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Quantitative d'Aix-Marseille - UMR-CNRS 6579
Ecole des Hautes Etudes en Sciences Sociales
Universités d'Aix-Marseille II et III

Document de Travail

n°2004-11

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Realisms, Realisticness and Experimental Economics

(Mars 2004)

DT-GREQAM

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ABSTRACT: This paper discusses the critics of unrealism and artificiality of economic experiments by exploring three kinds of realism. The standard justification of experiments settles for "empirical realism" and does not imply any kind of realisticness in the laboratory in assuming the universality of theories. A "local realist" stance, by postulating the validity of theories in a domain of application, offers the possibility to create this applicable domain in the laboratory and so to have an impact on theory. Finally, the "critical realist" position against experiments lying on the specificity of the conditions of the laboratory is weakened. In view of these positions, it seems inconsistent to unconditionally reject laboratory critics of theories.

Key words : Realism, parallelism, experimental economics, methodology.

JEL Classification : B4, C9.

RESUME: Cet article explore les critiques d'irréalisme et d'artificialité faites à l'encontre de l'économie expérimentale en explorant trois sortes de "réalisme". La justification standard des expériences repose sur le "réalisme empirique" et n'implique pas un quelconque réalisme des paramètres du laboratoire, en supposant que les théories sont universelles. En partant du principe que les théories sont valides dans un domaine d'application, un "réalisme local" offre la possibilité de créer un tel domaine dans le laboratoire et ainsi de tester les théories. Enfin, la position du "réalisme critique" contre l'expérimentation en sciences sociales, qui repose sur la spécificité des conditions du laboratoire, est relativisée. Au vu de ces positions, l'article montre que les critiques expérimentales des théories ne peuvent être rejetées inconditionnellement.

Mots clé : Réalisme, parallélisme, économie expérimentale, méthodologie.

Classification JEL: B4, C9.

Realisms, Realisticness and Experimental Economics

After 50 years of existence, and despite a lot of reluctance during its fledgling stages, a growing number of economists begin to accept experimental economics. Today, the *a priori* that considers economics as a non-experimental science is less and less widespread. But this enthusiasm for the laboratory - characterised by a continual increase in experiments published in the different economic reviews and a Nobel Prize for one of its pioneers, Vernon Smith - is far from being enough to erase all scepticism. As Smith states: “ Those of them who do experiments in economics think that we have helped to transform economics, in part, into an experimental science. I would expect, however, that an opinion poll among all economists would show a substantial majority replying “no” to the question [is economics an experimental science?]” (Smith, 2002, p.94).

Experimentalists are often accused to produce results which are not transferable outside the laboratory (e.g. Nelson, 1998; Loewenstein, 1999). This concept of appliance of experimental results in the “wild” world, better known as external validity or parallelism remains the Achilles’ heel of experimental economics (Loewenstein, 1999). And despite the importance for experimentalists to defend their discipline via such a concept, they stay silent, preferring to surf on the success wave: “problem of parallelism (or ‘external validity’) is the most important methodological problem of experimental economics. It is also one of the least discussed problems, despite the fact that much prejudice against experiments stems precisely from the suspicion that their results cannot be ‘exported’ outside the laboratory” (Guala, 2002, p.261).

Briefly, the critics argue that the gap of social setting between the laboratory and the wild world (either the particularity of the laboratory social setting or the absence of the social setting of the “wild” world in the laboratory) makes the experiment artificial or “unrealist” and does not permit to test theories[1]. To discuss such critics and to shed light on the role of social setting in the laboratory, I will apply the distinction of Mäki between realisticness and realism (Mäki, 1998, 1989) to this problematic of experimental economics. Whereas for Mäki the realisticness refers to the descriptive validity of the assumptions of a model, it will refer here to the adequacy between the situations present in the laboratory and in the wild world;

realism still referring to the philosophical thesis. As within the works of Mäki, I will show that there is not a necessary relationship between those two features.

