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Abstract: Many economists working within the framework of behavioral economics (BE) label the conventional way of modeling as *unrealistic*, and consider their own approach as *more realistic* than the standard practice. However, a *criterion* for realism is lacking in behavioral economics literature. This paper offers a simple criterion for predicating realism to economic models, and provides an illustration of such criterion at work on a particular BE model.

Keywords: realism, behavioral economics, philosophy of economics, economic models.

Introduction

The debate around *realism*[1] in economic modeling is 'perhaps the hottest topic in economic methodology' (Lehtinen 2002, p.1). One of the latest expressions of this discussion comes from the field of cognitive approaches to economics. Many economists, who work within the framework of Behavioral Economics (BE), label the conventional way of modeling as *unrealistic*[2].In turn, what they consider *realistic* is their own approach (or at least *more realistic* than the standard practice). The

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following are some of the main epistemological claims made by behavioral economists which supposedly reinforce their pretension that BE models are more adequate than their conventional rivals (all the quotations are taken from Camerer and Loewenstein, 2004).

1) 'Loss aversion is more realistic than the standard continuous, concave, utility function over wealth, as demonstrated by hundreds of experiments.' (p.4)
2) 'Behavioral economics increases the explanatory power of economics by providing it with more realistic psychological foundations.' (p.3)
3) 'At the core of behavioral economics is the conviction that increasing the realism of psychological underpinnings of economic analysis will improve the field of economics ....making better predictions of the field phenomena...' (p. 3)

These claims establish a strong connection between the *realism* of BE-models and their *explanatory* and *predictive* capabilities. This could be puzzling for those economists influenced by Friedman's methodological views, in which a link between *unrealism* and *predictive* capacities is suggested – the famous 'F-twist'. But unluckily a cogent criterion for *realism* is lacking in the Behavioral Economics literature. In the rest of the paper a criterion for predicating realism for economic models is proposed.

As an illustration, the paper offers an analysis of the criterion at work on a particular BE-model, presented in *Myopic Loss Aversion and The Equity Premium Puzzle* (Benartzi and Thaler, 1995; in what follows, *MLA*) and explains in what sense it might be considered *realistic* or *more realistic* than any of the conventional rival models. However, we that this particular analysis could be extended to other cases and might help to clarify in what sense many other BE-models might be called *realistic*.

Background: the debate about the realism of assumptions

Conventional economic models are plagued with unrealistic assumptions: agents endowed with perfect knowledge, unlimited computational capacities and perfect foresight, economies with no transaction costs, and so on. Many well trained students of economics and even some specialists wonder about how such theoretical constructions are related to real economies (Mäki, 2009b, Lehtinen, 2012). A vast

literature on economic models focuses on the problem of how unrealistic models can represent or control their intended targets (Sugden, 2000, 2008; Cartwright, 2007; Alexandrova, 2008). Indeed, for many heterodox economists, the unrealism of assumptions compromises the connections of these models with real systems, making them suspicious of being a mere "intellectual game" lacking any relationship with the real world (Lehtinen, A., 2012). It is claimed then that the commitment of mainstream models with unrealistic assumptions is a hindrance that should be amended with *more realism*.

On the other hand, the well-known essay written by Milton Friedman (Friedman, 1953) on the methodology of economics is considered by most economists as the official stance in defense of unrealistic models, linking their *predictive* relevance to their commitment to *unrealistic* suppositions – the 'F-twist' discussed by Samuelson, Musgrave and many others:

> 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). (Friedman, 1953, p. 14).

