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# *A Brief Epistemology of Economic Models. A Short Essay in Philosophy of Science*

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## 1. The matter at issue

The present paper criticizes the commonsensical belief that exact science is deterministic by showing that economics does not deal with uncertainty as effectively as natural science. In particular, this paper shows that while contemporary physics succeeded at solving the major epistemological troubles deriving from the indeterminacy of quantum mechanics, economics still fails at improving the predictive power of its models. Hence, the paper suggests that while physics managed to refine its method, economists still fail at finding a commonly shared method that bear stable results.

Now, according to the common wisdom, scientific explanations of given events establish stable and solid quantitative relationship between particular observables and general causes.

In truth, this amounts to state that science still follows the Newtonian paradigm and that the scientific method has not changed at all in over three and a half centuries since *Philosophiae Naturalis Principia Mathematica* (1687) was published. In fact, common wisdom builds upon Newton's paradigmatic idea that any scientific explanation of observable facts should be based on some very general principles (the fundamental laws) that are inductively derived from immediate evidence. In particular, the Newtonian framework demands that it must be possible to fix general principles and derive their remotest consequences so that particular events can be explained via *verae causae*, e.g. gravity, that relate general laws and particular empirical evidence, e.g. gravitation and Kepler's laws (Ruse, 1989).

In order to explain why the abovementioned common belief is wrong and simplistic, we can take advantage of an equally straightforward example that introduces the discussion that follows in the next pages.

Imagine a chess game and imagine all the chess pieces disposed at the two edges of the chess board. Then, imagine that two players play the game and that the chess pieces have a given configuration that designates a winner at the end of the game. The question to be answer is: is it possible to give a univocal scientific explanation of how given outcomes of the game obtain? Theoretically, it is. Yet it is also possible to object that there might be other ways to explain how the outcome of the chess game is determined. More specifically, my contention is that the chess game could be a good metaphor to show that alternative explanations in science are possible.

Indeed, there might be at least four explanations of how the outcome of the chess game obtains and all these explanations are in equally correct at least in principle:

1. If we were to reason in Newtonian terms, we would simply say that the outcome of the game is obtained by deducing the particular case of the game that is being played from some general principles (i.e., the rules of the game);
2. If we were to reason in terms of statistical mechanics, we would simply say that there must be a given winning strategy such that there's a given amount of probability that one of the two players will follow exactly that strategy and win the game;
3. If we were to reason in terms of Quantum Mechanics, we would say that we observe someone winning the game because of some given factors, but we can't identify the complete causal chain;
4. If we were to reason like social scientists, we would simply argue that the circumstances of the game (e.g., players' level of attention or experience) led to the observed outcome.

Besides that, what is remarkable is that, first, each of the possible explanations of the observed events is based on the assumptions that sustain the hypothesis that is proposed and, second, each of the possible explanations proposes different causal chains that explain the event that is observed.

In regard to this, the present paper aims at building a short epistemology of the economic models by taking advantage of the chess game and at exploring three factors that happen to be fundamental while explaining the events of the game, namely: assumptions, causality and uncertainty. In particular, the present paper stresses that these each of these factors play a crucial role in each explanation.

Upon this claim, the next section focuses on assumptions and on answer 2, the third section focuses on uncertainty and on answers 1 and 3, while the last section focuses on causality and answer 4.

## 2. On the importance of assumptions

As already remarked, Modern science is based upon the deterministic idea that any explanation of recurring particular phenomena is to be derived from general principles that are initially established.

Allegedly, this grounding belief is rooted upon the fact that the deductive system of Euclidean geometry has been the main geometrical framework available to scientists until the beginning of 20$^{th}$ Century. With respect to this, 19$^{th}$ Century's British philosophers Whewell and Herschel maintain that good rigorous method in science is to be based on hypothetico-deductive models (see Ruse, 1975).

Nevertheless, the rise of Boltzmann's pioneering work in Thermodynamics encourages scientists and philosophers to begin calling into questions two of the strongholds of modern science: first, the determinism of deduction; second, modern scientists' dogmatic idea that science is to identify the complete causal chains that explain why given particular evidence is observed, e.g. planets' motion, without any speculation (see *general scholium*, 2$^{nd}$ ed. of Newton's Principia, 1713). In contrast to these tenets, Boltzmann's pioneering work in thermodynamics opens a new chapter of the philosophical debate about science only by adding to it a new and simple philosophical question that challenges the method of Modern Science, i.e. can probability theory be legitimately applied to science?

More specifically, Boltzmann's main contribution to the history of science regards his endeavor to explain why we observe macroscopic events that are the outcomes of microscopic and chaotic events. Indeed, his work is one of the first attempts in the history of science to answer a very important research question, namely: considered that the second law of thermodynamics asserts the irreversibility of thermodynamic processes as transfers of heat always lead to an increase of entropy, how is then possible to describe the behavior of all those microscopic events that bring about particular macroscopic observable events?

In order to grasp the importance of this question, we shall go back to the chess game that was previously introduced and ask ourselves: how many chances are there that at the end of the game the chess pieces will be disposed exactly like they were disposed at the beginning of the game? Of course, the correct answer is 'basically none'. Yet this question allows us to see what Boltzmann grasped. In fact, the outcome of the game is going to be gradually determined by a gradually increasing disorder on the chessboard. This means that, in order to know who wins the game, there must exist a given winning strategy such that there's a given amount of probability that one of the players will follow exactly that strategy and win the game. Most importantly, the hypothesis regarding the existence of such a strategy predicts that there should be some configuration of the chess pieces on the chessboard caused by the players playing the game that represents the winning outcome and designates who wins the game. However, it is not possible to deterministically assign a winner to the game without formulating some assumptions on the game itself by, for example, assessing players' skills ex ante. Accordingly, it is not possible to forecast the outcome of the game by simply taking the rules of the game into account. For there might be other factors that might influence the course of events throughout the game.

Similarly, in Thermodynamics such an increase of disorder is known as entropy and Boltzmann's work in thermodynamics is pioneering because he has been the first physicist who *understood that the laws of thermodynamics are the outcome of statistical and probabilistic laws* [and proposed that observable] *macroscopic regularities are the outcome of casual interactions amongst molecules* (Rovelli, 2015, p. 12). Hence, like in our chess game, given thermodynamic events are the outcome of multiple casual interactions amongst gas molecules that resemble the moves of each player throughout the game to its end. That is, like for the chess pieces, there must exist an initial and a final gas molecules configuration that designate two distinct macroscopic events, i.e. the initial and the final status of the physical system that is investigated. Nevertheless, while the initial position of chess pieces is established by the rules of the game, this is not the case for molecules as their initial positions happen to be casual. For this reason, assumptions concerning the initial positions of molecules are a necessary condition for predicting the future states of the physical system that is studied.

In regard to this, Boltzmann's work is pioneering because it devises the first method to calculate entropy by means of probability. Indeed, his entropy equation expresses the direct relationship between the measured entropy and the amount of microstates that cause a given macroscopic event in an ideal gas. In particular, the equation assumes that, as observable macroscopic regularities are the outcome of an increasing disorder on the microscopic level of reality, each microstate takes place with the same probability of any other microstate. That is, like the outcome of our chess game is determined by the possibility that players make any move that is allowed, also any given macroscopic observable state of an ideal gas is caused by any possible microstate.

In its original formulation Boltzmann's entropy equation states that the more complex a system is, the higher are entropy and the number of microstates. In formal terms the equation is written as follows:

$$S = k_B \ln W$$

where $S$ is the entropy and is regarded as an adimensional factor, $k_B$ is Boltzmann's constant and $W$ is the number of microstates that is assumed to trigger the observable state of the gas. $\ln W$ ensures that entropy gradually increases till some point where it starts slowing down (i.e., when the event becomes visible). In this sense, the equation allows to describe the evolutional states of an ideal gas whose particles are assumed to be physically identical, to have identical probability distribution and to be statistically independent.

Furthermore, *according to Boltzmann, entropy is a measure for physical probability, and the essence of* [the Second Law of Thermodynamics] *is that in Nature a state occurs more frequently, the more probable it is* [However] *one always* *measures in Nature the differences in entropies, never the entropy itself, and to this extent one cannot speak of the absolute of a state, without a certain arbitrariness* (Planck, 1920). In this regard, Boltzmann's constant is obtained by dividing the gas constant $\bar{R}$ by the Avogadro constant $N_A$ and expresses the general relation between energy and temperature in an ideal gas. Specifically, the constant represents the random thermal motion of a single molecule, provided that the position of each microstate is probabilistically assumed. In this way, when employed in measurement, the constant can be used to measure the difference in entropies of different thermodynamic systems (i.e. systems with different amounts of $W$) that are not in equilibrium.

As far as this is clear, it is worth to remark that, for physicists, the usefulness of probability theory does not spring out of the fact that since particles cannot be observed, scientists can only rely upon the laws of probability when describing the behavior of a multitude of microscopic entities. By contrast, it's been the application of probability theory and statistics to physics that has allowed physicists to devise highly abstract models that replicate in general terms the particular events that take place on the microscopic level of reality. Indeed, the history of mathematics teaches us that probabilists formulated two important theorems that lay at the foundations of mathematical statistics and ease the work of social and natural scientists, namely: *Bernoulli's law of large numbers* that states that *as the number of identically distributed and randomly generated variable increases, their sample mean approaches their theoretical mean*;[1] and *Laplace' central limit theorem* that states that *the sum or average of an infinite sequence of independent and identically distributed variables, when suitably rescales, tends to a normal distribution.*[2] Most importantly, both theorems ease the normalization of values and allow natural and social scientists to calculate quite accurate point or interval estimations of unknown values. As a consequence, the models of contemporary physics that describe the particles behavior are usually very general and express only probabilistic relationships between the entities which reflect our conceptualization of the microscopic reality, i.e. our intuitions (Reichenbach, 1929). This explains why we can picture the molecules of an ideal gas as perfect spheres that behave like billiard balls that bounce on one another any time players hit them with a billiard cue. Indeed, each configuration of the balls depends on many other factors – the strength of the bumps, how long for the player interacts with the system, the players' experience etc. Hence, the consistency of a model is given by the power of the assumptions that support

[1] http://www.britannica.com/science/law-of-large-numbers

[2] http://www.britannica.com/topic/central-limit-theorem

its hypotheses and, most importantly, by its ability to combine all the relevant factors that may interfere with measurements in terms of probabilities.

