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Omniscience and rationality in Microeconomics

by
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It would be very difficult to discuss the question concerning the hypothesis of omniscience in microeconomics without relating this hypothesis to the more fundamental hypothesis of rationality (usually referred to as rationality principle or postulate) which is at the base of the very idea of an economic theory and even social sciences. Indeed omniscience is a quality which was typically attributed to *homo oeconomicus* whose essential characteristic is to be perfectly "rational". This association between omniscience and rationality goes back to the marginalist revolution which progressively brought economists to model economic agents as rational calculators who make each decision by systematically maximizing their utility through the standard application of more or less sophisticated mathematical methods. But since the very idea of such a maximization has no meaning if all relevant parameters and variables are not carefully taken into account, it became relatively common to associate *omniscience* (the required knowledge of such parameters and variables) and *rationality* (the disposition to make decisions which tend to maximize the degree of success in reaching a goal).

Two notions of rationality

However, this association of the two concepts which are involved in the making of *homo oeconomicus,* which was to become the very symbol of economic science, is not a license to confuse them. Consequently, it is very important to underscore that "abandoning the hypothesis of omniscience in economics" does not necessarily mean "abandoning the

rationality principle". The frequent confusion between these two potential movements of rejection stems from the fact that there are clearly two quite different versions of the rationality principle which are referred to by the same name in economic literature. To distinguish them, it will be helpful to refer respectively to "rationality-efficiency" and to "rationality-purposefulness". When a neoclassical economist considers an action rational if and only if it actually maximizes a positively valued magnitude like utility or profit, this economist is referring to what I call rationality-efficiency. And when an Austrian economist considers an action rational if and only if it is oriented towards the satisfaction of the agent's purpose, this economist is referring to what I call rationality-purposefulness. Naturally, these two notions of rationality are closely related: on the one hand, if an agent succeeds in maximizing a positively valued magnitude, it is normally because he or she intended (or had the purpose) to maximize it in order to reach satisfaction and, on the other hand, an action oriented towards the satisfaction of a purpose will at least tend to maximize any positively valued magnitude measuring the degree of that satisfaction. But in spite of their interdependence, these two notions are clearly different, at least insofar as the criteria of their applicability is different.

In the realm of rationality-efficiency, not all actions are rational; only those actions which actually maximize a predeterminate economic value (like profit, revenue, expected utility or possibly happiness) are deemed rational. An action which fails to maximize such a predeterminate positive value can hardly be considered as rational since any action whatsoever would be rational. Since the point of invoking the rationality principle is to predict what will happen when people freely decide to act, there is no longer any point in invoking it if any and all action count as rational. The beauty of the rationality principle is that it permits us to predict that agents (while free to do do what they like) will actually do what is objectively the most advantageous for them, but if those agents, due to their ignorance, are unable to determine the best point to reach, there is no longer any way to predict anything from analysis of their situation. It is in this context that omniscience has been closely associated with rationality.

What I called rationality-purposefulness is a quite different animal. According to this view of rationality, an action is rational when it is oriented towards the satisfaction of any purpose regardless of what it may be. Consequently, an action which fails due to misinformation might be characterized as rational as well as a successful action. Note that this point of view is less counter-intuitive than the opposite one: after all, who could blame an investor for being irrational if, after having carefully considered every available piece of information which looks worthwhile to be considered, he loses money due to a unpredictable move by a competitor which turns out to be very damaging to his investment? In such a view, omniscience is in no way a condition for rationality, but the problem is that any action, since it is by definition oriented towards the satisfaction of *some* purpose, will tend to be characterized as rational. Being unfalsifiable by hypothesis, such a conception of rationality would hardly be of any help in prediction. In such a view, irrationality would not correspond to a lack of efficiency (due to misinformation or to anything else) but to a lack of purpose which means to a situation in which a behavior would literally cease to be an action. In such a view, the mechanical response of a computer, while perfectly efficient, will not be characterized as rational except if it is perceived as the action of its programmer.

Classical economists and rationality

In any case, economists often seem to hesitate between these two opposite and extreme versions of rationality, but it is interesting to observe that, before the marginalist revolution, the rationality principle played an important role in economics without being reduced to anyone of these extreme versions. Rejecting a frequently held view, I claim that, indeed, no more than economic theory as such, the principle of rationality was born with the marginalist revolution. Its systematic application predates this so-called revolution by at least a century. Without going back to Aristotle, it is significant to observe that this principle starts to play a crucial role

when, by the second half of the XVIIIth century, economic theory ceased to be only a moral and political wisdom and became an explanative science.

