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The Mechanics of Physics in Finance and Economics: Pitfalls from Education and Other Issues

Emmanuel Haven†

Memorial University, St. John's, NewFoundland, Canada and IQSCS, UK

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Abstract

This contribution discusses attempts to answer the question how finance/economics and physics may join together as disciplines to uncover new advances in knowledge. We discuss pitfalls and opportunities from such collaboration.

1 INTRODUCTION

At this year's 'International Econometric Conference of Vietnam (ECONVN2019)' in Ho Chi Minh City, we encountered many presentations which revolved around the use of models. The prowess of each of those models was put to the 'test' so to speak, mainly in problems which revolved around forecasting some event, whether it be a price or a statistical quantity. In essence, we

†Corresponding author: Memorial University, St. John's, NewFoundland, Canada and IQSCS, UK. Email address: ehaven@mun.ca

may actually wonder why, in economics, and even more so in finance, we would be interested in anything else than forecasting. This sort of argument goes back to the idea that applied finance and economics are for a large part interested in that class of problems which lends itself to an exercise in forecasting. Granted, there are areas of finance and economics, especially such as mathematical economics or mathematical finance, which have much less interest in this end goal. They rather focus on the justification, mostly mathematical, of the modelling used. This is an extremely important part of any scientific endeavour. Unfortunately, most models which are used in applied branches of finance and economics, often have only a remote connection with the mathematized branches of the same disciplines. In other words, if there were to be a much more tighter connection between those mathematized and applied branches, we would probably be in a much better capacity to appreciate the pitfalls of applying models in finance and economics.

In this paper, we want to discuss some issues which may explain this disconnect (section 2) and we also consider further on in the paper, areas where collaboration between disciplines may lead to fruitful outcomes (section 3, 4 and 5). We conclude in section 6.

2 EDUCATION AS THE KEY ARGUMENT FOR THE 'DISCONNECT'?

As the paper by Hung Nguyen [29] shows, there is a very clear distinction to be made between explaining and predicting. We would want to claim that intuitively speaking, predicting a phenomenon may not lead to explaining the phenomenon. In fact, the worse of all worlds occurs, when we predict *and* we want to explain our prediction, but we 'forget' the assumptions our model is standing or falling on. The questioning of the applicability of models to specific problem situations is an obvious necessity. However, for a variety of reasons it is a difficult thing to do in many circumstances. The paper by Professor Nguyen brings forward some arguments and he also refers to Richard Feynman [17]. In the 1974 commencement address at Caltech, Professor Feynman had this to say: "In summary the idea is to try to give *all* of the information to help others judge the value of your contribution, not just the information that leads to judgment in one particular direction or another." This is a pure calling, and very very difficult to do for problems which are - hopelessly - multidimensional. Let me make this clear though: what Professor Feynman says is absolutely correct. His proposal is the most noble way of pursuing the truth. Nobody can doubt this. But I would want to humbly propose that it can be very difficult to pursue this quest, even though all of us should pursue it to some degree. Let me explain what I mean with 'to some degree'. Many problems in economics and finance do have precisely a type of character which is as follows: i) they have very often no control group at all (not because there is no wish to have one, but rather, because it is just not possible to have one); and ii) they attempt to capture a problem which can be influenced by many, which even by experiment are, non-distinguishable directions. Hence, it becomes extremely difficult to disentangle influencing sources for a given result. This is surely not always the case but it often is.

Let me give an example which can show that we may not even have to confine this issue we raise, to economics or finance. In economics, one of the driving 'mathematized' models in decision making is the so called maximum utility model. You maximize a utility function which attempts to formalize the relationship between goods consumed and the utility or satisfaction such consumption brings.

This maximization occurs under the constraint of a so called budget constraint. The demand function is in fact derived from that premise: i.e. we demand more goods at lower prices, as opposed to goods which are priced higher (with exceptions). Apart from the immediate issues with such a model (for instance: what is the meaning of 1 'util' of consumption etc..), there is maybe a more deep-seated query, which could go like this: "why would we want to maximize the utility received from consuming?" A biologist may answer this question, by saying that even in the fundamental building blocks of nature do we see minimizing/maximizing behavior in such primitive objects like cells. Do we know why? Maybe not. Thus, if we want to aggregate up from the microworld to the macroworld, we are faced with a host of enormously complex interacting processes. In the two examples we just mentioned, there are maybe foundational issues, i.e. 'why maximize' which if left unanswered, may leave us in limbo as to how to explain our theory. This in turn, may make it difficult to follow Feynman's pure calling. However, there are surely counterexamples to this argument. Quantum physics is phenomenally successful in predicting and very precise arguments can be made why 'this or that' result may not hold 100%. So the Feynman pure calling is entirely applicable. At the same time, quantum physics faces deep foundational issues.

