Von: "Foundations of Physics" <renlethjoyviterbo.pural@springer.com>
Mittwoch, 23. September 2009 15.54 Uhr

An: <ulf.klein@jku.at>, <ulf.klein@gmx.at>

Betreff: Submission Confirmation
Anlagen: Mime.822 (3 KB) [Anzeigen] [Speichern unter]

Dear Professor Ulf Klein,

Thank you for submitting your manuscript, "The statistical origins of quantum mechanics", to Foundations of Physics.

We will take your manuscript into consideration. If it is suitable for our journal, it will be reviewed through our peer review system. On average, an editorial decision may be expected within six months after the manuscript is submitted for review.

If your manuscript is not suitable for our journal (to be decided by the editorial board), you will be notified within three months.

During the review process, you can keep track of the status of your manuscript by accessing the following web site:

TEXT WHICH IS EITHER UNINTERESTING
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With kind regards,

Gerard 't Hooft, Chief Editor of Foundations of Physics

Von: ulf klein <klein.ulf@gmail.com> Donnerstag, 31. Dezember 2009 12.22 Uhr
An: <ulf.klein@jku.at>
Betreff: Fwd: Editorial Decision on FOOP1410
Anlagen: Mime.822 (17 KB) [Anzeigen] [Speichern unter]
---------- Forwarded message ----------
From: Foundations of Physics <renlethjoyviterbo.pural@springer.com>
Date: 2009/12/21
Subject: Editorial Decision on FOOP1410
To: klein.ulf@gmail.com, ulf.klein@gmx.at

Dear Professor Klein,

We have received the reports (see below) from our reviewers on your manuscript,
"The statistical origins of quantum mechanics", which you submitted to Foundations of Physics.

Based on the advice received, the editors have decided that your manuscript is rejected for publication. However, it could be reconsidered for publication should you be prepared to incorporate major revisions. In case you decide to prepare a revised manuscript, you are asked to carefully consider the reviewer comments (especially by reviewer #2) which are attached, and submit a list of responses to the comments.

Please find attached the reviewer comments for your perusal.

Submissions without original source files will be returned for these prior to final acceptance.

Please also submit your response as separate submission item.

In order to submit your revised manuscript, please access the following web site:

TEXT WHICH IS EITHER UNINTERESTING
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In case you decide to send in a revised manuscript, we appreciate receiving it before 22 Feb 2010

With kind regards,

Gerard 't Hooft
Chief Editor

COMMENTS FOR THE AUTHOR:

Reviewer #1: The paper seems well-written and contains results that seem correct and would be of interest to readers of Foundations of Physics. I have several suggestions for improvement of the manuscript, although I do not propose them as necessary conditions for acceptance. I would encourage the author to attempt to reduce the number of pages. I believe there are many lengthy discussions and derivations that can be ommitted and/or simplified.

Page 3:
1)I don't understand the comment about no need for initial values in what the author calls "type 3" theories. Indeterministic theories like QM do require one to specify the initial values of, say, the wavefunction in order to apply unitary evolution.

2)I believe Peres' intention with the term cryptodeterminism was not to refer to classical statistical mechanics, but to theories that attempt to reproduce quantum mechanics from underlying deterministic theories (like Bohmian mechanics), and in which the hidden variables are necessarily inaccessible. Thus the prefix 'crypto'.

Page 10:
3)(1st par.) The word entanglement has a very well-defined technical meaning, and it is being used here in a loose sense that may be misleading. The author should explain in which sense this dependence he refers to is technically equivalent to entanglement, and if this is not the case, omit that word.

Page 25:
4) On line 34, I would like a clarification. Is the author claiming that he can derive the Hilbert space structure from the Schrodinger equation, or that it may be obtained in the future? That was definitely not shown in this paper, and in fact the paper did not show anything about other postulates of quantum mechanics, e.g. why hermitian operators correspond to observables, and their eigenstates to possible outcomes of measurements. This sentence should be more clearly qualified. It may just not be true, and that is my suspicion, that the Hilbert space structure with all of its consequences can be derived from these assumptions at all. In fact, this fact (and the fact that no derivation of the tensor product structure is presented either) should be clarified in a prominent position in the paper to avoid misleading readers to believe that the author has derived ALL of the quantum mechanical formalism from his assumptions.

5) On that note, I would like to see some comments about the possibility of extending this work to discrete (spin) degrees of freedom.

Reviewer #2: The heart of the paper is an attempt to derive Schroedinger's equation (for one particle moving in one spatial dimension) from other, more basic assumptions. The idea, I gather, is to argue that some vaguely-stochastified version of classical physics -- plus some other additional constraints -- imply an equation that looks like Schroedinger equation, and so to argue in favor of a statistical interpretation of QM.
In my opinion, however, the whole thing amounts to a kind of numerological tinkering which provides no real insight into the nature of QM. For one thing, the author is too quick to identify QM with Schroedinger's equation. QM is actually a lot more than this, including, for example, the operators-as-observables structure and also measurement axioms. So the mere fact that one can start with some semi-familiar looking equations, and then wave one's arms cleverly for 10-20 pages, and end up with the Schroedinger equation, simply does not mean that one
has "derived QM". I am also troubled by the lack of any clear statement about the ontology of the proposed theory. The starting point suggests that the picture is (just as in classical mechanics) a particle following a definite trajectory. And the context-setting discussion in section 3 suggests that the idea is just that there is some (classical-stat-mech type) uncertainty about the initial conditions and/or the laws, which give rise to a theory with a statistical character. If that's the model, it would be good to be clear about that.

But, if that's the model, surely the author should be more aware of (and, in a paper like this, should discuss and cite) things like Nelson's stochastic mechanics and the de Broglie - Bohm pilot-wave theories, both of which are also theories involving particles (in the literal, classical sense) following definite trajectories.

I am also troubled by the failure of the paper to engage with any of the well-known central *puzzles* of QM. For example, Feynman famously explained how the 2-slit experiment contains the essential mystery of QM. How does the author propose to make sense of the 2-slit experiment? (It's because no answer to that kind of question is anywhere in sight, that I claim that no useful physical insight is provided in the paper.) And how about the kind of nonlocality that Bell taught us is required by any empirically adequate theory? Of course the author here only treats the one-particle case, but if the idea is supposed to be that a classical-ish model with underlying definite particle trajectories can reproduce the predictions of quantum mechanics, it seems relevant to confront the question of non-locality. If the generalization of the author's ideas to many-particle-systems involves non-locality (as it must according to Bell's theorem if it is to be empirically adequate) why
should I prefer it to Bohmian Mechanics?

In summary, it doesn't seem like this paper makes a useful contribution to the foundations of physics literature, and I would recommend against its publication.

The following three files (here merged to a single one) were uploaded to the Website of the Editorial Board of Foundations of Physics around January 10, 2010.:

Dear Professor 't Hooft,

Enclosed please find the revised version of my
manuscript FOOP1410:

"The statistical origins of quantum mechanics"

which I would like to resubmit for publication in
Foundations of Physics.

Also enclosed are two separate pdf files containing my response to the two referee reports. I followed most of the recommendations of reviewer #1 and tried to meet the requests of reviewer #2 as far as possible.

I could not follow the request of reviewer #2 to include a discussion of the double-slit experiment and of Bell's theorem. Such a discussion would lead to a considerable increase in length of the manuscript and would be in contrast to the recommendation of reviewer #1, who asked me to reduce the number of pages.

With best regards,

U.Klein

MY RESPONSE TO REVIEWER #1

Reviewer #1: The paper seems well-written and contains results that seem correct and would be of interest to readers of Foundations of Physics. I have several suggestions for improvement of the manuscript, although I do not propose them as necessary conditions for acceptance. I would encourage the author to attempt to reduce the number of pages. I believe there are many lengthy discussions and derivations that can be ommitted and/or simplified.