Let us consider the argumentation of Turgot who, no later than 1766, established that what he called a "current price" *has* to prevail on a market (and not simply *should* prevail, as earlier "just price" theorists would have said): "If one of the wine sellers were offering only four quarts for a bushel, the owner of the wheat will not give it to this wine seller if he knows that another will give him six or eight quarts for the same bushel" (*Écrits économiques,* p. 141). This sentence contains, in a nutshell, the central intuition on which all future price theories were based. The point for Turgot was simply that people are rational in the sense that they are not stupid; consequently it was legitimate to presume that they prefer to get more wine rather than less and that they will take the proper means (or, in the present case, they will make the proper deal) to obtain what they prefer.

It is true that Turgot implicitly referred to a theory of value which was much closer to the marginalist utility theory of value than was the labor (or production cost) theory of value which classical economists were committed to. This fact, however, does not imply that Smith and Ricardo's respective theories owe nothing to the rationality principle. On the contrary, look at how Smith explains why market prices will tend to oscillate around what he called a "natural price"[1]. In this theory, the rationality principle is, in a certain sense, implied by the very idea of a *natural price* which was defined as the price which is just sufficient to bring to the market the quantity of commodities required to satisfy the demand. If he did not suppose that producers are rational fellows, how could Smith be so sure that a higher price would be operative in convincing some producers to produce more commodities and to bring more of them to the market? The role of the rationality principle is even clearer when we consider the way Smith argues that the market price tends to be brought in line with the natural price.

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1 Smith A., *The Wealth of Nations,* book I, ch. 7.

Indeed, he explains that in the event of a supply of a commodity in excess of its demand the market price of this commodity would be below its natural price and that, consequently, either rents, wages or profits in this sector would be below their natural rates. Such a situation, according to Smith, would impel landowners, workers or employers to draw part of their resources away from this market. But how could Smith be so sure that they would react in such a way if it is not because he postulates that people are not stupid enough to keep renting, working or investing if they are not paid an amount which is sufficient to convince them to stay in such an activity? And how could Smith know that such a withdrawal would cease as soon as the price went back to a satisfactory level if it is not because he postulates that people are not stupid enough to indefinitely keep divesting in an activity which becomes more and more in line with their preferences? And how could he know that this withdrawal movement would produce this happy effect for the producers if it is not because he postulates that consumers are not stupid enough to stubbornly refuse paying a slightly higher but still reasonable price when they see that otherwise they can no longer find a sufficient amount of the commodity they need.

Had I chosen the example of Ricardo's economics, it would have been still easier to illustrate this point. Let us take his famous rent theory as an example: how could Ricardo be so sure that the farmers exploiting land of a higher quality would freely accept to pay a rent to their landowner if not because he postulates that these farmers are not stupid enough to take trouble to move to land of such a lower quality that their net profit would not be higher than the profit they presently obtain even after paying the required rent? Even the Marxian capitalists' compulsion to "increase their relative surplus value" according to a "law" of capitalism could not be understood if it was not postulated by Marx that these capitalists are rational enough to manage to be in position to cut their prices and to take away from their competitors a greater and greater share of a strictly limited market in order to maximize their

profit. And when Marx revisits Ricardian analysis, as he did in his transformation theory[1], the role of the rationality principle is still crucial in explaining why prices will diverge from values. Marx's argument indeed implies that capitalists are not stupid enough to keep investing in an industry where the rate of profit does not compare with the rate prevailing in other industries.

My point is not that Smith, Ricardo and Marx have *explicitly* made use of the rationality principle. I am not saying -- that would be absurd -- that they have *analyzed* the functioning of what is going on in the mind of landowners, workers, capitalists and consumers. This type of analysis was not systematically developed before the marginalists. My point is that their totally different type of theory -- which essentially concerned the repartition of production's surplus between classes rather than between individuals, *postulated* such a principle as a necessary condition of intelligibility. But if it is legitimate to characterize the contribution of classical economists in such a way, what was revolutionary about the so-called marginalist revolution? The answer is that marginalist economists required much more from the principle of rationality. They were not content, as were the classical economists, with invoking rationality as a general principle which permits us to understand the working of the mechanism responsible for the fact that the price level (the rent level or the level of any other economic variable) is relatively determinate, they wanted to accurately determine this level with the help of calculus. But the determination of such an exact position presupposes that this exact position exists and that it is relatively stable. In other words, it presupposes equilibrium, but the existence of such a stable equilibrium presupposes not only that the agents are rational but that they are well informed when they decide to act. In other words, this presupposes rationality-efficiency and omniscience. The classical economists, from Turgot to Marx, made frequent use of the rationality principle, but it is not clear whether the kind of rationality which played an important role in their respective theories was conceived as rationality-efficiency or

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1 Marx, K., *The Capital*, Book III, sections I & II.

as rationality-purposefulness. More precisely, this question had no relevance in their case. Because no attempt was made by them to derive precise price levels from an analysis of what is going on in agents' minds, the question of the nature of this mind activity (omniscient computation or purposeful decision) had not been raised.