Sometimes, we can subsume, as in physics, the complexity of a problem in an intuitively palatable prescriptive model. As an example, here again from economics, we can use the idea of this utility function we mentioned above. The so called degree of risk aversiveness of a decision making agent, could be encapsulated by the degree of concavity of his/her utility function. If 'agent 1' is more risk averse than 'agent 2', then agent's 1 utility function is 'more concave' than agent's 2 utility function. Such degree of concavity can easily be rendered in a very simple mathematical way. Assume agent 1, has utility function $u(w)$ and agent 2's utility function is $v(w)$, where w denotes the agent's wealth. The agent with utility function $u(w)$ is more risk averse than the agent with utility function $v(w)$ if: $u(w) = g(v(w))$ where $g(.)$ is an increasing/strictly concave function. In such a statement, one can find that there is very little to uncover in terms of assumptions.

Slightly more assumptions come in the following example. Assume we were to consider the maximum amount of wealth an agent would be willing to give up so as to avoid a risk: ε, which is a random variable with mean zero. Then using again the above utility function $u(.)$, and denoting the maximum amount of wealth to be given up as $\$amount(\varepsilon)$, one can write that, with the use of the utility function $u(.)$: $u(w - \$amount(\varepsilon)) = E(u(w + \varepsilon))$. In words, this means that the utility for reduced wealth (i.e. $w - \$amount(\varepsilon)$), is equal to the expected utility of getting into a gamble. This expectation is calculated with the aid of a probability.

In physics, we sometimes think of mean-field approaches to simplify the world. In economics or finance, we may find recourse in using expected values. But surely, in theories where humans are involved, especially via a subsumed decision making process, the pure calling of explaining 'everything' which may not help the purported conclusion, is a daunting task.

But what else may be at the 'root' now, of this so called dis-connect we mentioned at the beginning of this paper? In other words, what other arguments can we use to support the thesis that if mathematical and applied sides of a discipline do not communicate well, we may be in trouble with recognizing pitfalls of the models we use? This in turn then leads us to perform poorly on Feynman's pure calling. We believe another root cause may revolve around education. Let us explain.

Any graduate programme in applied finance/economics, will often have a course in so called 'applied modelling', a course which in essence, utilizes methods from statistics to relevant problems in economics or finance. Most of those courses are about one semester long, and are crammed with methodologies, which often are mechanically applied *without* much regard for the assumptions which support the models. Surely, the advent of the computer and the use of statistical software, when used in this mechanical fashion, only amplifies the problem. One can of course not generalize, but it is really not a difficult argument to make, that in the absence of a true regard for how assumptions can invalidate a model, one should not be surprised that there is a failure to reproduce results. Granted, the very phenomenon which one attempts to model is having such a complex source of events which drive it, that reproducibility may not even have to be contemplated. But, in those cases where reproducibility may be feasible, the culprit may lie in the erroneous use of the statistical method, or also, the use of a different (but comparable) data set.

Those problems have begun to be discussed with increasing frequency. We refer the interested reader to four key references which may -more than- whet the appetite. See Leek and Peng [27]; Wasserstein and Lazar [38]; Trafimow [36] and Briggs [10].

To pursue the argument somewhat further. I would like to invoke another reason, which again purports to education. We started the introduction to this paper with an argument where we mentioned that there is a disconnect between the applied and the theoretical communities in economics and finance. The intrinsic knowledge of mathematical finance, is virtually unshared by the applied finance community. To some large extent, this may also be the case in the economics community. This disconnect is due - to some degree- by the fact that graduate education can not lie emphasize on both domains. It is very hard, for pragmatic purposes to impose on graduate students that they need to be equally well versed in the mathematical and applied aspects of finance for instance. Apart from the additional time this would require for students to complete a graduate degree, it also would very much intensify the needed versatility of students, i.e. they would have to be able to pursue a rather more mathematically oriented degree. Such additional requirement would also impose differential types of mathematical knowledge depending on whether we are in the game of espousing theory and application in finance as opposed to economics. In finance, especially via the impetus given through the success derivative pricing has brought about, the mathematical emphasize would be on a good knowledge of stochastics and on the solving of partial differential equations (PDE). Especially, the solving of PDE's was at one point in the 1990's of paramount importance when derivative pricing was attempting to relax volatility parameters. However, in economics, the emphasize on PDE solving would be greeted with scepticism. Rather, a good knowledge of real analysis would be very welcome.

