SDI Review Form 1.6

Journal Name:	**Physical Science International Journal**
Manuscript Number:	**2014_PSIJ_11144**
Title of the Manuscript:	**Computational Solution to Quantum Foundational Problems**
Type of the Article	**Original Research Article**

General guideline for Peer Review process:

This journal's peer review policy states that **NO** manuscript should be rejected only on the basis of '**lack of Novelty'**, provided the manuscript is scientifically robust and technically sound.
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PART 1: Review Comments

	Reviewer's comment	**Author's comment** *(if agreed with reviewer, correct the manuscript and highlight that part in the manuscript. It is mandatory that authors should write his/her feedback here)*
Compulsory REVISION comments	-	
Minor REVISION comments	-	
Optional/General comments	**(1) I found this paper very nicely presented, styled, and written.**	
	(2) Since I am not an expert on computational complexity theory, I found it difficult to judge step-by-step correctness of the paper's content.	
	(3) I feel that the paper implies that equations of laws of physics must be solvable within a certain computational complexity level. I think this is like saying that a house-owner must hide house keys only in such a way that an intruder should be in a position to locate the hidden house keys. I do not find any reason for laws of physics to abide by a certain computational complexity requirement.	In my paper, I never implied the existence of any computational complexity requirement compelling natural laws to obey. I only suggested that some physical theories (i.e., mathematical models representing the real world) – particularly quantum theory and its fundamental Schrödinger's equation – might be computationally hard, i.e. infeasible. Undeniably, the defining characteristic of any mathematical physics model is that it makes falsifiable or testable predictions. So, if a mathematical model were never soluble in a reasonable amount of time (even with access to a supercomputer), then such a model would not have a realistically testable predictive content, and therefore the term "realistic" would be hardly applicable to it. This implies that if the Exponential Time Hypothesis (the computational hardness assumption, which if true would imply $P \neq NP$) held, then we would not be able to describe a microscopic system and a macroscopic measurement device (interacting with each other) within the frame of the same exact realistic mathematical model.

(4) I found this paper posted at http://arxiv.org/abs/1403.7686 .	Yes, it is correct: I uploaded a draft of the paper to arXiv – a repository of electronic preprints of scientific papers. However since the uploaded draft has not yet been published in any peer-reviewed scientific journal, by submitting it to Physical Science International Journal I have not violated the Journal submission rules, which clearly state that "submission of a manuscript clearly indicates that the study has not been published before or is not under consideration for publication elsewhere".
5) I found paper's comment by Scott Aaronson's at http://www.scottaaronson.com/blog/?p=1767#comme : **I can confirm that it's complete garbage.** **i) The author is simply mistaken that solving the Schrödinger equation is "NP-complete" in any interesting sense: his argument for that seems to rely on a rediscovery of the adiabatic algorithm, but he doesn't mention that the spectral gap could be exponentially small (and hence the annealing time could be exponentially large)—the central problem that's been the bane of Farhi and his collaborators (and, of course, of D-Wave) for the past 15 years…** **ii) Also, even if you thought (for totally mistaken reasons) that quantum mechanics let you solve NP-complete problems in polynomial time, that might (or might not) suggest to you that quantum mechanics should be replaced by something else. But until you'd actually found a replacement, and given some sort of evidence for its truth, I don't see how you could claim to have thereby "solved the measurement problem"!! …**	*Here my reply to Aaronson's critique goes (posted on ResearchGate.net, post "P!=NP and quantum nature of universe – any thoughts?"):* i) In the paper, the adiabatic algorithm was neither rediscovered nor used but merely mentioned as an example to support the following argument: The principal possibility of encoding specific instances of some decision problems (namely, NP-complete problems) in certain Hamiltonians implies that if existed, an algorithm capable of solving Schrödinger's equation for all Hamiltonians would be able to solve the given NP-complete problems as well. Therefore, the key paper's claim about NP-hardness of Schrödinger's equation is derived from the possibility of encoding NP-complete problems into certain Hamiltonians (the feature that has been pointed out in many great reviews; see for example [arXiv:1106.5875] and [arXiv:1306.1259]) and correspondingly has nothing to do with the particular quantum adiabatic evolution algorithm constructed by Prof. Farhi and his collaborators [arXiv:0104129]. Only if the above-mentioned generic algorithm were efficient, then Schrödinger's equation could be solved efficiently for any physical system (including a macroscopic object, an observer, and entire universe). But as the difficulties with Farhi's method indicate (mounting evidence that Farhi's algorithm takes exponential time in the worst-case for NP-complete problems) and theoretical conclusions made by Aharonov and co [arXiv:0405098] as well