Historically, the defense of unrealistic models posed by Friedman was largely successful. Their *motto* on theory appraisal ('don't pay attention to the accuracy of assumptions, but on accuracy of predictions') was consistent with mainstream modeling based on assumptions obviously 'wildly inaccurate' as 'descriptive representations of reality'. Behavioral Economics seems to accept the legacy of Herbert Simon (Simon, 1955), and *at the same time* the Friedman's criteria of prediction accuracy, two lines of thought that traditionally had been considered incompatible. [3]

Camerer (2014, p.1) is aware that BE models are challenging a much extended tradition:

> The behavioral economics approach I describe in this essay is a clear departure from the "as if" approach endorsed by Milton Friedman. His "F-twist" argument combines two criteria:
>
> 1. Theories should be judged by the accuracy of their predictions;

> 2. Theories should not be judged by the accuracy of their assumptions. The empirically-driven approach to behavioral economics agrees with criterion (1) and rejects criterion (2). In fact, criterion 2 is rejected because of the primacy of criterion 1, based on the belief that replacing unrealistic assumptions with more *psychologically realistic* ones should lead to better predictions. (*Our italics*)

Camerer does not make explicit in which of the many faces of 'realism' is he thinking. This could be controversial, because BE models, in fact, do use maximizing agents that seem to have computational capacities beyond the possibilities of 'real' human beings, in a sense that Simon (1955) rejected as a source of error in conventional models. Given this 'unrealism', it is not clear what definition of realism could be used to rationalize Camerer claims. Surely the concept of 'more psychologically realistic' assumptions needs to be commented. In order to fill this gap, a weaker (relative) notion of realism based on empirical (experimental) evidence provided by other disciplines is offered. It grounds economic models on psychological knowledge endowed with experimental support [4]. Here is the criterion proposed:

> Given two models, m1 and m2 which, ceteris paribus, differ in a set of causes C in order to explain a phenomenon F, it can be said that m1 is more realistic than m2 in reference to C when the way in which C is specified in m1 (but not in m2) is compatible with the most relevant and best available knowledge.

The proposed criterion expresses a very *modest* demand. It does not assert that a given model is *realistic* (or more realistic that another model) in general, because such pretension would presuppose evaluating all of the assumptions included in both models. What is rather stated is a *double relative notion*: a given model is considered realistic (or more realistic) *in relation* to another model *and regarding a* well-defined set of characteristics. Based on this criterion, it could be defended that *regarding the way in which an agent's preferences* are modeled, a BE-model like MLA is *more realistic* than its conventional alternative models. This assertion is supported by the claim that cognitive psychology is the relevant discipline for providing scientific evidence about how individual preference rankings are formed. This is puzzling for mainstream modeling, in which economics is – and perhaps must be - a discipline 'separated' from psychology (Hausman, 1990). What is precisely the kind of 'scientific

evidence' cognitive psychology provides? Briefly stated, cognitive psychology *provides experimental evidence* of the causal factors behind decision making and behavioral economics *uses* some of those causal factors as inputs for economic models able to explain or predict some economic phenomena.

Behavioral economics' causal factors

BE models typically use concepts taken from the realm of psychology. Cognitive and experimental psychology have reached several important results showing that in many circumstances individuals take decisions using a repertory of dispositions and heuristics. Instances of dispositions are 'loss aversion', 'myopia', 'adaptation', 'saliency', 'focus illusion' and 'mental accounts'. Heuristics include the 'default rule', 'fifty – fifty' and '1/n rule' (Benartzi and Thaler, 2001, 2007).

A remarkable result of cognitive psychology is that when an individual faces a situation in which his wealth changes regarding a given state, their utility (measured in absolute values) is greater when the change represents losses than when it represents gains. This fact, reflecting the emotional impact produced by an alteration in a given level of wealth, is called *loss aversion*. It has been shown that loss aversion influences decisions bringing about several typical patterns of behavior. Two of them are worth mentioning: the so called 'endowment effect' and the different disposition to assume risks facing alternative descriptions of the same choice situation (i.e., framing effects).

