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The paper presents three different reconstructions of the 1980s boom of game theory and its rise to the present status of indispensable tool-box for modern economics. The first story focuses on the Nash refinements literature and on the development of Bayesian games. The second emphasizes the role of antitrust case law, and in particular of the rehabilitation, via game theory, of some traditional antitrust prohibitions and limitations which had been challenged by the Chicago approach. The third story centers on the wealth of issues classifiable under the general headline of "mechanism design" and on the game theoretical tools and methods which have been applied to tackle them. The bottom lines are, first, that the three stories need not be viewed as conflicting, but rather as complementary, and, second, that in all stories a central role has been played by John Harsanyi and Bayesian decision theory.

1. Introduction

The aim of the paper is to present three alternative explanations for the post-1980 boom of noncooperative game theory. In previous works (see e.g. Giocoli 2003, Chs.4-6) I have explained how game theory as such[1] failed to receive a significant degree of attention by economists[2] in the first two decades after its "invention" by John von Neumann and John F. Nash, namely, the 1950s and 1960s. The simple question then is: how could it happen that a neglected sub-discipline managed in about a decade to conquer the "hearts and minds" of economists, eventually becoming the undisputed theoretical core of mainstream economics?[3] It is quite immediate to surmise that, if we accept 1980 (or, better, as will be detailed below, the last third of the 1970s) as the starting date for the rise of game theory, the events which sparked it must have taken place in the previous decade or so, namely, during the 1970s.

What I offer here are three explanations of the rise:[4]

- the beginning of the literature on the refinements of Nash equilibrium
- the reaction against Chicago antitrust theory and policy
- the application of game-theoretic tools to mechanism design problems.

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[1] Not so game theory as a tool-box of useful analytical techniques: see Giocoli 2003a

[2] Not so by mathematicians: see e.g. Owen 1982.

[3] Here the publication of Kreps 1990 may be taken as iconic of such a conquer: see also below, §4.

[4] The list is far from exhaustive as other, surely relevant explanations might be added, such as the definitive abandonment of the cooperative approach in favour of the noncooperative one (see e.g. Schotter and Schwödiauer 1980) or the increasing mathematical literacy of average economists. I have dealt extensively with these issues in Giocoli 2003; 2009.

In my fore-mentioned works I have more or less explicitly argued for the first explanation, crediting the boom of game theory to the huge amount of research spent on chasing ever more refined characterizations of strategic rationality and game solutions. This literature originated from the pioneering work of 1994 Nobelists John Harsanyi and Reinhard Selten who, in the second half of the 1960s, extended noncooperative theory to deal with, and actually solve, games of incomplete or imperfect information. In this paper I wish to go beyond this account: my main thesis is that what really caused the boom was the powerful combination between the refinements literature and the two other explanations. Indeed, it might even be argued that most of the former emerged just out of necessity, in order to tackle the concrete antitrust and mechanism design issues raised by the latter.

What goes totally untackled in the paper is a further concern which, as I argue below, is nonetheless crucial for a full reconstruction of the postwar history of game and decision theory. How, when and why did the idea that rational agents should be modeled as Bayesian decision-makers become, first, an accepted, and, later, the standard assumption in economic theory? Here I have no answer yet, but in the concluding § I will advance the suggestion that a promising research line goes in the direction of investigating the kind of decision theory which was being taught during the 1960s in top US business and management schools. Anyway, in what follows I will simply take for granted that a game theorist working in the second half of the 1970s was perfectly comfortable with the assumption of Bayesian decision theory as the kind of rationality to be attributed to players in a noncooperative setting.

2. The refinements literature and Harsanyi's contribution

The most straightforward explanation of the 1980s triumph of game theory may be found in the so-called refinements literature. By this name it is meant the description of how the standard definition of a Nash equilibrium in a game can be sharpened by invoking additional criteria derived from decision theory (Govindan and Wilson 2007, 1). The origin of this stream of research dates back to the 1950s, that is, immediately after John Nash had developed his solution concept for noncooperative games. It did not take long, in fact, for game theorists (including Nash himself: see Giocoli 2003, Ch.5; 2004) to raise those very issues which still lie at the roots of the refinements literature, viz., the problems of multiplicity, equilibrium selection and rational behavior under incomplete information. Take for instance Luce and Raiffa's 1957 classic. Within their overall negative evaluation of Nash equilibrium (see ibid., 104-5, 112), the authors put particular emphasis on the circumstance that the standard, fixed-point argument in favor of Nash's solution – namely, that of being an equilibrium notion with the property that knowledge of the theory supporting it would not lead any player to a choice different from that dictated by the theory itself – characterizes it as just a necessary condition of rational strategic behavior, but not a sufficient one. Multiplicity of equilibria in normal form games is widespread, so much so that, absent a convincing theory of how to select among them, Luce and Raiffa had to conclude that <<… [Nash] equilibrium notion does not serve in general as a guide to action.>> (ibid., 172).

Luce and Raiffa also realized that the normal form, while providing a general, and quite user-friendly, tool for modeling strategic situations, entailed the suppression of all information issues. Indeed, in the normal form no player can get any private information until *after* she has chosen her strategy for the whole game, that is, until there is nothing left she can do apart from mechanically implementing the strategy itself (see Myerson 2004, 3). This of course falls short of being a proper representation of strategic behavior in dynamic situations, that is, in all games where a player is called to act repeatedly, and thus can draw inferences about the other players' strategies, preferences or private information as the game proceeds. Normal form Nash equilibria simply do not distinguish between the case in which each player commits initially and irrevocably to her strategy throughout the game, and the case in which a player continually re-optimizes as the game goes on. The distinction is lost because the definition of Nash equilibrium presumes that players will surely adhere to their initially chosen strategies. As Govindan and Wilson (2007, 3-4) put it,

<<Most refinements of Nash equilibrium are intended to resurrect this important distinction. Ideally one would like each Nash equilibrium to bear a label telling whether it assumes implicit commitment or relies on incredible threats or promises. Such features are usually evident in the equilibria of trivially simple games, but in more complicated games they must be identified augmenting the definition of Nash equilibrium with additional criteria.>>.