Similarly, this happens to be an epistemological issue also in economics. In fact, while performing measurements, economists are often to deal with two problems that originate from uncertainty and other disturbing factors that affect measurement, namely: the effectiveness of econometrics and the reliability of the economic models. Most importantly, either issues are related to the consistency of economic theories and to the assumptions that lay at their foundations consequently. That is, either issues relate to the objectivity of economic theories. Upon this, our discussion shall now turn to the philosophical consequences of Lucas' critique of econometrics and to Friedman's arguments regarding the objectivity of economic theory. Once more, the example of the chess game will be our proxy.

Lucas criticizes standard econometrics in that it usually predicts the economic effects of new policies implementation on the basis of historical data. Specifically, Lucas argues that such models fail at taking into account that once governments introduce new policies, rational agents adjust their behavior in agreement to the novelties introduced by the policy. That is, agents' behaviors caused by a change in policy might discontinue previous trends. Thus, Lucas argues that if models estimate the effects of new policies assuming invariance in estimation, this might lead to an increase of forecast uncertainty due to the fact that models are not aligned with the possible implications of policies.

Now, in order to make sense of the critique, let us go back to the chess game previously introduced. Let us imagine that the two players (*player 1* and *player 2*) played *n games* and that at round *n player 1* won most of the games thanks to his skills. Then, imagine that the referee who chairs the game between *player 1* and *player 2* proposes to introduce new rules. Does it make sense to claim that the new rules (e.g. the bishop moves like the rook and vice versa) will fit the skills of both players and that *player 1* will keep leading the competition at round $n + 1$? Unfortunately, it does not make very much sense to claim so because any argument must be supported by assumptions, e.g. the two players are equally skilled or smart. Hence, predictions based on previous experience might turn out to be inconsistent unless they're deductively justified.

With respect to this very issue, new classical economists like Lucas usually claim that economics ought to employ an a priori approach. That is, like modern scientists, economists should consider the identified statistical relations amongst relevant facts as being the outcomes of unobservable interactions amongst unobservable variables, e.g. market forces. Accordingly, Lucas claims that the link between changes in policies and changes in trends is secured by agents' rationality and their ability to adjust their behavior to the changes in the economic environment. Nonetheless, undertaking this approach leads to a problematic and methodological issue: although assuming people rationality as coherent with the general principles of economics ensures deduction, wouldn't this amount to a simplification of the reality? Hoover (1994) suggests that this question leads to a necessary distinction between weak and strong apriorism.

If we endorse strong apriorism, then we basically suggest that *good econometrics starts with a hypothesis derived from theory which dictates certain expected signs and significance levels of coefficients. It then estimates a regression and checks whether the result accords with the a priori expectation – if yes, fine; if no, back to the theoretical drawing board. Bad econometrics tries out arbitrary (i.e. atheoretical) specifications until the results suit the investigator's prior beliefs* (Hoover, 1994, p. 303). Within this framework, agents are assumed to be fully rational and to aim at maximizing their utility (efficient market hypothesis!). Accordingly, given rationality, individuals respond to the changes in the economic environment proactively and *irrational behaviors* are excluded ex ante. Yet, if rationality and utility-maximization are assumed, how is then to possible to undertake strong apriorism to explain the massive purchases of subprime mortgages that led to the bankruptcy of Lehman Brothers in 2008?

By contrast, if we endorse weak apriorism, we suggest that *belief and inference stand in a relationship of mutuality: inferences are founded partly on unexamined beliefs; but these inferences, in turn, may suggest the modification of those beliefs. Thus, theory presents us with some a priori (in the sense of not currently questioned) restrictions on empirical investigations; while the empirical results help generate beliefs (or new theories) which are prior to further investigations* (Hoover, 1994, p. 305). Accordingly, weak apriorism makes it possible to adjust theory to data without arbitrariness as modifications would be justified by testing. For econometrics then becomes a quantitative method that allows to make forecasts on the basis of data that can either prove or disprove the theory.

Hence, within the framework of weak apriorism, econometrics allows economists only to set models that capture the invariant relations amongst the relevant variables on different levels of complexity and the effects of such interactions amongst these variables. At the same time, econometrics also provides economists with the evidence that justifies the hypotheses that are put forward by a given theory. That is, econometric models allow economists to carry out statistical inferences from multitudes of random observations and establish whether the observed trends and the estimated statistical errors confirm the initial hypothesis or not. Thereby, economists can either refine and expand the applications of the employed econometric models or reject their theories and formulate new models on the basis of what data display. That is, trends can be explained only as long as evidence supports the theory and statistical errors are not impeding. For this reason, within weak apriorism, agents are assumed to be rationally bounded. In this sense, deduction is adjustable to what data display and agents' behavior can be explained by circumstances.

Now, as long as economics sticks to hypothetico-deductive explanations whilst its predictive power often turns out to be weak nonetheless, is it possible to assert that economics is an objective science? And can economics be considered a positive (and, thus, exact) science?

According to Milton Friedman, any answer to these two questions requires a preliminary distinction between positive and normative economics. To him, the former justifies its claims quantitatively on the basis of evidence, while the latter expresses qualitative judgements on the value of policies and justifies its claims by reasoning by counterfactuals. For instance, an economist might either claim that the implementation of the Quantitative Easing program in the US was effective upon evidence obtained by means of regression or claim that, on the basis of evidence, shouldn't the FED have reacted to the slump as it did, the consequences of the recession would have been much worse. That is, economists can either reason strictly in quantitative terms or in both qualitative and quantitative terms taking data as a proxy for explanation.

Upon the distinction, Friedman argues that true positive economics should justify its claims rather in the first than in the second way because true economics is always made up of two components: the theory itself and the empirical data. Hence, he defines the former as a set of tautologies that are logically interrelated and the latter as the bulk of evidence that proves whether the hypotheses derived from the theory are founded or not.

In particular, in Friedman's views, *truly important and significant hypotheses will be found to have assumptions that are wildly inaccurate descriptive representations of reality, and, in general, the more significant the theory, the more unrealistic the assumptions (in this sense)* […] *a hypothesis is important if it "explains" much by little, that is, if it abstracts the common and crucial elements from the mass of complex and detailed circumstances surrounding the phenomena to be explained and permits valid predictions on the basis of them alone* (Friedman, 1953, p. 153). This is possible as far as the hypothesis of a theory is derived from its crucial and unrealistic assumptions. In this way, if the relationship between assumptions and hypothesis is symmetric (i.e., it is possible to derive the hypothesis from the assumption and vice versa), then it must also be possible to show how the assumptions lay at the foundations of the hypothesis. That is, the logical relation between assumptions and statements of the theory reflects the causal chain that the theory identifies.

Furthermore, Friedman acknowledges that there might be more than one assumption that bears truthful hypotheses. That is, for him, efficient and objective models are those that bear testable and reliable conclusions that can be further tested. Accordingly, successful hypotheses are those that confirm models that can additionally encompass and explain facts that are explained by other hypotheses. In this sense, implausible assumptions that distort the reality allow for high levels of abstraction. For they are remedies to the fact that, differently than in the case of natural sciences, controlled experiments in economics are not fully possible. Thus, the major objective of economic models is to describe facts by presenting the interrelations between the ideal-types that depict only the relevant aspects of the reality that might explain complex phenomena.

For instance, even though there is nothing like perfect competition in the real world, the explanatory power of such an ideal-type allows economists to explain how the competition between firms of the same industry takes place and how it is related to price-elasticity. In this way, economists can simplify the complexity of real-world-competition and explain all the relevant phenomena by means of statistical inference.

However, despite implausible assumptions might ease the job of economists, this does not mean that it is not costly to employ them. Indeed, if we were to take Friedman literally agreeing to the fact implausible assumptions and quantitative methods are the key elements of good theories, we would risk of ignoring many other qualitative factors that do affect measurements. In fact, Akerlof's seminal paper (1970) shows that information asymmetry happens to be a peculiar aspect in markets and often affects measurements. For qualitative aspects as moral hazard can be drivers of agents' behavior and eventually affect market development more than economists might even believe.

For example, imagine that our chess game is now played by a human being and a cyborg that is able to predict the moves of the human being. Then, assume also that the human being is not aware of playing against a cyborg. That is, like in the case of perfect competition, he believes that he is playing against another being alike him with equal knowledge. Most likely, the human being would lose most of the games because the cyborg would concretely have much more information than the human being. Besides, if we also assume that no one apart from the cyborg knows that there's a cyborg playing the game, then no assumption would lead to reliable predictions concerning the outcome of the game. In this sense, the cyborg would have much more information than the other player and would take advantage of it, whilst no one could exactly predict the outcome of the game just by assuming that the rules of the game are followed, that no one is cheating and that the players are equally smart.

It follows that, QED, information asymmetry and cognitive limitations affect behavior. Hence, any simplification of the reality that aims to explain economic processes might simply overlook important causal chains. In particular, while in Physics simplifications do not affect measurements because noise can be controlled during tests, this is not possible in economics because uncertainty, and external disturbance happen to affect measurement fundamentally. These two aspects will be the main focuses of the next two sections.