Claiming in such a way that the difference between marginalist and classical economists is not related to the role of rationality *per se,* but rather to some specific features of rationality which are associated with omniscience, is at odds with the common view according to which the sheer rationality of individual agents was the crucial point. I think nonetheless that I do not diverge so much from the traditional view according to which it is the use of calculus which is determinant. Nor do I diverge too much from the more recent thesis documented by Philip Mirowski according to which the marginalist revolution was mainly characterized by an unrestrained adoption of models provided by physics to analyze economic phenomena[1]. I claim that classical economists postulated that capitalists are rational in managing to make an acceptable level of profit, but in no way that they are engaged in constructing mathematical or almost physical models to characterize the way capitalists would be systematically maximizing profit. True, it is reasonable to claim that marginalist economists as well did not suggest that economic agents use this type of mathematical tool themselves since they rather suggest that capitalists manage to maximize their utility in a way in which the results are correctly mirrored by the use of calculus. However, the point here is not to know how, according to economists, the agents arrived at the decisions they make, but to know what economists consider the essential results of their analysis. And, as is well known, with marginalism, the essential result is the determination of an equilibrium position which is very roughly characterized as a position in which all agents' tendencies to engage in economic activities are adjusted in such a way that it is in no one's economic interest to proceed to any further transaction. As illustrated by the history of attempts to characterize such an equilibrium position and, to start with

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[1] Mirowski, Philip, 1989, *More Heat than Light,* New York, Cambridge University Press.

Walras' magistral first attempt developed with the help of a fictive auctioneer, the hypothesis of omniscience has been the almost unavoidable mean to cope with the disturbing variables which could otherwise impede decisions from being maximizing ones. For marginalist economists, the principle of rationality was the tool to accurately predict the result of economic choices without denegating that the choices were really made by agents who are in principle subjective and free agents. The trick was to postulate that such agents would freely choose the position which is the most advantageous to them. But what is the point of simply postulating, as classical economists did as well as marginalists, that an agent is not stupid enough to keep buying a good at an higher price than necessary, if this agent remains, as it is very probable, unaware of most cheaper opportunities? Such a rationality principle limited by ignorance can help in explaining why an agent makes a move once he learns about such an opportunity, but it cannot help in determining a general solution unless it is further postulated that all agents are aware of every possible opportunity on the market. Alternatively, an agent could discover an opportunity to make gain by selling goods on the market, but if the same agent discovers later that a superior gain could have been made had he restrained from selling those goods at the first occasion, he or she would regret the first move too late. Thus, such unpredictable impetuous moves due to ignorance would destroy the possibility of predicting the final equilibrium, a possibility that the rationality principle was invoked precisely to open. This is the reason why Walras introduced an auctioneer whose role was to make sure that all agents become aware of their best opportunities of gain before getting involved in any transaction. Provided with the help of an auctioneer or otherwise, omniscience quickly became the strict condition for reaping all the benefits of the marginalist revolution.

By contrast, classical economists postulated rationality, but they did not postulate omniscience because they did not intend to calculate such a precise equilibrium level. They did not postulate omniscience because for them the rationality of economic agents does not imply more than a general commitment to their own economic interest without requiring any

particular type of calculation. It is true that, in their scheme, the capitalists had to discover that the rate of interest is higher in some industries than in others, even if this might be difficult to assess accurately. But Smith or Ricardo did not pretend to establish any kind of result which supposes such an accurate assessment. In a sense, their view allowed for some *inexactness* since what was involved was not the instantaneous decisions of rigorous and ultra sensible calculators but a progressive transfer from one industry to another which left ample time for reaction and adaptation. The more or less prompt decisions of various economic agents committed to their own interest while more or less skilful in assessing the appropriate reaction perfectly fitted their scheme and explained that market prices roughly oscillate around natural prices or that rent tends to rise with the exploitation of lands of lower quality. For Smith, higher precision would have been counterproductive and for Ricardo, such precision, while desirable in itself, would clearly suppose that a solution be found first for the fundamental but apparently unsolvable problem raised by the absence of a fixed standard of value in economics.