Remarkably, Luce and Raiffa did try to modify the standard definition of a game by allowing players to have incomplete information, and thus to hold *beliefs* rather than knowledge, about the strategic situation (see Luce and Raiffa 1957, §12.4). Unfortunately, their complicated technique led nowhere.

Despite the early discovery of the limits of Nash equilibrium, we can safely date the real beginning of the refinements literature to Selten 1965. In that paper Selten explicitly raised the issue of the adequacy of the normal form, and of the related necessity to investigate more carefully the extensive form, as the central questions of noncooperative game theory.[5] This opened large,

[5] On Selten's paper see Myerson 1999, 1076.

uncharted prairies along two research lines.[6] On the one side, Selten's acknowledged the all-too-frequent case of games whose normal form representation had too many Nash equilibria, some of which seemed clearly irrational when the game was examined in extensive form in that they required an agent to play a strategy she would refuse to play if actually called to. This forced the imposition of stronger necessary and sufficient conditions for rational strategic behavior in extensive form games – stronger, that is to say, than Nash's necessary condition for the normal form. As is well known, Selten's answer to the problem of excluding intuitively unreasonable Nash equilibria was the new notion of subgame perfect equilibrium, but this was just the first entry in what in the next couple of decades became a long list of ever more refined equilibrium concepts.[7] On the other side, at the root of Selten's 1965 puzzle lay the intuitively appealing idea that extensive form games sharing the same normal form should have the same set of solutions. Hence, further refinements of Nash equilibrium have been developed which may be directly applied to games in normal form.[8] These refinements enjoy the property that the solution theory based upon them, when applied to the extensive form, guarantees that extensive form games sharing the same normal form representation will have the same solutions.

Many refinements have been proposed.[9] Generally speaking, each contribution to this literature starts with a list of the properties which appear theoretically desirable for a refinement concept to enjoy. Especially in the literature's early years, the dream was to find the "magic bullet", i.e., *the* solution concept capable of solving all games, be they in normal or extensive form. While this dream – which was just a revised version of von Neumann's original goal of providing a *complete* characterization of rational strategic behavior (see von Neumann and Morgenstern 1953, 31) – was quickly abandoned, the search strategy followed by game theorists entailed that the refinements were mostly developed incrementally, one after the other, and often relying on *ad hoc* criteria.

Two major groups of refinements can be identified (see Govindan and Wilson 2007). The first consists of those equilibrium notions which require sequential rationality as the game progresses.[10] The other includes the notions which warrant the credibility of the equilibrium by considering perturbed games where every contingency – even very unlikely ones – occurs with positive probability. Thus, while refinements in the first group exclude unreasonable equilibrium by imposing a stronger notion of rationality upon the players, those in the second accept that players may make "mistakes", thereby making no equilibrium truly unreasonable.

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[6] Note that the distinction is made here just for expository reasons, since the two lines have never been really separated.

[7] Another landmark notion worth mentioning here is the sequential equilibrium in Kreps and Wilson 1982.

[8] See Myerson 1991, 215.

[9] To mention a very rough datum, a simple Google scholar search for the expressions "refinements of Nash equilibrium" and "Nash equilibrium refinements" delivers about 400 hits.

[10] A strategy is sequentially rational for player i at information state s if i would actually want to do what this strategy specifies for him at s when information state s actually occurred.

Yet, even the ultimate and most sophisticated refinement may still allow for multiple equilibria in many games (the most obvious example being the well-known Battle of the Sexes). This explains why, following Myerson (1991, 241-2), it is correct to distinguish between true refinements on the one side – namely, solution concepts intended to offer a more accurate characterization of rational behavior in games – and selection criteria on the other. The latter are any objective standard which can be used to determine the focal equilibrium expected by every player to occur in case of multiplicity. Note that a selection criterion, differing from a refinement, requires more than the players' rationality. What is also called for is, in Myerson's terminology, a "cultural" feature of the players' environment capable of inducing them to focus on a specific selection criterion, so much so that if players, on account of the common cultural feature, expect each other to behave according to one of the equilibria, then they may rationally fulfill these expectations.

The twin goals of refining the Nash equilibrium and selecting among several equilibria, fueled by the two theoretical drivers of eliminating unreasonable equilibria and ensuring the consistency of a game's solutions in normal and extensive form, constituted a powerful mix for the rise of game theory. Historians have explained how, starting from the postwar period, economics has detached itself from its old role model, mechanical physics, and has replaced it with a new one, mathematics. Indeed, neoclassical economics in the second half of the 20th century has increasingly resembled mathematics in terms of methodology (read, the axiomatic approach) and, above all, of the discipline's sociology, namely, of things such as: how Ph.D. students are selected and taught, what is considered a relevant contribution, how scientific progress is defined, and so on and so forth (see Weintraub 2002; Giocoli 2009). The refinements literature fits perfectly in such a picture, so much so that until very recently I would have unreservedly embraced these two statements: *i*) the boom of game theory in the late 1970s – early 1980s has been due to the flow of contributions on the refinements literature; *ii*) this literature has come into existence because ever more economists have applied their ever increasing mathematical skills[11] to a math-style, problem-solving setup, namely, that of providing a new theorem showing how a given solution concept might be either generalized or turned into another, more refined one.

Yet, I now recognize that the limit of an explanation based on these statements is that it is too "internal" to the subdiscipline of game theory and so it downplays the extent of the revolution brought to the broader realm of economics by the other founding paper of the refinements literature, namely, Harsanyi's landmark analysis of games with imperfect information, published in three parts in *Management Science* (Harsanyi 1967-68). The content and the import of Harsanyi's paper have

[11] Rizvi 1994 highlights how a role in this story might have been played by the inflow into game theory of the gifted mathematical economists who, precisely in the same period, were abandoning general equilibrium theory due to the negative implications of the SMD theorem.

been masterfully reconstructed by Roger Myerson in various papers (see Myerson 1999, 2001, 2004, 2008). Here I just wish to highlight a few features.

