On the current state of game theory

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> "The formal language greatly limits the audience that really understands game theory; the abstraction blurs factors that natural thought takes into account and the formality creates an illusion that the theory is scientific" (Ariel Rubinstein, 2013).

Abstract
It emerges from a state-of-the-art review on game theory recently made by one of the most renowned specialists, Larry Samuelson, that the very idea of "applying" that theory to the study of concrete problems – in industrial economics for example – makes no sense. The restrictive nature of the underlying hypotheses is compounded by the problem of the multiplicity of "solutions" to its models. The countless "refinements" elaborated by game theorists to reduce the number of solutions only made matters worse while submerging game theory under the welter of mathematics – the arbitrary character of the choice of "the" solution is carried over the choice of "the" refinement. Arbitrariness also prevails in the so-called "evolutionary" approach of game theory, which has little to do with the initial idea of that theory (rational behaviors). The only area where game theory – in its "cooperative" form – had a certain utility was in the (prescriptive) field of matching between individuals who have an interest therein. The aim of the organizer-planner is not to find "the" optimal solution – which generally does not exist – but rather to do "for the best".

One of the most widespread myths in economics, but also in sociology and political science, is that game theory provides "tools" that can help solve concrete problems in these branches – especially in economics. Introductory and advanced textbooks thus often speak of the "applications" of game theory that are being made, giving the impression that they are revolutionizing the social sciences. But, looking more closely, we see that the few examples given concern mostly the usual "stories" (prisoners' dilemma, "chiken", battle of sexes, entry deterrence, store chain paradox, centipede game, etc.) of "old" game theory. Take the four volume set *Handbook of Game Theory with Economic Applications* – a Handbook that provides an extensive account of what has been done in the field of game theory from its beginning, especially in economics, but not exclusively. Despite its title, there is not the slightest trace of a concrete example of an application, nor do we find any numerical data in its thousands of pages. This is not surprising. Mathematical reasoning requires clear and explicit enunciation of the assumptions used in its demonstrations. In particular, the assumptions concerning the *information* available to each player – his payoffs and those of the other players for each outcome of the game, the rules of the game, etc. – are so restrictive that there is no concrete situation in the world where they could possibly be verified, not even roughly (Guerrien, 2004, p. 2-3). As Ariel Rubinstein, another renowned game theorist puts it:

> "Nearly every book on game theory begins with the sentence: 'Game theory is relevant to …' and is followed by an endless list of fields, such as nuclear strategy, financial markets, the world of butterflies and flowers, and intimate situations between men and women. Articles citing game theory as a source for resolving the world's problems are frequently published in the daily press.

> But *after nearly forty years of engaging in this field, I have yet to find even a single application of game theory in my daily life*" (Rubinstein, 2013, my italics).

Most serious game theorists know this but they are reluctant to admit it[1]. In his recent overview of the subject "Game Theory in Economics and Beyond", published in the *Journal of Economic Perspective*, Larry Samuelson, a prominent game theory specialist, gives a typical example of this contradictory [schizophrenic] attitude. Though it was probably not his intention, Larry Samuelson's paper gives us a good picture of the impasse that game theory is in. After telling us that the theory, and its "tools" have become "essential", – notably in economics where it leads to "extraordinarily useful models" –, the description that he gives of the state that the theory is in contradicts this rosy characterization. This is the case, for example, when he calls the mass of "refinements" of Nash equilibrium as "a menagerie".

He is even less satisfied with the so-called "evolutionary" game theory, a branch that was "surrounded by a great deal of excitement" in the 1990s and has since "receded into the background."

Apart from this contradictory attitude, Larry Samuelson, like many of the other prominent game theorists, makes some elementary logical mistakes. A typical example concerns his mischaracterization of Cournot's model "solution" (equilibrium).

The Cournot's "solution": an inconsistent presentation

The only game model on which Larry Samuelson focuses in his paper is the old-fashioned Cournot duopoly (1838), typical of what he calls "classical" game theory. Even though he does not mention any concrete application of this model – not even a vague one as Cournot's springs of mineral water –, L. Samuelson argues that it is "extraordinarily useful". To describe the model, he draws two (reaction) curves and tells us that *each player* can deduce the equilibrium strategy (quantities of output) *the same way* "an analyst observing the game" can deduce it:

> "Under the classical view of game theory, one should be able to deduce the equilibrium play from the specification of the game and the hypothesis that it is commonly known that the players are rational. An analyst observing the game should be able to make such a deduction, *as should the players in the game"* (Samuelson 2016, p. 110, my italics).