Thus, without a good grasp of both faces of knowledge in such complex disciplines like economics and finance, it is extremely difficult to assess results in the way that Richard Feynman was prescribing them. Let me give a maybe too simple example. Any graduate student in finance knows that in academic finance, we want to de-emphasize the use of the past as a beacon for the future. In fact the theory of martingales, which underpins a lot of mathematical finance, holds exactly the opposite assumption, i.e. the expectation of a future asset value , S_{t+1}, given the information we have now, $\mathcal{F}_t$, is such that the conditional expectation of that quantity, $S_{t+1} : E(S_{t+1}|\mathcal{F}_t) = S_t$. Whilst past information is very valuable in so called technical analysis and in a lot of very pragmatic tips about how to invest wisely, academic finance seems to go the opposite way. Where does the truth lie? Can we better uncover that truth if we were to be knowledge-able of both the applied and theoretical faces of finance? Very probably so.

I want to push the argument even further. Apart from espousing theory with practice, via the knowledge dual of mathematics/applied statistics, we can pose the following question: what about the connections economics and finance might want to have with other disciplines? The answer to this question may come in different guises. In sociology for instance, one has studied the financial markets from a sociological perspective and the resulting conclusions are very interesting (see MacKenzie and Millo [28]). What about other disciplinary connections? The connection with physics that economics and also finance has, was (and continues to be) studied. But to come to the argument that a dual degree in physics and economics (or finance) may lead to breakthroughs which could answer the pure argument that Richard Feynman preconized for a theory to be scientific, is a little farfetched. Or maybe not? After all, physicists shall not be afraid to claim that, probably one of the most celebrated theories of finance, so called Black-Scholes option pricing theory ([5]), is in essence a heat equation resulting from the financial manipulation on an asset which is assumed to follow a geometric Brownian motion process. Hence, two types of PDE's appear here: respectively, a regular PDE and a stochastic PDE. But aside from this very well crafted theory, have we come across other theories in economics which really can show an intimate connection with some area of physics? The answer to that question is much more difficult. Hence, the argument that education should provide for a 'triumvirate' education of physics; mathematics and finance theory/applications is much more remote. This is not to say that in fact, the very finance industry, has actually picked up, upon this absence of interdisciplinarity in academia: i.e. many quant traders and bankers, have often dual degrees in physics and maths and combine this knowledge with the finance knowledge they get served up, once they embark upon a career in the finance industry.

3 THE 'DISCARD OF DETAIL' ARGUMENT

Most of the approaches which are steeped in physics, more specifically statistical mechanics, when applied to problems in finance and economics, will provide for tools which can augment prediction. However, the explanatory power of what one observes via the use of physics, is not necessarily augmented. As an example, it remains not so obvious to explain why financial data has embedded power laws. Does this characteristic help us better to understand financial data? Maybe not?

I have often brought forward the argument, that if there is no physics model embedded in financial or economics theory, progress in those disciplines via the interdisciplinary conduit will be modest. A good counterexample, which does precisely provide for a model is the work on statistical microeconomics by Belal Baaquie [3]. The Hamiltonian framework is introduced and the work shows how the augmented information on the equilibrium price and its dynamic evolution can be captured by respectively the potential and kinetic energy terms making up the Hamiltonian.

As we have remarked before in other work, the connections with physics are difficult and very tricky to fathom. The key issue, I believe, in order to really use physics ideas in social science, is that one has to have an openness of mind which allows for the discarding of detail. What do I mean? Let us give an example. The use of Brownian motion in financial option trading is clearly an invention which came out of first in mathematics, via the use of Louis Bachelier's [4] work on arithmetic Brownian motion in the theory of games. We all know Einstein's work on Brownian motion. But why should a stock price process conform to a Brownian motion?