First of all, note that Harsanyi’s motivation also stemmed from the limitations of the Nash equilibrium for normal form games. As I said before, a game in normal form assumes that all players have the same information, or, as Myerson effectively puts it, that <<the “beginning of the game” must be a point in time when all players have the same information.>> (Myerson 1999, 1076). Yet, this restriction is unacceptable when we have to model situations where agents have some, long-standing differences in information, because <<…it demands that our model must begin with some point from the distant past.>> (ibid.). Harsanyi 1967-68 showed how to avoid this difficulty by constructing a Bayesian game of incomplete information.

A (consistent) Bayesian game is one where the players’ different beliefs at the beginning of the game are due to their having observed different random variables about which all players have common *prior* beliefs. This entails that, thanks to Harsanyi, it is not necessary anymore to impose *ad hoc* informational differences whenever the analysis of real world phenomena requires them, because these differences can themselves be explained in terms of the heterogeneity of players’ experiences. In other words, while in a Bayesian game, the game model itself is assumed to be common knowledge among players, we do not need to assume anymore that players have the same information.

The players’ different information is described by a collection of random variables, called the players’ types, each of which is private information of one player. The actual value of each players’ type is omitted from the model, which instead includes a probabilistic description of what each type of each player believes about the other players’ types.[12] This corresponds to a very peculiar modeling strategy, because the perspective from which Harsanyi’s games are analyzed is that of someone who only knows the information common to all players, i.e., the information summarized by the game model itself. Hence, the Harsanyian analyst is asked to deny herself the possibility to exploit any knowledge of any player’s actual type: such an information, being a private one, must be treated as a random variable because this the only way the analyst may correctly appreciate the uncertainty of the other players who do *not* know it and can only formulate beliefs about it. These beliefs are said to be consistent if the players’ type-contingent beliefs can all be derived by Bayes’s rule from the fore-mentioned common prior distribution.

[12] More specifically, a Bayesian game is a mathematical model that specifies: 1) the set of players, 2) the set of feasible actions for each player, 3) the set of possibly types for each player, 4) each player’s expected payoffs for every possible combination of all players’ actions and types, 5) for each possible type of each player, a probability distribution over the other players’ possible types describing what each type of each players would believe about the others’ type (see Myerson 2008, 4).

The second remarkable feature refers to the actual circumstances of the early presentation of Harsanyi's paper (see Aumann and Maschler 1995; Myerson 2004). This took place at a 1965 game theory conference in Jerusalem, where the audience included people like Robert Aumann, Michael Maschler and Reinherd Selten. The three, together with Harsanyi, spent the following years working together in a research project of the US Arms Control and Disarmament Agency (ACDA). Problems of information in games became a central focus of ACDA, where Harsanyi found the proper environment to refine, and spread, his ideas. These events help explain why Bayesian games found immediate application in the refinements literature, so much so that they quickly became the latter's most essential tool. For example, Harsanyi 1973 offered a new interpretation of mixed-strategy equilibria, filling a gap as old as game theory itself: by letting each player have some minor private information which changes the payoffs only slightly, any mixed-strategy equilibrium of a normal form game could be transformed into a non-randomized equilibrium of a Bayesian game, where each type chooses an optimal action without randomization (Myerson 1999, 1077; 2008, 5). Indeed, it is hardly an exaggeration to claim that, absent Harsanyi 1967-68, very little of the refinements literature would exist in its present form. Just think of the analytical possibilities opened by the interpretation of an extensive form game as a Bayesian game: any action in the latter may be represented as a plan describing what the player would do in any situation after the beginning of the game as a function of what the player may learn during the play, so much so that a player's strategy becomes a function that specifies a feasible action for each other player's possible types. Or think of Kreps and Wilson's 1982 sequential equilibrium, arguably the most fundamental solution concept for extensive form games, which explicitly requires that each player's system of beliefs be consistent with the game structure.

It would be tempting to constrain Harsanyi's work entirely within the boundaries of the refinements literature. This would obviously strengthen the above-mentioned "internalist" explanation of game theory's boom. However, the importance of the 1967-68 paper far exceeds those boundaries, as it is apparent if we pay attention to the fact that what Harsanyi gave us was no less than a general methodology, a standard analytical framework, for the modeling of situations where economic agents have different information – a kind of situation which is ubiquitous in the real world and which lies at the foundation of the whole of information economics. Such a broader view of Harsanyi's contribution goes a long way into explaining the reasons behind the modern rise of game theory. The clearest supporter of this interpretation is again Roger Myerson. So we may well give him the final word in this section:

<< [Harsanyi's] influence has been the basis for a profound revolution in social science. Any academic discipline must rely on a general methodology to provide a framework for inquiry and debate. […] After Harsanyi's (1967-8) great

study of incomplete information, Vickrey's auction games, Akerlof's (1970) market for lemons, and Spence's (1973) labor markets, and Rothschild and Stiglitz's (1976) insurance markets could all be viewed as interesting examples of people playing Harsanyi's Bayesian games. Having this common framework of such informational problems enabled us to apply insights from the study of any one of them to all the others, and thus the new economics of information was born.>> (Myerson 2004, 18).

3. The reaction against Chicago antitrust[13]

It is rather well known that the 1930s marked a watershed in industrial economics. On the one side, the first general models of imperfect and monopolistic competition were proposed; on the other, the new structure-conduct-performance (SCP) approach was developed in Edward Mason's Harvard seminar.[14] The SCP approach – which predicted anticompetitive outcomes as the inevitable consequence of non-perfectly competitive market structures – was to dominate industrial economics from the late 1930s to the early 1970s. Remarkably, these were also the years of active antitrust enforcement by the US Supreme Court and the Department of Justice. Following more than a decade of relative neglect of antitrust, the Supreme Court effectively revitalized the Sherman Act via the introduction of new per se prohibitions in rulings such as *Interstate Circuit* (1939) and *Socony Vacuum* (1940).[15] Exactly as dictated by the SCP approach, the focus in antitrust law shifted from conduct to market structure, while typical SCP notions like market shares and the various indexes of concentration became the basic tools of judicial analysis. The zenith of SCP-style antitrust enforcement came in the 1960s. Three key rulings – *Brown Shoe* (1962), *Philadelphia National Bank* (1963), *Von's Grocery* (1966) – testified the Supreme Court's turn towards structuralism: at the heart of the Court's evaluation in all these cases were market shares, their history and future, as well as their effect on market structure.