This is *a mistake* since one of the main assumptions in Cournot's model is that *each duopolist knows only his own cost function* – from which he deduces his reaction function. Unlike "an analyst observing the game", each player ignores everything about the other player. "Rational" players *cannot* therefore "deduce" the equilibrium strategy of the game, and equilibrium can only happen by accident (Bénicourt and Guerrien, 2008, p 320).

Larry Samuelson relies on his (erroneous) reasoning on Cournot's model to justify the importance generally given to (Nash) equilibriums by game theorists:

[1] David Kreps also expresses skepticism, but less clearly than Ariel Rubinstein.

> "This immediately answers an obvious question: Why are we interested in the equilibrium of a game? In the classical view, the equilibrium implication of a game will be *obvious* to rational players, and will *just as obviously* be reflected in their behavior" (Samuelson, 2016, p. 110, my italics).

He thereby falls under David Kreps's criticism:

> "When economic analysts invoke the notion of a Nash equilibrium, they are asserting, at least implicitly, that the situation in question has (or will have) a self-evident way to play. When, as often happens, they don't say why it is that there is a self-evident way to play, then it is left to the reader either to supply the reason or to be suspicious of the results" (Kreps, 1991, p. 32).

The fact that the (Nash) equilibrium is not an obvious way to play (a way that is self-evident) is, for Kreps, the main weakness of game theory:

> "The great weaknesses of game theory are that it is fuzzy (to say the least) on just when and, if so, why equilibrium analysis is relevant, and on what to do when equilibrium analysis is irrelevant" (Kreps, 1991, p. 36).

The "menagerie" of (Nash) equilibrium refinements

At the beginning of his article, Larry Samuelson misses out the problem of the relevance of Nash equilibrium as a "solution" (prediction) of game models. He prefers to focus on what he considers to be the major problem of game theory: the multiplicity of (Nash) equilibria.

Game theorists have addressed this issue by imposing additional restrictive conditions on equilibria – restrictions that they call "refinements" – but with little success. This is vividly described by Larry Samuelson:

> "The equilibrium refinements literature was not a complete success. Instead of producing an equilibrium refinement that could command consensus, the literature gave rise to *an ever-growing menagerie of refinements*. New refinements tended to give rise to examples highlighting their weaknesses, followed by successor refinements destined to serve as the raw material for the next round. This seemingly endless cycle prompted Binmore (1992, p. 1) to liken the refinements quest to Hercules' quest to kill the Hydra, with two new heads appearing in the place of each predecessor" (Samuelson, 2016, p. 111-112, my italics).

Carried away by his criticism, and in *total contradiction* with what he previously said about Cournot's model, he questions the very relevance of Nash equilibrium:

> "At the same time that many game theorists were busy inventing and reinventing refinements of Nash equilibrium, *difficulties appeared in the attempt to show that at least Nash equilibrium,* much less refinements of Nash equilibrium, *could be deduced from the specification of the game and the hypotheses that the players are commonly known to be rational*" (*ibid*, p. 112, my italics).

The quest for equilibrium refinements inevitably leads to the question of what exactly is a rational behavior, in particular in sequential games – such as the centipede game or the repeated "prisoner's dilemma". As Robert Aumann explains,

> "the interesting aspect of the refinements is that *they use irrationality to arrive at a strong form of rationality*. In one way or another, all of them work by assuming that irrationality cannot be ruled out, that the players ascribe irrationality to each other with a small probability. True rationality (sic) requires 'noise'; it cannot grow in sterile ground, it cannot feed on itself only" (*Game Theory* entry of *The New Palgrave Dictionary of Economics*, 2008).

Unfortunately, Aumann does not tell us anywhere what he means by "strong form of rationality" or by "true rationality" …

"Any behavior" can be "explained" in industrial economics

Larry Samuelson has equally harsh words for the use of game theory in industrial economics. "Heavily empirical" before the 1970s, industrial economics "became enthusiastically theoretical" in the 1980s, thereby succumbing to the trend of game theory. For Larry Samuelson, the systematic recourse to (noncooperative) game theory, with its countless (Nash) equilibrium refinements, can explain anything:

> "Strategic models came to be used to explain price discrimination, advertising, entry deterrence, limit pricing, and a host of other phenomena. The difficulty was that an impression soon formed that a sufficiently determined modeler could construct a model explaining any behavior, no matter how counterintuitive" (Samuelson, 2016, p. 118).

It is perhaps for this reason that:

> "the strategic revolution in industrial organization did not maintain its momentum" (*ibid,* p. 118).

