue integration in the case of a rapidly varying phase.

Regarding the arguments given in the current version of the manuscript, I would like to see further discussion of the order-of-limits problem that this work implicitly raises. Does it matter whether one takes $\varepsilon \to 0$ or $\hbar \to 0$ (i.e. the "deterministic" or the "standard") limit first? If I can take these limits in either order and still recover Newton's equations of motion then that should be stated in the paper. The argument in its present form seems to imply that there is something special about choosing to combine the limits such that $\varepsilon/\hbar$ is fixed.

As for the treatment of earlier research in this area, I am not very familiar with the literature on this subject. It did occur to me that a reference to Ehrenfest's original work might be appropriate, but references to such "classics" are somewhat optional. On the whole Prof. Klein seems to have tried to do justice to previous studies that have made efforts in the same direction.

Finally, I list a few minor problems with the English:
1. Below Eq. (7), second sentence "Note that using *these* variables..."
2. Last sentence of first paragraph of Section 6 "There are no in-principle constraints on..."
3. p. 9, last sentence of first paragraph, "But, despite intense research,..." I.e. there is no "of" needed here.
4. p. 9, fifth paragraph, second-last sentence, "...becomes *negligibly* small..."

Report of referee on paper of U. Klein (MS249..)

This is a nice paper which presents a clear discussion of significant issues and merits publication in AJP, after some modification of the presentation.

Title: The Physical Review has long had a policy of not allowing a title in the form of a question. I don't know about AJP, but I for one would not accept it, especially in this case, since it sets up the reader to think an answer will be provided. An alternative title might be

"h → 0" and the connection between quantum and classical mechanics"

Abstract: It's too short and does not do justice to the scope of the paper. I suggest something like the following.

"An analysis is made of the relation between QM and CM, in the context of the so-called "h → 0" limit. The various ways in which this limit may be interpreted is reviewed. It is shown that the Schrodinger equation for a single particle moving in an external potential V does not, except for special cases, lead, in this limit, to Newton's equation of motion for the particle."

(Remark: In general the phrase "h →0", time-honored though it is, should be avoided in a partly pedagogical paper, since h is not dimensionless. See below.This is why I have put quotes on it.)

Introduction: I wish the author would change this. It reads like a colloquium lecture rather than a sober discussion of an important topic in a journal of record.

In the first paragraph, the author sets up a straw man, inventing a student-teacher situation by quoting one sentence from Dirac's book . Then, by way of dramatizing the subject matter, he gives as possibilities i) Dirac is wrong ii) The answer is not known. He thereby sets up what I would regard as a misleading, not to say false dichotomy. As I am sure the author is aware, it has been known for a long time (see e.g. the papers of M. Berry), that this so-called limit is singular. This fact should surely be mentioned by the author before he launches into his otherwise cogent analysis of a specific situation. It is not necessary to be told that various great physicists were in agreement with Dirac to justify the author revisiting this subject and to speculate on what various other authors took for granted. I would scrap all that. It is enough to say that Dirac's dictum is too broadly stated and requires modification, instead of the dramatic "Dirac is wrong."

Remark: Apart from the question mark, the current title, often used by others, is also unsatisfactory for presentation to readers of AJP because h is not dimensionless : One must specify some features of the physical system under consideration and divide h by some quantity with the dimension of action before the question makes sense, as one has to do in special relativity, using the ratio v/c where v is some comparison velocity. [The bracket (v/c) on the left-hand side of (3) is best omitted–it makes it look as if v/c is a multiplier of gamma.]

There are many ways of introducing the subject in a less polemical and more scholarly way, in keeping with the tenor of the rest of this otherwise fine paper . I am sure the author is capable of finding one.

MS 245950, "What is the limit $\hbar \to 0$ of quantum theory ?"

Dear Editor,

Please find enclosed the revised manuscript MS 245950 which I would like to resubmit for publication in AJP.

In the following I describe my response to the two referee reports and the corresponding changes made in the manuscript (first and second referee are referred to as R1 and R2). The points raised by R2 and R1 are listed in consecutive order; there is no overlap between the recommendations of the two referees.