## 3. On the relevance of uncertainty

Hitherto, the discussion has focused on explaining why the rigidity of the hypothetico-deductive models often clashes with the explanatory power of social sciences and of those branches of physics that deal with particles like statistical mechanics. On this basis, the present section aims at explaining why it is important that social and natural scientists find effective ways to deal with theoretical and practical uncertainty. In this respect, this section recounts how the rise of quantum mechanics (QM from now on) sweeps the determinism of Modern Science (notably, Laplace's) away and paves the way to a new indeterministic epistemological stance that clashes with two strongholds of Modern Science and Modern Philosophy, i.e. the Modern concepts of substance and causation (notably Kant's most). Eventually, this section draws a comparison between how particle physicists and economists deal with indeterminacy in their models.

Now, the common belief that is criticized in this paper typically stands upon Laplace's idea that *we ought to regard the present state of the universe as the effect of its antecedent state and as the cause of the state that is to follow*.[3] In the language of Modern physics, this means that our knowledge of the current state of the universe, i.e. its position and momentum, together with the knowledge of the laws of (Newtonian) physics provide us with enough information to predict the future states of the universe. In formal terms, Laplace's determinism is expressed by his rule of succession that states that if we successfully observe an event $n$ times, then the probability $p$ that it will occur again is obtained by applying the following formula:

$$p = \frac{(n+1)}{(n+2)}$$

[3] http://plato.stanford.edu/entries/determinism-causal/

The principle denotes Laplace's attempt to justify the underlying inductive reasoning of Newtonian mechanics simply by supposing that the more an event is observed, the likelier it is that it will occur again. Regardless of the ingenuity of Laplace' idea, his principle is at fault at least in two respects: first, it holds only as long as the observed entities are macroscopic; second, it assumes that we observe always the same entities. Eventually, the principle can be falsified simply by pointing out that if $n = 0$, then $p = 0.5$ nonetheless. That is, for Laplace, any never-observed fact is always half-true (Gower, 1997).

Specifically, if we were to follow Laplace's line of reasoning and apply his principle to predict the outcome of our chess game, we would risk of making two mistakes.

First, suppose we know nothing about the skills of the players and of their scores throughout the previous games. Would we able to formulate assumptions that can help us predict the outcome of the game? Of course, we would not. Thus, given our knowledge and since we observed no game, we'd only predict that either players have equal chances to win the game, i.e. 0.50.

Second, suppose that we have already observed many games and that player 1 has always won, then we would correctly predict that player 1 is likelier to win the game only by applying the principle. Common wisdom demands this to be true (and partially it is), though this is the case as long as circumstances are favorable. That is, if we were to stick to the determinism of the rule of succession, we'd make predictions on the outcome of the game that ignore any possible negative instance as, for example, the cases where player 1 might be constrained by external factors, e.g. he has headache and cannot focus as he should, and loses the game.

Moreover, the determinism of modern science is rooted onto the modern idea that the universe changes teleologically. In fact, according to Newton, every observable change in motion in the universe is observable because it is relative to an absolute space and an absolute time. In this sense, the changes in the universe that we observe are relative to absolutes.

Such teleological conception of the universe is best represented by Kant's idea that substance is something that endures over time regardless of the changes of its properties. Upon this, Kant maintains that human minds are able to express relations amongst entities because their intuitions locate entities in space and expresses time relations amongst them. That is, human minds only observe portions of space and the changes that take place in those portions of space. Indeed, for Kant, experience is possible mainly because of the category of relation (substance-and-accident; cause-and-effect; reciprocity, i.e. locality). Hence, Kant argues that experience is generally possible because human minds perceive the effects of the changes in substance and are aware of the causal chains that bring about those changes. That is, human minds perceive the effects of the changes in substance because they are aware of the fact that each change is univocally caused and that it is always anteceded by its cause. It follows that it is human mind's awareness of regularities that let us suppose that given effects ought to have always the same cause and occur again and again.

Now, let us attempt to revert the modern line of reasoning by means of our chess game. Imagine that we initially observe *player 1* winning more games than *player 2* throughout *n* games. Then, imagine that we watched all games behind a glass wall, while we shall observe the game *n+1* without standing behind the glass wall, even though we're aware of the fact that our presence might affect the performances of either player. Imagine also that while *player 1* usually comes under stress when the audience is present and loses his attention because of the stress, *player 2* is rather excited of being observed by the audience because this pushes him to do his best throughout the game. The question to be now answered is: can we deterministically assume that *player 1* will win the game? Of course, we can as long as odds show that he can win the game. Nevertheless, we cannot ignore the fact that our interaction with the players might actually affect the outcomes. That is, we're left with a considerable amount of uncertainty regarding the outcome of the chess game. Hence, we can't deterministically assume that *player 1* will win the game again. Indeed, should we find out that our interaction with the players might affect all the successive games we attend as audience, then we might find out that no outcome can be deterministically predicted as uncertainty is an uncontrollable variable.

This example serves as the best proxy to show how QM sweeps away the deterministic stance of modern physics. In fact, in QM substance can be denoted as the vector that describes the state of the system that is being observed, i.e. the vector $|\varphi\rangle$. In simple words, $|\varphi\rangle$ identifies a system that *persists in time, whereas the observables, as accidents of the substance, change with time. Each objective property can without difficulty be referred to the state* $|\varphi\rangle$ *in the sense that each property can either be attributed to the system identified by* $|\varphi\rangle$, *or not* […] *it is the constancy of* $|\varphi\rangle$ *that defines what is meant by a change with time* (Mittelstaedt, 1976, pp. 191–121). That is, opposite to Kant's concept of substance, $|\varphi\rangle$ represents only the bundle of properties of the system that are measured at the time of measurement. Its constancy depends on how long the observation lasts.

It is exactly $|\varphi\rangle$ that lies at the foundations of Heisenberg's uncertainty principle. Indeed, the latter lies upon Heisenberg's conjecture that *electrons do not always exist. They exist only when someone looks at them* […] *they materialize* *in a predictable place with a predictable probability* [because] *when no one disturbs them, they're not in any specific place* [That is] *it is not possible to predict where the electron will be, it is* [instead] *possible to calculate the possibility that it will appear in some place* (Rovelli, 2014, p. 23). In formal terms, Heisenberg's principle can be expressed via two inequations:

1. $\Delta x \Delta p \geq \frac{\hbar}{2}$, where $x$ is the position of the electron, $p$ is its momentum and $\frac{\hbar}{2}$ is Planck's constant divided by $4\pi$;
2. $\Delta E \Delta t \geq \frac{\hbar}{2}$, where $E$ is the energy state of the electron, $t$ is the time of observation and $\frac{\hbar}{2}$ is Planck's constant divided by $4\pi$.

The first inequation refers to the fact that it is not possible to measure momentum and position of an electron at once, whereas the second inequation refers to the fact that *if energy changes slowly, the rate of change of all expectation values of any observable operator must also be slow*.[4] Herein the symbol $\Delta$ substitutes the two standard deviations $\sigma$ of the parameters so that the importance of uncertainty can be stressed properly.

Now, in order to grasp the meaning of the two inequations, let us imagine of shining light on an electron in order to determine its position and momentum. The interaction of the light wave with the system allows to acknowledge an important fact. Indeed, given that Planck's law states that photons with short wavelength have large energy, we observe that: first, the more the wavelength of light decreases, the more the precision of the measurement of $x$ (i.e. the position of the electron) increases; second, the more the wavelength increases, the more of the measurement of p (i.e. the momentum of the electron) increases. Accordingly, *if the energy is known with certainty at one time, then the state is unchanged for all time, i.e. all probability distributions are time independent* [...] *the statistical uncertainty in energy is related to the time rate of change of (the probability distributions of) observables* [so the second inequation] *relates the time scale $\Delta t$ for a significant change of a given (initial) state to the statistical uncertainty of the energy $\Delta E$ in that state.*[5] In simpler words, this means that as in QM time is not an observable, changes in energy are observable whether the expectation values change *while* measurements are carried out.

It follows that Heisenberg's principle tackles the abovementioned strongholds of Modern physics at least in three respects:

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4 https://brilliant.org/wiki/heisenberg-uncertainty-principle/

5 http://ocw.usu.edu/physics/classical-mechanics/pdf_lectures/13.pdf

1. The first inequation states clearly that the two operators $x, p$ do not commute. This means that it is not possible to perform any measurement of $x$ or $p$ without considering their variations (or standard deviation from the mean of the initially assumed probability distribution) together. That is, the more we know about $p$, the less we know about $x$ and vice versa. Plainly, this is where uncertainty lies.
2. The principle states that we are to interact with the system via light waves in order to know something about the system, i.e. find out something about $|\varphi\rangle$.
3. The principle invalidates the causation principle of Newtonian physics as, following Heisenberg, neither is it possible to know position and momentum at once nor is it possible to determine them by performing two independent measurements. Indeed, given the principle, we can predict where an electron will be, but, given $|\varphi\rangle$, we can't be sure of measuring the position or the momentum of the same electron twice.

Thus, considered that it is impossible to assign fixed positions to quantum objects due to quantum superposition, QM conflicts with the modern concept of causation. For Heisenberg's principle leads us to a necessary trade-off between momentum and position at each measurement that shatters Laplace's determinism and the Modern concept of causation. In fact, uncertainty means precisely that physicists can only identify effects and speculate about their causes.

On this basis, the German physicist Max Born maintains that uncertainty forces scientists to abandon determinism and proposes that since QM cannot say much about individual events (i.e., particular events), it only allows for causal statements concerning general events. Indeed, quantum *systems are very different from more familiar physical systems. As long as no interference in terms of measurement is attempted, the quantum systems evolve according to the deterministic Schrödinger equation. The causal aspect comes to light when deliberate measurements are made on the system* [and as] *we* [deal] *with a collection of particles* [the] *causal interference produces events, which happen* [only] *with degrees of probability* (Weinert, 2005, p. 250).