MY RESPONSE: The paragraph on p.10/11 including Eqs.(33)-(36), which is of a formal nature and of secondary importance, has been eliminated. The calculation on p.12 has been condensed by eliminating Eq.39. Also Eq.43 on p.13 and Eqs.53 and 54 on p.15 have been eliminated. Some points had to be added but the total lenght of the manuscript could be decreased by 1/2 page.

Page 3:
1)I don't understand the comment about no need for initial values in what the author calls "type 3" theories. Indeterministic theories like QM do require one to specify the initial values of, say, the wavefunction in order to apply unitary evolution.

MY RESPONSE: I thought it was clear from the context that I was talking about initial values for particle trajectories; of course initial values for the wave function are still required. A remark on p.3, line 8 has been added to clarify this point.

2)I believe Peres' intention with the term cryptodeterminism was not to refer to classical statistical mechanics, but to theories that attempt to reproduce quantum mechanics from underlying deterministic theories (like Bohmian mechanics), and in which the hidden variables are necessarily inaccessible. Thus the prefix 'crypto'.

MY RESPONSE: I referred to the term "cryptodeterminism" found (probably introduced) in a paper by Moyal - in the introduction of Moyals paper (Ref. 29) this term is used to describe exactly the aspect of classical statistical mechanics referred to in my manuscript. But I agree that the twofold meaning of this term could lead to misunderstandings. To clarify this point I added a reference and a remark on p.3, line 21

Page 10:
3)(1st par.) The word entanglement has a very well-defined technical meaning, and it is being used here in a loose sense that may be misleading. The author should explain in which sense this dependence he refers to is technically equivalent to entanglement, and if this is not the case, omit that word.

MY RESPONSE:
I eliminated the word entanglement from the text and from the list of keywords.

Page 25:
4) On line 34, I would like a clarification. Is the author claiming that he can derive the Hilbert space structure from the Schrodinger equation, or that it may be obtained in the future? That was definitely not shown in this paper, and in fact the paper did not show anything about other postulates of quantum mechanics, e.g. why hermitian operators correspond to observables, and their eigenstates to possible outcomes of measurements. This sentence should be more clearly qualified. It may just not be true, and that is my suspicion, that the Hilbert space structure with all of its consequences can be derived from these assumptions at all. In fact, this fact (and the fact that no derivation of the tensor product structure is presented either) should be clarified in a prominent position in the paper to avoid misleading readers to believe that the author has derived ALL of the quantum mechanical formalism from this assumptions.

MY RESPONSE: What I mean (on line 34 of p. 25) is that the Hilbert space structure can be obtained from the time-dependent Schrödinger equation by means of mathematical analysis, abstraction and generalization. But I did not mean that I did it in this manuscript or intend to do it; this is a task which can be done e.g. as part of a course in quantum mechanics. This non-axiomatic approach to QM is followed in several (older) textbooks. I added a reference on line 36 of p.25 to clarify this point.

Let me just recall here that an attempt to solve the time-dependent Schrödinger equation by separation leads immediately to the eigenvalue problem for the Hamilton operator H. No axiom is required to define hermitian operators; their existence is a consequence of the obvious requirement for real expectation values. To solve the eigenvalue problem of H one is automatically led to ask for commuting operators; here also symmetries and conservation laws enter the game; but again no extra axioms are required, all this is follows from the mathematical structure of the basic differential equation (which includes its symmetries).The same is true - making use of the mathematical concept of completeness of a set of functions - with regard to the definition of transion probabilities, etc. etc. The Hilbert space is a derived concept, developed (also historically) from a mathematical analysis of Schrödingers equation and is of secondary importance as far as foundations of QM are concerned. I hasten to add that the Hilbert space structure is an extremely elegant and powerful mathematical device. What I mean is just that it is not a "conditio sine qua non" for formulating the foundations of QM.

The first (german) edition of "Mathematical Foundations of Quantum Mechanics" by von Neumann appeared in 1932 but the axiomatic approach of this work remained widely unknown for many years. In an encyclopedic reference work (H.A. Kramers, "Die Grundlagen der Quantentheorie - Quantentheorie des Elektrons und der Strahlung", Akademische Verlagsgesellschaft) published in 1938, complete quantum mechanical solutions for many fundamental problems of atomic and many-body physics, including even radiation, may be found without any reference to Hilbert space.

However, I agree partly with the criticism of the referee as regards my claim that I have derived QM. I should be slightly more modest here. I derived Schrödingers equation for a one-dimensional configuration space (as well as the rule for calculating expectation values). According to my view (explained above) one may claim to have derived QM if Schrödingers equation for a 3N-dimensional configuration space has been derived and if gauge fields and spin degrees of freedom have also been taken into account. I believe that all this can be done but have not (yet) done all of that (only 3D and gauge fields, see below). I added a corresponding remark on p.25, line 44. The generalization of the present formalism to three dimensions and gauge fields has already been completed; a corresponding remark and reference has been added on p.25, line 44 and on p.27, line 60.

5) On that note, I would like to see some comments about the possibility of extending this work to discrete (spin) degrees of freedom.

MY RESPONSE: I think I know how to include spin degrees of freedom but this question has not yet been studied in detail (will be done next). I included spin in the list of open questions on p.28, line 17.

MY RESPONSE TO REVIEWER #2

Reviewer #2: The heart of the paper is an attempt to derive Schrödinger's equation (for one particle moving in one spatial dimension) from other, more basic assumptions. The idea, I gather, is to argue that some vaguely-stochastified version of classical physics -- plus some other additional constraints -- imply an equation that looks like Schrödinger equation, and so to argue in favor of a statistical interpretation of QM.

MY RESPONSE: I derived an equation which has the same mathematical form as Schrödingers equation, with all quantities appearing in it having the same dimension and the same meaning as in Schrödingers equation. So what does the reviewer mean by "an equation that looks like Schrödinger equation" ? According to all rational points of view the equation I derived *is* Schrödingers equation and not only "an equation that looks like Schrödinger equation". His remark does simply not make sense. His report contains several other similar comments (see below: "..numerological tinkering..", "..wave one's arms..") where rational reasoning is replaced by rhetoric artistry.I will simply ignore all such comments in the remaining part of my reply keeping my own standards in scientific discourse.

In my opinion, however, the whole thing amounts to a kind of numerological tinkering which provides no real insight into the nature of QM. For one thing, the author is too quick to identify QM with Schrödinger's equation. QM is actually a lot more than this, including, for example, the operators-as-observables structure and also measurement axioms. So the mere fact that one can start with some semi-familiar looking equations, and then wave one's arms cleverly for 10-20 pages, and end up with the Schrödinger equation, simply does not mean that one has "derived QM".

MY RESPONSE: I derived the usual quantization rules as regards the operators x,p,H and the rule for calculating expectation values. The fact that these operators represent observables follows more or less automatically - by mathematical reasoning and abstraction- from Schrödingers equation : an attempt to solve the time-dependend Schrödinger equation by separation leads immediately to the eigenvalue problem for H. To interpret the eigenvalues as possible measurable values of H is then an easy step. The transition probabilities also appear more or less automatically if the probability interpretation of the wave function is combined with the purely mathematical property of completeness of sets of functions. In fact, Schrödingers equation is really the heart of QM insofar as the rest of QM can be obtained from it automatically by logical and mathematical reasoning. Older textbooks on QM follow this path while it is often hidden in modern books by axiomatics.