R2:

1.) Title,

The title suggested by the referee is similar to my title, but slightly misleading because the 'connection between quantum and classical physics' is a wide field which includes for example the transition from microscopic to macroscopic physics. Such questions do not belong to the subject of my manuscript, which describes a very specific problem, namely the problem posed by Dirac (and described by him in very similar words), and I think this problem is more precisely described by the present title.

Question mark in title:
R2 notes that a question mark sets up the reader to think an answer is provided. I do provide, e.g. on p. 4, an answer, at least as far as the 'standard limit' is concerned: "The two equations (8) and (10) which will be referred to as probabilistic Hamilton-Jacobi theory (PHJ) constitute the classical limit of Schrödinger's equation or 'single-particle' QT, respectively." (from the context it is clear that the 'classical limit' is the limit \hbar \to 0). Thus, following the criterion of R2, the question mark is not misplaced.

\hbar is not dimensionless (Two remarks of R2).
As the referee himself points out, it is long-standing common usage to refer to the vanishing \hbar without dividing it by a reference quantity of dimension of action. In fact, I do not see any way to settle this point of R2 without creating confusion. To write \hbar/S could lead to the misleading impression that this is the essential parameter for the transition from QT to NM (as v/c is for the transition from from SRT to NM) . Here the situation is much more complex, as shown in my manuscript (the density \rho is also important). See in particular the remarks at the beginning of section 3 and the second paragraph of section 6.

2. Abstract (second point of R2)

The abstract has been rewritten, following closely the suggestion of R2.

3. Introduction (third point of R2)

The student-teacher situation and the two virtual statements of the teacher (the 'dramatic' "Dirac is wrong" and the "The answer is not known") have been eliminated. Thus there is no possibility any more to misunderstand these statements. The (silent) student still appears briefly in the second paragraph. The first paragraph contains now not more than a coherent selection of facts describing the motivation for the present work. The second student-teacher scene in the last paragraph of section 8 has also been removed, and replaced by an equivalent but more abstract discussion.

4. R2 suggests to incorporate the statement "...this so-called limit is singular" in the introduction. I mentioned the singular nature of this limit in the second paragraph of the introduction and a second time, in a more technical context and with a reference, on p.2, see text following Eq.(7).

5. The discussion of the situation of the Coulomb problem (p.9, upper part) is an important part of my argumentation and should not be abandoned. The nonexistence of the coherent states of the Coulomb problem is the important point. R2 does not explain why 'is it not necessary to be told that various great physicists...' (is it also 'polemical' ?). I think this historical remark makes the manuscript more readable and contributes to an understanding of the situation.

6. I eliminated the misleading notation (bracket on the left-hand side) in Eq.(3).

R1:

1. The few minor errors mentioned by R1 in the first paragraph (and reported at the end) of his report have been corrected.

2. My way of introducing 'completeness' (in the upper part of p.10) is in fact somehow short and abrupt. I should mention that these statements are not born out of a spontaneous idea but represent my summary of a detailed analysis of the meaning of the crucial term 'completeness' in QT. I added a remark and a reference in order to indicate that more should be said about this point but cannot be said due to lack of space.

3. In the next paragraph (the one starting with 'Einstein...'), the word 'deliberately' has been removed and the following sentence has been reformulated. I hope this decreases what R1 calls 'slightly grating'. A comment in brackets has been added near the end of this paragraph. In the following paragraph (the one starting with 'The fact that NM...') the term 'dogma' as a characterization of the correspondence principle has been eliminated.

Remark: R1' comment on Bayesian understanding of probability shows that his interpretation of QT is very different from mine (which is the 'statistical' one). I appreciate his tolerance of deviating opinions. The statistical interpretation of QT does not introduce hypothetical ensembles in order to describe single particles. Despite its minority character (which explains my interest in discussing questions of interpretation), the statistical interpretation belongs to the established interpretations of QT. A reference to a (very good but slightly outdated) review article describing this interpretation in detail has been added at the end of section 8

4. The problem raised by R1 in his fourth paragraph is that everything I derived might possibly only hold for the particular version of quantum-dynamics (Schrödinger picture) which I used in the manuscript and might break down for other versions of quantum-dynamics (Heisenberg picture) of QT.