In this sense, Max Born argues that since it is not possible to assign deterministically probabilities to individual events because we cannot know a priori how probably they'll occur, physicists can only assert that they're caused. In order to clarify Max Born's suggestion, we shall take advantage of a fiction example devised by Louis de Broglie (see Weinert, 2005, p. 251). Let $A$ be a fire gun that shoots electrons at a crystal and let $B_1$, $B_2$, $B_3$ … $B_n$ be the individual events caused by $A$ on the crystal surface. As we have given up determinism, it is easy to see that it is impossible to predict where and when a given event $B_i$ takes place on the crystal. Therefore, all that can be asserted is that all the $B$-events have a cause. This means that QM account of causation drops the necessary locality condition as events are caused, but the causal chains are not directly observable. Besides, determinism is dropped in virtue of the fact the relations between events can be expressed only in probabilistic terms, whilst events are also caused by our interaction with system during measurements. This is why QM laws report rather the existence of general facts than the existence of individual events. Indeed, contra Modern Physics, in QM entities do not have a fixed initial position and their momentum is initially unknown.

In this respect, statistics comes in handy to physicists because it allows them to set abstract model that describe the behavior a wide domain domain of objects whose behavior cannot be deterministically described individually. Of course, this increases the chances of the parodox occurrence and unexplained facts, but, as we shall see in the next section, physicists have so far managed effectively to solve most of the problems arising from the discoveries of QM.

Now, in order to grasp the magnitude of the epistemological shift introduced by the rise of QM, we shall go back to our chess game. As mentioned earlier, it might be the case that our interaction with the players might substantially influence the outcome of the game. Hence, as long as we do not interfere with the game (as we observe the game behind the glass wall), the game goes on following the previous trend, i.e. player 1 leads the game. Conversely, our interaction with the system alters the status of the system and, for this reason, we observe events that discontinue the previous trend, i.e. player 2 wins game $n + 1$.

All that we acknowledge is a discrepancy between the two observations due to some cause. Nevertheless, what matters is that there might be many other factors that might substantially influence the course of the events, e.g. the brightness of the room or player's physical conditions. Most of these factors might be potentially unknown to us. That is, all that we can say about the outcome of the game is that there's a given amount of probability that the outcome will be the predicted one as long as there are good chances that the amount of information we have reflects the state of affair that is being observed.

For example, this happens to be the case when experiments are carried out: if the research equipment does not work properly and we're not aware of it, then the outcome of the experiment might not be the predicted one. Thus, contra modern physics, any principle that predicts the outcome of the game expresses solely a general law that describes only what would generally happen under given conditions and predicts any possible outcome only with different degrees of probability. This is what uncertainty amounts to in QM.

Herein, we maintain that QM and social sciences are quite similar exactly in this respect. In fact, also social scientists are to write down general laws that subsume the behavior of a large multitude of individuals and ascribe possible causes to the observed behaviors. Yet social scientists are to deal with greater uncertainty than physicists. For resulting paradoxes and anomalies often limit the credibility and the explanatory power of their theories.

The case of economics happens to be emblematic. In fact, uncertainty plays a crucial role in economic forecasting. For instance, when the data concerning the GDP growth rate of a country are reported by the media, journalists often stress whether or not those data confirm forecasts. Usually, if the forecasts of the GDP growth rate are accurate, then the uncertainty is reported as being low, whilst if they are not accurate, uncertainty is reported as being high. The difference between the forecasted state of affairs is known as *forecast error* and displays economists how strong a predictive power the employed models have.

However, *modern 'capitalist' economies are exceedingly complex, with the macro-aggregates that are observed being the outcome of myriad interrelated decisions and actions taken by large numbers of heterogeneous agents with conflicting objectives. Such economies both evolve and are subject to sudden shifts precipitated by institutional, political, social, financial, legal, and technological change* (Clements and Hendry, 2002, pp. 526–527). In this sense, forecasting models are imperfect tools because they don't allow economists to predict the economic consequences of possible shocks or to take into account all the relevant variables that might affect output over the period that is taken into consideration.

Nevertheless, *forecast uncertainty depends upon the variable being forecast, the type of model used for forecasting, the economic process actually determining the variable being forecast, the information available, and the forecast horizon* (Ericsson, 2001, p. 7). More specifically, the fitness of the model to the information available proves being very relevant. For this reason, economists often attempt to predict the forecast error before the actual outcome is revealed because they aim at establishing what the likeliest outcome might actually be. In turn, the difference between the actual outcome, the actual forecast error and the predicted forecast error can be useful to evaluate how reliable a given econometric model is. In fact, models can be improved in order to obtain less forecast uncertainty and more statistical accuracy. That is, considered that models are to fit the information available and explain how the latter affects the variable that is forecasted, good models should allow economists to predict an interval of forecast uncertainty such that the actual forecast error falls into that range. Yet it is not very easy to establish what uncertainty amounts to and to what extent its influence can be measured.

On the one hand, in statistics and econometrics, the coefficient $R^2$ (the coefficient of determination) indicates in percentages how well data are replicated by a given model in such a way that: if the model fully predicts the outcome measured, $R^2$ is 100%; whereas if it does not, $R^2$ amounts to 0%. So uncertainty is trivially measurable by solving $1 - R^2$.

On the other hand, in case of financial markets, uncertainty expresses how random external events affect financial trading and it is measurable in terms of fluctuations so that future values can be predicted on the basis of the previous values. For instance, if investors are skeptic about the possibility that a given company will perform well in the future because of an external shock, e.g. a penal lawsuit against, then uncertainty (i.e. risk) rises and the equity price of the company's shares decreases. For the lower is the risk, the higher is the price of an asset and vice versa. The fluctuation is calculated as the difference between the current market value of the stock and the average of its past values expressed in terms of variation.

Furthermore, when predicting the future values of real variables during recessions, e.g. consumer demand during an economic slump, uncertainty happens to play an important role. In this respect, the econometric model that conjoins theoretical (i.e. forecast error) and real uncertainty (fluctuations) is known as VAR (*Vector Autoregressive Model*). The latter allows economists to take into account the linear interdependencies between the independent variables that may affect the dependent variable that is forecasted. More specifically, the model conjectures that each variable that is part of the regression is dependent on its past values. In this way, it is then possible to estimate what factors affect a dependent variable in the long-run as the time-series of each independent variable are considered.

For instance, when the global recession hit the UK in 2009, the Bank of England clarified that uncertainty does affect economic output, yet *establishing that uncertainty 'causes' fluctuations in output in the United Kingdom is not straightforward* [In this sense] *an advantage of* [VAR] *is that it allows uncertainty and economic growth to introduce exogenous 'shocks' to the uncertainty equation* [and] *then* [to] *observe how that affects other variables within the system, such as output* (Haddow et al., 2013, p. 106). Accordingly, VAR is a resourceful tool because it allows economists to measure the effects of different independent variables on the dependent variable over time, taking also uncertainty into account. In fact, as economic slumps can ease increases of volatility in markets and make market forecasts likelier to be false, it might useful to acknowledge how uncertainty affects each independent variable under examination over time and how this affects the dependent variable along the whole time-series taken into consideration. Certainly, this might improve accuracy in predictions, yet VAR cannot say much on how possible future external shocks might affect the variable that is forecasted. That is, VAR cannot say much on how future possible fluctuations might affect output, but it can reveal what factors did affect output in the past. The problem is that the sources of shocks are not always strictly financial or economical, e.g. BREXIT. For there are many qualitative factors that economic models fails at identify and that often concern consumer behavior.

In fact, real uncertainty is a peculiar feature of the economic environment itself and it primarily impacts real activity, i.e. industries. In particular, increases in real uncertainty are often due to many factors such as lack of precise information among consumers (individual uncertainty), individuals' inability to predict the nearest future, adverse public policy and business initiatives that might deplete markets of resources. When forecasting future values, economists ought to consider how such factors might affect future outputs.

However, evidence from the recent economic stagnation across EU shows that poorly performing forecasts are often due to the fact that models often ignore that, during weak economic times, individuals are often more prudent in making their decisions because they attempt to avoid risky investments. In spite of the optimism of standard economic models, this creates market leakages that economists can hardly predict and quantify. In this sense, models often ignore the importance of microeconomic events.

Indeed, the level economic uncertainty is mostly visible on the microeconomic level especially during slumps, e.g. shops out of business. Yet its effects are often various and unpredictable as they can be either highly positive or extremely negative or both at once.

For example, during a deflationary recession, uncertainty could lead firms of a given industry to get rid of their inventories in order to reduce costs as consumers might be willing to buy firms' products at lower prices. This could relaunch investment and contain downsizing thanks to enhanced cost management. At the same time, the same deflationary slump could also thrust firms of other industries to pennilessness, provoking an increase of unemployment and a decrease in consumption. In either cases, the economy as a whole would be affected.

With respect to this, *literature has taken three approaches* [in order to identify the effects of uncertainty] *One approach relies on timing: that is, estimating the movements in output, hiring, and investment that follow jumps in uncertainty* […] *A second approach uses structural models calibrated from macro and micro moments to quantify the potential effect of uncertainty shocks* […] *A third approach exploits natural experiments like disasters, political coups, trade changes, or movements in energy and exchange rates* (Bloom, 2014, pp. 167–168). Nevertheless, considered that the effects of uncertainty can be either positive or negative or both at once, it can't be established *ex ante* whether agents will respond positively or negatively to shocks. All that can be remarked is that such factors, e.g. decrease in investment, pave the way to slumps. Yet the relationships amongst indicators, jumps in uncertainty and recessions might not hold deterministically.

If such contentions hold true, then it is easy to grasp why the overall complexity of both qualitative and quantitative microeconomic factors might have several implications that might impact economies as a whole. Indeed, when uncertainty is high, macroeconomic forecasts often turn out to be devious and complicated primarily because of the uncertainty in the business environment.

Upon this, it is possible to see the bond between theoretical and real uncertainty. It is enough to remind that different macroeconomic models may be equally efficacious in forecasting and still have different degrees of explanatory power nonetheless. In fact, when economists are to come up with good forecasts, they often end up combining different models as no singular regression fits the rough data as they are.