Thus, starting from the mid-1930s and for more than thirty years, a remarkable and increasing consistency existed on competition matters between judicial decisions and conventional economic thinking.[16] Such an almost perfect overlap helped open and support the era of aggressive contrast against monopolization and restraints of trade which lasted until the 1970s. Yet, as it had already happened before (see Martin 2007), the antitrust pendulum was to swing back. A reaction against the excesses of structuralism inevitably came, and it came from the Chicago school of economics.

The Chicago counterrevolution in antitrust has been founded on four pillars. First of all, a theoretical pillar, the so-called *tight prior equilibrium*, or "good approximation", hypothesis (Reder

[13] The first part of this § follows Giocoli 2010.
[14] Mason 1939 is traditionally considered the manifesto of the SCP approach.
[15] See Martin 2007.
[16] See Kovacic and Shapiro 2000, 51-52

1982), that is to say, the idea that any economic system exhibits a spontaneous tendency to reach a situation of Pareto-optimal equilibrium provided it is not disturbed by exogenous interferences, like those by government, antitrust authorities or courts. Secondly, an empirical pillar. According to Chicago economists, the data and observations used to found and validate the SCP approach were simply *wrong*: for example, the structuralist claim that the causation went from the number of firms in a market to the amount of profits each firm could earn had actually to be reversed since only the most efficient, i.e., most profitable, firms were those capable of surviving competition. The third pillar had to do with the viewpoint from which to evaluate competition and explain business conduct. Given that Pareto-optimality was the "natural" situation of markets (see the first pillar), *efficiency explanations* of business behavior had to be privileged with respect to market power ones. Two corollaries followed. First, the focus of antitrust analysis should be on market performance, as well as on the conduct determining it, while the structuralist viewpoint had to be abandoned. Second, the measure of market performance had to be consumer welfare (<<…the only legitimate goal of antitrust…>> in the words of Bork 1978, 7). The fourth and final pillar was pragmatic, but perhaps even more important than the previous three. I refer to the special ability of Chicago scholars to translate their economic arguments into *operational principles* that courts and lawyers might easily understand and apply.[17]

The combination of the four pillars proved irresistible. Starting from the early 1970s, and reaching their maximum influence about a decade later, Chicago antitrust arguments conquered US courts and, eventually, the Supreme Court. The landmark event is unanimously considered the 1977 *GTE Sylvania* ruling,[18] when the Court rigorously applied a Chicago-style economic argument to overrule the per se prohibition of restrictive distribution practices and bring non-price vertical restraints back to the rule-of-reason realm. The general lesson of *GTE Sylvania* was simple, but epoch-making: reliance on competition (read: a competitive market structure) to deliver good market performance had to be abandoned and replaced, in general, by a case-by-case evaluation of the net welfare impact of every single business practice, and, in particular, by an efficiency assessment of allegedly anti-competitive behaviors. Many other rulings, covering the other areas of antitrust, from collusion to dominance, from mergers to predatory pricing, followed and consolidated this crucial principle. The focus on efficiency explanations made much more difficult for antitrust authorities to win a case involving, say, vertical restraints or monopolization. Less than a decade after *Sylvania*, Chicago economists could be satisfied with their achievement: several

[17] On the crucial role of legal scholars as intermediaries for the application of economists' insights to legal problems in terms that courts can readily comprehend, see Kovacic 1992, 297.
[18] Continental T.V. Inc. v. GTE Sylvania Inc., 433 U.S. 36 (1977).

business conducts had been declared per se *legal*, while a case-by-case evaluation was warranted for almost all the remaining types of behavior.

It is crucial for our story to realize that, exactly when the Chicago approach made its breakthrough, by convincing ever more US courts of the validity of the economic arguments supporting pro-competitive explanations of several, supposedly anti-competitive, business conducts – *exactly then*, a series of new results in industrial economics seemed to prove the contrary, namely, that there could well be an anti-competitive rationale behind these very same conducts! Remarkably, these results were all based on the application of *game-theoretic tools* to models of imperfect competition. And the kind of game-theory employed in these models was that developed in the 1970s by Harsanyi, Selten and all the other scholars of the refinements literature. The coincidence is so impressing that it seems reasonable to argue that, at least as far as industrial economics is concerned, and regardless of their ability to "sell" their ideas to legal scholars and courts, the Chicago economists' approach has never been dominant or mainstream. Indeed, since the early 1980s, the advent and quick rise to dominance in industrial economics of game-theoretic methods has set the record straight in the marketplace of ideas, so much so that it is now customary to speak of a *post-Chicago* approach to competition issues.

Now, the question is: may we apply a *post hoc, propter hoc* logic and thus conclude that the boom of game theory has been *caused*, or induced, by the necessity to counter, in academic circles, as well as in courts, the Chicago attack against active antitrust enforcement? Let me exemplify this viewpoint by referring to the case of *predatory pricing*. By this expression it is meant, generally speaking, the case of a firm which sets prices at a level implying the sacrifice of profits in the short-run in order to eliminate competition and get higher profits in the long run.[19] Hence, the two basic elements of predatory behavior are the existence of short-term loss, on the one side, and the existence of market power enabling the predator to raise prices in the long run and recoup the losses suffered during the predatory phase, on the other. The standard account of predatory behavior satisfies these two requirements. It is the so-called *deep pocket story*: a big firm may drive its small rivals out of business by exploiting its ability (due, for instance, to the extra profits earned in other markets) to survive for a significant period of time the losses originating from a below-cost (viz., predatory) price, while small firms have no such ability and are thus forced to eventually give up. While this story sounds quite convincing,[20] a robust theory supporting it has been proposed only in the 1980s, by Benoit 1984. But, and here is the key point, Benoit's paper, as well as several others providing a rationale for predatory behavior, all stem from Harsanyi's formalization of games with incomplete information.

[19] Motta 2004, 412. The following paragraphs closely follow Motta's exposition of predatory pricing (ibid., §7.2).
[20] This also was the Supreme Court's opinion: see e.g. *Utah Pie vs. Continental Baking Co.*, 386 U.S. 685 (1967).