   To make clear that this is not the case, I added in the introduction some remarks explaining in more detail the fact that the concept of a physical theory (and also the concept of a limiting theory) used in the manuscript is based on *predictions* (these remarks are also relevant in view of the the second, more general critical comment of R1 dealt with in point 5). These predictions are real numbers which can be calculated by means of the quantum-mechanical formalism and do *not* depend on the dynamics; the various descriptions of quantum dynamics are related to each other by unitary transformations which leave all predictions invariant. This point is also addresses in the second paragraph of section 8.

   A second comment of R1 in the fourth paragraph concerns Heisenberg's operator equations of motion and their relation to NM. Equations of motions for Hilbert-space operators are abstract objects which do, except in very special cases, only share formal similarities with c-number equations. To discuss such points would require much space. I added a comment (...analogy Poisson brackets and commutators..) in the introduction to make even more clear that structural similarities do not belong to the subject of this work. There is no mathematically well-defined way to create c-number equations from operator equations by means of a limiting process \hbar -> 0 (if such a process existed its inverse would be an extremely attractive, 'continuous' quantization method).

5. In the fifth paragraph R1 addresses the path-integral approach to quantum mechanics.

   I added a longer paragraph (after the second paragraph of section 8) discussing the path integral method in the limit \hbar –> 0. There I explain why the fact, that the dominant contribution to the Feynman propagator comes from the classical path, does not imply that the classical limit of Feynman's formulation is NM. The classical limit of Feynman's version of QT is the same classical statistical theory (Eqs. 8 and 10) as the classical limit of Schrödinger's equation. Any other result would be strange in view of the fact that Feynmans quantum mechanics is equivalent to Schrödinger's formulation. In the second paragraph of section 8 I point out that my results are general results of quantum theory and do not depend on a particular formulation.

6. In the second paragraph on the second page, R1 addresses the order-of-limit problem.

   The two limiting processes \epsilon -> 0 and \hbar->0 do in fact not commute with each other, as shown in sections 3-5. In section 3 the standard limit \hbar->0 is performed for arbitrary states including wave packets with fixed width \epsilon. In section 4 the deterministic limit \epsilon -> 0 is then performed as second limit, after the limit \hbar->0. In section 5 it is shown that the inverse process (first \epsilon -> 0 then \hbar->0) cannot be performed since the limit \epsilon -> 0 (for \hbar fixed) does not exist. Thus the two limiting processes clearly do not commute. In section 6 both limits are performed *simultaneously* and this limit (performed in such a way that the ratio \epsilon over \hbar->0 is kept fixed) is in fact something special since the \hbar->0 transition from QT to NM can be performed in this way (by combining both limits), at least for a few potentials.

All this has already been explained in the manuscript but further explanation making it even more explicit may be useful. Following this suggestion of R1, I added a paragraph at the beginning of section 6. A small corresponding change has been made in the first paragraph of section 8.

All points raised by the referees have been discussed. Points 5, and also 4,6, of R1 required extensions rather than modifications of the manuscript but seem useful for potential readers. They led to an increase in length of the manuscript of about a page.

Sincerely

U.Klein

Von: AJP Editorial Office <ajp@dickinson.edu> Mittwoch, 2. Mai 2012 21.24 Uhr
An: ajp@dickinson.edu; Ulf.Klein@jku.at
Betreff: AJP: decision on your manuscript #24950
Anlagen:
2 Anlagen
AJP MS24950-2 R1's report.pdf (68 KB) Anzeigen
AJP MS24950-2 R2's report.txt (1 KB) Anzeigen

Dear Professor Klein,

Attached you will find copies of the reviewers' reports on your manuscript "What is the limit $\hbar \to 0$ of quantum theory ?," MS 24950. As you can see, these reviewers favor publication, but one still raises a number of important points for you to consider and address. In addition to these comments, I might add that while I find your manuscript to be pretty clearly written, I do not think it will be very easy for non-specialists to follow. Please bear this in mind as you revise your manuscript.

As before, any revised manuscript should NOT just make the specific changes suggested by the reviewers. Instead carefully consider the recommendations in context, and revise the manuscript to make sure that there exists a coherent and logical story line for the manuscript.