It follows that the epistemological problems of economic models lay in the fact that economists are to build bundled models that often ignore the internal structure of each of the models that are part of the bundle. Specifically, when economists consider *several models, each of which represents considerable thoughtful labor and has been used for forecasting, it is unlikely that the main differences among them are well described by ranking them according to their distance from the truth. More likely, they each represent a judicious compromise between parameter parsimony and honest representation of uncertainty* (Sims, 1988, p. 166). Furthermore, if different models are employed and combined into a single regression, this may lead to having to face the problems arising from their different forecast errors and relative forecast significance. This means that economists are to be ingenious and be ready to deal with many possible different anomalies at once, but at what cost?

From a theoretical point of view, the problem can be solved by making use of a statistical method called *ordinary least square method*. The latter allows to draw the fittest regression line to observed data from a population sample in order to fit the data to a given model.

From a pragmatic point of view, the problem is unfortunately thornier than anyone may ever think. In fact, despite statistics supplies economists with excellent means for data investigation, macroeconomic forecasting is not just a matter of making univocally true probabilistic statements concerning events that will take place in the future. The problems of macroeconomic forecasts lay in the absence of a commonly shared way of predicting events amongst economists.

For instance, different private and public agencies can forecast the output of a country and estimate different results. The latter are then released to the public that interprets them upon what they believe to be true. This means that agents assess the reliability of the forecasts themselves. Eventually, if agents are rational, they plan their future actions on the basis of the forecasts they consider the plausible. Yet is it actually possible to rationally rank the different projections of different models without committing any sharpshooting Texas fallacy?

Corder (2005) answers this question by showing that it is possible to denote different features of the forecasts made by governmental agencies. In particular, he observes that different techniques of forecasting may bring about different outcomes or be differently biased. For instance, this can be observed by looking at the forecasts of three US economic indicators (i.e. CPI, Unemployment rate and yield of Treasury bills) that are taken into account by OMB (Office of Management and Budget), CBO (Congressional Budget Office) and SSA (Social Security Administration) when they calculate and forecast the growth rate of the US GDP. Accordingly, Corder establishes three criteria to evaluate the performances of the forecasts of the three agencies, namely: forecast accuracy, forecast bias and forecast efficiency. To him, these criteria should then explain how the three agencies deal with forecast uncertainty.

In terms of forecast accuracy, evidence (the data of the period 1976–2008) suggests that CBO's and SSA's forecasts of the GDP growth rate are often more accountable than those of OMB because the latter agency is far too optimistic when carrying out forecasts. That is, whereas CBO's and SSA's forecasts of the value of the indicators are quite realistic, OMB always estimates unrealistic hikes of the value of the indicators because of an over-estimation-bias. On the other hand, none of the three agencies makes reliable predictions of future interest rates because they all model rather an assumed monetary policy than an actual one based on the information that FED makes available.

In terms of forecast bias, all the three agencies carry out highly biased long-term forecasts. For instance, whereas all agencies predict an increase of the GDP growth rate and a decrease of unemployment, evidence suggests that the opposite takes place.

Eventually, the efficiency of the forecasts often depends upon the reliability and the availability of the information that is used for forecasting. Evidence shows that, when forecasting, the three agencies often either make use of fragmentary information or use other agencies' outcomes in order to predict the future values of macroeconomic indicators.

This reveals that *persistent long-term bias and relative inefficiency of macroeconomic forecasts suggest agencies weigh or incorporate different types of* *information in different ways in their forecasts. In the short-run, these alternative weighting schemes do not bias forecasts, but, in the long-run, each of these weighting schemes introduces similar types of biases in forecasts* (Corder, 2005, pp. 68–69).

Epistemologically speaking, this problem concerns both deduction and causation. Indeed, like for quantum physicists, economists ought to employ approaches that avoid determinism and take up causal talking nonetheless. Nevertheless like in the example of the chess game mentioned above, each forecast depends only on the initial conditions that are considered. Hence, in order to forecast future outcomes, economists are to combine different models and choose arbitrarily different pieces of information that will support deduction. That is, when issuing long-term forecasts, economists attempt to construct reliable models that can effectively deal with the uncertainty deriving from the unpredictability of human behavior and from a multitude of destabilizing factors that are part of the economic environment. That is why each model is based on different assumptions that are subjectively chosen.

Practically speaking, the abovementioned problem regards assessing whether or not economists adjust information and models to the data they want to interpret. That is, the findings should confirm whether the formulated hypothesis is sound rather than being confirmed by theories. But is it really possible to assess the validity of a model in this way? And can economists really avoid any sharpshooting Texas fallacy?

Julian Reiss (2013) suggests that it is possible to assess the validity of a model only as long as a distinction between correlation and causation is marked. For him, the former indicates relations between variables, whilst the latter expresses tendencies.

As to correlation, Reiss notes that the statistical correlation amongst events proposed by a theory regards rather associating particular instances than writing general laws. For example, the so-called Phillips curve associates an increase of inflation with an increase in employment, yet evidence from stagflation in the 70s shows the opposite, i.e. high inflation and high unemployment. Accordingly, against the common belief that this paper criticizes, many instances from the economic history show that *first, most factors we regard as causes are not universally associated with their effects* […] *second, not all effects follow their causes* […] t*hird, causes may act at a temporal or spatial distance* […] *fourth, constant conjunction, temporal priority of the cause and contiguity are insufficient for causation* (Reiss, 2013, pp. 91–92). For these reasons, identifying a correlation between particular instances is a preliminary condition to put forward general laws.

As to causation, Reiss proposes that causal relations in economics express rather general tendencies than general laws. That is, it must be possible to mark off the difference between the final statement of a theory and the general tendencies it describes. This means that the latter are general descriptions of the particular events that are identified by a theory and describe general tendencies that do not take into account any possible disturbing factor. Indeed, if anomalies are taken into account, the laws turn out to have exceptions. For example, an acknowledged general tendency is the increase of inflation that follows the implementation of an expansionary monetary policy. If it is assumed that wages grow together with inflation, then it is obvious to suppose that firms will be willing to hire more people as their business will generate more revenues of higher value. Yet is this always the case? Unfortunately, it is not. For instance, expansionary monetary policies that attempt to sustain the economy for too long may cause a rise of productivity and a stagnation of wages that in turn can cause high employment and inflation close to zero or negative, e.g. what happened to the US economy during the years 2009–2015 as firms have easy access to cheap borrowing and do not need to increase the prices of their services or products.

Hence, following Reiss, we acknowledge that any evaluation of the validity of an economic theory should be based on the distinction amongst the general hypothesis of the theory, the general laws it puts forward and the particular instances it considers. Besides, following Reiss, it is also possible to make sense of why in economics possible interpretations of the same facts are possible.

In order to clarify this point, we shall go back to our chess game once more. If we now posit that the outcome of the game cannot be deterministically deduced from the laws of the game, that our interferences with the player are not the only disturbing factor and that other disturbing factors, e.g. players' physical conditions, are unknown to us, then it is easily elicited why different explanations of the game are possible and why they all possibly make sense in different degrees. Indeed, any explanation of the outcome of the game and any future prediction regarding the outcome will depend univocally on the assumptions made by the scientist who attempts to make sense of the chess game. Eventually, it will be the public who establishes whether or not any explanation sounds reasonable and objective.

In this sense, each theory has their own general statements, puts forward their general laws and identifies the particular instances that are pertinent to explanations of the theory. Yet objectivity is limited to the how convincing a theory is for the public who evaluates it.

This leaves room for discussing of how our interaction with the system and other disturbing factors might successively affect outcomes. This will be the main focus of the next section.

## 4. On the importance of causation

The problems highlighted so far regard the credibility of a theory that explains given observables. Our discussion has led us to grasp the importance of the fact that different explanations of given observables can reach similar conclusions, even though they identify different causal chains. Upon this, the present section deals with the biggest issue that follows from the previous discussion, namely the identification problem in QM and in economics.

Now, as remarked above, the philosophical consequences of QM have forced scientists to abandon the Modern deterministic picture of the universe. Besides, the puzzles of the realm of particles have pushed physicists to admit that different explanations of the same facts are possible because of our ignorance concerning the causal chains that bring about given observables.

This very fact has led to the rise of two schools of thought in QM: the so-called Copenhagen interpretation of QM that commonly refers to Nils Bohr and Werner Heisenberg; and their rivals, namely Albert Einstein, Erwin Schrödinger and their followers.

The Copenhagen interpretation of QM stands upon two irrevocable tenets: first, the acknowledgement of the fact that the states of quantum systems change with time in agreement with Schrödinger's wave-function; second, their claim that it is our interaction with the system that determines the outcome of measurements. Upon this, they claim that particles are entangled. That is, if it is assumed that particles are somehow interlinked, then the measurement of the status of a given particle provides us with enough information concerning the status of another particle. Hence, they claim that QM is a complete theory.

By contrast, their rivals claim that the tenets of the Copenhagen interpretation are inconsistent because they suppose that actions-at-distance are possible. Thus, they claim that QM is not complete because the Copenhagen interpretation of QM identifies effects only, but does not explain how the entanglement happens to take place. That is, the Copenhagen interpretation just ignores the possible existence of hidden causal chains that might explain the quantum entanglement.

For the sake of clarity, we can take advantage of our chess game in order to make sense of the interpretation dilemma.

Recall the variant of the chess game presented above where we supposed that our interaction with players affects the outcome of the game. Then, imagine that we know nothing about the players and that we just suppose that usually the most skilled player is likelier to win the game. Hence, when we begin observing the game we acknowledge that player 1 leads the game. Nevertheless, at the end of our observation we conclude that player 2 is eventually the winner of the game. How is it possible? Clearly, something must have happened since we began observing the game: either our presence influenced the game or what we observed happened because of some underlying reasons that are unknown to us.