However, part of the reviewers criticism is justified here. I claimed that QM has been derived in my paper and in doing that I assumed tacitly that the present derivation of Schrödingers equation can be extended to arbitrary number of spatial dimensions, gauge fields and spin. I am convinced that this can be done but have not yet shown. Consequently, I formulated my claim as to the derivation of QM in a more modest way (see p.25, line 44)

I am also troubled by the lack of any clear statement about the ontology of the proposed theory. The starting point suggests that the picture is (just as in classical mechanics) a particle following a definite trajectory. And the context-setting discussion in section 3 suggests that the idea is just that there is some (classical-stat-mech type) uncertainty about the initial conditions and/or the laws, which give rise to a theory with a statistical character. If that's the model, it would be good to be clear about that.

MY RESPONSE: For didactic reasons (to show the contrast) I wrote down the definition of classical momentum and Newtons equation at the beginning of section 3. Has this led the referee to misunderstand my paper completely ? These classical equations do not belong to the set of fundamental assumptions defining my theory. No trajectories (solutions of these classical equations) are used anywhere in the manuscript. The quantities x and p of my theory are random variables, as has been pointed out several times, see e.g. p.4, line 22.

In my opinion there is a second fundamental misunderstanding in the reviewers report which is important because it is very widespread. It could be called the "deterministic dogma". Whenever a physicist points out an (obvious) fact like "the wave function does not provide a complete description of an individual particle" it is concluded that this person votes for a "hidden variable theory". In this way only deterministic theories are allowed and other possibilities are excluded. The underlying assumption is that every theory must be

deterministic with regard to single events. This assumption has
attained over the status of a (deterministic) dogma. But this
assumption is not necessarily true.

It is also possible that the fundamental laws of nature are of a
statistical nature only (deterministic laws for ensembles only, not
for individual events). This is hard to accept because it calls
into doubt the very basis of our scientific (deterministic) thinking.
But this is the very content of QM (without any additional assumptions).
This interpretation is compatible with all experimental results and it
is free of all fundamental conceptual problems (measurement problem,
impossibility to measure the wave function of a single particle [see
the book by Alter and Yamamoto, Ref.1] etc), which are unsolved for
so many years and which are, according to the present point of view,
unsolvable because they do not belong to the kind of questions a
statistical theory can answer.

I have explained most of this already in section 2 and 3 but the referee
has not accepted it because he believes in the deterministic dogma. To
make this point clearer and more explicit I mentioned and briefly
discussed the origin of the misunderstanding, i.e. the deterministic
dogma. This is done on p.25, line 27.

But, if that's the model, surely the author should be more aware of (and, in
a paper like this, should discuss and cite) things like Nelson's stochastic
mechanics and the de Broglie - Bohm pilot-wave theories, both of which are
also theories involving particles (in the literal, classical sense)
following definite trajectories.

MY RESPONSE: There is a certain overlap of ideas between the
present theory and Nelson's stochastic mechanics. The latter is
also based on assumptions which lead to QM. I added a comment on
the relation between these two approaches on p. 27, line 25.

There is no overlap of the present theory with the de Broglie - Bohm
pilot-wave theory. The latter is a modification of QM, which differs
both with regard to the fundamental equations and some of its predictions
from QM. I do not see any need to discuss it because there is no reason
to assume that it is superior to QM.

I am also troubled by the failure of the paper to engage with any of the
well-known central *puzzles* of QM. For example, Feynman famously explained
how the 2-slit experiment contains the essential mystery of QM. How does
the author propose to make sense of the 2-slit experiment? (It's because no
answer to that kind of question is anywhere in sight, that I claim that no
useful physical insight is provided in the paper.) And how about the kind
of nonlocality that Bell taught us is required by any empirically adequate
theory? Of course the author here only treats the one-particle case, but if
the idea is supposed to be that a classical-ish model with underlying
definite particle trajectories can reproduce the predictions of quantum
mechanics, it seems relevant to confront the question of non-locality. If
the generalization of the author's ideas to many-particle-systems involves
non-locality (as it must according to Bell's theorem if it is to be
empirically adequate) why
should I prefer it to Bohmian Mechanics?

MY RESPONSE: The central puzzles discussed in Feynman's book on QM are

mostly a consequence of ill-posed questions. If a statistical theory
like QM is used to describe the behavior of single particles, then a
lot of mysteries and puzzles will be created automatically. These matters,
including Bells theorem, have been discussed from the point of view
of the statistical interpretation many times in the literature [see
also my own very detailed notes (in german) on Feynmans 2-slit
experiment which are available on the internet:
"http://www.tphys.jku.at/group/klein/dslit.pdf"].
My paper presents a single, clearly defined argument in favor of
the statistical interpretation and it took me 28 pages to explain
this argument in detail. It is not a review article and I cannot
include here such space-consuming problems as the 2-slit experiment
or Bell's theorem.

In summary, it doesn't seem like this paper makes a useful contribution to
the foundations of physics literature, and I would recommend against its
publication.

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von Foundations of Physics <renlethjoyviterbo.pural@springer.com>
an klein.ulf@gmail.com;
ulf.klein@gmx.at
Datum 3. April 2010 10:48
Betreff Major Revisions requested FOOP1410R1
Gesendet von editorialmanager.com

Details ausblenden 3. Apr. (Vor 10 Tagen)

Dear Professor Klein,

We have received the reports (see below) from our reviewers on your manuscript,
"The statistical origins of quantum mechanics", which you submitted to Foundations of Physics.

Based on the advice received, the editors have decided that your manuscript could be reconsidered for publication should you be prepared to incorporate major revisions. In case you decide to prepare a revised manuscript, you are asked to carefully consider the reviewer comments which are attached, and submit a list of responses to the comments.

Please find attached the reviewer comments for your perusal.

PLEASE NOTE: YOUR REVISED VERSION CANNOT BE IN .PS OR .PDF AS TYPESETTING CANNOT MANIPULATE THE FILES. IN ORDER TO PROCEED WITH PUBLICATION YOUR ORIGINAL SOURCE FILES ARE REQUIRED.

Submissions without original source files will be returned for these prior to final acceptance.

Please also submit your response as separate submission item.

In order to submit your revised manuscript, please access the following web site:

TEXT WHICH IS EITHER UNINTERESTING
OR PRIVATE (USERNAME, PASSWORD)

In case you decide to send in a revised manuscript, we appreciate receiving it before 07 Jun 2010

With kind regards,

Gerard 't Hooft
Chief Editor

COMMENTS FOR THE AUTHOR:

Reviewer #1: I will maintain my recommendation for publication. However, I would like to reinforce my opinion that the gaps between the present derivation and the full structure of quantum mechanics are not to be neglected.

In particular, the step from a single particle to many particles could well prove to be unsurmountable by this approach. To use an example mentioned by the author at the conclusion of the paper, Schrodinger's and de Broglie's original interpretation of |\psi|^2 as representing the density distribution of an "extended particle" breaks down precisely when one tries to generalise that interpretation to many particles, due to the possibility of entangled states. This work may well suffer from a similar naivety, but in the absence of a proof, I would recommend publication if only so that the idea could be scrutinized by a larger audience.

In support of the general approach and as evidence that it could be of interest to at least some other members of the community, however, I would mention a recent paper by Goyal (http://arxiv.org/abs/0910.2444v1). Perhaps the author would like to explore that paper due to the similarities with the present work.

Reviewer #2: I have read the author's responses to the earlier referee reports and also the revised manuscript. The author apparently didn't agree with anything I wrote in my previous report (which is maybe not too surprising since the report was rather negative) and so didn't in any substantial way revise the manuscript in response to my previous criticisms. I am completely unswayed by the author's (unfortunately dismissive) response to those criticisms, though, so my summary judgment about the paper remains just what it was before: the paper doesn't make any useful contribution to the foundations of physics literature and so doesn't rise to the level needed for publication in this journal.