Abandoning Omniscience but not Rationality

In making this detour through classical economics, I do not mean to propose an apology for classicism against marginalism and still less to suggest that the abandonment of the hypothesis of omniscience, which had been adopted by marginalists, would bring us back to classical economists. One may legitimately think that the hypothesis of omniscience is a dead-end way to analyze what is going on in the actual economic world while continuing to have good reasons to reject the theory of value put forth by classical economists and adopt a theory of value much more akin to the marginalist theory. My point was only to dissociate rationality and omniscience and to suggest that the abandonment of the hypothesis of omniscience does not imply the rejection of the postulate of rationality which was much more fundamental and much more permanent in the history of economic thought.

It is because omniscience associated with rationality-efficiency and with models of general equilibrium turn out to be a dead-end, insofar as economics is supposed to explain how the actual economic world works rather than be either a purely normative theory or, alternatively, what Alexander Rosenberg called a "branch of mathematics" that we are now faced with the abandonment of the hypothesis of omniscience. Probably no one has been more vocal about the necessity of abandoning this hypothesis than G.L.S. Shackle. For Shackle, explaining how the actual economic world works implies making room for time in economics, e.g. making room for a future which would actually be seen as a source of surprise and of uncertainty, a view which, according to him, is clearly incompatible with omniscience. And for Shackle, omniscience is so closely associated with the very idea of rationality that he flatly concludes from this that one has to "reject rationality or time"[1]. In fact, what should be rejected, in such a view, is clearly what I called rationality-efficiency rather than the principle of rationality as such or rather than what I called rationality-purposefulness. This is the position that Shackle implicitly adopts when he recommends "the abandonment of formalisation as an ideal"[2] and when he says that since the subject-matter of economics is "desires and needs, valuations, […], the drawing of inferences from a composite of knowledge and conjecture"[3], this science is "fundamentally, essentially, imprecise and blurred"[4]. Shackle was not the only nor the most influential economist who took such a stand against the peculiar view of rationality characterized by formalization and omniscience which was becoming the trade-mark of neoclassical economics. Some Austrian economists, especially in the XXth century have adopted a very similar position. Mises, Lachmann and Hayek made strong and largely acclaimed objections against pseudo formalization and against

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1 Shackle, G.L.S., 1972, *Epistemics & Economics,* Cambridge, Cambridge University Press, p. xvi

2 See Coddington, A., 1975,"Creaking Semaphore and Beyond: A consideration of Shackle 'Epistemics and Economics', *British Journal of Philosophy of Science,* 26, p. 156.

3 Shackle, G.L.S., 1972, *Epistemics & Economics,* Cambridge, Cambridge University Press, p. 71

4 *Ibid,* p. 72

pseudo-omniscience in economics. In some sense, they have constructed a whole economic theory based on the acknowledgment of ignorance and uncertainty while emphasizing more than any other economist the role of rationality, but rationality understood as rationality-purposefulness.

In such a context, a radical abandonment of the hypothesis of omniscience preserving both the principle of rationality which seems to play an irreplaceable role in economics and the new theory of value which was introduced by marginalism could have been accomplished by the adoption of the Austrian program in economics. But, as is well known, after more than a century of various theoretical accomplishments, Austrian economics remains a relatively marginal tradition even if the hypothesis of omniscience has become less and less recommendable among neoclassical economists. Thus, the problem of abandoning the hypothesis of omniscience is the following. As such, omniscience is perfectly separable from rationality and consequently its abandonment should not be a serious problem for an economic theory based on the rationality principle. But, as we have seen, omniscience is precisely the associate ingredient which made the rationality principle so attractive to marginalist and neoclassical economists since it opens the possibility of exactness and predictability. In this context, the prospect of adopting an approach like the Austrian one which excludes exactness and makes prediction almost impossible even in principle was hardly acceptable by those for whom economics was modelled by the prevailing image of science. Such a prospect was so unacceptable that if the price to pay to avoid it was to abandon not only omniscience but the rationality principle itself, it could still look like a good deal. But is economics possible without a commitment to the rationality principle? This trilemma can be usefully reformulated with the help of some phrases made popular by Alex Rosenberg. On the one hand, the type of economic theory which postulates omniscience and which is mostly illustrated by general equilibrium analysis can hardly be applied to the actual economic world and, in this sense, is nothing but a "branch of mathematics". On the other hand, the type of analysis which is

perfectly illustrated by Austrian economics and which explains economic phenomena by invoking the rationality of human action proposes explanations which are more akin to those provided by "folk psychology" than to those provided by respectable sciences. Finally, since, according to Rosenberg, this last feature is associated with the role of rationality per se (or with the role of intentionality), economists, if they want to maintain their commitment to a progressive and predictive science, have no choice but to abandon the whole of economic theory which is based on rationality and wait for the development of neurology (as the basis of cognitive sciences), on which a really positive economics hopefully could rest.