After giving credit to Thomas Schelling for pointing out the importance of "the personal and cultural context", L. Samuelson mentions Keynes and emphasizes the need for "careful work on the art of choosing models". However, he does not provide any successful example of this "art" apart from a rapid reference to the hypothetical problem between a president of the United States and a "Middle Eastern dictator" whose rationality is not ensured…

And about Jean Tirole?

Jean Tirole, we are told, won the Nobel Prize in economics because:

> "he has made important theoretical research contributions in a number of areas, but most of all he has clarified how to understand and regulate industries with a few powerful firms" (Press release of the *Economic Sciences Prize Committee of the Royal Swedish Academy of Sciences*, October 2014).

He was supposedly also rewarded for his proposing a “unified theory” based on “his analysis of firms with market power” (*ibid*).

Although the issue of firms with “market power” is at the very origin of “industrial economics”, Larry Samuelson’s article – which was published two years after the award of the Nobel Prize to Tirole – says nothing about this “unified theory”, in which he apparently does not believe. Neither does he mentions any paper published by Jean Tirole. He only blames him for failing to speak of the cooperative approach in his game theory textbook (Fudenberg and Tirole, 1991).

Firms with market power – whether on the selling side or on the buying side – will meet at some time and they will need to bargain. What does game theory have to say about this?

And the bargaining problem, “basic point of departure for studying economics”?

After reminding us that:

> “Edgeworth identified the bargaining problem as the basic point of departure for studying economics”,

Larry Samuelson observes that, despite the efforts made by game theorists,

> “economic reasoning was not helpful in identifying which of the typically many efficient outcomes might appear” (Samuelson, 2016, p. 121).

Therefore,

> “one cannot easily point to examples where bargaining methods have been overhauled in response to game theoretic insights” (*ibid*, p. 121).

The difficulty stems from the fact that the various procedures proposed by game theorists for overcoming the indeterminateness that characterizes bargaining situations are “too sensitive to fine details of the model”.

Among these “details”, there are

> “the timing of offers and counteroffers, the specification of the information structure, the length of the horizon, the length of a time period, and other details” (*ibid,* p. 121).

Each of these details can have a significant impact on the “outcome” of the model, regardless of the solution concept used.

For Larry Samuelson:

> “given a choice from a collection of models that give sharply different results, with little guidance as to which is appropriate, it is not surprising that one might avoid using such models” (*ibid,* p. 122).

Therefore, there is not much to expect from game theory when it comes to the bargaining problem.[2]

A similar statement can be made about evolutionary game theory which enjoyed its days of glory at the end of the 20th century.

Evolutionary game theory: another disappointment

Samuelson reminds us that during the 1990s, evolutionary game theory was "surrounded by a great deal of excitement" in the economics profession (p 115). There was hope that it could remove the indeterminateness related to the multiplicity of Nash equilibria.

Instead of deducing "the equilibrium actions from the structure of the game", players can proceed "to a trial-and-error process that (one hopes) tends to push them toward equilibrium" (Samuelson, 2016, p. 115).

This seems reasonable at first sight… but it is not. Rational individuals notice that their forecasts are mistaken and progressively modify them by taking into account what they observe during the process. Hence, the equilibrium that they reach – if they ever reach it… – depends on what happened all along the path that they followed to reach it (it is "*path dependent*") – so it cannot be deduced from the specifications of the initial model.

Samuelson illustrates the idea of an evolutionary game by returning to Cournot's model:

> "Evolutionary game theory puts the dynamic process back into the picture. Interestingly, Cournot (1838) motivated his equilibrium for the duopoly as the limiting outcome of a best-response-based adjustment process" (*ibid,* p 115).

This justification of equilibrium only makes sense if we assume that firms behave like robots that are programmed to make offers and counteroffers on the basis of a reaction function given once and for all – whereas "conscious" players modify their behavior in accordance with what they observe during the process.[3]

The evolutionary approach radically alters the essence of game theory. It does so, not by introducing "dynamics",[4] but by turning players into robots whose "algorithm" is given by a list of instructions – that game theorists call a *strategy*. Players *do not choose* strategies, each of them *is* (reduced to) a strategy. A game is therefore given by a set of strategies and rules that specify the order and the number of times ("rounds") those strategies meet in pairwise contests.

[2] Ebay's case is notable. Built originally around the idea that "appropriate prices" (whatever that may mean) would emerge from generalized bargaining, the website had to change its policy: nearly 90% of the exchanges are now taking place with a price set by the sellers. There are no absolute rules that prevent (some) players' "strategic behavior" from destroying confidence in the game.