Let me, though, try to briefly clarify some points that the author doesn't seem to have really understood from my earlier remarks. To begin with, I am not at all dogmatically insistent on determinism. What I insist on, rather, is clarity. For example, I remarked earlier that the ontology of the theory being proposed by the author is unclear. He seems to have understood this as a complaint that the theory isn't deterministic, but that simply isn't what I said or meant at all. The point was this: in most general terms, the author's paper is an attempt to derive the Schroedinger equation (and thus QM in general) from F=ma. Well, F=ma is a theory that is fundamentally about the motion of point particles. If you tell me that you have F=ma in your theory, but that you aren't positing an associated ontology of particles, then I simply don't know what you are talking about. More precisely, I don't know what your theory is talking about. And clearly, the situation isn't
then improved by engaging in (what I previously called) "numerological tinkering" to convert F=ma into Schroedinger's equation. I still simply don't understand what the theory is fundamentally supposed to be *about*. Please note that this is not at all an insistence on a particle ontology (the ontology of, say, the many worlds theory, or GRW theories with "mass field" or "flash" ontologies, are perfectly clear even though they don't involve particles) nor is it an insistence on determinism (again, the dynamical laws of GRW are perfectly clear even though they are fundamentally stochastic).

Another criticism I made before which the author did not understand was my remark that he merely generated something that "looked like" Schroedinger's equation. He objected that if it looks like Schroedinger's equation, it *is* Schroedinger's equation. But this is in general not the case at all. Here is just one example. In the polar decomposition of the wave function in Schroedinger's actual equation, the phase S is not a single-valued function. (It can be multi-valued so long as all the values are equivalent mod h.) This is admittedly not so important when one is working with a one-dimensional configuration space, but (obviously) it is crucial even in the context of relatively simple one-particle situations (in 2-D or 3-D), e.g., the Hydrogen atom. But such a mathematical feature can in no apparent way be derived in a scheme like the author's. At best, it would have to be put in as a further arbitrary axiom. Incidentally, I also find that the author's
understanding of and treatment of the literature pertaining to Nelson's stochastic mechanics is seriously deficient. For example, as I understand it, it is precisely this issue of the impossibility of deriving the appropriately multi-valued character of S from "F=ma plus stochasticity" that led Nelson himself to abandon his own program (or at least the

aspect of it which pertained to trying to derive QM from something classical-ish plus noise).

I will also repeat my previously-expressed concern that the author seems rather oblivious (if not hostile) to the several "bread and butter" issues in the foundations of quantum physics which, it seems to me, should be addressed in at least some way by anybody proposing what amounts to a fundamentally new understanding of QM. I mean, for example, the 2-slit experiment, Bell's nonlocality theorem, and the measurement problem. There are a lot of proposals out there already (such as MWI, GRW, deBroglie-Bohm, Nelson, etc.) for how to understand QM, and they each have a unique and reasonably clear position on this constellation of important issues. If the author wants to propose something new that will in some way compete with those existing proposals, he probably should strive for understanding them better and clarifying how his ideas are superior in at least, say, some one way to some one of these existing theories. Otherwise, people like me who understand the issues and
appreciate the subtle relative virtues and vices among them, will just continue to wonder: why should I care about some superficial attempt to derive something that anyway only superficially resembles *part* of QM from relatively implausible and arbitrary axioms. If it provided a glimmer of some new insight into, say, the wave-particle duality revealed in the 2-slit experiment, or some promising new way of trying to address the measurement problem, or something like that, the superficiality could maybe be forgiven. But without this, not.

So, the author can take those comments for whatever (if anything) they are worth to him. For the editor, I will just repeat that I don't think this paper belongs in Foundations of Physics (especially considering its length). Since evidently the other referee thinks more highly of it than I do, perhaps a third referee should be asked to look at it.

Reviewer #3: In recent years, the challenge of finding simple, physically reasonable principles from which quantum theory can be derived has received considerable interest. In this paper, the author attempts to reconstruct a particular, important part of quantum theory, namely the one-particle Schroedinger equation from physical principles.

I believe that the paper is potentially an important contribution to the literature on this subject, and the author clearly makes a serious effort to articulate the rationale for his proposed principles. However, there are several important issues --- some to do with the presentational style and some to do with the derivation itself --- which ought to be addressed before publication is considered in Foundations of Physics.

General Comments

The presentation mixes together (a) long verbal discussions and justifications, (b) the postulates, mathematical assumptions and the derivation itself.

This is most unfortunate. Readers would be very well served (and grateful) if the paper were reorganised so that these two components were sharply separated.

I would like to see the paper reorganised into two parts:

Part I: a clear and brief statement of the postulates, a crisp derivation of the Schroedinger equation, clearly pointing out (and labelling) all auxiliary assumptions. This should be brief (at most 6 pages), with cross-references to part II. This section should conclude with an itemized list of all postulates and auxiliary assumptions. A model for this part would be Reginatto's PRA paper (which the author cites).

Part II: the remainder of the paper, where verbal discussions and supporting material is given.???

Detailed Comments

Abstract

1. "kind of statistical metamorphosis" is unhelpfully vague; "a set of two relations" is linguistically complex given that this is the abstract.

2. The claim that Schroedinger's equation is derived from three postulates is inaccurate: there are at least 4 postulates, and a number of auxiliary assumptions (some of which the author points out in the paper, and some which he does not point out). While I appreciate the author wishing to present his work in the strongest way, it is important that he find a

more balanced and accurate way of describing the achievement of the paper.

Sec 1. Introduction

?1. p2, lines 10-25. Here, several attempts to derive Schroedinger's equation are cited, but no indication is given about in what way these other works are lacking (do they fail to solve the problem of deriving Schroedinger's equation?) or in what way the present work is different or an improvement. It is important that this be made clear to the reader, so that they are properly motivated to read onwards.

Sec 2. On probability

1. Item 3 in the list: "..but with unknown laws" is confusing (although I understand what is meant)

Sec 3. Statistical Conditions??. In equation (3), p-bar is defined. While this would make sense in a particle-based picture, it is not clear what this would mean in an ensemble picture. To see the problem, note that in quantum theory, the momentum operator is not operationally implemented by measuring position at two successive times (as it would be classically). So, what "p" means operationally in classical and quantum theory differs greatly. See arXiv:0910.2444 for more details.

2. p4, lines 25-32: it is assumed that there are two separate probability distributions, one over x, and one over p. i.e. it is assumed that the probability distribution over phase space factorises. This is a highly non-trivial assumption which needs to be flagged at the very least, and certainly discussed. See e.g. [G. Kaniadakis, "Statistical origin of quantum mechanics", Physica A 307, 172 (2002)] for an example of an approach that does not make this assumption at the outset.

Sec 4. On random variables.

1. The discussion in this section is verbose. Many readers would stop reading at this point. I suggest the author condense the discussion to the bare essentials (one or two paragraphs at most) that are necessary to the mathematical derivation, and put any necessary supporting material into an Appendix.

2. The variable transformation Eq.(14) is not reversible, and is therefore not trivial. An essential feature of the Schroedinger equation has been inadvertently brought in at this point. See Timothy C. Wallstrom's well-known article on single-valuedness of the wavefunction (Phys. Rev. 49, 16131617 (1994)) for discussion on this point. See also Smolin's quant-ph/0609109.

Sec. 5.

1. The verbal discussion (following line 44) should be substantially shortened.

Sec. 7. Entropy as a measure of disorder?

1. This discussion should be substantially shortened and moved to an appendix. It is tangential to the main purpose of the paper.

Sec 8. Fisher information

1. p16 from line 42 onwards, and p17: This discussion should be shortened and moved into an appendix (merged with the discussion from Sec. 7)

Sec. 11 Discussion.

1. p23, line 57 "it is completely unclear why they work" --- this is not accurate; see for example Bohm's textbook Quantum Theory and arXiv: 0910.2444 (and references therein)

2. p24, lines 7-9 "..just a consequence..." is unclear to me.