	iii) As additional problems, the author appears to conflate the P vs. NP problem with the question of whether NP-complete problems can be efficiently solved in the physical world, a common novice mistake. And also, he seems comically unaware of everything that's been discovered in quantum computing theory over the past 20 years relevant to the issues he's writing about—as if he just emerged from a cave.'	as Bernstein and Vazirani [*SIAM J. Comput.*, 26(5):1411–1473] suggest, this efficient algorithm in all likelihood does not exist, which backs up another claim of my paper [arXiv:1403.7686]. ii) In the paper, there is nothing – not a single sentence – that suggests (or insinuates in some way) replacing quantum mechanics by something else. Quite the contrary, the crux of the paper is to show that quantum mechanics is a well-defined theory with clear interpretation and as such, it has no need of the "measurement problem" resolution. Namely, the impossibility of deriving a non-symbolical description of an arbitrary macroscopic system from the Schrödinger equation might be the reason the world (or just its portrayal) split into a macroscopic domain following classical mechanics and a microscopic domain following quantum mechanics. Such a 'solution' to the measurement problem has been proposed by Borh: His position is that classical concepts are autonomous from quantum theory and cannot be derived from it. As stated by Bohr's correspondence principle (in its strong form), it is inappropriate to treat macroscopic objects (e.g., measuring instruments) in purely quantum mechanical notions. iii) The paper was dedicated to evaluation of the assumption of the generic efficient solvability of the Schrödinger equation. Hence, only sources, which were considered necessary to prove the point, were referenced in the article. Beside that main goal, another question concerning the origin of probabilities in quantum theory was discussed. Accordingly, no new classes (such as BQP and QMA) extending the classical complexity classes P and NP were considered in the paper since allowing for those classes (of decision problems solvable probabilistically) would be *circular* for the problem of the probability origin. After all, the paper [arXiv:1403.7686] was about the quantum

		reduction postulate and the emergence of Born's rule but not about quantum computing or simulating Hamiltonian evolution (neither it was about quantum computational complexity theory per se) as Aaronson's critique implies. It seems like S. Aaronson read some other article not mine. Otherwise, it is very difficult to explain why his harsh and rather derogative comment is barely related to the contents of the paper [arXiv:1403.7686].

	(6) If I were to accept Scott Aaronson as an expert on the subject matter of this paper, I feel that publication of this paper in this journal may harm this journal's credibility.	Provided that you used an open *hypothetical* conditional clause here, I presume that you do not *really* feel this way about the paper. Nevertheless, I believe that the clarification I presented above on your comment #5 clearly demonstrates that Aaronson's criticism is based on a superficial reading of my paper. Besides, I have fully revised all the paper's sections relating to computational complexity removing completely any mention of Farhi's adiabatic algorithm, please observe.
	(7) I think that the subject matter of this paper is too specialized and outside the scope of this journal.	As the Journal home page reads, "Physical Science International Journal is an open access journal that publishes original research articles … in all areas of Physics, Chemistry and Earth Sciences. Subject matters include study in all areas of Physics, Chemistry, Engineering, Material Science, Astronomy, Natural Science, Earth Sciences and other related fields: Fundamental physics, applied physics, atomic, molecular and optical physics, nuclear and particle physics, astrophysics and physical cosmology, artificial intelligence, neural processing, physics in medicine and biology, plasma physics, biophysics, econophysics, geophysics, neurophysics, psychophysics, wireless and optical communications, quantum mechanics". In view of that almost all-encompassing scope of the Journal, I cannot see how the subject matter of my paper might be too specialized or outside the scope of Physical Science International Journal. I hope this satisfies yours comments. Thank you for your time and consideration.