Indeed, to justify the external validity of their results, experimentalists settle for the traditional defence of experiments in economics (Plott, 1982; Smith 1976, 1982) that lies on an empirical realist epistemological position that does not necessitate the realisticness of the social setting of the laboratory. But it is far from being the only philosophical justification of the use of the experimental tool. What is then the link between other forms of realism and the necessity of having a "real" social setting in the laboratory? Of course it is impossible to deal with all other philosophical positions. I will discuss here just two others: Francesco Guala's (1999) and Nikos Siakantaris' (2000) who have both questioned the Plott and Smith traditional defence of experimental economics. Guala, by referring to Grether and Plott (1979) develops the view that even blocks of experiments cannot systematically condemn a theory. Siakantaris is more unconditional: he argues that experiments are not of a great use in social sciences.

In this paper, I will bring together these three different points of view on the use of experimental economics and compare their different ontologies regarding the *Way the World Work*s (Mäki, 1999). These three conceptions of experiments refer to three different kinds of realism, which have their own vision of theory. The standard justification (empirical realist position) views theory as general and therefore the laboratory as one of the places where to test the theory that does not reclaim any kind of realisticness (Section 1). A localist realist stance (Cartwright, 1983, 1989, 1999) supposes theories to have their own domain of application. In this last case the role of the experimenter should be to find a connection between the social setting of the laboratory and the one of theory's applicable domain (Section 2). Finally, I will discuss the transcendental realist argument of Siakantaris prohibiting experimentation in social science because of the impossibility to have any bridge between the laboratory settings and any other situation, and weaken his epistemological statement against the use of the laboratory (Section 3).

1. Empirical Realism: the Plott-Smith justification of EE.

The most common justification of the use of the experimental tool can be attributed to Charles Plott and Vernon Smith. These are the two economists most often quoted in experimental papers when authors refer to some methodological issues on the use of experimental economics. Their positions present some differences. But these differences are more on the insistence of some parts of argumentation than on some fundamental differences in the way to look at experimental economics. Whereas Plott insists on some methodological (logical) considerations, Smith tries to go further and flirt with the deep water of ontological considerations. In this section, I will develop the thinking of these authors in a complementary way rather than to try to oppose them.

1.1 Standard methodology of experimental economics

The Plott-Smith position is associated to empirical realism, even if there have not been any explicit affiliation to such philosophers in their methodological papers. Smith quoted a few times Popper and Lakatos[2] and while he has done specifically an article on Lakatos and experimental economics, the references to Lakatos appear just in the title, the content being not philosophical. The justification is the following: "We are theoretical experimentalists, not methodologist-philosophers, and we are not competent to appraise Lakatos. In the pages to follow we provide a practitioner's perspective" (Smith and al., 1991, p.201). In the methodological papers of Plott, no references to such philosophers are present. This is surely due to the disregard of Plott to methodological consideration. As he states "economists should keep an open mind about experimental methodology and should judge work by the statement of results rather than by methodological principles" (Plott, 1987). Despite the absence of explicit philosophical references, I will show that their skeptical attitude towards knowledge and their ontological position refer to empirical realism.

The position of Plott and Smith seems the easiest (among the sensible) way to promote experimentation in economics. And, following Plott, this simplicity could be seen as paradoxical because of the traditional reluctance about the laboratory in economics: "Naturally occurring economic processes are so complex that the complete experimental control with multiple replications defies the imagination. Yet, despite of that seemingly insurmountable obstacle, the methodological posture taken by experimentalists is straightforward" (Plott, 1986, p.732).

This insolent position regarding the two centuries during which economists have kept the laboratory in the dark stand in one sentence: theories are to be general and so have to apply to the special cases of the laboratory. This position lies on the following difference between theories and experiments.

On one hand, theories are general. They "involve sweeping statements about broad classes of phenomena" (Plott, 1991A, p.89). They are supposed to apply in every cases likely to happen without any restrictions due to particulars: "for example, the law of supply and demand and the resulting notion of an equilibrium price are supposed to be characteristic of markets regardless of the item that is being bought and sold" (Plott, 1991A, p.89)[3].