The first pattern arises under conditions of exchange. Individuals who suffer loss aversion value an object more when they already have it than when it is not in their possession. This phenomenon, called the *endowment effect* (Thaler, 1980), has been demonstrated in many different experimental circumstances (Knetch, 1989; Kahneman et.al., 1991). Tversky and Kahneman (1981, 1986) showed that loss aversion is one of the causal factors generating reversion of preferences under uncertainty. This happens when a same choice situation is described alternatively in terms of losses and gains. Consider for instance the case of the so-called *Asian disease* (Kahneman, 2003a, pp. 1458).

Imagine that the United States is preparing for the outbreak of an unusual Asian disease, which is expected to kill 600 people. Two alternative programs to combat the disease have been proposed:

If Program A is adopted, 200 people will be saved.

If Program B is adopted, there is a one-third probability that 600 people will be saved and a two-thirds probability that no people will be saved.

Framed this way most people choose Program A over Program B, showing risk aversion. Consider now this new pair of options:

If Program A' is adopted, 400 people will die.

If Program B' is adopted, there is a one third probability that nobody will die and a two-thirds probability that 600 people will die.

When options are described in this way most people prefer Program B' over A' even when it is easy to note that A is *equivalent* to A' and B is equivalent to B'. In both cases exactly *the same* options are described. The only difference is that in the first choice setting the options are described in terms of saved (gained) lives while in the second one in terms of lost lives. This change in terminology should be irrelevant for truly rational individuals but apparently these subjects think differently because their preferences within one frame are *reverted* when the frame is shifted. The way in which options are *described* seems to be more important for them than the options themselves. This finding represents a broadening of the domain of application of loss aversion, showing that under some conditions it influences attitudes regarding risk. The theory that provides these results is known as Prospect Theory (PT). Moreover, PT affords a quantitative measure of loss aversion, which ordinarily ranges between 2.25 and 2.50.

The fact that well defined changes in the manner in which options are described (i.e., changes in the framing or the reference point) results in a *systematic* reversion of preferences suggest that PT has two fundamental virtues:

1. It identifies a dominant causal factor of the decisions that a majority of a group of individuals adopt under uncertainty.
2. It allows predicting and controlling (manipulating) those decisions.

These features of Prospect Theory represent an extraordinary success identifying some *causal factors* of human decisions. Let's see what notion of *causality* is involved here. Psillos (2004) points out that there are two main concepts of causality:

1) causation as *dependence*
2) causation as *production.*

Claim (1) asserts basically that to say that *c* causes *e* is to say that *e* consequently depends on *c*. Dependence is understood in a counterfactual sense: "if the cause hadn't happened, the effect wouldn't have happened"; claim (2) expresses a different idea: to say that *c* causes *e* is to say that something *in* the cause produces (brings about) the effect or that there is something (e.g., a mechanism) that links the cause and the effect. In this paper this second claimis set aside because it presupposes the notion of *mechanism* and it is controversial that this concept may be applied to the realm of psychological events. Instead it will be shown that PT can be considered a legitimate causal theory in the first sense of causality. Kincaid (2004) and Guerring (2010) offer *empirical* accounts of causality as *dependence*. According to Kincaid we may assert that X causes Y if it is possible to use this relation for making *predictions* [5].

> A claim to know a causal factor is dubious to the extent that it does not allow us to explain and predict. (Kincaid, 2004, p. 172) [6].

Guerring offers a different (but also empirical) answer to the problem of how it may be established that X causes Y. He states that this is the case when co-variation between both factors is found in experimental settings, saying that

> ... one might observe that in a properly conducted experiment (i.e., with a randomized treatment and isolated treatment and control groups) it is often possible to demonstrate that some factor causes a particular outcome even though the pathway remains mysterious (perhaps because it is not amenable to experimental manipulation). We do not hesitate to label these arguments as

> causal and as definitive (assuming proper experimental protocols have been followed and replications have been conducted in a variety of settings. (Guerring, 2010, p. 1505)

As was shown at the beginning of this section, Prospect Theory involves a *causal connection* according to both standards. It successfully satisfies the co-variation standard and also allows for predictions about what kind of changes in the reference point will produce what changes in the individual ranking of preferences over lotteries. It is remarkable that these predictions are not only true in experimental settings but also in applications to real, non-controlled situations (Benartzi and Thaler, 2007, Benartzi and Thaler, 2001). An additional virtue of PT is that the identified causal factor (the frame) may be *imposed* on the decision makers. This allows manipulation of their decisions. Summarizing, PT allows manipulability and control of a causal factor of the decisions taken by a majority of individuals under conditions of uncertainty.