Is it reasonable? If one were to have a very close attention to detail, one would discard such analogy. Why should trading be continuous when manifestly it is not? Why should the time evolution stock prices follow, be along a path which is continuous but nowhere differentiable? Do we need non-zero quadratic variation? In summary, a very close attention to detail, would probably have discarded Brownian motion as a reasonable description of the stochastic behavior of asset prices over time. But instead it became a mainstay. It is the key stochastic differential equation which drives option pricing theory.

Vladik Kreinovich and co-authors [23] propose some important stepping stones which may allow a newcomer to enter the world of the interdisciplinary applications which connect physics with economics and finance. As an example, in that paper it is argued that symmetries may well be natural in economics. The example of the measurement of GDP is indeed scale invariant. The authors advance good examples which show the shift invariance and additivity as key properties which also exist in economics. But there are characteristics from physics, which I would say, do not translate well in economics. A key characteristic which is an issue, I believe, is whether the economy can be seen as a conserved system. In some regards it can, but one can easily come up with examples where conservation is not valid. As one knows, there are essential results from basic physics which will not hold if conservation is not in place. Is that an issue? Does this problem refer us back to what we mentioned before: i.e. a need for a degree of 'discard of detail'? I leave it up to the reader to decide. As further examples, we have mentioned before that there are other issues like the objectivity of time and the time reversibility. Both are characteristic of a lot of physical processes but they are not essential when we consider financial processes. Again, can the 'discard of detail' ability help us here?

4 PUSHING HARD THE 'DISCARD OF DETAIL' REQUIREMENT: A STEP FORWARD IN EXPLAINING VERSUS PREDICTING?

An area where the 'discard of detail' requirement may be even more prevalent is in the application of the quantum-like formalism in social science. From the outset, for any new readers, this new approach refers to the use of a subset of formalisms from quantum mechanics which are applied in a social science macroscopic environment. There can be scope for an analogy of a quantum mechanical phenomenon in the decision making process of individuals. We discuss this more below. In the area of finance though, the prowess of the imported quantum formalism comes more to light with its connection to a specific form of information measurement. We discuss this more in the next section.

The quantum-like formalism is probably most well known in its applications to psychology and more specifically decision making. Please consult the oeuvres of Khrennikov [25]; [24]; [20]; [26] and Busemeyer [11].

In Aerts and D'Hooghe [1], one goes beyond just the use of a formalism. In fact the approach the authors follow is really very much concerned with explaining a phenomenon, i.e. in this case, the process of decision making. We note again that although quantum physics as a theory has been, very probably, the most successful theory ever devised by humankind in correctly predicting quantum phenomena, there are very deep foundational issues in quantum mechanics which remain unresolved. For instance, the interpretation of the meaning of the wave function, a key building block in that theory, is still open for debate. Aerts and D'Hooghe [1] propose two possible layers in the human thought process: i) the *classical logical layer* and ii) the *quantum conceptual layer*.

A key argument is the subtle difference between both layers. In the quantum conceptual layer, so called 'concepts' are combined and it is precisely those combinations which will function as individual entities. In the classical logical layer, one combines also concepts *but* those combinations will *not* function as individual entities. This subtle distinction leads to an explanation of two well known effects: the so called 'disjunction effect' and 'the conjunction fallacy'.

The disjunction effect, made furore the first time it was uncovered (and then systematically confirmed in subsequent experiments) by Shafir and Tversky [33]. It invalidates a key axiom (the so called 'sure-thing' principle) in subjective expected utility, a framework devised by Savage [32] and heavily used in many economic theory models. This violation of the sure thing principle is also known as the Ellsberg paradox [16]. It is best illustrated with a so called two stage gamble where you are you are either informed that: i) the first gamble was a win; or ii) the first gamble was a loss; or iii) there is no information on what the outcome was in the first gamble. What Tversky and Shafir observed was that gamble participants exhibited counter-intuitive behavior in their gambling decisions. In essence, gamblers agree to gamble in similar proportions, when they have been informed whether they either won or lost. The issue which is counter-intuitive is when gamblers are not informed. Busemeyer and Wang [12] show that a quantum approach can work here. The so called 'no information' state is now considered as a superposition of both informed states.