[3] Cournot's reaction function is based on the *conjecture* that "the other player will not modify his offer if I modify mine" – while it is what he does all along the equilibrium "search" process.

[4] In the introduction to their *Theory of Games and Economic Behaviour,* Von Neumann and Morgenstern explain that it will take "at least a century" to introduce dynamics into game theory.

In principle, the outcome of an evolutionary game is simple to determine: feed the (given) strategies (lists of instructions) and rules of the game into a computer and look at the gains of each player (strategy) – gains can be expressed in terms of offsprings or in anything else.

Even if they radically differ in their conception, the "conscious choice" and the "robotic behavior" approaches of game theory are both unfortunately often thrown together into the same pot. This confusion is maintained because the concept of Nash equilibrium and that of evolutionary stable strategy (which is specific to the evolutionary approach) share a certain *formal similarity*. In both cases, any "deviation" from "equilibrium" by a player – a modification in his choice or a (random) "mutation" in his (unique) strategy – results in a lower payoff for him.

Samuelson eventually notes that after the "excitement" of the 1990s – as attested by the countless "tournaments" organized with various alternatives of the win-win game – evolutionary game theory has progressively "receded into the background" (Samuelson, 2016, p. 116).

This can be interpreted as a "success". However, in retrospect, it appears that it is a meager success. The evolutionary approach has, in particular, failed to solve the problem of the multiplicity of Nash equilibria that cripples game theory:

> "Evolutionary models do not consistently lead to any of the standard refinements of Nash equilibrium, much less produce a consensus on what a useful refinement might be" (*ibid,* p. 116).

Larry Samuelson gives an example – perhaps an opportunity to mention one of his publications… – in which the "evolutionary dynamics" do not eliminate the (weakly) dominated strategies whereas they "should be eliminated in every (Nash) equilibrium refinement". This would be sufficient to explain why evolutionary theory "has receded into the background" (*ibid,* p. 116).

Some successes? Auctions and mechanism design

For Larry Samuelson,

> "Two of the obvious successes of game theory are auctions and matching" (*ibid,* p. 121).

In both cases, the theoretician adopts the point of view of someone who seeks to organize the relationships between the participants in the best way – on a voluntary basis or in return for a compensation. Therefore, the aim is not to explain *how things are* but rather *how they should be*, according to a criterion specific to each situation.

After mentioning the "obvious successes" of auction theory, Larry Samuelson acknowledges that:

> "modeling an auction gives rise to a seemingly endless series of choices— are values common or idiosyncratic, are the bidders risk neutral or risk averse, is there a resale market, will the bidders collude, are the bidders

> symmetric, and so on—again without definitive indications as to which is the obvious modeling choice" (*ibid,* p. 122).

Once again, we end up "without definitive indications as to which is the obvious modeling choice" (*ibid*, p 122).

The game theorist can propose the type of auction that he deems appropriate to a specific situation. However, he must make a certain number of assumptions – notably concerning the behavior of the bidders. There can be no guarantee that these assumptions are verified and therefore that the solution proposed is the "best" one – whatever that may mean. The person who effectively organizes the auction can, as Samuelson says, only gain "useful insights" from it. The "obvious success" in the application of game theory to auctions is not so obvious…

Auctions are a specific case of "mechanism design", an approach that proceeds in the "reverse order" of what is usually done in standard game theory: the game theorist sets a goal – a function to be maximized for example – and then seeks ("designs") the rules of the game ("the mechanism") that would allow the goal to be achieved, or at least to get as close to it as possible. In the case of an auction organized by a seller, the goal can be to obtain the highest price – the one that the bidder with the highest values is willing to pay. Mechanism design is therefore closer to the approach adopted by an engineer – or a central planner that establishes rules and incentive systems – than to that of a person who seeks to explain what happens or to predict what will happen.[5]

Matching is, according to L. Samuelson, the other "obvious success" of game theory. The organizing "engineer" now *decides* how the players are "matched" by trying to do what is best according solely to their preferences. His problem is, at first glance, relatively simple because he does not need to care – as in the case of an auction – about the players' behavior, the information available, their beliefs, etc. The matching problem is quite common: students that seek to get into the "best schools" and schools that wish to recruit the "best students", marriage candidates, organ donors and recipients… The aim of the organizer is to match people in such a way that there is no match (*A*, *B*) by which *both A* and *B* would be individually better off than they are with the element to which they are currently matched.[6]

This stability property – no one has an incentive to replace his partner in the absence of (another) person that is inclined to do so – is used to characterize certain concepts of solution within game theory. Simple in principle, the determination of stable matchings is complicated in practice because matchings are derived from rankings – and not from numbers that have a meaning in themselves. This "technical" difficulty is compounded by an "ethical" issue: the organizer must generally choose between several stable matchings and then "favor" certain players at the expense of others. The theorist or the practitioner can simply find the "solutions" (stable matchings), give them to the organizer and let him choose the one he prefers.