In this sense it could be useful to reread the claim of Camerer and Loeweinstein (2004, p.1) By 'more *psychologically realistic* [assumptions]' he seems to refer to 'assumptions that are *causal factors* empirically grounded in an experimental setting in the field of psychology'. But why are these discoveries *relevant* for economy? This is something that needs to be defended on different grounds.

The explanatory role of behavioral economic models: from psychologically causal factors to economically *relevant* causal factors

In this section it is explored the way in which BE models might fulfill a potentially *explanatory* role. Though cognitive psychology provides a wide set of dispositions and heuristics endowed with potential causal power over decisions, its epistemological relevance *in the domain of economics* is not obvious. At first sight many of these dispositions and heuristics –like those already described in the *Asian Disease* experiment– are concerned only to the domain of pure theory of decision making and nothing is suggested about further applications to the specific field of economic

phenomena. It cannot be known beforehand whether a particular psychological feature, for example *loss aversion*, keeps any connection with already known patterns of saving and investment. When could be sensible to assert a causal connection among one of these *psychological* components of the human intuitive system and an *economic* (i.e., social) phenomenon? BE-models provide the answers to this question. More specifically, it is claimed that one of the main purposes of a particular BE-model is to explore what specific human disposition or heuristic may be causally *relevant* in connection to a given pattern of economic behavior.

Typically, relevance evaluation is performed in two steps. First, modelers have the knowledge of a set of well tested hypothesis concerning the particular effects that specific propensities and heuristics produce on decision making in controlled circumstances. This is the tool box provided by empirical psychology. Second, the modelers intend to *use* this type of knowledge to explain the formation of a particular economic pattern that is independently known. To this end a BE-model is constructed and the conjecture that a specific disposition or heuristic (or a set of them) Xi brings about a given pattern p is implemented. For instance, Benartzi and Thaler (1995) show that the so called Equity Premium Puzzle (EPP) –a pattern resulting from the individual distribution of investments between bonds and assets which is found anomalous from a conventional approach– could be explained away assuming individuals have loss aversion and suffer from myopia (i.e., they react on the frequency of the information about the returns of each type of investment) [7].

Now it can be asserted more precisely in what sense a *realistic* BE model could have *explanatory power*. Say that *Xi* is a human disposition or a heuristic supported by experimental evidence and *P* a known economic pattern with no proven connection with *Xi* yet. In these terms, the explanatory power of a given model could be defined as follows:

A model has *explanatory power* when it shows that under circumstances *C*, *given Xi* the pattern *P* ensues.

This definition can shed light on the reasons why many heterodox economists find neoclassical models inadequate from an explanatory standpoint. Indeed, under this criteria conventional economic models do not have *explanatory* power. The causal

factors of many models are not *compatible with the most relevant and best available knowledge*. Moreover they do not presume that the 'explanatory' variables used in their models have experimental support, nor find it a faulting characteristic that it is necessary to cure. Friedman (1953) provides the classic illustration of this position. According to him the theory of the firm does not assert that the entrepreneurs do what its axioms literally affirm (for instance, they do not equal (combine) the curves of marginal costs and income in order to decide their level of production). Entrepreneurs behave *as if* they would do such things. The same can be said of Expected Utility Theory (EUT): It says nothing about the formal aspects of the preferences of *real* people. It rather states that those agents who at the end of the day have been successful in the market have behaved *as if* they had had complete and transitive preferences [8].