On the other hand, experiments realized in the laboratory are special cases. A usual critique[4] of experimental economics that experiments doesn't reflect the "outside" world lies on a misconception of the experiment. Experimental economists do not want at all to mirror the wild world[5]. No sensible experimentalist could pretend that the situation in his experiment copies exactly the situations that occur in the nature. The phenomena created in the laboratory are of course less complex.

What experimentalists wish to do is just to stage the theory. And the theory can be staged in the natural (rich) world, as well than in some simple situation. Staging the theory in a simple setting is more convenient because of matter of internal validity: it permits the experimenter to have a better control over the different variables. So the experiment should be at least "as *rich* as the theories they test" (Smith, 1980, p.350). If the variables "are not parameters of the theory, then the criticism of "unrealism" applies equally to the theory and the experiment" (Smith, 1982, p.937).

Moreover, this simplicity comparing to the wild world does not take away any value for the experiments. Indeed, even if the experiments are really simple compared to phenomena which occurs in nature, "they are just as real" (Plott, 1991A, p.905). In a laboratory real people earn real money in a real situation (Plott, 1982, p.1520[6], 1991, p.905[7]; Smith, 1976, p.275[8]). So, what happens in the laboratory is a real situation; the experiment is just a special case with the particularity to happen in a "controlled wild".

Finally, theories which are general should of course work for special cases: "General theories intended for application in complex markets should be expected to work when applied to the simple special case" (Plott, 1986, p.732). We have seen that the laboratory is at an intermediate stage of complexity between the abstract theory and the "wild world". But what is important is that the laboratory is more concrete than the theory. So, the laboratory has to be a domain of application of the theories that pretend to be general: "The experimental laboratory […] consists of a far richer and more complex set of circumstances than is parameterized in our theories. Since the abstractions of the laboratory are orders of magnitude smaller than those of economic theory, there can be no questions that the laboratory provides ample possibilities for falsifying any theory we might wish to test" (Smith, 1982, p.936). Indeed, since what happened in the laboratory is a special case of what happens in the wild, such a single case cannot be used to infer a theory (induction), but can be used to test it. It consists in the popperian falsificationism: "[…] universal theories are not deductible from singular statement. But they may be refuted by singular statements, since they may clash with descriptions of observable facts" (Popper, 1976, p.86). That's why such a single case does not need to be as rich as the real world, but need at least to be as rich as the theory.

So, according to these authors, experiments realized in the laboratory can be a tool to test some theories. The test of a theory consists in "comparing its message or its outcomes with the experimental observations" (Smith, 1994, pp.113-114). The more the theory is in accordance with the observation, the "better" (in the sense of more powerful) is the theory.

What to do when a theory seems to be rejected by experience? The first point is that one test is far from being enough to assert that a theory does not give results according to observation. This is due to the awareness by the experimentalists of the Duhem-Quine problem. The Duhem-Quine underdetermination thesis asserts that a theory can never be tested in isolation. We have to add auxiliary hypothesis to stage the theory in the laboratory. The population chosen for the experiments (gender, nationality), the structure of the stake (high or low stake), the place where the laboratory is are auxiliary hypotheses that we test in the same time with the theory. So when the predictions of the theory is not in accordance with the observation, we can not know if it is due to some failure in the theory or in the auxiliary hypothesis. Experimental economists can not ignore the underdetermination thesis. But should they stop experimenting because of it? Surely not. The best cure against the Duhem-Quine problem is to multiply experimentation and to try to see if the results are robust when varying the auxiliary hypothesis. By doing such a kind of sensitivity analysis, we test and even measure the effect of auxiliary hypothesis in order to diminish the Duhem-Quine critics (Sawyer and al., 1997).

So to obtain a set of experimental results, we need to replicate experiments[9]. The methodology of experimental economics has done everything to permit everybody to replicate results. All details of the protocols are included in the papers or are available upon request. Furthermore, the practice of experiments is homogeneous among all the experimental economics society. In this way, experiments can be reproduced. Of course, for institutional reasons due to publication objectives, the experiments are never exactly replicated. But in introducing new treatments, experimentalists use to do control treatments, which consist generally of the replication of former experiments. In this way, it is possible to obtain a set of experimental results that contradicts the theory.