3. This section ought to be reorganised as follows:

(a) Give an itemised list of all postulates (qualitative statement, followed by quantitative statement).
(b) Give an itemised list of all subsidiary assumptions (of which there are several).
(c) Give brief (maximum 2 lines in each case) itemised discussion of any of these as the author wishes.
(d) Give 2-3 paragraphs of general discussion as the author wishes.

Note from the managing editor:

The author should have particular attention for the comments of the third referee.

27.05.2010:

Dear Professor 't Hooft,

Enclosed please find the revised version of my
manuscript:

"The statistical origins of quantum mechanics"

which I would like to resubmit for publication in Foundations of Physics.

Also enclosed are three separate pdf files containing my response to the three referee reports.

Reviewer #3 addressed two kinds of issues, the presentational style and the derivation itself.

As far as the derivation issues are concerned, I have shown in my comments that most of his points of criticism are based on misunderstandings or errors.

As far as the presentational style is concerned, reviewer #3 requested - despite the fact that he essentially recommended the paper as a whole - a radical reformulation and complete rewriting and shortening of the manuscript.

This request of reviewer #3 created a rather frustrating dilemma for me, because, in my opinion, his suggestions would make the paper incomprehensible and unreadable. As a matter of fact, it is the author, who is finally responsible not only for the scientific content but also for the comprehensibility of a paper. In this context I would like to recall that no one of the other referees (not even reviewer #2) criticized my style of writing; the first reviewer wrote: "the paper seems well-written".

On the one hand reviewer #3 requested shortening of 'verbose' parts on the other hand he himself misunderstood most essential aspects of the paper, which indicates that not less but more 'verbose' explanations should be provided.

Nevertheless, to follow the suggestion of reviewer #3 as far as possible (without destroying readability) the manuscript has been shortened considerably. Some new remarks had, however, to be added in response to referee requests.

I would like to mention that the statistical approach reported in the manuscript has been generalized in the meantime to gauge fields and spin, see: http://arxiv.org/abs/1005.2734

With best regards,

U.Klein

AUTHORS COMMENTS ON REPORT OF REVIEWER #1 (FOOP1410R1)

Reviewer #1: I will maintain my recommendation for publication. However, I would like to reinforce my opinion that the gaps between the present derivation and the full structure of quantum mechanics are not to be neglected.

In particular, the step from a single particle to many particles could well prove to be unsurmountable by this approach. To use an example mentioned by the author at the conclusion of the paper, Schrodinger's and de Broglie's original interpretation of |\psi|^2 as representing the density distribution of an "extended particle" breaks down precisely when one tries to generalise that interpretation to many particles, due to the possibility of entangled states. This work may well suffer from a similar naivety, but in the absence of a proof, I would recommend publication if only so that the idea could be scrutinized by a larger audience.

Authors comment:

The present approach has been generalized in the meantime to 3 dimensions, gauge fields and spin; see: http://arxiv.org/abs/1005.2734. I agree that the extension to many particles is a crucial step. My theory has not yet been extended in this direction but I do not see any obstacles. The reason for my optimism is the enormous generality of the basic assumptions, continuity equation and ehrenfest-like relations. Of course, the calculation has still to be performed.

In support of the general approach and as evidence that it could be of interest to at least some other members of the community, however, I would mention a recent paper by Goyal (http://arxiv.org/abs/0910.2444v1). Perhaps the author would like to explore that paper due to the similarities with the present work.

Authors comment:

I read the paper by Goyal and found a remarkable similarity of the basic ideas; a comment on this interesting attempt to improve understanding of the correspondence rules has been added in section 10 to the manuscript.

AUTHORS COMMENTS ON REPORT OF REVIEWER #2 (FOOP1410R1)

Reviewer #2: I have read the author's responses to the earlier referee reports and also the revised manuscript. The author apparently didn't agree with anything I wrote in my previous report (which is maybe not too surprising since the report was rather negative) and so didn't in any substantial way revise the manuscript in response to my previous criticisms. I am completely unswayed by the author's (unfortunately dismissive) response to those criticisms, though, so my summary judgment about the paper remains just what it was before: the paper doesn't make any useful contribution to the foundations of physics literature and so doesn't rise to the level needed for publication in this journal.

Authors response:

The referee confirms his rejection of the manuscript. Let me just note that his first report did not contain any concrete points of criticism (concerning the physical theory presented in the manuscript) which could be taken into account; rather my approach was rejected as a whole for metaphysical reasons which I - I must admit- not really understand. His only concrete suggestion was to include a discussion of the 2-slit experiment and Bells theorem. But this I could not do because the first referee requested me to *shorten* the paper; I tried to meet the second referee's request as far as possible by adding some remarks and mentioning references pointing to the appropriate literature.

Let me, though, try to briefly clarify some points that the author doesn't seem to have really understood from my earlier remarks. To begin with, I am not at all dogmatically insistent on determinism. What I insist on, rather, is clarity. For example, I remarked earlier that the ontology of the theory being proposed by the author is unclear. He seems to have

understood this as a complaint that the theory isn't deterministic, but that simply isn't what I said or meant at all. The point was this: in most general terms, the author's paper is an attempt to derive the Schroedinger equation (and thus QM in general) from F=ma. Well, F=ma is a theory that is fundamentally about the motion of point particles. If you tell me that you have F=ma in your theory, but that you aren't positing an associated ontology of particles, then I simply don't know what you are talking about. More precisely, I don't know what your theory is talking about. And clearly, the situation isn't
then improved by engaging in (what I previously called) "numerological tinkering" to convert F=ma into Schroedinger's equation. I still simply don't understand what the theory is fundamentally supposed to be *about*. Please note that this is not at all an insistence on a particle ontology (the ontology of, say, the many worlds theory, or GRW theories with "mass field" or "flash" ontologies, are perfectly clear even though they don't involve particles) nor is it an insistence on determinism (again, the dynamical laws of GRW are perfectly clear even though they are fundamentally stochastic).

Authors response:

I am trying hard to understand what the referee means but there seem to be huge differences in fundamental methodic assumptions.

The correct classification of a physical theory in terms of philosophical categories seems to be a matter of primary importance for the referee. But for me it is not. "Ontology ...is the philosophical study of the nature of being... Traditionally listed as a part of the major branch of philosophy known as metaphysics, ..." I don't believe in the primary role of philosophical (metaphysical) thinking in physical theory construction. I believe logic and observation is enough and even better. Philosophers have developed their categories over centuries but mostly with a macroscopic world in mind. For me the world around us is real but not necessarily completely comprehensible. In particular, I am convinced that nature does not care about our philosophical categories. In this context it is interesting to note that the renowned philosopher (and physicist) Karl Popper is a pronounced advocate of the statistical interpretation; see e.g.: Karl R. Popper, "The open universe - an argument for indeterminism", Roman and Littlefield, 1982.

I did nowhere assume that F=ma. With regard to this point, I think I understand now a little bit better how the referee misunderstood my approach. The referee believes I assumed first the validity of F=ma and *then* "converted" it to Schrödingers equation. Something similar happens in Nelsons approach by adding stochasticity. But not in mine. Please read the relevant parts of the manuscript once again carefully. At *no time* the validity of F=ma was assumed (I am repeating here what I wrote in my reply to the first report). May be I should repeat an obvius point: my theory is a field theory, trajectories occur nowhere.