Behavioral economics models and conventional models: 'as-if' and 'how is that possible' models

Let's call 'as-if strategy' to this interpretation of the informational content of theories and models. Some specialists consider the use of this strategy illegitimate, considering it uninformative and 'unrealistic' [9]. Some of them consider the 'as-if argument' a convenient device for avoiding the charge of unrealism directed to the conventional way of modeling [10]. This criticism is not accurate, however, because the as-if strategy may perfectly well be part of the explanatory project of those BE models which are *realistic* in the sense presented in this paper. To consider this possibility let's see the next figures:

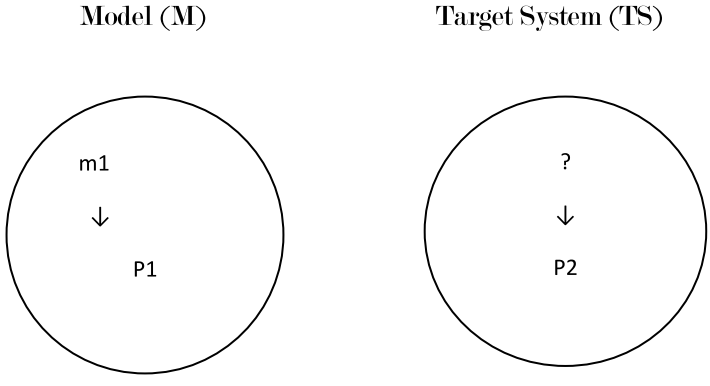

Figure 1: Model and target system

Suppose that in a given Target System a pattern P2 is observed. Researchers suggest that it has been generated by some unknown causal factor. In order to explain P2 they build a model like the one represented on the left hand of the figure. There, another pattern P1 (similar to P2 or compatible with it) is deductively obtained from a set of psychological factors (preferences and expectations) m1 provided by cognitive psychology and used as causal factors. A user of the model may then legitimately assert that 'real' (ordinary) individuals belonging to TS behave *as if* their decisions had originated in the workings of m1 (this can be stated independently of how probable it is that these decisions have 'really' been generated in this way).

Maybe m1 is effectively acting in TS but a user of the model does not need to commit to this to assert that in TS pattern p2 arises *as if* it were generated by m1. In this context the as-if argument simply means that within the framework of the model it is possible to deduce patterns like the ones observed assuming m1, which is strictly true in this case.

There is another reason why the a critical rejection of the as-if strategy is flawed: it is perfectly adequate for a particular kind of models (the so called *how is that possible* models), which show what factors *could* cause some observed results. Both conventional and BE models explore what 'mechanisms' or concatenation of causal factors could possibly generate a given economic pattern. Benartzi and Thaler (1995) show that

investors behave as if they suffered from myopia and loss aversion with respect to returns from their investments in assets and bonds [11].

However, conventional and BE models are two very different types of *how is that possible* models. This fact is quite clear when the restrictions imposed on the solution of their respective problems in each case are made explicit. Conventional models are constrained only by mathematics and logic. The possibility they establish between the posited behavioral mechanism and its results is just *logical* in nature. For example, in order to 'explain away' the Equity Premium Puzzle conventional economics does not hesitate in positing a risk aversion of 30 points, something that seems to be psychologically impossible. Psychology does not impose restrictions on this model. Many BE models face an additional restriction: the way in which they model agents' preferences and expectations should be *compatible with the best available empirical knowledge* (specifically, with the experimental results provided by experimental *psychology*). So it could be perfectly possible (*empirical* possibility) that the dispositions or heuristics isolated within de model could have caused in *our* world (not just in an arbitrary or imaginary world) the observed pattern under examination [12]. In Friedman's terms, both kinds of models appeal to the *as-if* strategy. The whole difference lies in the evidence supporting the posited explanatory factors. In the conventional way of modeling no *empirically* supported factors concerning human conduct are involved.