Modern Economists' Search for an Acceptable Approach

For virtually every member of the economic profession, this would surely be too high a price to pay and for them, the abandonment of the rationality principle as such has not been seriously considered. Actually, it would be very difficult to find examples of genuine attempts at dispensing with the principle of rationality in economic analysis. One of the very few such attempts was offered by Gary Becker in his very atypical paper of 1962 entitled "Irrational Behavior and Economic Theory"[1]. To show that *irrational* behavior could fit as well as rational behavior when it comes to deriving the essential tenets of economics[2], Becker built an economic model featuring two extreme cases of irrationality (i.e. of misadaptation of an action to a situation). In this model, "impulsive" behavior and "inert" behavior correspond respectively to decisions taken at random and to decisions always maintained as identical no

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1 Becker Gary S. (1962), 'Irrational Behavior and Economic Theory', *Journal of Political Economy,* LXX, Feb., pp. 1-13.

2 This thesis gave rise to a brief controversy with Israel Kirzner. See Kirzner, Israel M. (1962), 'Rational Action and Economic Theory', *Journal of Political Economy,* LXX, Aug., pp. 380-385 and the replies from each author: Becker Gary S. (1963), 'A Reply to I. Kirzner', *Journal of Political Economy,* LXXI, Feb., pp. 82-83; Kirzner Israel M. (1963), 'Rejoinder', *Journal of Political Economy,* LXXI, Aug., pp. 84-85. I have analyzed the implications of this debate in Lagueux Maurice (1992), "Kirzner versus Becker: Rationality and Mechanisms in Economics" in *Perspectives on the History of Economic Thought.Selected Papers from the History of Economics Society Conference 1991*, R. Hebert, ed., Aldershot, Hants., Edward Elgar Publishing, 1993, pp. 23-37.

matter how the parameters of the situation might change. With the help of these models and the usual tools of economic analysis, Becker manages to show that constraints imposed by budget lines force such impulsive or inert consumers to limit their behavior to what is compatible with opportunity sets made available to them after any change in price, and that this very fact implies that "regardless of the decision rule used" the average behavior of these consumers will be reflected in a negatively inclined demand curve. Becker very ingeniously tried to establish that other essential theorems of microeconomics can be derived from such models without resorting to any principle of rationality, but in his other papers, which have been much more widely read, he was quick to forget this 'morceau de bravoure', which incidentally was flatly rejected by most of the few economists who referred to it. In any case, similar explicit attempts to dispense with rationality seem to be virtually non-existent in economic literature. Certainly, the various attempts made by economists to draw upon the explanative power of natural selection are possible candidates for consideration, but if most attempts of this type tend to reduce the role of rationality, it is difficult to find one which really denies it insofar as room is made for sensible adaptation, for learning by trial and error and even for deliberate processes. Armen Alchian who is the ancestor of all those builders of Darwinian models in economics insisted so heavily on the complementary character of his approach that it would be a mistake to too quickly associate such models with a rejection of the rationality principle.[1] Clearly, it is difficult to absorb the idea that the science which, since Lionel Robins, was so often characterized as dealing with *choices* made to satisfy wants in contexts of scarcity could be totally independent from the reasons which motivate those who strive to satisfy such wants.

Most economists who have relinquished their once favorite activity, which consisted of building abstract models designed to describe the interplay of omniscient agents, instead of

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[1] See Alchian, Armen A., 1950. 'Uncertainty, Evolution and Economic Theory', *Journal of Political Economy*, 58, June, 211-222; see also Alchian, Armen A., 1953, "Biological Analogies in the Theory of the Firm: Comment", *American Econommic Review,* 43, 3, p. 602.

totally dispensing with rationality, preferred to neutralize the alleged anti-scientific consequences which seem to be associated with both versions of the rationality principle. This has been done in two opposite directions. Rationality-purposefulness has been criticized for being too loosely defined and rationality-efficiency for being too unrealistic. On the one hand, rational action conceived as purposeful is of no help to scientific prediction, because if any purpose whatsoever can satisfy the requirement of rationality, a specific action cannot be predicted on this basis. On the other hand, rational action conceived as efficient is also of no help to scientific prediction, because such efficiency requires hypotheses like omniscience which are too unrealistic to explain what is actually going on in the real world. In both cases, the rationality principle, whose main merit was to make predictions compatible with free human decisions, turns out to be of no serious help in predicting actions taken in the highly complex context of human psychological motivations. The rationality principle has been blamed for this failure either for being a pure general principle whose content is too poor in matter of specific intentions and motivations or for being related to the unmeasurable and slippery world of intentionality and of motivation. Consequently, for those who were looking for a way to solve the problem and to restore the scientific character of economics, it was tempting to either analyze the motivations of economic agents more seriously or, on the contrary, try to avoid as much as possible any reference to intentions and motivations.