If we were to follow the line of reasoning of the Copenhagen interpretation, we'd simply conclude that a change in outcomes took place. Hence, we would claim that our interaction with the game caused a change in the course of events. That is, the initial and the final state of the game are somehow entangled.

By contrast, if we were to follow the line of reasoning of the rivals of the Copenhagen school, we would rather claim that it is not enough to claim that we have observed a change in the course of events of the game. For this amounts to ignoring that there might be some underlying reasons that explain why we observe a change in the course of events of the game. That is, our knowledge of the causal chain is incomplete.

This dilemma can be best explained by means of Schrödinger's famous thought experiment known as Schrödinger's cat. Its main tenet is that, given quantum superposition,[6] we shall admit of our ignorance about the fate of the CAT inside the box. That is, we can't know whether the cat is alive or dead unless we open the box. The conclusion to be drawn is that it is our interaction with the system itself that determines whether the cat is alive or dead because, before we do so, the cat is both alive and dead due to superposition. In simple words, this means that our inability to predict whether or not the cat survives follows from our inability to predict ex ante which event will eventually take place and when it takes place.

Furthermore, the dilemma shifts us back to the beginning of this paper and to the fact that common wisdom often sticks to the Newtonian picture of the Universe. Interestingly, the problems that follow from quantum superposition and quantum entanglement regard the dilemma of whether the principle of locality in tenable in the case of QM.

Modern Physics supposes that objects have defined positions in the universe that can be elicited by means of the information available (momentum and position above all). Besides, Modern Physics stands upon the idea that every observed effect is always anteceded by a cause. Conversely, QM denies the two Modern tenets because of Heisenberg's principle and because of the problems that follow from quantum entanglement. In respect to this, the so-called EPR paper (Einstein, Podolsky and Rosen, 1935) points out that the conflicts between QM and Modern Physics result into methodological paradoxes.

[6] Quantum superposition supposes that before we perform any measurement, the system is any possible state.

For example, imagine particle A, an electron, and particle B, the positron of particle A. If we assume that the system is in any state before measurement because of quantum superposition, then we also claim that we do not know anything about particle A and B until we perform an observation. Yet when we interact with the system and we perform a measurement on particle A, we observe something strange: if we observe particle A rotating with speed 1, then we observe particle B rotating with spin –1. Why? Because quantum entanglement demands that a change in the electron (with negative charge) will result in a change in the positron (its counterpart with positive change). This phenomenon leaves an important question without answers namely: if the change of spin of particle B is instantaneous, doesn't this amount to say that the transfer of energy takes place faster than light? If this were the case, then the change of speed would violate the theory of relativity.

The EPR paper points precisely to this problem and claims that QM is incomplete exactly because it cannot explain how it is possible that the "information" between the two particles seems to travel faster than light. That is, QM identifies incomplete causal chains. On this basis the rivals of the Copenhagen interpretation suggest that QM physicists should suppose that there might be hidden local variables that are unknown and that complete the causal chains with the missing elements for the sake of locality. Is this really tenable, though?

Bell (1964) attempts to solve the paradox by supposing that if locality were really to apply to the quantum realm, then it would be possible to express the causal relationship between the electron and the positron in terms of probabilistic correlations. Hence, he proposes that if hidden variables are admissible in QM, then it must be possible to devise experiments that confirm the assumed probabilistic correlation. Yet assuming locality leads to experimental results that contradict the assumed probabilistic correlation. This sweeps locality away: actions-at-distance in the quantum realm are possible simply because locality pertains only to the macroscopic world.

It follows that QM leads to accept that the laws that apply to the behavior of macroscopic systems simply do not hold in the quantum realm. Therefore, postulating ontologies that encompass hidden parameters that complete missing causal chains leads to empirical falsification.

Upon this, the puzzles deriving from quantum experiments and QM discoveries have pushed quantum theoretical physicists to devise a commonly accept model that might explain how and why the interactions between particles take place. Today this branch of QM is known as quantum field theory and its main paradigm is best known as the Standard Model. The latter postulates that there might exist two main classes of elementary particles, i.e. the fermions and the bosons, that might explain some of the secrets of matter.

At the same time, the rise of the Standard Model pushed experimental physicists to devise experiments that check the consistency of the Model by recording the outcomes of the interactions between bundles of particles at very high speeds. In particular, these experiments are carried out for long periods until particular dominant features resulting from the collisions can be identified. Eventually, it is the statistical significance of results that measures how confidently scientists can claim to have found what the model predicts. For instance, when Higgs' boson was discovered in 2012 after many years of experiments, CERN's physicists claimed that data displayed statistically significant results concerning the fact that the identified particle had the features of the so-called Higgs' boson.

Thus, the quantum field theory marks off an interesting interdependence between models and experiments. For experiments are now meant to replicate the abstract conditions of models in the real world for potentially infinite times. Within this framework, the resulting statistics tell us whether and how frequently the hypothesis falling from the models is repeatedly observed.

With respect to this, Van Fraassen (1980) proposes that contemporary science proceeds following what he calls "a modal frequency interpretation" of probability theory. The latter expresses how empirical data and the expectation values of the theories come to converge. In particular, he proposes that it must be possible to distinguish between ideal experiments that can be carried out infinitely times and real experiments that can be carried out only for a finite number of times. Hence, he establishes that sound theories are those that display an overall fit between the frequencies displayed by real experiments and the probabilities that are predicted by the ideal experiments of models.

In order to grasp the importance of Van Fraassen's suggestion, we can take advantage of our chess game once more and assume that we're now aware of the fact that our interaction with the game results into a change in the course of events. If our knowledge is accurate, then we can cause changes in outcomes simply by changing the initial conditions. Imagine that we now have some initial information about the players, e.g. we know whether the players have sight-related problems, and we also know how quickly they respond to a change in external stimuli. It follows that if the information we hold is accurate, then we can predict how likely specific changes in outcomes follow specific changes in initial conditions.

For instance, if we know that players have given sight-related problems that might lead them to lose their concentration at some point of the game, then we can predict that a change in outcomes might follow from a change in the intensity of light in the room caused by us. This means that we can imagine many ideal situations where the intensity of the light is changed and predict all the possible outcomes following from the changes probabilistically. Eventually, the measured frequencies confirm or disconfirm the predicted outcomes.

Accordingly, imagine that we know that player 1 is more astigmatic than player 2. Then, assume also assume also that at the time we run the observation player 1 is leading the game. If we know exactly how astigmatic both players are, then we know that a given change of the intensity of the light in the room might affect both players in different ways. This very fact provides us with enough information to predict the possible outcomes. In fact, we might predict that if the intensity of light changes by a given particular amount, then player 2 has higher changes to win the game because he will be more focused than player 1 with that amount of light. Eventually, if the frequencies of the predicted outcomes derived from experiments converge to the prediction of the model of the game, then the findings support the model and the model is can be considered accurate.

Now, although this example might simplify the understanding of QM, this does not mean that the example is not flawed. Indeed, the main assumption underlying the example is that players will respond to external stimuli like particles. This amounts to claim that players lack any cognitive ability. That is, if it holds for physics, it cannot hold for economics.

Nevertheless, the flaw in example allows to grasp where the identification problem lies in the economic theory. Indeed, if we now assume that players are fully rational and they respond to external stimuli in a peculiar different way than particles, then we can identify many causality-related problems in the economic models. The thorniest amongst this problem regards the inquiry on whether or not agents can be considered rational.

On the one hand, Neoclassical Economics puts a strong emphasis on individual choices. Adherents to this current firmly believe that individuals are utility-maximizers who're able to create rational expectations on the basis of the information available to them and plan their future actions accordingly. In particular, Neoclassical economists usually claim that individuals are rational utility-maximizers because they're able to adapt to changes in the economic environment in order to maximize their future gains (Roncaglia, 2003).

On the other hand, some economists (see Krugman, 2009; Stiglitz, 2010) complained that the causes of 2008 subprime mortgage crisis casts several doubts on the soundness of the Neoclassical paradigm. For the events of 2008 revealed that market participants might not always seek utility rationally and that the actual effects of moral hazards might be hardly quantified. In the same way, other academics (see Sapelli, 2008) express strong distrust towards Neoclassical

Economics by pointing out that its models are often inaccurate representations of the reality that bear imprecise predictions because they do not take factors else than variables into account, e.g. lack of reforms or inefficient macroeconomic mixes that tackle the expectations of agents.

Although establishing whether or not criticism of Neoclassical economics is sound goes beyond the scope of this paper, it is worth to note that critics might have a point when they argue that market participants might not always be fully rational. Accordingly, this paper suggests that a legitimation of the concept of bounded rationality needs be carried out. For there might be a discrepancy between the way economists think individuals grasp the informational available to them and the way individuals do actually grasp the information that is available to them.

In fact, if economists do acknowledge the constraints in individual choices deriving from the lack of precise information, the impossibility of individual omniscience and the shortages of time (see Simon, 1982), then it should be possible to establish *till what extent* an individual action can be considered rational. That is, it should then be possible to define formally how and by how much the information available to individuals lead them to take an action. Hence, I suggest the relationship between the concept of bounded rationality and the folk concept of intentionality be investigated. The reasons behind my suggestion can be exemplified by the problematic consequences of the *representative agent view* that follow from Lucas Critique.

As mentioned above, Lucas (1976) claims that the predictive power of econometric models is undermined by the fact that they do not take into account that once governments introduce new policies, rational individuals will adjust their behavior to the changes introduced by the new policy (as Neoclassical Economics dictates!). Yet, as Hoover (2001) points out, Lucas Critique demands macroeconomics to have micro-foundations that rely upon the assumption that individual responses to the changes in policies will be homogeneous as they will reflect the rational actions of a typical utility-maximizers that partakes in market activities. Plainly, this seems impossible as, following this line of reasoning, also herd behaviors would be perfectly homogeneous and so there would be no room for discrete choices. Hence, Hoover suggests that endorsing the possibility of micro-foundations amounts to admit that the macro aggregates are nothing, but the outcome of the rational behavior of a representative agent. Clearly, this would bring about paradoxical consequences that clash with the General Equilibrium Theory (see Woodford, 2003). From the point of view of this paper, Hoover's claim is agreeable and, furthermore, gives more sparks on why a clarification of the concept of bounded rationality is so important. Indeed, if we agree with

Hoover that it is impossible to speak of a single representative agent, then we implicitly admit that we should explain how inside-industries-trends are possible.