Epistemic properties of BE models

Taking into account the previous considerations let's go back to the three claims shared by many behavioral economists mentioned at the beginning of the paper. Claim (1) clarifies the sense in which many economists of Behavioral Economics attribute *realism* to their models: unlike conventional models, BE-models often incorporate explanatory factors that have been already identified in experimental (psychological) settings. Claim (2) says that by substituting imaginary preferences and expectations for psychological traits supported by the findings of cognitive psychology in conventional models, the *explanatory* capability of the newly created models should increase. However, the association between more realism and augmented explanatory power must be taken cautiously. The incorporation of dispositions and heuristics of Prospect

Theory contributes to the *potential* explanatory capacity of BE models, but for asserting that a particular BE model is in fact explanatory in a concrete situation, additional empirical information is needed. This information needs to be found in the *economic* realm in support of the claim that the particular set of causal factors isolated in the model has been acting in the particular case under examination. Only having this sort of evidence we are justified to claim that those factors isolated within the model do in fact (not only potentially) explain the observed pattern. Thesis (3) claims that the fact that BE models are *more realistic* than conventional models, explains their better predictive performance. Leaving aside the merits of Friedman's assertion, thesis (4), formulated this way is problematic. The introduction of well tested dispositions or heuristics within models is good, but they do not guarantee better predictions. The causal credentials that PT has acquired in experimental settings do not entail that it will preserve this same causal power regarding another domain of phenomena simply because a logical link has been established within a model. The particular factors isolated might be causally *irrelevant* regarding the type of economic pattern that has to be explained. This is perhaps why Camerer asserts a simple 'suggestion'.

Conclusion

The debate about the realism of economic assumptions has lead the philosophy of economics into a *cul de sac*. To some extent the problem lies on the many meanings of the words 'unrealism' or 'unrealistic'. This paper provides a criterion for deciding when a model is *more realistic* than one of its rivals. It is a *relative* criterion that focuses on the foundations that the models involved can exhibit in reference to a particular set of potential causal factors.

It is *not* claimed that model A, if found more realistic than another model B (according to this criterion), is *better* than B, nor is it asserted that better predictions are obtained from A as a consequence of it being more realistic. But some positive connection can be sustained between satisfying realism in this sense and the potential *explanatory* role that economic models may perform. Besides, this analysis helps to temper some rough treatments of the *as-if* strategy showing that it is compatible with *realistic* BE models. In cases like the one studied in the present paper the as-if

arguments involved show which factors *could* (according to experimentally confirmed knowledge) cause some observed results.

The fact that real agents under risk or uncertainty make decisions based on psychological dispositions and heuristics has strong empirical foundations and justifies that they be considered as *potential* causal factors of economic behavior. It follows that explanatory relevance can be attributed to BE models that *use* those heuristics or dispositions to model human choices and succeed in deriving economic patterns from them.

From an epistemological point of view, the empirically based approach to model building could be extended to other forms of modeling in economics. Other behavioral factors, different from the ones discovered by cognitive psychology, but which also have empirical support as potential causes of human decisions, might be incorporated in economic analysis. This has been achieved with success in the case of ethical considerations (the well-known cases of fairness and reciprocity).

Endotes

[1] As Wade Hands (2001) pointed out, the term 'realism' in economics is not necessarily related with its meaning in philosophy (a meta-theoretical view about the existence of those entities mentioned by scientific theories or models). Following Mäki (1989, 1998d) Hands endorsed the distinction between 'realisticness' and 'realism' in order to avoid this misunderstanding (see Hands, 2001, pp.328-329) However, we prefer to use the term as usual, mainly because this is the way it is presented in the bibliography this paper explores. We are indebted to an anonymous referee for having stressed the convenience of this remark.