Predatory behavior has always been quite hard to assess. Indeed, the basic difficulty is implicit in its definition: how can a court distinguish low prices due to genuine competitive behavior from low prices due to the willingness to eliminate competition? This problem explains why, starting from the 1960s, the predation story became an easy target for the harsh criticism of Chicago scholars, who obviously denied that the observation of a "low" price charged by a firm endowed with market power might ever lead to an accusation of predatory behavior. The attack began with McGee 1958. In that paper, various critiques were raised against the standard story. The two most important ones were, first, that the small firm, with a "small pocket", may well obtain funds from the financial market, thereby resisting against the predation, and, second, that it is far from clear that predatory behavior is the most profitable one for the big firm, given that there may well exist alternative, more profitable strategies (say, a merger).

McGee's paper, which fully reflected the Chicago view of antitrust, has had an enormous impact in the application of the School's approach to antitrust cases. In the words of McGee's 1980 reassessment of his earlier contribution, the key point is that <<… if they are to be broadly applicable, theories of business behavior should concentrate on policies that pay.>> (McGee 1980, 295). In the case of predatory pricing his argument proved that this was *not* the case, that is, that there were no serious reasons to assume that the predator's total future gains – i.e., the motivation behind predation itself – could ever exceed the short term loss caused by the predatory strategy. McGee's conclusion was that predatory pricing, if it ever existed, was quite rare and, thus, undeserving of the attention, and the resources, of competition authorities. Doing otherwise would imply a high number of "false positives", i.e., of wrong condemnations as predatory of simple price rebates, that is, of the first and foremost instance of competing behavior.

During the 1960s and 1970s various proposals came from SCP quarters in order to rescue the predation case from McGee's critiques. These attempts were characterized by a recourse to one form or the other of old-style, cost-based rules, the most significant one being Areeda and Turner's 1975 average variable cost rule.[21] The point is that none of these rules managed to effectively counter the gist of McGee's argument, namely, that there seemed to be no business rationale for the predation strategy in the first place. The dispute was eventually settled by the Supreme Court which, in the famous 1986 *Matsushita* case, embraced the Chicago story and concluded that "…predatory pricing schemes are rarely tried, and even more rarely successful" and that "mistaken inferences in cases like this one are especially costly, because they chill the very conduct the antitrust laws are designed to protect". Thus, "[i]f the factual context renders respondents' claims

[21] According to which a price below reasonably anticipated average variable cost (used as a proxy of unobservable marginal cost) should be presumed unlawful in court (Areeda and Turner 1975, 733). For a brief survey of other rules, see McGee 1980, 304-320.

implausible – *if the claim is one that simply makes no economic sense* – respondents must come forward with more persuasive evidence to support their claim than would otherwise be necessary…"[22] Nails in the coffin of predatory pricing, one might be tempted to say.

However, starting from the late 1970s game theory has offered some convincing explanations of why predatory pricing may, after all, be a sound, profit-maximizing business behavior. As remarked by Motta (2004, 415-6), the common thread in recent models of predation is that such a behavior may be explained only in a context of imperfect/incomplete information, that is, when players have some uncertainty. The key idea is that the predator may try to exploit the prey's less-than-perfect knowledge (or that of the outside investors who finance it) and thus behave so that to make the rival believe that no profits can be made in that industry. As a result, either the prey will exit the market, or a potential entrant will abstain from entering, or its lenders will not be willing to provide the necessary funds. For this manipulation of beliefs to be possible, it is necessary that some uncertainty exist: in a market where all firms have perfect knowledge, McGee's argument is correct and no predation would ever be observed because everybody knows beforehand either that the predator will be successful in its strategy to exclude the rival, who therefore will never costly challenge the incumbent, or that predation will be unsuccessful and thus will never be tried in the first place. Thus, what we have here is an instance of a real life business behavior which is simply inexplicable in a simple perfect information context and which can only find a rationale – and the related possibility of condemnation – in the richer analytical setup of imperfect/incomplete games.

Modern, game-theoretic models of predation can be divided into three broad categories: reputation models, signalling models and financial market models. All date back, in their first versions, to the late 1970s – early 1980s and all share a common tool-box, whose key components are, on the one side, Harsanyi's setup for games of incomplete information and, on the other, the equilibrium concepts of perfect Bayesian equilibrium (PBE) and sequential equilibrium. Let me briefly exemplify this literature by referring to one of its most famous papers, namely, Kreps and Wilson 1982a.[23]

The key idea is that a price war *today* may find its rationale in the attempt to create a reputation of being a strong and aggressive incumbent to discourage *future* entry. Selten's chain store paradox (Selten 1978) had demonstrated that, in case of an entry game, the application of backward induction leads the entrant to always enter and be accommodated by the incumbent, regardless of the (finite) number of times the game is played. Hence, no predation would ever occur in such a game. Note that Selten's result bode ill for the success of game theory: once more, a logically

[22] Matsushita Elec. Indus. Co. v. Zenith Radio Corp., 475 U.S. 574 (1986). The quotations are at pp. 589, 594 and 587.
[23] Other classic papers are Milgrom and Roberts 1982, Benoit 1984, Saloner 1987. Here I follow once more Motta 2004, 416-8.

impeccable game-theoretic reasoning, this time in the form of subgame perfect equilibrium, entailed an implausible description of real world behavior – a result that Selten himself deemed paradoxical as he acknowledged that the desire to build a reputation of toughness capable of deterring further entry seemed to provide a good reason for real world incumbents to prey on entrants. Kreps and Wilson simply introduced some uncertainty on the incumbent's type (i.e., whether weak or strong) and proved that this sufficed to show that predation would occur in a PBE.

Assume there is some probability that the incumbent is *not* weak. A strong incumbent would fight entry by setting a low price: yet, this is *not* predatory behavior, but just a (Chicago-style) manifestation of the incumbent's competitive edge. The key issue, however, is that now a weak incumbent also has an incentive to fight entry by setting a low price: this in order to make the potential future entrants believe the incumbent is a strong one and thus they had better avoid entry. But setting a low price is precisely an instance of predatory behavior: a weak incumbent, in fact, is deemed to lose money from such a low price and its losses can only be justified in view of, and recouped thanks to, the deterrence of future entries. Kreps and Wilson's main insight is clear-cut: even a small departure from perfect information might justify predatory pricing in a finite, but long enough, horizon.