In the first direction, a move was taken in order to replace the too schematic rationality-efficiency (which required omniscience) by a more realistic kind of rationality which fit much more closely to the actual behavior of economic agents. The most systematic and consistent theory developed along these lines is probably Herbert Simon's "bounded" rationality theory. Instead of suggesting that economic agents simply maximize their utility, that entrepreneurs especially simply maximize their profits, and that to reach this goal they have to accurately assess (thanks to their postulated omniscience) every variable which might affecting their actions, why not suppose that entrepreneurs are content with profits which look good enough

and start to react in a significant way only when they become aware that these profits start to fall to an unsatisfactory level? This approach is clearly more realistic and susceptible of being developed in a research program based on empirical inquiries and psychological investigations, but resorting to the notion of satisfaction introduces a degree of conceptual imprecision which was considerably at odds with the type of analysis which economists were used to. In any case, it is doubtful that this type of research could generate a genuine economic theory, by contrast with psychological investigations or with a theory of industrial organization. Be that as it may, there is a price to pay for abandoning the hypothesis of omniscience and, in this case, the price was considered too heavy by most economists who did not adopt Simon's more realistic approach.

The other possible solution that I have mentioned looked much more attractive to economists. At first glance, since omniscience was primarily invoked to ensure the possibility of maximizing utility, it would have been sensible to think that the abandonment of omniscience would imply the abandonment of maximization and, as we have seen, this was the road taken by Simon. However, neoclassical economists, who could hardly abandon the idea of maximization without sacrificing the formalization so characteristic of their approach, choose instead to modify the maximandum by introducing uncertainty into it. The gambit was to take expected utility as maximandum, i.e the utility of possible outcomes multiplied by their respective probability. The economic agent is no longer omniscient since he or she doesn't know whether the option taken will provide the maximal utility while nonetheless knowing perfectly that it is the option which, on average, will provide the greatest chance of increasing maximally this utility. There is no question here of denying the significance of the multiple theoretical contributions which are based on expected utility, but as a way of eliminating the hypothesis of omniscience, one has to admit that it a questionable success. As was insistently

observed by Shackle[1], such a distribution of probability implies knowledge of all possible outcomes and all possible circumstances which can affect them in such a way that the maximization of expected utility can hardly require less omniscience that the genuine maximization of utility. Be that as it may, taken in its crude form, the theory of decision based on expected utility met various types of difficulties which are illustrated by a few paradoxes such as Newcombe's paradox where it strongly suggests to be one-boxer even though it is a dominated strategy, or St. Petersburg paradox where it suggests to buy a specific lottery ticket even though it is indisputably a poor and intuitively unacceptable deal. Clearly, another type of formal theory was required to cope with situations involving those more radical forms of uncertainty that are not amenable to subjective probabilities.

In any case, insofar as it tended to reduce the role of the psychological concept of utility, such a theory could be seen as a logical continuation of the trend in which economics has been engaged since the early developments of marginalism. It is well known indeed, that the history of microeconomics has been characterized by a progressive depsychologization of the conceptual basis of utility theory which had been formulated by Jevons with a relatively important psychological content. The substitution of ordinal for cardinal utility did a lot to reduce the role of the mental element in economic theory but other steps had to be taken in this direction. If rationality cannot be serviceable without implying an unrealistic omniscience, why not reduce the technical role attributed to rationality as much as possible ? The point was to develop an economic theory which, without denying the essential role of rationality, would be independent of any kind of subjective process associated with rationality. Here again, it is well known that the crucial step was taken by Paul Samuelson with his theory of revealed preferences. Preferences ceased to be relatively unstable mental states, untestable and virtually inseparable from beliefs and actions, and became perfectly observable features of behavior. In

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[1] See, for example, Shackle, G.L.S., 1966, *The Nature of Economic Thought,* Cambridge, Cambridge University Press, ch. 7 and 8.