This is where the folk concept of intentionality kicks in. In fact, the latter allows to interpret behavior upon an analysis of how individuals formulate the assumptions that push them to act. That is, intentionality might let us find a link between individuals' understanding of the available information and the way they set goals to achieve on the basis of that information. With respect to this, philosophers of mind made huge efforts through the last fifty years in order to find a formal definition of the folk concept of intentionality. Eventually, they ended up acknowledging that the latter is somehow related to the concepts of purpose and the concept of consciousness.

As per the concept of purpose, Anscombe (1963) defines intentional actions as acting for a reason. In her account, if one's able to explain why he's taking a given action, then he's acting intentionally. In further developments, Dennett (1981) defines intentionality in terms of 'direction of fit' towards a goal. More specifically, he distinguishes between three kinds of stances: the physical stance (that pertains to physics and chemistry), where the future states of a given system can be predicted by knowing the laws governing it and its current state; the design stance (that pertains to biology and computers) that is a teleological stance such that once we are aware of the features of a given system, we can predict its future status; the intentional stance (that pertains to minds and software), where future actions are taken on the current status of the system. According to Dennett, the minds of rational individuals are intentional systems because individuals are able to formulate beliefs by processing the information available to them and act consequently. This is due mainly to the design and the physical properties of brains. In this sense, behavior is predictable as long as one is aware of the conditions that makes a given behavior possible.

As per the concept of consciousness, Searle (1983) argues that *intentional* mental states can either refer to the content of a proposition, e.g. *the king of France is bald*, or refer to the extension of that proposition, e.g. the proposition *the king of France is bald* turns out to be true because there's no King of France. Hence, Searle argues that Intentional mental states must be satisfiable. This means that all mental states, e.g. beliefs or hopes or perceptions, are fulfilled as long as the necessary conditions of satisfactions are fulfilled. Therefore, an intentional action is carried in order to accomplish a goal that is expressed by the content of a proposition that must be satisfied.

Despite the two accounts (Searle's and Dennett's) might seem to claim the same, they actually do not. Consider the following belief: 'if there's an apple in the fridge, I will eat it'. Under the first interpretation, I believe there's an apple in the fridge and I express my willingness to eat it; under the second interpretation, I will eat the apple that is in the fridge as long as my belief that an apple is in the fridge is satisfied by the state of affairs, i.e. the apple is in the fridge.

Interestingly, recent developments in philosophy, questioned the claim that intentionality can solely be defined in terms of propositional attitude. This seems to be quite evident once outcomes of future actions, the risk they entail and their consequences are considered. Indeed, the abovementioned definitions begin creaking once they are tested against folks' perception on intentionality.

Malle and Knobe (1997) mark off the difference between intentional and unintentional actions by carrying out an experiment in which random individuals are asked to define an intentional action. Incredibly, most of the surveyees respond that an action is intentional as long as someone is able to carry out the course of action they believe they will.

Upon this, Knobe (2003a) benchmarks the two abovementioned accounts of intentionality by devising a survey that relates skills to achievements and morality. Accordingly, he constructs a vignette-case whose main character, Jake, is in need of money and can gain the amount of money he needs either by participating in a rifle contest or by killing an old rich aunt. Each case is divided into two subcases where two assumptions are dominant: either Jake is a skilled shooter or he's not.

In the rifle contest vignette case, Jake is to shoot a bull in the eye to get the money he needs. In both subcases, Jake successfully accomplishes his goal. When 37 surveyees are asked whether Jake acted intentionally, their answer is that he acted intentionally in the subcase where Jake's assumed to be a skilled shooter, while he did not in the subcase where Jake is not assumed to be a skilled shooter.

In the old rich aunt murder case, Jake gets the heritage of his aunt only if he manages to kill her while she's in her house by shooting through the window. In both subcases, Jake successfully accomplishes his goal. When 37 surveyees are asked whether Jake acted intentionally, their answer is that he did so regardless of whether he's a skilled shooter.

Thus, QED, folks relate intentionality to the ability to accomplish a goal (i.e., skill). Besides, the data show that when folks judge whether an action is intentional, they put strong emphasis on the morality of the action that is judged. This is evident in the old rich aunt murder case: folks consider the action intentional in either subcases.

In another experiment, Knobe (2003b) puts even more emphasis on morality by constructing two vignette-cases that stress the importance of the side-effects of the goals of the action. Each vignette-case presents a given side-effect deriving from the action that is performed. The vignette-cases recount the story of the vice-president of a company who wants to implement a program that aims at increasing the profits of the business: in the first case, the vice-president implements the program, profits are increased and the environment is helped (first side-effect); in the second case, the vice-president implements the program, profits are increased, but the environment is harmed. Interestingly, when 78 surveyees are asked whether the vice-president acted intentionally, they answer that he did in the second case, while he did not in the first case.

Clearly, this shows that there might be a strong discrepancy between how philosophers formally define intentionality and how folks perceive it. Specifically, while philosophers put more emphasis solely on propositional attitudes and purposes, folks attach more importance to skill and morality.

In this regard, it is important to note two important facts: first, Dennett's and Searle's accounts of intentionality might only work as normative theories while interpreting Knobe's vignette-cases; second, the vignette-cases depict imaginary events where the determinism of the two accounts works with no hindrances. In concrete cases, the outcomes of events might be determined by external factors as already proposed earlier. For instance, the company's program does not harm the environment as long as no technical problem comes up and the money of Jake's aunt are inherited as long as no one sees him shooting. That is, in real cases, we might overlook that the link between the goal that is pursued and the side-effects it will bring holds as far as conditions are favorable.

Importantly, Kahneman and Tversky (1979) suggest that agents are rather risk takers than parsimonious and cautious in their choices. Indeed, their Prospect Theory undermines the tenets of the so-called Expected Utility Theory (one of the cornerstones of Neoclassical Economics). According to EUT, agents are utility-maximizers who rationally seek prospects such that:

$$EV = \sum_{i=1}^{n} (p_i) u(x_i);$$

where $EV$ is the overall expected utility; $x_i = x_1 \dots x_n$ are the potential outcomes; $p_i$ are the respective probability of each outcome; $v$ is a value that assigns probabilities to the outcomes. Within this framework, individuals are assumed to be risk adverse and to seek for gains as long as their absolute wealth is increased and never undermined.

Conversely, Kanheman and Tversky (1979; 1986) devise experiments whose findings question the validity of EUT. Their main findings show that:

1. Agents overweight certain outcomes.
   To show this, the two psychologists devise experiments where agents are required to choose between certain gains and gains with given probabilities. For instance, when asked to choose between earning, say, €100 with certainty and winning, say, €400 with probability 0.80, most of agents choose the first option. Hence, agents prefer to make choices with certain outcomes.
2. Agents underweight small probabilities.
   To show, the two psychologists devise experiments where agents are required to choose between a certain loss and a loss with a given probability. For instance, when asked to choose between loosing €100 with certainty and loosing €400 with probability 0.80, most of agents choose the second option. Hence, agents are risk-takers rather than risk adverse.
3. Most of the agents chooses intuitively without foreseeing actual outcomes.
   To show this, the two psychologists devise experiments where agents are asked to partake in a two-stage fictitious game: in the first stage, they're asked to choose between winning nothing with probability 0.75 and access to the second stage with probability 0.25; in the second stage, they're asked to choose between winning, say, €500 with probability 0.80 or win, say, €100 with certainty. Most of the agents chooses the second option in either cases. Hence, agents do not foresee that the probability of the actual gain is only 0.25.
4. Risk- and certainty-seeking regard wealth as well.
   To show this, the two psychologists devise experiments where agents are asked to choose between a certain gain/loss of, say, €50 and a gain/loss of, say, €100 with probability 0.50. Either values will be either deducted or added to their wealth. Consistently with the previous findings, most of the agents chooses the gain €50 with certainty and to lose €100 with probability 0.50.

Hence, the two psychologists conclude that the value of two prospects $x$ and $y$ with distinct probabilities $p$ and $q$ (respectively) can be quantified by the following statements:

$$V = \begin{cases} if\ p+q<1\, or\ x \geq 0 \geq y\ or\ x \leq y\ then\ V(x,p;y,q) = \pi(p)v(x) + \pi(q)v(y) \\ if\ p+q=1, x>0>y\ or\ x<0<y\ then\ V(x,p;y,q) = v(y) - \pi(p)[v(x) - v(y)] \end{cases}$$

where $\pi$ is a function that describes the fact that agents overweight certain outcomes and underweight outcomes with small probabilities. In this sense, prospects are weighed by the agents upon their potential gains or losses, but why is it so?

Kahneman (2003) argues that agents face choices by 'activating' two interdependent systems: the system of intuition and the system of reasoning. The first is activated once one responds to external stimuli and processes that content automatically, while the second is activated once contents are to be cognitively represented and analyzed. Importantly, the latter corrects the mistakes of the former. In this sense, agents' framing bias is always due to the fact that they intuitively opt for the dominant choice, i.e. the one that bears the highest value. That is, agents respond to external stimuli and choice-situations by activating mostly the first system. Accordingly, as evidence shows, despite all agents are assumed to be able to reason, system 2 can correct the mistakes of system 1, but cannot prevent intuitive wrong choices. This explains why agents misweigh prospects: they simply do not analyze the informational available properly. Indeed, against Bernoulli's idea that agents will always make the most reasonable choice, Kahneman and Lovallo (1993) show that agents often commit 'narrow framing' biases, namely: they are unable to process correctly all the relevant information that is available to them and, accordingly, often overlook and opportunities.