[5] The model of perfect competition – with its "center" that "gives" prices and changes them until their equilibrium value is found, etc. – can be considered as an example of *mechanism design* intended to achieve a form of social welfare (Pareto optimality). For this aim to be achieved, agents must act as price takers and there must be a complete system of markets.

[6] Matching is often associated with the idea of a "market" – for marriage, for organ donation, etc. A very strange market, indeed, without prices and where the participants merely inform the "organizer" about their possibilities or preferences…

Does research into matching techniques fall in the field of game theory? Not if one considers that game theory deals with "decision-makers whose actions mutually affect one another" (Aumann, 2008) since there is only one "decision-maker" in the case of matching – the "engineer" or the organizer – who seeks to solve a problem related to what we commonly call "operational research". Accordingly, there is no trace of the "matching" issue found in game theory textbooks nor in the several popular texts related to this field.

Is the "cooperative" approach of game theory a road to salvation?

Research into "satisfying" matchings falls within the so-called "cooperative" – "coalitional" to be more precise – game theory. Larry Samuelson blames game theorists for abandoning the cooperative approach in the 1970s, in favor of the non-cooperative one. The cooperative approach actually held a prominent place in Von Neumann and Morgenstern's seminal book *Theory of Games and Economic Behavior* (1944). Andrew Schotter, a Morgenstern student, explains that

> "von Neumann and Morgenstern (VNM) were looking for a way to break the circularity of the 'I think he thinks that I think' logic of strategically interdependent situations. They wanted to provide a way for players to behave that was independent of their expectation of what their opponent intended to do" (Schotter, 1992, p. 106).[7]

Hence the special attention VNM gave what they called "orders of society" – *institutional arrangements* in current language. Multiplicity of orders simply reflects the variety of existing societies, where individuals form groups (coalitions) in which they "cooperate". As is suggested by the word "order", theoreticians are particularly interested in situations where groups maintain themselves – where they are stable.[8] The other cooperative solution concepts – VNM stable sets, bargaining sets, kernel, core, nucleolus, etc... – are in the same vein.

Larry Samuelson does not ask why the non-cooperative approach of game theory – despite its impasses – almost entirely replaced the cooperative approach in the 1970s. The most plausible explanation is ideological, as often in economics. Indeed, the non-cooperative approach – which implies that each solution must be derived from individual maximizations – is consistent with the canons of "microeconomic foundations", the sole scientific or rigorous approach accepted by the consensus established in the 1970s within the mainstream paradigm. Unhappily, the only great "success" that can be claimed by game theory in practice – matching, which is a type of planning – is not really in conformity with this new zeitgeist …

Conclusion

Initially developed by mathematicians interested in situations such as those of certain parlor games, with specific rules, game theory progressively became a label for all situations, real or imaginary, in which the gains of each participant, in money or in anything else, depends, at

[7] The "minimax" strategy for zero-sum games, the only non-cooperative concept in *Theory of Games and Economic Behavior*, supposes that "a certain return is *guaranteed*, no matter what one's opponent does" (p. 106, Schotter's italics). It aims at safety rather than maximum gains.
[8] Schotter believes that, given his "Austrian" convictions, Morgenstern would have been reluctant to use game theory for "social planning" and for "institutional engineering", as mechanism design does.

least partly, on the choices made by the other participants. Since most activities in social life correspond to this type of situation, the scope of game theory is, in principle, unlimited. In practice, the constraints that the use of mathematics requires implies that this scope is reduced to more or less complex "fables" or "proverbs" where the requirements concerning the information available to the players are so unreal that they are never verified in practice, not even roughly.

As Ariel Rubinstein clearly says in *Frankfurter Allgemeine*:

> "game theory is a collection of fables and proverbs. Implementing a model from game theory is just as likely as implementing a fable. A good fable enables us to see a situation in life from a new angle and perhaps influence our action or judgment one day. But it would be absurd to say that "The Emperor's New Clothes" predicts the path of Berlusconi …" (Rubinstein, 2013).

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