The conjunction fallacy was another very interesting paradox. It was uncovered by Tversky and Kahneman [37] and it shows that experiment participants make decisions which contradict Kolmogorovian probability theory (i.e. the probability of an intersection of events A and B is seen as more probable than the probability of either event A or B)

Those two fallacies can call in for the use of a more generalized rule of probability which can be found in quantum mechanics: i.e. the probability rule which accommodates the interference effect. It is by no means the only rule of probability which can solve this issue. More generalized rules, beyond the one of quantum probability, can also be used. See Haven and Khrennikov [19]. We do not expand on it here. Within the setting of the two layers that Aerts and D'Hooghe proposed, there is a very clear attempt to explaining the outcome of the experiments. Interested readers should consult Sozzo [31] and Aerts, Sozzo and Veloz [2] for more information.

We close this section of the paper with the words that indeed we do push hard the 'discard of detail' argument here, as in effect we try to use, besides the formalism of quantum mechanics, elements of the philosophy of quantum mechanics. This indeed is an example of where we think quantum mechanics may reside even at the macroscopic scale of a human decision making process.

The next section of the paper makes the 'discard of detail' argument less hard to push, and it does so with as result that there may well be less explaining but more prediction.

5 PUSHING *LESS* HARD THE 'DISCARD OF DETAIL' REQUIREMENT: A STEP FORWARD IN PREDICTING RATHER THAN EXPLAINING?

As we mentioned before, the inroads in decision making that the quantum formalism has made are important. The proof of this statement can be found in the fact that publications in this very area of applications have now appeared in top journals.

The 'discard of detail' argument is maybe somewhat less hard to push when we consider applications to finance. Here, it is just the formalism which really makes the difference rather than the formalism *and* the philosophy (the thought process) of quantum mechanics per sé. The formalism we push here, revolves around the possible fact that the wave function can have an information interpretation. But even more distinguishing from mainstream quantum mechanics, is the fact that the formalism we follow uses a trajectory interpretation of quantum mechanics. In effect, an ensemble of trajectories exist if the so called 'quantum potential' is non-zero. This potential is not quite comparable to a real potential. This approach, also known under the name of Bohmian mechanics, requires the concept of non-locality, which says that the wave function is not factorizable. The key references are by Bohm ([8], [9]) and Bohm and Hiley ([7]).

The mathematical set up on deriving the quantum potential can be summarized in a couple of steps. We follow here Choustova [13]. The ideas of using Bohmian mechanics in a finance environment were first devised by Khrennikov and Choustova ([14]; [25]). The wave function in polar form can be written as: $\psi(q,t) = R(q,t)e^{i\frac{S(q,t)}{h}}$; where the amplitude function $R(q,t) = |\psi(q,t)|$; and the phase of the wave function is $S(q,t)/h$, with h the Planck constant. Note that q is position and

t is time. We substitute $\psi(q,t) = R(q,t)e^{i\frac{S(q,t)}{h}}$ into the Schrödinger equation:

$$ih\frac{\partial\psi}{\partial t} = -\frac{h^2}{2m}\frac{\partial^2\psi}{\partial q^2}+V(q,t)\psi(q,t); \quad (1)$$

where m is mass; i is a complex number and $V(q,t)$ is the time dependent real potential. It is best to consider the left hand side first of the above PDE when substituting the polar form of the wave function. This then yields:

$$= ih\frac{\partial R}{\partial t}e^{i\frac{S}{h}} - R\frac{\partial S}{\partial t}e^{i\frac{S}{h}}. \quad (2)$$

The right hand side of the PDE, when substituting the polar form of the wave function yields, after simplification:

$$\frac{\partial^2 R}{\partial q^2}e^{i\frac{S}{h}} + \frac{2i}{h}\frac{\partial R}{\partial q}\frac{\partial S}{\partial q}e^{i\frac{S}{h}} \\ +R\frac{i}{h}\frac{\partial^2 S}{\partial q^2}e^{i\frac{S}{h}} - \frac{R}{h^2}\left(\frac{\partial S}{\partial q}\right)^2 e^{i\frac{S}{h}} \quad (3)$$