Once this set of experimental evidence is obtained, we can judge the theory. Plott suggests an alternative for the theories that do not correspond to the observations of the laboratory's special cases: "As models fail to capture what is observed in the special cases, they can be modified or rejected in light of experience". So, experiments are mainly useful to reject or improve theories. The rejection happened when the "model may be so poor at capturing observed behavior" (Plott, 1982, p.1520). Otherwise, the theory should be refined in order to integrate the new observations; the theory should be theoretically progressive, that means that the theory should have empirical excess content over its predecessor (Lakatos, 1978).

If the theory is not rejected, i.e., the observations of the laboratory do not contradict the predictions of the theory, the theory is provisionally accepted ("corroborated" to use the term of Popper). But the work of the experimentalist does not stop there. Indeed, once a theory hasn't been rejected by experimental or other empirical studies, it is "thereby established as a behavioral law *with some claim to generality*" (Smith, 1982, p.942, my emphasis). This is the role of the experimentalist to do more experiments to find the limits of the theory. That's what Smith calls "boundaries experiments" in reference of the types of experiments made by Kaplan (1964, p.150).

The role of the boundary experiment is to find a better theory by the way of new extensions. This corresponds to the skeptical attitude of the empirical realists: "only tests undertaken in a critical spirit – attempted refutations – should count" (Popper, 1976, p.103). Indeed, according to Smith, "the hallmark of science is to be found in a constructively skeptical attitude toward knowledge" and this attitude is "the principal contribution of Popper's falsificationist methodology" (Smith, 1976, p.265).

The goal of this principle of theories' refinement is to go closer to the truth even if we can never reach it. But what is important is that the truth exists and we can observe it. This ontology of the social can be seen principally in the notion of parallelism of Vernon Smith.

1.2 Standard epistemological justification of experimental economics

Smith defines parallelism as follows: "propositions about the behavior of individuals and the performance of institutions that have been tested in laboratory microeconomies apply also to non laboratory microeconomies where similar *ceteris paribus* conditions hold" (Smith, 1982, p.936). Smith considers that his proposition is equivalent to that quotation of Shapley : "As far as we can tell, the same physical laws prevail everywhere" (Shapley, 1964, p.43, quoted by Smith, 1976, p.274, 1980, p.349, 1982, p.936)[10]. Then, parallelism means that each law has the pretension to be universal. Every behavioral law is the same for every human all around the world and for every time. From a practitioner perspective, if we isolate some conditions that lead to some consequence, and if these conditions are again gathered together whatever the time and wherever it takes place, the same consequence will happen, when *ceteris paribus condition hold*. The *ceteris paribus* clause should be interpreted here as the same relevant causally things acting in both contexts (lab and real world). The correspondence between the laboratory and the real world (represented by the concept of external validity) is thereby established. And because of this correspondence, the relevance of the laboratory is ensured.

Of course some gap between the laboratory and the wild world – some unrealisticness - remains. It leads to two critics of experimental economics: experiments are *artificial* and *unrealist*.

When we perform an experiment, the environment (constituted by the laboratory) around the selected conditions is not ignored and can have an effect on the consequence we will observe. This corresponds to the critics of *artificiality* of the laboratory. But as it has been explained before the laboratory need not to be a replication in miniature of the " outside world " but just a place where the theory can be staged. According to Smith (Smith, 1976, 1982), respecting four precepts - *nonsatiation* (utility is a monotone increasing function of the monetary reward), *saliency* (reward depends on the actions of the subject), *dominance* (the rewards structure dominates any subjective costs) and *privacy* (each subject is given information only on his own payoff alternatives) - is enough to observe a real microeconomy as postulated in standard theory. So the laboratory is one of the several places where we can observe economic behavior.