The modern literature on predation is now a standard feature of any intermediate industrial economics textbook. From the historian's viewpoint, one may hardly downplay the role played in this literature by Robert Wilson and his students/colleagues at Stanford Graduate School of Business (GSB).[24] If we pay attention to the fact that Wilson himself has been taught Bayesian decision theory, as a PhD student in the early 1960s, by no less than Howard Raiffa at Harvard Business School,[25] our story comes full circle. Harvard's SCP approach, epitomized by the standard predation story, had clear interventionist implications from the viewpoint of antitrust policy. Both the approach and, later, its policy implications suffered a setback when the efficiency arguments of the Chicago school made their way in the literature and, later, in courts. In the case of predation, these arguments simply claimed that there were no business motivations supporting it. Yet, starting from the 1980s, the rationale underlying old per se prohibitions has been rescued by those game theorists, such as Wilson and the other Stanford GSB scholars, who have applied Bayesian techniques to a long list of business behaviors, including predatory pricing. These were the

[24] The impact of Bob Wilson (at Stanford GSB since 1964) in the development of game theory would deserve a deeper investigation, as is apparent by even a quick glance at the list of those who have been his students, colleagues, or both. To name just a few, Paul Milgrom (PhD Stanford 1979), John Roberts (at Stanford since 1980), Peter Hammond (at Stanford from 1979 to 2007), David Kreps (PhD Stanford 1975), Jean-Pierre Benoit (PhD Stanford 1983), Garth Saloner (PhD Stanford 1982), Claude d'Aspremont (PhD Stanford 1973), Alvin Roth (PhD Stanford 1974), Bengt Holmstrom (PhD Stanford 1978), Bob Rosenthal (PhD Stanford 1969). Some information on Bob Wilson's influence can be found in the 2002 Festschrift *Game Theory in the Tradition of Bob Wilson* available at www.bepress.com/wilson

[25] On Raiffa's role in the diffusion Bayesian decision theory see the interview in Feinberg 2008.

techniques which Wilson had learned at Harvard and which had been first extended to game theory at the turn of the 1970s by Harsanyi and the other ACDA people. From here, it is rather tempting to conclude that one of the crucial factors explaining the 1980s explosion of game theory might have been the opportunity to rehabilitate Harvard's SCP approach and its policy implications, as a reaction against the *laissez-faire*, hands-off approach to antitrust of the Chicago school. This of course does not necessarily mean that game theorists were urging a return to pre-1970s antitrust policies and practices, nor that the rise of the so-called post-Chicago approach to industrial economics was the deliberate outcome of a precise policy view. Indeed, apart from the elegant generalizations of old results, the concrete (i.e., judicial) effect of the new literature has often been meager.[26] My point here is simply that, while in its first three decades of life game theory had been held back by a lack of useful applications, the opportunity to use Harsanyi and co.'s methods and results in such a prominent field like competition theory and policy did open a new era where game theory scholars could profitably (even in a literal sense…) apply their expertise, thereby effectively promoting the discipline's diffusion.

§3. Mechanism design problems

Mechanism design theory (MDT henceforth) provides a unified framework for analyzing the great variety of institutions (such as markets and firms) which allocate economic resources, with an emphasis on incentives and private information and the goal of identifying optimal institutions. More specifically, a mechanism is a specification of how economic decisions are determined as a function of the information that is known by individuals in the economy. The basic insight of MDT is that incentive constraints should be considered as important as resource constraints in the formulation of any economic problem. In every situation where individuals have private information and may perform hard-to-monitor actions, agents must be given the proper incentives to share their information and exert the due efforts. MDT tells us that the need to provide those incentives may impose constraints on the economic system whose relevance is no lower than that of traditional scarcity constraints.

The previous summary corresponds to a modern view of a "mechanism" and of MDT (in fact, it is taken from Myerson 1989). In his 1960 seminal work, Leonid Hurwicz defined a mechanism simply as a communication system in which participants send messages to each other and to a message center, and where a pre-specified rule assigns an outcome, like an allocation of goods, for every collection of messages received (Hurwicz 1960). Such a definition naturally led 1960s researchers

[26] The Chicago approach is still predominant in antitrust courts, especially in the US: see Martin 2007; Page 2008.

to emphasize the "cost side" of mechanisms, i.e., how to design an efficient mechanism from the viewpoint of informational and computational costs. Incentive issues came to the fore only in 1972, following Hurwicz's second seminal contribution, which first formulated the notion of incentive-compatibility (Hurwicz 1972), thus paving the way to the modern analysis of mechanisms where self-interested agents are endowed with private information. A mechanism is *incentive-compatible* if it is a dominant strategy for each participant to report her private information truthfully. In addition, every agent must be willing to participate to the mechanism, that is, participation should not make her worse off. Notably, Hurwicz 1972 demonstrated a negative result, namely, that in a standard exchange economy no incentive-compatible mechanism satisfying the participation constraint can produce Pareto-optimal outcome. In other words, the existence of private information precludes the attainment of economic efficiency.[27]

As is well known, MDT may exhibit a very noble origin. The idea of institutions as means to communicate widely dispersed information, as well as that of comparing different institutions according to their ability to work as communication mechanisms, date back to the 1930s great socialist calculation controversy and, in particular, to Hayek's contribution to the debate.[28] The young Leonid Hurwicz was among the mathematical economists who took Hayek by his word and tried to build a general framework for analyzing the different institutions as mechanisms for coordinating the behavior of individuals in a society. In short, MDT represents no less than the most mature version of the economists' more-than-two-century long inquiry into the problem of resource allocation.