Another criticism I made before which the author did not understand was my remark that he merely generated something that "looked like" Schroedinger's equation. He objected that if it looks like Schroedinger's equation, it *is* Schroedinger's equation. But this is in general not the case at all. Here is just one example. In the polar decomposition of the wave function in Schroedinger's actual equation, the phase S is not a single-valued function. (It can be multi-valued so long as all the values are equivalent mod h.) This is admittedly not so important when one is working with a one-dimensional configuration space, but (obviously) it is crucial even in the context of relatively simple one-particle situations (in 2-D or 3-D), e.g., the Hydrogen atom. But such a mathematical feature can in no apparent way be derived in a scheme like the author's. At best, it would have to be put in as a further arbitrary axiom. Incidentally, I also find that the author's
understanding of and treatment of the literature pertaining to Nelson's stochastic mechanics is seriously deficient. For example, as I understand it, it is precisely this issue of the impossibility of deriving the appropriately multi-valued character of S from "F=ma plus stochasticity" that led Nelson himself to abandon his own program (or at least the aspect of it which pertained to trying to derive QM from something classical-ish plus noise).

Authors response:

This paragraph contains a concrete point. The reviewer claims that my theory cannot be extended to three spatial dimensions because the function S can be multivalued if a transformation from \rho and S to \psi is performed. This point is strictly speaking irrelevant for the present one-dimensional treatment but is nevertheless important because I implicitly assume in the manuscript that my theory can be generalized to three spatial dimensions.

The referee claims that the assumption of a multi-valued phase would require "a further arbitrary axiom". Such assumptions are quite common but are actually not needed in my theory. The referee overlooked the fact, that only the gradient of S occurs in the fundamental continuity equation (we are talking here about Eq. 7, generalized to three spatial dimensions). Only the gradient of S occurs in physically relevant equations and must be single-valued. The function S itself may be multi-valued (of course in a way which is compatible with a single-valued gradient). Therefore, the possibility of a multi-valued S is built in in my theory from the very beginning and no additional axiom is required to allow for it. The claim of the referee that a further axiom is required in a 3D-generalization of the present theory is not valid.

In fact, the present theory has already been generalized in the meantime to three dimensions (and spin); the multi-valuedness of the phase is, as is well-known, intimately connected with the existence of gauge fields. An e-print on that is available, see: http://arxiv.org/abs/1005.2734. One of my published papers on such matters contains a similar derivation which may also be helpful: see Ref. 20.

Summarizing this paragraph of the reviewers report, no weak point has been found in my derivation neither in the present treatment nor in the 3D-generalization. The referee's claim that the equation I derived is not Schrödingers equation but only an equation that "looks like Schrödinger's equation" is completely unfounded.

I will also repeat my previously-expressed concern that the author seems rather oblivious (if not hostile) to the several "bread and butter" issues in the foundations of quantum physics which, it seems to me, should be addressed in at least some way by anybody proposing what amounts to a fundamentally new understanding of QM. I mean, for example, the 2-slit experiment, Bell's nonlocality theorem, and the measurement problem. There are a lot of proposals out there already (such as MWI, GRW, deBroglie-Bohm, Nelson, etc.) for how to understand QM, and they each have a unique and reasonably clear position on this constellation of important issues. If the author wants to propose something new that will in some way compete with those existing proposals, he probably should strive for understanding them better and clarifying how his ideas are superior in at least, say, some one way to some one of these existing theories. Otherwise, people like me who understand the issues and appreciate the subtle relative virtues and vices among them, will just continue to wonder: why should I care about some superficial attempt to derive something that anyway only superficially resembles *part* of QM from relatively implausible and arbitrary axioms. If it provided a glimmer of some new insight into, say, the wave-particle duality revealed in the 2-slit experiment, or some promising new way of trying to address the measurement problem, or something like that, the superficiality could maybe be forgiven. But without this, not.

So, the author can take those comments for whatever (if anything) they are worth to him. For the editor, I will just repeat that I don't think this paper belongs in Foundations of Physics (especially considering its length). Since evidently the other referee thinks more highly of it than I do, perhaps a third referee should be asked to look at it.

Authors response:

In the last paragraph of his report the referee repeats several points mentioned already in his first report ignoring almost completely my response to various points in his first report. As a matter of fact, I addressed several points (relation between the present theory and Nelsons theory, various remarks concerning the statistical interpretation, remark that the measurement problem is an ill-posed problem, added references,..) raised in his report. He ignores all of these except the one about determinism. Of course, I could do this only in a quite cursory manner as a result of length limitations. The referee seems to believe that my interpretation of QM is something completely new. He ignores my remarks that my interpretation is already described in the literature and that I am, of course, completely unable to discuss such complicated and length-consuming topics as the 2-slit experiment or Bells theorem. This could only be done in an review article and it would partly be a repetition of what already appeared in the literature. The reviewer mentions now also the 'measurement problem': According to the statistical interpretation the "measurement problem" is an ill-defined problem (unsolvable by definition). Complete information on the statistical interpretation (including 2-slit and Bell) may be found in the published literature [see e.g. the general review Ref. 2, and a discussion of the pseudo-problems created by erroneous application of probability theory to the 2-slit experiment in L.E. Ballentine, Am. J. Phys. vol.54, p.883 (1986)], for everybody, who is willing to accept the possibility that off-mainstream interpretations of QM may be useful.

AUTHORS COMMENTS ON REPORT OF REVIEWER #3 (FOOP1410R1)

Reviewer #3: In recent years, the challenge of finding simple, physically reasonable principles from which quantum theory can be derived has received considerable interest. In this paper, the author attempts to reconstruct a particular, important part of quantum theory, namely the one-particle Schroedinger equation from physical principles.

I believe that the paper is potentially an important contribution to the literature on this subject, and the author clearly makes a serious effort to articulate the rationale for his proposed principles. However, there are several important issues --- some to do with the presentational style and some to do with the derivation itself --- which ought to be addressed before publication is considered in Foundations of Physics.

General Comments

The presentation mixes together (a) long verbal discussions and justifications, (b) the postulates, mathematical assumptions and the derivation itself.

This is most unfortunate. Readers would be very well served (and grateful) if the paper were reorganised so that these two components were sharply separated.

I would like to see the paper reorganised into two parts:

Part I: a clear and brief statement of the postulates, a crisp derivation of the Schroedinger equation, clearly pointing out (and labelling) all auxiliary assumptions. This should be brief (at most 6 pages), with cross-references to part II. This section should conclude with an itemized list of all postulates and auxiliary assumptions. A model for this part would be Reginatto's PRA paper (which the author cites).

Part II: the remainder of the paper, where verbal discussions and supporting material is given.???

Authors comment:

Here, in the first part (General Comments) of his report, the reviewer proposes a radical reorganization and separation of the whole manuscript in two parts, the first one containing - as I understand it - the postulates and derivations and the second part containing discussions as well as other unspecified things (denoted by ???).

His suggestion leaves open the question whether or not the present arrangement of sections should be kept or how it could be improved. More questions appear if one reads the second part (Detailed Comments) of the report. There one finds, several suggestions concerning the organization of the manuscript without any reference to the above separation in two parts.

Besides the unclear nature of the suggested separation, I think that such a separation (of explanations and postulates) would make the paper incomprehensible and unreadable (Note that the paper suggested by the referee as a "template" has a much simpler structure and is much shorter). The mixing of verbal discussions and postulates seems to be unavoidable in order to make the physical meaning of the postulates accessible *before* these are formulated in mathematical terms. I believe that just the readers of "Foundations of Physics" will be interested in "verbal", detailed and coherent explanations of the mathematical formulas they find in this journal.

Detailed Comments

Abstract

1. "kind of statistical metamorphosis" is unhelpfully vague; "a set of two relations" is linguistically complex given that this is the abstract.

Authors comment:
"kind of statistical metamorphosis" : I spent a lot of time - both at the time of writing and now again trying to meet the referee's criticism - searching for an expression which characterizes the unusual relation of classical mechanics to the theory proposed in sections 1-3 in an adequate manner. I found no better expression than "metamorphosis". For example "version" sounds more definite ("less vague") but is actually less precise. Note that the referee did not suggest an alternative.