itself, this approach had nothing to do with the abandonment of the hypothesis of omniscience, but in the mind of its adepts it provided the point of departure for a well grounded scientific inquiry. In such a context, it looked possible to abandon the hypothesis of omniscience and general equilibrium analysis, without being forced to adopt either an informal comprehensionist approach like Shackle's and the Austrians' or an institutionalist psychological approach like Simon's. In such a context, abandoning the hypothesis of omniscience implied nothing more than making room for uncertainty and risk in the model designed to analyze those revealed preferences. After all, if it is generally admitted that the hypothesis of omniscience must be rejected, it is because it is self-evident that uncertainty and risk are typical to the context in which economic decisions are made. Economists have taken a long time realizing this, but since mid-XXth century, one cannot deny that theories that make room not only for probability but for radical uncertainty and risk have become more and more popular among them.

It seems that game theory is the most popular answer to this theoretical requirement. Game theory uses, as a subsidiary tool, some less problematic elements from expected utility theory, but bases typically its assessment of decision on an analysis of strategies taken as a whole. This implies careful consideration of movements to be expected from opponents in situations which can be exemplified by business competition, warfare as well as any type of game. Since the reactions of opponents are one of the main sources of uncertainty in the economic world, game theory seems at first glance to be a particularly apt way to analyze and even predict the behavior of economic agents without invoking omniscience. While analyzing perfectly rational decisions based on rational preferences, game theorists are typically strongly committed to the idea that they are neither referring to preferences determined by degrees of *utility* nor suggesting that these agents choose to act in a given way *in order to* maximize their

utility[1]. They tend to insist on the idea that preferences are given through agent's observable behavior to which they adopt Samuelson's theory of revealed preferences.

The question of interest for us about game theory understood in such a way is knowing to what extent it is free both from the looseness associated with rationality-purposefulness and from the unrealisticness associated with rationality-efficiency (and, consequently, with the hypothesis of omniscience). On the one hand, insofar as it strictly adopts the theory of revealed preferences and develops a strictly logical analysis of behavior, game theory cannot be accused of the vices which are attributed to rationality-purposefulness. On the other hand, insofar as it proposes strategies which accurately describe the behavior of rational people involved in the most concrete games and situations without providing them with any unrealistic knowledge, it seems that it can hardly be accused of cheating by grounding perfect efficiency on the hypothesis of omniscience. However, matters becomes more complex when one realizes that game theory tends to be more than a theory of games *strictu sensu*. Game theory tends to be a general theory of human behavior and it is precisely due to this larger scope that this theory is considered here.

This point is important because by definition a game is nothing but a set of rules which are supposed to be perfectly known by all players. When Shackle objected to the use of probability in economics because it implies knowledge of all possible outcomes, he would have been wrong had he referred to games because, due to the fact that they are defined by a set of rules, games -- even highly complex games like chess -- are reducible to a number of possible outcomes which are determinable at least in principle. But he would have been right had he refer to normal economic activity which, not being a game defined by a finite set of rules, is not reducible to a determinate number of possible outcomes. Thus, treating an economic problem as a game is somewhat similar to *providing agents with information* about

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[1] See Binmore, Ken, 1992, *Fun and Games*, Lexington, MA, Heath and Co, pp. 97-98.

real life situations equivalent to the information that players in a game *strictu sensu* get from the rules. Another difference between games and usual behavior is crucial: players by definition are engaged in a situation where only one goal is considered, namely winning. Cheating and complacency are not considered when game theory is applied to a game *strictu sensu* and does not need to be considered since, with this type of behavior, the game would be auto-destructive. But with general human behavior, if we except a few atypical situations like direct confrontation in war or stock exchange activity, usual situations are much more complex and involve typically agents who are far from being guided solely by a single goal. Incidentally, it is because profit cannot be considered as the single goal of capitalist entrepreneurs that Simon's approach make sense. Thus, treating an economic problem as a game is somewhat similar to *providing agents with information* about their opponent's goals equivalent to the information that players in a game *strictu sensu* get from the singleness of goals. Consequently, in the context of games, it makes sense to postulate that expert players have common knowledge of the rules of the games, of the opponents' goal, and of all the logical consequences of this knowledge. However, in the context of real life situations or of economic situations, such a postulate becomes a highly artificial hypothesis which is not so far from the hypothesis of omniscience adopted by general equilibrium theorists.