But, what has all this to do with the 1980s boom of game theory? The answer is, quite a lot. As early as 1980, in their JEL survey on the discipline's latest developments, Andrew Schotter and Gerhard Schwödiauer identified Hurwicz's problem and approach as one of the three, institutionally-oriented sources of the new surge of interest in game theory.[29] Generally speaking, MDT defines institutions as noncooperative games and compares different institutions in terms of the equilibrium outcomes of these games. Following Hurwicz's notion of incentive compatibility, and in view of his 1972 negative result, it came indeed natural to other economists to ask whether Pareto optimality could be attained by considering a wider class of mechanisms than exchange economies and by imposing game-theoretic equilibrium concepts less demanding than the dominant strategy one. Moreover, it was rather straightforward to generalize Hurwicz's MDT problem to the question of what kind of mechanism would maximize any given objective function, be it a profit or

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[27] For an assessment of Hurwicz's contribution to MDT see Royal Swedish Academy of Sciences 2007; Myerson 2007.
[28] See e.g. Hayek 1945.
[29] The other two being social choice literature, which today would be classified within MDT, and Martin Shubik's general equilibrium analysis with price-making agents, which on the contrary has had little impact in the following years (see Schotter and Schwödiauer 1980, 480-1).

a social welfare one. Interpreted in the latter sense, the range of potential applications of MDT became practically boundless: to name just a few, MDT has been applied to issues such as the provision of public goods, the design of auctions and public procurements or the regulation of monopolists, and has led to a radical reinterpretation of entire sub-fields, such as firm theory or social choice theory.

The landmark moment for the general applicability of MDT came in the 1970s with the formulation of the *revelation principle*. The principle states that, for any general coordination mechanism, any equilibrium of rational communication strategies for economic agents can be simulated by an equivalent incentive-compatible direct-revelation mechanism (DRM), where a trustworthy mediator maximally centralizes communication and makes honesty and obedience rational equilibrium strategies for the agents (Myerson 2008a). A DRM is a special kind of mechanism where a mediator is assumed to exist who can communicate separately and confidentially with every agent in the economy. Each individual is asked to report all his private information to the mediator who, in her turn, after receiving the reports, confidentially recommend some action to each individual. Any rule specifying how the mediator's recommendations are determined as a function of the reports received is a DRM (Myerson 1989; 2008a). A DRM is said to be incentive-compatible if honesty in reporting to the mediator and obedience to the latter's recommendation is an equilibrium strategy for each individual. Usually, an incentive-compatible DRM can be formulated in terms of a system of linear inequalities which makes it a simple and intuitive mathematical object. Of course, a DRM is an ideal mechanism, with no real world counterpart. Yet, the crucial insight of the revelation principle is that for *any* equilibrium of *any* general mechanism, there is an equivalent incentive-compatible DRM. This entails that, by limiting our analysis to mathematically handy incentive-compatible DRM, we can characterize the outcome of every possible equilibrium of every possible realistic mechanism with no loss of generality. Thus, thanks to the revelation principle the analyst may deal with seemingly intractable problems of mechanism design under conditions of private information by simply investigating the properties of the set of linear inequalities of the corresponding incentive-compatible DRM.

The revelation principle has been first formulated by Gibbard 1973 who followed Hurwicz in the use of dominant strategies as equilibrium concept. The decisive step forward came at the end of the 1970s when several researchers (Holmstrom 1977; Rosenthal 1978; Dasgupta, Hammond and Maskin 1979; Myerson 1979) independently re-formulated the principle, in the case of a purely informational problem (i.e., adverse selection), applying a broader solution concept, Bayesian Nash equilibrium.[30] In the case of a pure moral hazard problem the principle had been already developed in Aumann's 1974 theory of correlated equilibrium. The much-awaited synthesis, i.e., the revelation principle for general Bayesian games with incomplete information with both hidden information and hidden action, came with Myerson 1982. Thus, in a few years the key results of MDT were fully derived and this took place in the context of Bayesian games and with the use of Bayesian Nash equilibrium as solution concept. Since then, mechanism design problems have always been formulated in Bayesian terms, that is, according to the modeling strategy first envisioned by John Harsanyi.

What we have here is a third, very convincing, answer to the question "why the game theory boom?". The exceptional width of applications of MDT and the circumstance that the latter has never been separated, since the late 1970s, from Bayesian game theory speak by themselves. Every economist wishing to contribute to MDT – that is, to any issue in modern microeconomics having to do with private information – had first to learn how to model and solve a Bayesian game. This simple truth is epitomized by what is commonly considered the first intermediate microeconomics manual entirely built around game-theoretic notions, namely, David Kreps's 1990 *A Course in Microeconomic Theory*. What may be found in the third and fourth parts of the manual, after ten initial chapters covering standard material, is a sub-course in game theory and information economics which carries the reader step-by-step towards a glorious finale in Chapter 18, dedicated to "The revelation principle and mechanism design". In short, by 1990 the time was ripe for the author of a successful handbook to make economics students aware, first, that the core of modern microeconomics lay in Bayesian game theory and, second, that what game theory was for was the design of mechanisms.

Three final remarks on this section. First, it is a nice irony of the history of game theory that the discipline's vindication and eventual triumph came from applying its tools to institutional design and analysis. As is well known, a deep interest in institutions was in fact among the main themes and motivations of von Neumann and Morgenstern's *Theory of Games* (see e.g. von Neumann and Morgenstern 1953, 41, 43).[31] Second, among the fields where MDT has been applied we find industrial organization. For example, Baron and Myerson 1982 is considered the foundational paper for the modern analysis of monopoly regulation. This may lead us to conclude that the distinction between our second and third explanation is somehow blurred, since many of the 1980s

[30] Note that, while the authors correctly identified the relation between mechanism design and noncooperative theory, the classic survey by Schotter and Schwödiauer hardly mentioned John Harsanyi and Bayesian equilibrium as the focus was just on either dominant strategy or Nash solutions (see Schotter and Schwödiauer 1980, 480-1, 493 ff.).