[2] 'Modern mainstream economic theory is largely based on an *unrealistic* picture of human decision making. Economic agents are portrayed as fully rational Bayesian maximizers of subjective utility. This view of economics is not based on empirical evidence, but rather on the simultaneous axiomization of utility and subjective probability ... However it is wrong to assume that human beings conform to this ideal' (Selten, 2001, p. 13, our italics).

[3] But see E.M Sent (2005) for an alternative account of the relationship between BE and Simon's original view. We are grateful to an anonymous referee for this insightful reference.

[4] Darrell and Maier-Rigaud (2012, p. 292) criticized 'neoclassical theory for its refusal to integrate social scientific research, especially from social psychology and sociology', and pointed out the failure of conventional economics 'to design models that take into account key elements that drive economic outcomes in real-world markets. Half a century of research that conveniently disregarded essential institutional and behavioural characteristics of the markets...'

[5] Kincaid also provides a weaker characterization of causality. He asserts that X is cause of Y if X *influence* the occurrence of Y.

'What is a force? It is a causal factor. A force is *causal* in that it influences something. It is a *factor* in that it need not be the only influence present.' (Kincaid, 2004)
This is more a definition of causality than a method for identifying the cases in which we can say that a factor X causes Y. The question arises of how do we know that X causes Y? Kincaid's answer is that we are legitimated to assert a causal link when we can (correctly) predict that given X will follow Y.

[6] Kincaid's main interest is the clarification of the notions of law and explanation. To avoid unnecessary complications in this paper we leave aside both issues. What will be retained is the connection made by Kincaid between causal knowledge and predictive capacity.

[7] EPP refers to an economic pattern that is different from the so called endowment effect. It does not arise from the fact that people value more assets when they sell them than when they want to buy them. EPP shows that investors (in the long run) do not prefer to invest in assets rather than in bonds even if the returns of assets is clearly higher than the returns on bonds. Endowment effect does not exist in this case for two reasons. Firstly, because supposedly this effect cannot happens regarding money. Secondly, because what EPP describes is an unreasonable distribution of resources among two kinds of goods which at first are not possessed by individuals.

[8] Regarding the problem of whether or not agents follow those rules described in the axioms of EUT when taking risky decisions, Friedman y Savage (1948) offer a rather negative answer:

'The hypothesis does not assert that individuals explicitly or consciously calculate and compare expected utilities. Indeed, it is not at all clear what such an assertion would mean or how it could be tested. The hypothesis asserts rather that, in making a particular class of decisions, individuals behave as if they calculated and compared expected utility and as if they knew the odds' (F-S, 1948, p. 298).

[9] 'Past economists have tolerated the 'as if' neglect of real phenomena, but it no longer satisfies scholars in this new age of exploration for evolutionary understandings of origin and development. We are interested specifically in the human mind and human social organization. We obtain little insight in this respect from overly-capacious and unfalsifiable principles that apply to any organism or behavioral entity' (Hodgson, 2012, p. 100).

[10] 'The first challenge to the status of game theory as a universal theory comes mainly in the form of research by cognitive psychologists, who have questioned the expected utility model used in game theory to analyze decision making. There is strong empirical evidence to challenge the behavioral assumption that actors are utility maximizers. Indeed, several decades of research have offered evidence that is hard to ignore. Yet the response of game theorists to this criticism has varied considerably. One response takes the form of Milton Friedman's classic "as if" argument and summarily dismisses this entire line of criticism. The only thing that matters for these game theorists is a model's predictions and that the alleged universalism of the expected utility model provides a basis for making predictions. Thus, it is of no consequence that these predictions are based on assumptions about behavior that may be 'wildly inaccurate descriptive representations of reality'. The simplicity and coherence of a theory that allows for prediction trumps any concerns about the realism of the expected utility model' (Munck, 2001, p. 179).

[11] For a different assessment of what is called here *as if strategy* in Behavioral Economics see Berg and Gigerenzer (2010).

[12] Shefrin and Thaler (1988) offer another excellent case of realistic *as-if* explanation.

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