"a set of two relations": I modified this sentence to make it linguistically less complex.
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2. The claim that Schroedinger's equation is derived from three postulates is inaccurate: there are at least 4 postulates, and a number of auxiliary assumptions (some of which the author points out in the paper, and some which he does not point out). While I appreciate the author wishing to present his work in the strongest way, it is important that he find a more balanced and accurate way of describing the achievement of the paper.

Authors comment:

All postulates and auxiliary assumptions used in this manuscript have been pointed out explicitly. I counted the existence of a continuity equation of a *particular* form as a single assumption (this seemed natural to me because the existence of a continuity equation itself is more or less evident). But this has been mentioned explicitly. Two points below which the reviewer qualifies as additional assumptions (his point 2 of section 2, and his point 2 of section 4) are actually errors of the reviewer (see below). Nevertheless, I used in the manuscript several times more modest formulations "essential assumptions"..etc, to meet his criticism

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Sec 1. Introduction
?1. p2, lines 10-25. Here, several attempts to derive Schroedinger's equation are cited, but no indication is given about in what way these other works are lacking (do they fail to solve the problem of deriving Schroedinger's equation?) or in what way the present work is different or an improvement. It is important that this be made clear to the reader, so that they are properly motivated to read onwards.

Authors comment:

Following this useful suggestion, I added a remark, pointing out which features of my work can be considered as an improvement as compared to previous related works.

Sec 2. On probability

1. Item 3 in the list: "..but with unknown laws" is confusing (although I understand what is meant)

Authors comment:

I added a remark, trying to make this point clearer.

Sec 3. Statistical Conditions??. In equation (3), p-bar is defined. While this would make sense in a particle-based picture, it is not clear what this would mean in an ensemble picture. To see the problem, note that in quantum theory, the momentum operator is not operationally implemented by measuring position at two successive times (as it would be classically). So, what "p" means operationally in classical and quantum theory differs greatly. See arXiv:0910.2444 for more details.

Authors comment:
p-bar is the statistical expectation value of a random variable as explained in detail in this section. Therefore one has to assume that a measuring device for a quantity named "momentum" exists (as well as for position and time). Ensemble properties - like expectation values or probabilities - can only be verified by performing a large number of identical experiments on particles. Thus I cannot see here any contradiction between the particle picture and the ensemble picture. The momentum operator in quantum mechanics is of course not implemented operationally by measuring position at two successive times; there is no way at all to define the momentum operator in quantum mechanics operationally because it is not an observable property, only its eigenvalues. The reviewer points out that the definition of p-bar is not clear but does not explain what exactly he means. Does he mean that measurement of a quantity p of a microscopic particle is fundamentally different from that of a macroscopic body ?. There are strong indications (see also several points below) that the reviewer misunderstood the character of the theory outlined in section 3. The operational meaning of my quantities is not in contradiction to the paper arXiv:0910.2444 (which details are to be found in this paper ?).

Something with p-bar was unclear to the reviewer but he was not really specific about that; maybe I was unable to understand him. To avoid possible misunderstandings I added some more verbose explanations of the theory in section 3.
----

2. p4, lines 25-32: it is assumed that there are two separate probability distributions, one over x, and one over p. i.e. it is assumed that the probability distribution over phase space factorizes. This is a highly non-trivial assumption which needs to be flagged at the very least, and certainly discussed. See e.g. [G. Kaniadakis, "Statistical origin of quantum mechanics", Physica A 307, 172 (2002)] for an example of an approach that does not make this assumption at the outset.

Authors comment:
The referee made an error here. If one calculates an expectation value of a quantity A depending only on (say) p, one needs only a probability w(p) - it may be the residual density of a density w(x,p) but this has nothing to do with factorization. Factorization may be an option if the expectation value of a quantity B(x,p) is to be calculated. Then I need a probability density w(x,p) [factorization means w(x,p)= a(x)b(p)]. There is nowhere in this manuscript a probability depending on both x and p. Thus, this criticism of the reviewer is not justified and no additional assumption needs to be flagged.

The theory of Kanadiakis is based on particle trajectories in phase space and is completely different from mine. Probabilities in this theory depend in fact on points (x,p) in phase space. As pointed out in great detail in several

verbose parts of the manuscript the present theory is basically a *configuration space* theory. The highly non-trivial characterization of the present class of statistical theories - containing quantum mechanics as a special case - is explained in several verbose parts of sections 2,3,4,7,8 of the manuscript
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Sec 4. On random variables.

1. The discussion in this section is verbose. Many readers would stop reading at this point. I suggest the author condense the discussion to the bare essentials (one or two paragraphs at most) that are necessary to the mathematical derivation, and put any necessary supporting material into an Appendix.

Authors comment:
A shortening of this verbose discussion will increase the danger of misunderstandings (see above) of these non-trivial matters. A separation of explanations and mathematical derivations seems absurd; it would make this section unreadable. The most reasonable and convenient place for verbal explanation of formulas is of course immediately before these formulas.
----

2. The variable transformation Eq.(14) is not reversible, and is therefore not trivial. An essential feature of the Schroedinger equation has been inadvertently brought in at this point. See Timothy C. Wallstrom's well-known article on single-valuedness of the wavefunction (Phys. Rev. 49, 16131617 (1994)) for discussion on this point. See also Smolin's quant-ph/0609109.

Authors comment:
No new feature has been brought in inadvertently by the variable transformation Eq.(14). The referee has failed to notice that only the gradient of S occurs in the fundamental continuity equation (we are talking here about Eq. 7, generalized to three spatial dimensions). Only the gradient of S must be single-valued. The function S itself may be multi-valued. Therefore, the possibility of a multi-valued S is built in in my theory from the very beginning and no no new feature or assumption has been brought in. (As it happens, exactly the same error has been made by the second referee). Thus, this criticism of the reviewer is not justified and no action required on this point.

Wallstrom's paper, quoted by the referee is entitled "The inequivalence between the SchrÃ¶dinge equation and the Madelung hydrodynamical equations". The Madelung hydrodynamical equations are - as is well-known - obtained from SchrÃ¶dinger's equation by separating real and imaginary parts and then *performing an additional derivation*. My theory has nothing to do with Madelungs equations and related theories (I believe Wallstrom is right, but this is a point that has nothing to do with the present theory). Smolin's paper which is, surprisingly, also quoted by the referee, criticizes Wallstrom's conclusion and is also completely unrelated to the present work.

It may be of interest to note that my theory has been generalized in the meantime to three spatial dimensions (see: http://arxiv.org/abs/1005.2734.); the multi-valuedness of the wavefunction is closely related to the appearance of gauge fields.

Sec. 5.

1. The verbal discussion (following line 44) should be substantially shortened.

Authors comment:
A moderate shortening of the discussion has been performed. A substantial shortening of the verbal discussion would increase the danger of misunderstandings of these non-trivial matters.

Sec. 7. Entropy as a measure of disorder?

1. This discussion should be substantially shortened and moved to an appendix. It is tangential to the main purpose of the paper.

Authors comment:

A moderate shortening of the discussion has been performed. A substantial shortening of the verbal discussion would increase the danger of misunderstandings of these non-trivial matters. The precise characterization of the role of uncertainty in quantum mechanics as compared to classical physics is not tangential but central to the paper. A separation of verbal discussions and mathematical derivations has not been performed because it would reduce the readability of the manuscript.

Sec 8. Fisher information

1. p16 from line 42 onwards, and p17: This discussion should be shortened and moved into an appendix (merged with the discussion from Sec. 7)

Authors comment:

A moderate shortening of the discussion has been performed. This section
has been merged with the next one. The discussion of the role of uncertainty in quantum mechanics as compared to classical physics is of central importance and non-trivial. A separation of verbal analysis and mathematical derivations has not been performed because it would reduce the readability of the manuscript .

Sec. 11 Discussion.