Notably, McFadden and Train (1996) suggest that perceptions might lead actors to make wrong choices because they substantially affect memory. In the same way, McFadden (2013) shows that the neoclassical account of indirect utility that considers prices, quantity of goods, the attributes of consumer experience and the primitive character of consumers may fail once other factors are considered. More specifically, against the *representative agent view*, he argues that consumer experience and the primitive features of the typical consumers might be influenced by memory and also by external factors. Hence, he points out that these factors might affect preferences in the long-run and justifies his claim by pointing out that: first, current researches in psychology show that memory is selective in recording rather successes or losses than normal activities, e.g. one can better remember the day he found €100 on the sidewalk on the way back home from work than any day he walked back home; second, current researches in anthropology show that group dynamics affect how people make choices, i.e. follower effect are concrete facts that pertain to any society. In this sense, choices cannot be assumed to deterministically rational like in the Neoclassical frames because many other factors affect the way agents deal with risks and future choices. In formal terms, this means that models of choice are not made up only of linear relations.

Once this is acknowledged, it is then reasonable to question also whether agents assess probabilities in a Bayesian a priori way on the basis of individual criteria. That is, once uncertainty due to external factors is acknowledged, does it make sense to claim that probabilities can be assessed only from a subjective perspective?

Popper (1956) proposes that quantum mechanics is to be based on propensity theory of probability because, provided that physicists test theories by performing controlled experiments, physical conditions are the enablers for the experiments to be successful so that if the same conditions are replicated, the same experiment will most likely yield similar results. In this sense, propensity theory is to be thought as *the tendency of a given type of physical situation to yield an outcome of a certain kind, or to yield a long run relative frequency of such an outcome*.[7]

Now, let us reconsider Popper's suggestion within the framework of economics. Following his suggestion, we would acknowledge that a variation in initial conditions brings about consequential outcomes. If this were the case in economics, then agents would uniformly react to a change in initial conditions, i.e. a change in the information flow. Yet this would amount to say that all agents process the information available to them correctly and uniformly. Interestingly, experience contradicts this tenet. Indeed, as Smith suggests, *in many experimental markets, poorly informed, error-prone, and uncomprehending human agents interact through the trading rules to produce social algorithms which demonstrably approximate the wealth maximizing outcomes traditionally thought to require complete information and cognitively rational actors […] when people have more information they can identify more self-interested outcomes than Nash (and competitive) equilibria, and use punishing strategies in an attempt to achieve them, which delays reaching equilibrium* (Smith, 1994; p. 340). Besides, experimental evidence displays also that agents do not come to have common expectations upon common knowledge, e.g. public announcement, but via learning experiences.

This posits that the propensity theory might be adequate for economics as long as it is acknowledged that external factors have objective probabilities that affect agents in different ways. That is, agents might evaluate the external stimuli discretely. This explains why discrete choices and preferences become hard to identify for economists, while trends are easy to identify.

Nevertheless, if agents are risk-takers and they might process the available information in different ways, then do agents consider also the side-effects, i.e. the consequences of their actions, discretely? Unfortunately, any answer to this question takes us to dilemma.

[7] http://plato.stanford.edu/entries/probability-interpret/#ProInt

If all agents accept risk and process the information available to them uniformly and correctly, then they are aware of the consequences of the actions they perform. Thus, it makes sense to claim that the so-called *ceteris paribus* clause employed in economic models holds. Yet, as remarked by Smith, this is not case.

By contrast, if agents accept risk and do not process the information available to them correctly and uniformly, then they might not be aware of the consequences of their actions simply because their individual propositional attitudes might rely upon incomplete assumptions. Hence, agents' actions might be based upon defeasible reasoning and it would not make too much sense to claim that the *ceteris paribus* clause holds true. For new external conditions and other hidden variables might lead them to rethink their choices.

The dilemma is exemplified by the case of the Greek crisis. Allegedly, the spending review recently carried out by the Greek government was caused by the fact that many German retirees decided to invest their savings in Greek 10-year bonds as they sought risk in order to gain big returns. Then, once Greece risked of defaulting, the German retirees refused to sell the Greek bonds to the European Central Bank for reduced asset prices. The question to be answered is: did German retirees act intentionally? If we were to stick to folks' perception of risk, we would say that they did. Nevertheless, claiming that they acted intentionally amounts to say that they also accepted the investment risk and correctly processed the information about Greece they had. Either ways, it would not make too much sense to claim that they did act intentionally within a propositional framework.

Buchanan and Vanberg (1991) argue that markets can be defined as creative processes exactly because poorly informed agents that are motivated by resource allocations seek opportunities. Yet, contra Neo Classical theory, they claim that while markets function as nonteological processes, whilst it is scientists who interpret them that identify the telos in the market under investigation. That is, agents can't be defined as omniscient market participants because the subjective perspective of the explanation pertains to economists rather than to agents.

It follows that the fact that agents might be poorly informed implies that the reliability of any prediction depends univocally on the explanatory power of the model that is employed to describe the events that take place in the economy. Yet it is worth to note that economic models overall rely on assumptions about the rationality of agents.

Remarkably, we can then identify two problems: first, as economic models often rely on folk psychology, then they also rely on propositional logic, which is deterministic deterministic by definition; second, if markets function as nontelelogical processes, then assuming that agents are rational in a folk psychological glance implies that agents make choices deterministically. As shown above, this is not always the case.

Recall Schrödinger's cat thought-experiment. Lambertini (2013) argues that economics and QM are similar because in either sciences observers can't predict ex ante what state of affairs they will observe. As mentioned earlier, the peculiarity of Schrödinger's cat paradox is that it highlights that performing an observation to check whether the cat is alive or dead amounts to make the world collapse in one of the two possible states. In regard to this, Lambertini claims that the indeterminacy highlighted by the paradox can be thought of as a 2x2 noncooperative symmetric game with mixed strategies, but with information, which turns into a particular pure strategy game.

To see what Lambertini means, we can recall our chess game. The latter is a mixed strategy game where players assign weights to the possible moves they can make upon the moves of their adversary until the game collapses into a particular pure strategy game where the payoffs are only two: either victory or defeat.

Nevertheless despite the argument might seem sound, it contains a flaw. Indeed, it holds as long as a close world assumption is employed. That is, the player can only either win or lose the game as well as the cat can only be either dead or alive. This means that any intermediate state is excluded by definition.

While this holds true in the case of QM because physicists can run controlled experiments where changes in outcomes can be estimated upon changes in initial conditions, this does not hold in economics. In fact, if it is assumed that overall changes in the information flow affects the way people make discrete choices, this means that economists ought to take intermediate states into account when forecasting future economic outcomes. That is, particular outcomes might obtain with changing intensities.

For example, the implementation of the quantitative easing program in the EU was supposed to raise expectations and improve the economic conditions of the EU members. Yet the policy did not succeed at improving the economic outlook because of external factors, namely because of weak political leadership within the EU and lack of reforms that would reduce unemployment and foster economic integration. More specifically, while the policy successfully stabilized financial markets and lowered the yields of the bonds of many troubled states like Italy and Spain, it did not boost consumption and it did not affect inflation too much. That is, the policy raised the expectations of financial markets and did not entangle those of consumers. The possible reasons behind these events are a multitude and they might all affect agents somehow. Some reasons might be: political instability within the EU, consumers' growing distrust towards the financial system and the fears deriving from the escalation of tension with Russia. Hence, the information communicated to people by social and traditional media might have an impact on people especially now that people distrust politics and believe anything they read in the internet to be true. The suggestion of this paper is that economists should take this very fact into account.

Conversely, contemporary macroeconomics tries to explain the effects of monetary shocks by constructing models that take both micro- and macro-factors into consideration. Most notably, the widely employed DSGE model encompasses preference of agents, the productive capacity of agents and the institutional framework that regulates the economy. In this way, economists can construct very abstract models of the economy and predict what changes would follow from monetary or technology shocks. Romer (2016) complains that the economic models based on the DSGE model contain what he calls facts with unknown truth values. For him, these facts are employed in models only for the sake of deductions as models often relate facts using imaginary forces that do not reflect any actual state of affairs. Hence, he argues that the identification problem in economics concerns exactly the fact that economic models abstract too much from the reality. That is, they evaluate the implications of policies without taking into consideration actual factors such as actual agents' preferences, actual technology shocks and the actual implications of given policies.

In this sense, contemporary macroeconomic models posit the existence of hidden variables. Yet the identification problem in economics seem to be thornier than in the case of QM as models express relationships between facts that can't be verified empirically. Upon this, the suggestion of this paper is that economic models are incomplete because they do not identify all the relevant causal links. Therefore, this paper suggests that economics should focus more on understanding how agents choose and what influences their choices.

## 5. What's on the way

The question that this paper leaves open to further discussion regards whether the apriori approach of economics is tenable or not.

The previous discussion showed that while physicists managed to deal with the uncertainty deriving the puzzles of QM by taking up a strong empirical approach, economists rather followed the opposite path and constrained their field with the rigidity of the deduction of mathematical formalism. Yet this paper has shown that the determinism of deduction of formalism together with the determinism of propositional attitudes leads to several paradoxical conclusions that have been highlighted so far. Accordingly, it seems that the apriori approach of economic models shall be refined.

Thus, the main suggestion of this paper is that economists ought to investigate more how agents make choices and how their process the information that is available to them. In this way, new findings might reveal how agents react to change of information and whether agents foresee the consequences of their choices. In this regard, this paper suggests that economists should attempt to understand how qualitative and quantitative factors affect agents' choices together. In particular, it seems that such a combination might reveal what else than incentives causes agents to act. In this sense, it seems that lab experiments and further investigation of decision theory might help economics to improve its methods. This would certainly improve the predictive power of economics.

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