If you wish to revise your manuscript along the lines indicated, we would continue its editorial consideration once it has been resubmitted using the procedure indicated on the AJP website <ajp.dickinson.edu>. If you do resubmit, please indicate in a single cover letter how you have responded to the various comments of the reviewers. DO NOT send separate replies for each reviewer.

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Sincerely,

David Jackson

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Daniel V. Schroeder, Associate Editor
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Attachments:

R1's report
R2's report

Second review of AJP MS 24590, "What is the limit ħ→0 of quantum theory", by U. Klein

I have read the revised version, and appreciate Dr Klein's efforts to clarify issues such as the "order of limits" problem. A number of the comments in my earlier report have been dealt with satisfactorily.

However, I still consider a number of statements in this paper regarding the connection between classical and quantum mechanics to be too strong. In this, I find myself in agreement with Referee 1, who urged Dr Klein to tone down his statements. While that has been done in certain places, the MS as it presently stands assumes that there is only one correct way to define this limit: the way in which Dr Klein defines it at the start of his paper.

The discussion Dr. Klein has added of the path-integral formulation illuminates the conflict between our views on this point. I would say "I am satisfied that classical mechanics emerges from quantum mechanics if I take a localized state, evolve it in time, and obtain a localized state, with the position around which the latter is localized obtained from Newton's equations of motion for the potential in question". That the overlap ${}_{t'}\langle x'|x\rangle_t$ (of eigenstates of the position operator, x(t), within the Heisenberg formulation, at different times) comes out as predicted by Newton's equations of motion in the limit ħ→0 is clear within Feynman's path-integral approach. I would argue that it was this sort of emergence of classical physics from quantum dynamics that Dirac was talking about in his book (hence his use of wave packets). Hence I bridled at Dr Klein's statement on p. 1: "A closer look at Dirac's book shows, however, that at least as Dirac's original intentions are concerned, this statements should in fact be interpreted in this simple way." How can Dr Klein know Dirac's intentions so precisely? Surely the text is open to interpretation in this respect? I find this statement to be tendentious and unnecessary.

Of course, as emphasized by Dr Klein, the equivalence of different formulations of quantum mechanics means that the overlap ${}_{t'}\langle x'|x\rangle_t$ must–given my assumptions–behave in a classical fashion no matter which formulation one uses to calculate it. In my first referee report I encouraged Dr Klein to address the central question of the paper within other formulations of quantum theory since those formulations focus on different quantities (Schrodinger: wave function, Heisenberg: operator, Feynman: state overlaps). By working in the Schrodinger picture Dr Klein biases himself to think in terms of wave functions. This may push him into making stronger statements about the classical limit than are strictly justified; only overlaps of wave functions are actually measurable.

Indeed, equations in physical theories are always set up in order to track certain variables, and the variables chosen pre-condition our way of thinking. ψ is clearly a rather different kind of variable to x and p, and so a number of different ways of understanding the way that definite values of the latter emerge from the former in a "classical limit" may be possible–including the one I have suggested above.

Another example of this “variables pre-condition thinking” occurs on p.3 where it is asserted that Eq. (5) defines “a probabilistic (indeterministic) theory”. In fact, the equation is completely deterministic for the quantity ρ. It is just that the possibility to trace the motion of individual particles has been lost, by elimination of the original variables x and p in favour of this quantity ρ. The narrow notion of determinism employed by Dr Klein here also afflicts his discussion of quantum mechanics. Quantum mechanics is a deterministic theory. Given a Hamiltonian, H, we can reliably predict the time evolution of the (measurable) wave-function overlaps. It is just that quantum mechanics is not deterministic for classical variables such as x and p. But who says that determinism must be defined in terms of these variables?