Similarly, some conditions acting in the “wild” world are not present in the laboratory, that’s why experiments are viewed as *unrealists*. This environment can have an effect but:

- Either these environmental conditions (that are not pertinent for what we want to test) are not a part of our analysis and merit some other experiences on itself (boundaries experiments). For example, it is possible to study the life expectancy independently of the fact that the subject is a smoker or not. That does not mean that cigarettes have no impact on life expectancy but it can not be a part of our analysis.

- Or these conditions can be a part of our analysis and we should reconsider the pertinent conditions (from an experimental as well as a theoretical point of view) linked to our behavioral law. For instance, if we study the falling of the tree’s leaf depending on the age of the leaf, it would be pertinent to integrate the power of the wind who is one of the principal causes of it. If this parameter is present in the theory or *implicit* to it, the power of the wind should figure in the experiment. Otherwise, this parameter should be integrated in the theory and so in the experiment. Others studies (field experiments, others laboratory experiments, cases studies, psychological, sociological, anthropological studies …) can help us to determinate the causally significant powers.

These annex factors to the conditions of our experience, even if they can modify our result, do not condemn our experience. Indeed, to avoid a chaotic situation we have to postulate that minor change in the environment will induce minor change in the results. So, despite such variation in the context, the results will be qualitatively the same: “whatever the context of the particular microeconomy – the laboratory (using induced values), the primary market for U.S. Treasury bills, or the auctioning of scarce job interview slots among Chicago Business School graduates whose bids are denominated in “points” and constrained by a fixed endowment of such points – parallelism says that the incentives effects of different bidding rules are *qualitatively the same*; if rule A produces lower bids than rule B in one market, it will do so in other markets” (Smith, 1982, pp.936-937, my italics). This continuity in the consequence of causes insures the experimentalist to always obtain some relevant results when they take into account at least only the parameter of the theory.

Here is the standard argumentation for experimental economics. This justification lies on a traditional positivist philosophy. Same cause, same effect. But recently few authors have developed alternative realist ontologies that furthermore can be found in experimental economics literature. That's the case of Nancy Cartwright's localist realism.

2. Localist Realism: The Grether-Plott scope's reduction of experimental economics.

In several books and papers Nancy Cartwright has recently criticized this fundamentalist stance. After having briefly recapitulated Cartwright's philosophical position, I will apply it like Guala (1999) to the Grether and Plott preference reversal analysis (Grether and Plott, 1979) and see the application of her stance to experimental economics.

2.1 Cartwright position

In the Millian tradition, there are two contemporary philosophical approaches which are quite close to each other: Nancy Cartwright's (1983, 1989, 1999) and Dan Hausman's (1992). We choose to develop here the former because Hausman is merely descriptive about how economics is practiced; he doesn't develop a general epistemological view. The powerfulness of Cartwright's thought is precisely to have developed an alternative philosophy to the humean received view[11] even though her approach is also naturalist. This last point will facilitate our transversal studies of realism related to experimental economics. The difference between these two kinds of realism is the following one. In opposition to the fundamentalists for whom laws of nature are basic and everything can be explained as an account of them, Cartwright develops a more Aristotelian approach where "fundamental laws are not about what things do but what it is in their nature to do" (Cartwright, 1999, p.82).

Indeed, according to Cartwright there exist more essentialist entities: capacities that can be assembled in infinite (endless) ways to produce laws. Science has to study capacities, that are the factors that will act if unimpeded. Capacities have three elements: potentiality (what a factor can do and not what it really does), causality (capacity claims are about what results a factor can produce) and stability (the potential to produce the effect is robust over some variation in circumstances) (Cartwright, 1998). Capacities are present in things and are responsible of the causality we find in science: "*Aspirin relieve headache*. This does not say that aspirins always relieve headaches, or always do so if the rest of the world is arranged in a particularly felicitous way or that they relieve headaches most of the time, or more often than not. Rather it says that aspirin have the capacity to relieve headaches, a relatively enduring and stable capacity that they carry with them from situation to situation; a capacity which may if circumstances are right reveal itself by producing a regularity, but which is just as surely seen in one good single case" (Cartwright, 1989, pp.2-3). So the causal claims of science come from the understanding of the capacities. But when are they producing a regularity?