1. p23, line 57 "it is completely unclear why they work" --- this is not accurate; see for example Bohm's textbook Quantum Theory and arXiv: 0910.2444 (and references therein)

Authors comment:

I used a less extreme formulation and added a comment on useful attempts (arXiv: 0910.2444) to understand the correspondence rules.

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2. p24, lines 7-9 "..just a consequence..." is unclear to me.

Authors comment:

The commutator algebra of operators in quantum mechanics (not the concrete realization but the abstract algebra) coincides with the Poisson bracket algebra of observables in classical mechanics; both are determined by the structure of the space-time symmetry group. I added a short remark to make this point clearer.

----

3. This section ought to be reorganised as follows:

(a) Give an itemised list of all postulates (qualitative statement, followed by quantitative statement).
(b) Give an itemised list of all subsidiary assumptions (of which there are several).
(c) Give brief (maximum 2 lines in each case) itemised discussion of any of these as the author wishes.
(d) Give 2-3 paragraphs of general discussion as the author wishes.

Authors comment:

Such an abstract and bureaucratic separation in 'qualitative' and 'quantitative' statements, 'itemized' and 'general'

discussion, etc is absurd and would lead to complete frustration of readers. Instead, points of physical interest have to be discussed in order - with each point including all relevant aspects.

Gmail ulf klein <klein.ulf@gmail.com>
Your Submission FOOP1410R2
1 Nachricht
Foundations of Physics <renlethjoyviterbo.pural@springer.com> 19. Juli 2010 15:55
An: Ulf Klein <klein.ulf@gmail.com>
Dear Professor Ulf Klein,

We have received the reports from our advisors on your manuscript FOOP1410R2 "The statistical origins of quantum mechanics".

With regret, I must inform you that, based on the advice received, the Editors have decided that your manuscript cannot be accepted for publication in Foundations of Physics.

Below, please find the comments for your perusal.

I would like to thank you very much for forwarding your manuscript to us for consideration and wish you every success in finding an alternative place of publication.

With kind regards,

Gerard 't Hooft
Chief Editor

Comments for the Author:

Reviewer #1: After reviewing the comments from the other referees, the author's replies, and his new manuscript, I put forward the following comments:

1. I disagree with the authors' rebuttal of another referee's remark on being clear about the ontological meaning of the present manuscript. While disregard for philosophical debate is common in most physics journals, readers of Foundations of Physics are generally versed in basic philosophical matters and interested in philosophical as well as physical clarity. The author dismisses the referee's relevant comment with vague words such as "I am convinced that nature does not care about our philosophical categories". By that same token nature doesn't care about our physical theories either, which doesn't stop us from trying to understand nature by employing philosophy as much as physics, especially in a field which is at the boundary of the two disciplines such as the foundations of physics.

The main goal of the present manuscript is to provide an argument for the statistical interpretation of quantum theory by rederiving Schrodinger's equation from statistically inspired assumptions. The value of the manuscript is thus based on how well it reaches its goal to provide support for an interpretation of the theory. The fact that an equation can have multiple derivations is hardly a noteworthy fact and wouldn't merit publication unless said derivation carries with it an important message or a different perspective on the theory. Perhaps without being fully aware, the author is thereby engaged in philosophical debate. It would thus be desirable that he be aware and acknowledge this fact and strive to make a philosophically sound case for his work. In fact, throughout the paper he does indeed cite a number of philosophers of physics when it is in the interest of promoting his view. It would be an unprofessional double standard to thus dismiss referee #1's question by
appealing to being uninterested in philosophical debate.

I suspect that after careful thought the author would presumably argue that his work is not concerned with ontology;

that he wants to motivate the postulates of quantum theory by statistical reasoning, and necessarily requires a fundamental reference to "measurement", as is evident in part of his reply to referee #3: "one has to assume that a measuring device for a quantity named "momentum" exists". In this way I understand this work to be analogous to Chris Fuchs' program to rederive quantum theory from information-theoretic principles, while maintaining a fundamental unanalysed role for measurement.

2. Referee #3 seems to have reinforced my opinion that the present work is potentially interesting to the readership of Foundations of Physics as it fits within a broad research program of reconstructing quantum theory from foundational principles. However, he or she has also brought to my attention that in its present form the paper is perhaps not as well articulated and clear as it would be expected or desired for this journal. I agree with most of the suggestions from referee #3 towards making the manuscript clearer and more readable. The radical reorganisation suggested would most likely make the paper more attractive and readable.

However, after struggling with these considerations, I still maintain my recommendation for publication. While the present manuscript is perhaps less interesting and clear as it could potentially be, and while I seem to have disagreements with the author, it seems that the manuscript meets the minimal requirements for publication.

Reviewer #2: I have read the author's revised submission and also reviewed the earlier referee reports and the author's responses to them. My assessment remains essentially unchanged.

To begin with, I don't find there to be any particular unclarity with simply taking the existence of a complex-valued field, obeying a certain evolution equation (namely Schroedinger's), as the axiom of a physical theory. So I am, from the beginning, suspicious of attempts to *derive* the wave function and its evolution equation from allegedly more fundamental axioms. It's not that doing so is in principle pointless -- just that the proposer must discharge the burden of showing that the proposed alternative axioms are clearer, or simpler, or easier to accept, than the default ones.

So (to summarize everything I have said in previous reports), my problem with this paper is that it does not discharge this burden. In his response to my earlier comments, for example, the author takes offense at the suggestion that "F=ma" is somehow presupposed in his derivation, and underscores what he takes to be an "obvious" point -- that his is a field theory. But none of this is at all obvious or clear. In fact, the entire paper involves a kind of sustained equivocation between the idea that the theory is fundamentally about particles following definite trajectories (with the element of stochasticity being based simply on perhaps-inevitable ignorance about initial conditions and/or the laws governing the trajectories) and the very different idea that the theory is fundamentally about fields (which merely have some ultimately-coincidental analogies to probability distributions which might arise in a particle theory).

This is what I meant when I said previously that the *ontology* of the proposed theory was unclear. The author seems to think that this is a foreign and unscientific "metaphysical" sort of complaint. But I hardly think it is unscientific or metaphysical to insist that the proposer of an allegedly fundamental theory be able to say clearly what the theory is fundamentally supposed to be about (e.g., particles or fields).

This is also, by the way, the reason I suggested that the connection between the ideas proposed in the paper, and such things as Nelson's stochastic mechanics, the de Broglie - Bohm theory, and the measurement problem, be fleshed out more precisely. If, for example, the author means to commit unambiguously to the field ontology for his theory -- in which (e.g.) rho(x,t) is regarded as the fundamental "observable" -- then the theory, as proposed, becomes *immediately* contradictory with experimental data. He suggests that the field ontology is empirically viable on the grounds that the field can be identified with the relative frequency of particle positions observed on ensembles of identically-prepared particles. But it is *not* the ensemble statistics which are fundamentally observed in experiments; it is rather the outcomes of *specific* experiments, which (of course) always find a given particle at some one particular location. (In the paper, this is precisely the
point at which the author waffles, and vaguely hints that, for this reason, his field rho(x,t) should be thought of as merely an epistemic probability distribution for the position of a genuine particle.) Of course, maybe, as in orthodox QM, one could resolve the inconsistency by positing some additional dynamics for the field -- something like the "collapse postulate" which comes into play when a "measurement" occurs. But then one is clearly ensnared by the measurement problem (which the author evidently regards as unsolvable, by definition). Moreover, the question I began with -- namely, why I should prefer the proposed theory instead of one which simply begins by postulating the usual wave function obeying a certain set of dynamical laws -- now comes back into sharp focus.

So I just don't think that this paper makes any kind of useful contribution to the literature. It is far too confused and muddled and inconsistent about basic foundational issues (like determinism and ontology), and certainly (relative to whatever elements of useful novelty it may contain) far, far too long.