None of this is in contradiction with Dr Klein’s analysis which shows, very clearly, that what he calls the deterministic limit must be taken first, or, at least simultaneously, with the $\hbar \to 0$ limit. (By the way, just out of curiosity: is a linear relation between ε and $\hbar$ necessary for this? I.e. what is the minimum power of $\hbar$ in $\varepsilon \sim \hbar^n$ under which the classical equations would emerge? And how am I to understand the dimensionful quantity which must appear on the right-hand side in such a relation.) In my definition of “the classical limit” it is clear that $\varepsilon \to 0$ is taken right at the start of the calculation. By emphasizing that, absent this step, classical mechanics is not recovered, Dr Klein is performing an important service to students, and to the community. However, I think he is overstating what his calculations have shown when he says (e.g. in the Abstract) “classical mechanics cannot be regarded as the limiting case of quantum mechanics for $\hbar \to 0$”. To accept this statement requires that I accept his definition of what this limit must entail. The addition of some qualification here, e.g. “This shows that classical mechanics cannot be regarded as emerging from quantum mechanics–at least in this sense–upon straightforward application of the limit $\hbar \to 0$.” Analogous changes at the numerous points throughout the paper where similar statements are made are necessary for the paper to be suitable for publication in the *American Journal of Physics.*

first referee

I accept the author’s responses to my comments. However, I have one last suggestion for the title:

"Analysis of the \hbar --> 0 limit of quantum mechanics."

In my opinion this describes the paper perfectly, no question about it!
I leave it for the editor and author to decide this matter.

I extend my congratulations to Prof. Klein for an excellent, lucid analysis.

MS 245950, "What is the limit $\hbar \to 0$ of quantum theory ?"

Dear Editor,

Please find enclosed the revised manuscript MS 245950 which I would like to resubmit for publication in AJP.

In the following I list the changes made in the manuscript. All recommendations have been taken into account.

Sincerely

U.Klein

R2's report:

I accept the author’s responses to my comments. However, I have one last suggestion for the title:
"Analysis of the \hbar --> 0 limit of quantum mechanics."
In my opinion this describes the paper perfectly, no question about it!
I leave it for the editor and author to decide this matter.
I extend my congratulations to Prof. Klein for an excellent, lucid analysis.

my response:

I am very glad to read this last comment on my work. The title suggested by R2 is indeed very good. I leave the decision to change or keep the title to the editor (for me both are equivalent).

R1's report and my response:

Second review of AJP MS 24590, “What is the limit $\hbar \to 0$ of quantum theory”, by U. Klein

I have read the revised version, and appreciate Dr Klein’s efforts to clarify issues such as the “order of limits” problem. A number of the comments in my earlier report have been dealt with satisfactorily.

However, I still consider a number of statements in this paper regarding the connection between classical and quantum mechanics to be too strong. In this, I find myself in agreement with Referee 1, who urged Dr Klein to tone down his statements. While that has been done in certain places, the MS as it presently stands assumes that there is only one correct way to define this limit: the way in which Dr Klein defines it at the start of his paper.

----

my comment:
Please note that referee 1's (who is denoted by R2 here) critical remarks referred to the original version and not to the present, revised version of my manuscript.

The discussion Dr. Klein has added of the path-integral formulation illuminates the conflict between our views on this point. I would say “I am satisfied that classical mechanics emerges from quantum mechanics if I take a localized state, evolve it in time, and obtain a localized state, with the position around which the latter is localized obtained from Newton’s equations of motion for the potential in question”. That the overlap t′<x′|x>t (of eigenstates of the position operator, x(t), within the Heisenberg formulation, at different times) comes out as predicted by Newton’s equations of motion in the limit $\hbar \to 0$ is clear within Feynman’s path-integral approach. I would argue that it was this sort of emergence of classical physics from quantum dynamics that Dirac was talking about in his book (hence his use of wave packets). Hence I bridled at Dr Klein’s statement on p. 1: “A closer look at Dirac’s book shows, however, that at least as Dirac’s original intentions are concerned, this statements should in fact be interpreted in this simple way.” How can Dr Klein know Dirac’s intentions so precisely? Surely the text is open to interpretation in this respect? I find this statement to be tendentious and unnecessary.

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my response:
I replaced the sentence “A closer look at Dirac’s book shows, however, that at least as Dirac’s original intentions are concerned, this statements should in fact be interpreted in this simple way.” by the sentence: "But this answer is not really satisfying."