[31] Also see Schotter and Schwödiauer 1980, 481-2; Giocoli 2000. Once more, the role played by the sheer transition from von Neumann's difficult and largely inconclusive cooperative approach to Nash's simpler and more effective noncooperative program should never be forgotten: see above, fn.4.

contributions to competition economics which applied Bayesian game theory to provide a rationale for some anti-competitive business behaviors were actually developed within the broad context of MDT. This is apparent when we acknowledge that several of the protagonists of MDT (such as Milgrom, Roberts, Holmstrom, Rosenthal and Kreps) came from Stanford GSB which, as we saw in §3, was also central to the anti-Chicago reaction. Yet, and here is my last remark, if we look at the research center which provided the impulse for the development of MDT two places, other than Stanford, should feature prominently: the University of Minnesota and Northwestern University in Chicago.[32] The former was Hurwicz's home and an obvious formation center for mechanism-oriented economists. The latter was home of the Managerial Economics and Decision Science (MEDS) department at Kellogg School of Management. There, under the leadership of Stanley Reiter, himself a student of Hurwicz,[33] the Center for Mathematical Studies in Economics and Management Science (CMS-EMS) was created in 1971 to bring together economists, mathematicians, and social scientists who used mathematical methods and models. As may be read from the CMS-EMS website: <<A major focus of research at the Center has been the use of game theory in the analysis and design of systems, organizations, and institutions for managing and regulating economic and political activities>>[34]. Even a casual look at the list of the CMS-EMS Discussion Papers (all available on line) in the late 1970s – early 1980s, plus the names of the economists who worked at MEDS in the same period (Myerson, Milgrom, Holmstrom, Roberts, John Ledyard, Mark Satterthwaite), reveals the major role played by Reiter and the MEDS in the development and spread of both game theory and MDT. It is a safe bet to foresee that future works in the history of modern microeconomics will have to focus on the events going on at MEDS.

§4. Conclusion: a clear protagonist and a missing ingredient

Which of the three suggested explanations (refinements, antitrust, mechanism design) is then *the* motive behind the 1980s boom of game theory? Note that the real action in all three cases began in the last third of the 1970s, so much so that sheer chronology is of little help to discriminate among them. My own views have changed through time, moving from the first, to the second, to (more recently) the third explanation, but the real answer is that the question is purely rhetorical since the

[32] With the possible addition of Harvard, where two of the 2007 Nobelists for MDT, Eric Maskin and Roger Myerson, earned their PhD (both in 1976) under the supervision of Kenneth Arrow, who worked there from 1968 to 1979, before returning to Stanford, and who, among his many co-authorship with Leo Hurwicz, published in 1977 the volume *Studies in Resource Allocation Processes* (Arrow and Hurwicz 1977).

[33] Reiter earned his PhD in 1955, at the University of Chicago, under the supervision of Hurwicz.

[34] See www.kellogg.northwestern.edu/research/math

most correct assessment is that the game-theoretic revolution of neoclassical economics has been caused by a *combination* of the three. And if one still insists in requiring that a weight be assigned to the three explanations, the solution can only be found in a quantitative analysis to be performed searching the JSTOR and similar databases. Thus, my concluding remarks are not dedicated to arguing in favor of one explanation or the other. What I'd rather do is to underline who is, to my view, the common, and undisputed, hero of all the three narratives and to highlight the important piece of information that is still missing in order to complete our story.

The common hero is, little surprise at that, John Harsanyi. As I have argued at length above, his 1967-68 three-part paper lies at the foundation of practically everything that goes today under the headline of modern game theory. The sense of sincere reverence and admiration transpiring from the writings that the prominent game theorist and 2007 Nobel winner Roger Myerson has dedicated to Harsanyi bears witness to the latter's achievement. Hence, having paid due tribute to John von Neumann and John Nash as the revered founding fathers of the discipline, it is not an exaggeration to claim that the single most important character in the post-1950s history of game theory has been the 1994 Hungarian Nobelist – a figure who, unfortunately, still awaits a full-fledged intellectual biography capable of embracing both his adventurous life and the depth of his scientific contributions.

The missing piece of information can be summarized in a very simple question: how, when and why did Bayesian rationality become the kind of rationality attributed to economic agents in mainstream economic models? As I have argued in Giocoli 2003, the history of how 20th-century economists have characterized the rational agents populating their models is quite complicated. Among the unsolved puzzles, features that of understanding how Bayesian decision-making, which Leonard Savage had proposed in the early 1950s as a criterion for teaching his *colleague statisticians* how to draw the most correct inferences from their data (Savage 1954), could turn out being a substantial failure in statistics (where most statisticians happily stuck to the classical inferential techniques Savage so openly criticized) and, at the same time, an enormous success in economics. When did rational economic agents become Bayesian decision makers? The question is clearly crucial for our story because the popularity of Harsanyi's game theory could never materialize without Bayesian rationality having already entered the tool-box of a sufficient number of mainstream economists. The fact that rationality considerations could be explicitly extended, in Harsanyi's setup, to cover each player's beliefs about the rivals' beliefs about herself, or, in other words, that players could be modeled as capable of also theorizing, according to rationality criteria, on the other players' thought processes, was *very* far from granted.

Central to Bayesian game theory is the double assumption that rational players *i*) quantify via subjective probability distributions *all* the uncertainty they face, including the actions and beliefs of other players, and *ii*) maximize their utilities subject to these distributions. But this is something that, to make just a name, John von Neumann would have never accepted, as he time and again denied that agents in a game might perform the first activity, let alone the second one. Subjective probability itself was an instrument conspicuously absent from the tool-box of ordinary postwar economists, regardless of de Finetti's and Ramsey's 1930s efforts. Even the success enjoyed by Abraham Wald's "games against nature" as a useful technique for dealing with uncertain decision environments when no objective probability distribution is available (see Wald 1950) shows the distance separating postwar social science from the Bayesian approach. And still, Harsanyi's work did find fertile ground in the late 1960s – early 1970s, so much so that it managed to trigger the game-theoretic revolution. Who had fertilized the Bayesian fields in the previous decade or so?

I have no answer to this crucial question, but a possible research line should, to my view, look at management studies and business schools. Two clues and a general remark lead towards this direction. The clues are, first, the fact that a giant of contemporary game theory like Bob Wilson could learn his Bayesian skills under Howard Raiffa at Harvard Business School, and, second, the fact that Harsanyi's paper was published not in *Econometrica*, but in *Management Science*, hardly the obvious outlet for a highly theoretical piece of economic analysis. The general remark is that the stone guest of the whole history of postwar neoclassical economics, namely, operations research, featured in management Ph.D. programs much more prominently than in economics ones. And, of course, statistical decision theory was a crucial ingredient in operations research teaching. In a nutshell, I believe that a further, interesting question is the following one: did the Bayesian revolution in game and decision theory come to economics from business, rather than statistical, studies? The (in)famous "five-minute science" eagerly awaits its small revenge…

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