When the Schrödinger equation PDE is re-considered with the substitutions on the left and right hand sides, one obtains:

$$ih\frac{\partial R}{\partial t}e^{i\frac{S}{h}} - R\frac{\partial S}{\partial t}e^{i\frac{S}{h}} \\ = \frac{-h^2}{2m}\left[\begin{array}{c} \frac{\partial^2 R}{\partial q^2}e^{i\frac{S}{h}} + \frac{2i}{h}\frac{\partial R}{\partial q}\frac{\partial S}{\partial q}e^{i\frac{S}{h}} + \\ R\frac{i}{h}\frac{\partial^2 S}{\partial q^2}e^{i\frac{S}{h}} - \frac{R}{h^2}\left(\frac{\partial S}{\partial q}\right)^2 e^{i\frac{S}{h}} \end{array}\right] \\ + V\psi$$

After some additional cleaning up (multiplication of the above with $e^{-i\frac{S}{h}}$), separation of real and imaginary parts, leads to, for the imaginary part:

$$\frac{\partial R}{\partial t} = \frac{-1}{2m}\left[2\frac{\partial R}{\partial q}\frac{\partial S}{\partial q} + R\frac{\partial^2 S}{\partial q^2}\right] \quad (4)$$

And for the real part:

$$-R\frac{\partial S}{\partial t} = \frac{-h^2}{2m}\left[\frac{\partial^2 R}{\partial q^2} - \frac{R}{h^2}\left(\frac{\partial S}{\partial q}\right)^2\right]+VR \quad (5)$$

If the imaginary part is now multiplied with (both left handside and right handside) by $2R$, one obtains:

$$2R\frac{\partial R}{\partial t} = \frac{-1}{2m}\left[2R2\frac{\partial R}{\partial q}\frac{\partial S}{\partial q} + 2RR\frac{\partial^2 S}{\partial q^2}\right] \quad (6)$$

, which can be re-written as:

$$\frac{\partial R^2}{\partial t} + \frac{1}{m}\frac{\partial}{\partial q}\left(R^2\frac{\partial S}{\partial q}\right) = 0 \quad (7)$$

This is a famous equation in physics, known as the “continuity equation”, and it expresses the evolution of a probability distribution, since $R^2 = |\psi|^2$. If we divide the real part by $-R$, one obtains:

$$\frac{\partial S}{\partial t}+\frac{1}{2m}\left(\frac{\partial S}{\partial q}\right)^2+\left(V - \frac{h^2}{2mR}\frac{\partial^2 R}{\partial q^2}\right) = 0 \quad (8)$$

This is the Hamilton-Jacobi equation when $\frac{h^2}{2m} << 1$ and $\frac{h^2}{2mR}\frac{\partial^2 R}{\partial q^2}$ is negligibly small. The term $-\frac{h^2}{2mR}\frac{\partial^2 R}{\partial q^2}$ is the so called quantum potential.

The quasi-classical interpretation of quantum mechanics becomes quite clear now when we can consider the Newton-Bohm equation, which is:

$$m\frac{d^2 q(t)}{dt^2} = -\frac{\partial V(q,t)}{\partial q} - \frac{\partial Q(q,t)}{\partial q} \quad (9)$$

and $Q(q,t)$, being the quantum potential, depends on the wave function which evolves according to the

Schrödinger equation. The initial conditions are $q(t_0) = q_0$ and $q'(t_0) = q_0^{'}$ (momentum).

This is an important result for us, since we want to model a financial process with the above ordinary differential equation. However, we are - of course - faced with caveats. Let us enumerate some:

- There is a proportional relationship between the quantum potential and so called Fisher information (see Reginatto [30]), which incidentally has a connection with a widely used concept in econometrics, i.e. the so called Cramer-Rao bound. Even though, we can connect the quantum potential with the idea of information, as we do in the above framework, we have to assume that the wave function follows the Schrödinger PDE. This is surely a topic for much further discussion when we consider the social science environment in which some version of that Schrödinger PDE would have to be embedded in.

- The above ordinary differential equation is an extension, as many readers will have seen of Newton's second law. The path generation attached to this ODE does not exhibit non-zero quadratic variation. See Choustova [14] for a discussion under which stringent conditions such non-zero quadratic variation can still obtain.

- The "doing away" of the Planck constant in a social science environment is obvious. But can one think of an equivalent scaling parameter in the social sciences?