Of course, as emphasized by Dr Klein, the equivalence of different formulations of quantum mechanics means that the overlap t′<x′|x>t must–given my assumptions– behave in a classical fashion no matter which formulation one uses to calculate it. In my first referee report I encouraged Dr Klein to address the central question of the paper within other formulations of quantum theory since those formulations focus on different quantities (Schrodinger: wave function, Heisenberg: operator, Feynman: state overlaps). By working in the Schrodinger picture Dr Klein biases himself to think in terms of wave functions. This may push him into making stronger statements about the classical limit than are strictly justified; only overlaps of wave functions are actually measurable.

Indeed, equations in physical theories are always set up in order to track certain variables, and the variables chosen pre-condition our way of thinking. ψ is clearly a rather different kind of variable to x and p, and so a number of different ways of understanding the way that definite values of the latter emerge from the former in a "classical limit" may be possible–including the one I have suggested above.

Another example of this "variables pre-condition thinking" occurs on p.3 where it is asserted that Eq. (5) defines "a probabilistic (indeterministic) theory". In fact, the equation is completely deterministic for the quantity ρ. It is just that the possibility to trace the motion of individual particles has been lost, by elimination of the original variables x and p in favour of this quantity ρ. The narrow notion of determinism employed by Dr Klein here also afflicts his discussion of quantum mechanics. Quantum mechanics is a deterministic theory. Given a Hamiltonian, H, we can reliably predict the time evolution of the (measurable) wave-function overlaps. It is just that quantum mechanics is not deterministic for classical variables such as x and p. But who says that determinism must be defined in terms of these variables?

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my comment: I agree: the equation for \rho is completely deterministic.
I did not talk about the equation but about the (physical) theory which
entails an equation and an interpretation of the mathematical symbols
occuring in the equation. The point is that experiment tells us that \rho
in single-particle QT is not measurable in single particle measurements:
as R1 points out, the possibility to trace the motion of individual particles
has been lost. The theory [which entails the form of the equation as well
as the physical meaning of the variables (\rho)] is unable to predict the
data observed in single particle (photons,electrons) experiments (most
clearly visible in the beautiful double-slit data of Tonomura,
see: http://www.hitachi.com/rd/research/em/movie.html). It is quite
common to use the term 'indeterminism' in the sense I am using it.

my response:
This is an important point to make completely clear. I added a detailed
explanation of the term 'indeterministic theory' in the last paragraph of
the introduction.

None of this is in contradiction with Dr Klein's analysis which shows, very clearly, that what he calls the deterministic limit must be taken first, or, at least simultaneously, with the $\hbar \to 0$ limit. (By the way, just out of curiosity: is a linear relation between ε and $\hbar$ necessary for this? I.e. what is the minimum power of $\hbar$ in $\varepsilon \sim \hbar n$ under which the classical equations would emerge? And how am I to understand the dimensionful quantity which must appear on the right-hand side in such a relation.)
my comment: (only the linear relation leads to a proper cancellation of

singularities)
In my definition of
"the classical limit" it is clear that $\varepsilon \to 0$ is taken right at the start of the calculation. By emphasizing that, absent this step, classical mechanics is not recovered, Dr Klein is performing an important service to students, and to the community. However, I think he is overstating what his calculations have shown when he says (e.g. in the Abstract) "classical mechanics cannot be regarded as the limiting case of quantum mechanics for $\hbar \to 0$". To accept this statement requires that I accept his definition of what this limit must entail. The addition of some qualification here, e.g. "This shows that classical mechanics cannot be regarded as emerging from quantum mechanics–at least in this sense–upon straightforward application of the limit $\hbar \to 0$." Analogous changes at the numerous points throughout the paper where similar statements are made are necessary for the paper to be suitable for publication in the American Journal of Physics.

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my response:

I replaced the statement"classical mechanics cannot be regarded as the limiting case of quantum mechanics for \hbar$\to$0" in the abstract by the statement "This shows that classical mechanics cannot be regarded as emerging from quantum mechanics – at least in this sense–upon straightforward application of the limit $\hbar \to 0$." Analogous changes have been made at all other places (p. 8, middle of p.9, middle of p.10, end of p.10) where similar statements are made.