The problem is aggravated when we consider some implications of revealed preferences theory. Indeed, this theory postulates a kind of rationality which is neither rationality-efficiency nor rationality-purposefulness but what can be called "rationality-consistency". According to this view of rationality, preferences have to be stable and transitive. Although this is usually presented as a weak requirement, it is extremely strong. On what bases can we postulate that preferences have to be transitive? To borrow an example of Alex Rosenberg, it is clear that it is not irrational to express "a preference for caffeinated coffee over milk at breakfast,...milk over decaffeinated coffee at lunch, and the latter over caffeinated coffee at

dinner"[1]. Naturally, it is not irrational because circumstances change with the time of the day; but we may ask, along with Rosenberg, why it would be more irrational to prefer caffeinated coffee to milk on Monday, milk to decaffeinated coffee on Tuesday and the latter to caffeinated coffee on Wednesday. Tastes can change, and the only way for revealed preferences theories to avoid reintroducing a discussion of tastes which the theory was designed to avoid is to established that it is a complex pattern of successive and interrelated preferences which is revealed. Rosenberg correctly observes that such an *ad hoc* structuring of highly artificial complexes of preferred commodities makes the theory untestable, but Shackle would also have observed that by systematically eliminating change in tastes, it systematically eliminates time from the analysis, just like general equilibrium theories did.

It is true that rationality-consistency which is postulated by revealed preferences theory does not reintroduce anything like omniscience, but it reintroduces a kind of mechanistic rigidity which was criticized by Shackle for being incompatible with time. But why is it so important to make room for time in economics? It is important because human beings who are involved in economic activity are typically engaged in choices which are made complex due to uncertainty which time is mainly responsible for. But since it is impossible to analyze economic choices without postulating that they are guided by rationality, what is wrong with modeling this rationality by postulating a mechanistically enforced transitivity? It is wrong for reasons very similar to those for which modeling rationality by postulating an efficiency which is enforced through a quasi mechanistic omniscience is wrong. In both cases, the modeling has transformed an economic world which is characterized by uncertainty, unexpected consequences, guess-based adjustments and "animal spirits" into a mechanical and robotic process whose principal merit is that it fits pretty well into a manageable mathematical algorithm.

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[1] Rosenberg Alexander, 1992, *Economics - Mathematical Politics or Science of Diminishing Returns?,* Chicago, The University of Chicago Press, p. 121.

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As we can see, the problem is not so much with omniscience as such, but with this type of modeling. It is a problem because such modeling is normally designed to be tested. There would be no problem indeed with postulating omniscience or any other artificial way to arrive at tractable results if this device permitted successful prediction and if it was a step in the progression towards increasingly satisfactory explanative models. But, since it would be difficult to characterize the theoretical constructions of economists in such a way, it is reasonable to become less permissive when the gap continues to increase between heroic mathematical models and the human activity that these models are supposed to describe and when the main merit of these models tends to be the merit of being an interesting "branch of mathematics". This is the reason why hypotheses like the hypothesis of omniscience tend to be abandoned. But when they are abandoned, economists must find other paths. They might choose to develop, as Simon did, more concrete models which open new avenues for collaboration with other social sciences but drastically limit the prospect of further developing the high theory which has been the glory of economics. Alternatively, they might choose to develop new models like those associated with game theory which tend, however, to reintroduce a postulate very akin to omniscience when they pretend to describe not only specific phenomena like games *strictu sensu* but also economic activity as such. Finally, they might also, like the Austrians, choose to base informal analyses of economic phenomena on a larger understanding of rationality, an approach which can be illuminating without offering very convincing prospects for the development of the progressive science that most economists have in mind. If the hypothesis of omniscience was something like a marvelous dream, it is hardly surprising that its abandonment engenders various perspectives which, while being more appropriate to the analysis of actual economic world, are naturally much less enchanting.

Résumé:

L'hypothèse d'omniscience a joué un rôle important en micro-économie, même si l'on trouve de plus en plus gênant d'y recourir explicitement. Cette hypothèse ayant souvent été associée au principe de rationalité, lequel est beaucoup plus fondamental pour toute réflexion économique, deux façons de concevoir ce dernier principe sont distinguées en vue de dissiper cette confusion. Une discussion de la façon dont les économistes classiques ont eux-mêmes recouru au principe de rationalité sans pour autant invoquer l'omniscience vient éclairer ces distinctions. Les principales critiques adressées à l'hypothèse d'omniscience, en particulier celles véhiculées par G.L.S. Shackle sont ensuite examinées et quelques unes des avenues empruntées par les économistes du XXe siècle (économie autrichienne, pragmatisme d'Herbert Simon, théorie de la décision, théorie des jeux) sont discutées et comparées du point de vue de leur aptitude à développer une analyse théoriquement féconde sans recourir à des hypothèses aussi peu réaliste que l'hypothèse d'omniscience.