However, all is not that bleak. It is true that such a setting, when applied to finance, may well not do so well in explaining . At least, we may not seem to have available the elegance of the multiple layers of thought arguments we discussed above. In recent work Shen and Haven [34], used real commodity return data to find the functional forms of both the real and quantum potentials. This follows up on work which was first presented by Tahmasebi et al. [35]. The findings show that the quantum potential, via its connection with an information measure, does capture some aspect of public information. The real potential, does capture the expected part of public information, i.e. that the most likely daily returns on commodities are close to 0% and that any deviation from that equilibrium state is unlikely. There is an interpretation to be given to the steep walls of both the quantum and real potentials, and it is really the gradient of the forces, which themselves are the negative gradient of the potential, which may say more about the differential information contained in that body of public information. More work is to be done, with the inclusion of hopefully, the use of the extended second law of Newton to predict price behavior. The title of this section of the paper in that respect is quite correct: there is the promise of predicting rather than explaining.

6 CONCLUSION

This paper may now beg for a potential

key question, which may go as follows. Would anybody who did not hear about using the quantum mechanical formalism applied to social science wanting to 'come on board' after reading this paper? We think we can make some arguments that the line of thinking we proposed here may have rich avenues which can expect to deliver results. But, and there is an important ' but', it may require some level of distance from some expectations.

First of all, as discussed in this paper, the Feynman commencement address at Caltech, which we denoted with the words 'pure calling' may not be applicable to a full degree. As we explained, we may not be able to fully abide with that constraint. We surely do not contest it but the nature of the work done, even when within discipline, such as with economics, may not warrant that we can heed - fully - the call of the 'pure calling' to make a cheap play on words.

Secondly, the discard of detail attitude we described in this paper, is a necessity to do the type of work we propose here. It is really very easy to come up with arguments why physics formalisms and ideas may just *not translate* into social science. The simple arguments of the absence of a lab, as in physics, comes to mind if one wants to start initiating a discussion on this. Progressively more sophisticated arguments still come easy. The issues of conservation problems and reversibility of time spring to mind. There are many others, and they are really not hard to argue for.

Thirdly, we hope we could show to some extent, that the propensity to explain, can be achieved with some of the quantum mechanical ideas. We proposed the layers of thought arguments. When penetrating even more into the decision making area, we can become much more technical about using the interference term. But we do not have to reside there only. We can use more generalized probability measures. In finance, we are less well versed in explaining with the methodology we proposed here. There is, at present, more of a tendency to see if prediction (rather than explaining) could function. We do fully agree that even at this very moment in time, prediction has not yet occurred via that model.

We have not talked in this paper about other approaches, still sourced from quantum mechanics in the forms of quantum field theory applications and work revolving on the use of open systems. There is sprawling activity in both areas. In the open systems approach, work by Polina Khrennikova ([21]) on political decision making has made important inroads. In the use of elementary ideas from quantum field theory, work by Fabio Bagarello [6] and Polina Khrennikova [22] has led the way to, here also, new horizons. The future may be brighter than one thinks.

I want to close this paper by cycling back, one more time, to the quote by Richard Feynman. Even though we may in certain areas of intellectual endeavour have difficulty to come up with a good collection of precise arguments why an obtained result should be taken 'cum grano salis', it is of paramount importance that we all realize that to not engage at all with Feynman's remark is the worst outcome of all. Not engaging at all with his proposed exercise, maybe a symptom of one of the three errors that Hambrick [18] described as 'actions that bring palm to face'. One particular mistake is the so called Dunning-Kruger effect, which in essence makes us to overrate success (i.e. we may think we know much more whilst we do not at all). Dunning [15] himself described it as *meta-ignorance,* ignorance of ignorance.

I am not suggesting that we are at this dire level of state of affairs at all. Absolutely not! However, in the absence of us not knowing what the real truth is, it shall be very useful to go into this healthy exercise Feynman proposes: i.e. to de-construct, as best as we can, the theory or result we just proposed. In effect, a very good example of this de-construction is quantum physics itself. We all know it is a theory which has been tested, over and over again, and has delivered as no other theory in humanity has. Even though this huge success is unmistakable, there is a very sprawling activity in quantum mechanics which deals with the foundations of quantum physics. Highly achieved quantum physicists have participated in numerous debates on this very topic. This is an example to follow. In fact, one of the protagonists of the movement which applies quantum formalisms to social science is Professor Andrei Khrennikov whom we mentioned multiple times in this paper. He is also the founder and organizer of the longest held series of conferences - ever, which precisely deal with the topic of the foundations of quantum physics. Hence, we are in outstandingly good company to pull the quantum-like applications in social science to ever higher levels of achievement.

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