To produce a regularity, capacities have to be assembled in order to constitute a nomological machine. A nomological machine is "a fixed (enough) arrangement of components, or factors, with stable (enough) capacities that in the right sort of a stable (enough) environment will, with repeated operation, give rise to the kind of regular behavior that we represent in our scientific laws" (Cartwright, 1999, p.50). Cartwright calls these regular behaviors: law-like regularities. So, laws-like regularities are the result of a nomological machine. Depending on when the machine is at work, the regularities will be observed.

As nomological machines work in special circumstances theories are domain specific, contrary to the standard empirical scientific view. In the so-called fundamentalist approach, laws are universal: unconditional and unrestrictive in scope. For Cartwright, laws are local: "Laws can be true but not universal. We need not assume that they are at work everywhere, underlying and determining what is going on. If they apply only in very special circumstances, then perhaps they are true just where we seem them operating so successfully – in the artificial environment of our laboratories, our high-tech firms, or our hospitals. I welcome this possible reduction in their dominion; but the fundamentalist will not" (Cartwright, 1999, p.37). Laws are not universal anymore. There is now a multitude of them: "Reality may well be a patchwork of law" (Cartwright, 1999, p.34).

To underline her argument, Cartwright takes the example of the Newton's and Coulomb's law. What is the force exerted by two charged bodies? The Coulomb's law of electrostatic attraction and repulsion says that between two bodies of charge q_1 and q_2, the force exerted is $F=\frac{q_1 q_2}{4\pi\varepsilon_0 r^2}$. But the bodies do not actually take this force. Indeed, they are also subject to the law of gravitation. The law of gravitation says that between two bodies of masses m_1 and m_2, a force $F=\frac{Gm_1 m_2}{r^2}$ is exerted. Does it mean that the Coulomb's law is a false one? According to Cartwright the response is negative[12]. When the right nomological machine is at work for the Coulomb’s law (especially when the two bodies have “very small masses so that the gravitational effects are negligible” (Cartwright, 1999, p.82)), it is in the nature of the bodies to exert such a force. And it says even more: even if the bodies are subject to gravitational force, the charged bodies will have the tendency to exert such an electrostatic force.

2.2 Apparition of this stance in experimental economics

In economics, Grether and Plott (1979) have expressed this view that limits the scope of economic theory. Indeed, psychologists have found an anomaly that radically contradicts the economists’ vision of preferences. This phenomenon is the so-called *preference reversal*. The subject is faced with two different tasks. The first is to choose between two bets with the same expected monetary value. The first bet, the “P-bet”, offers a huge probability of winning a small amount of money, whereas the second bet, the “$-bet”, offers a lower chance to win a larger prize[13]. Most of the subjects choose the P-bet. But when subjects are asked to price the two gambles, most of them put a higher price on the $-bet. This phenomenon is highly problematic for economic theorists because it throws some doubts on preferences as considered in economics.

Beginning with Grether and Plott (1979) who first discussed this phenomenon in economics, economists tried to find a rational explanation for the preference reversal phenomenon.

After analyzing the responses to the preference reversal phenomenon, Guala concludes “when an experimental phenomenon is particularly indigestible, conservative scientists tend to insinuate that it cannot occur in the intended domain of a theory” (Guala, 1999, p.561). The defense strategy against the psychologists was to limit the scope of economics. By doing so, they also limit the role of experimental economics. Indeed, laboratory experiments cannot systematically test a theory. If we consider that economic theories have their own domain of application, for testing a theory, the laboratory must be in the domain of application of the theory.

2.3 Testing theories?

As it has been seen in the introduction, a purpose of the experiments is to test theories. If we adopt this philosophical stance of reduction in the scope of economic theories, is it still possible?

A first problem is to find an intersection between the laboratory and the domain of application of the theory. Can the pertinent features of the theory's domain be introduced in the laboratory setting? We can legitimately think that the answer is positive. Indeed techniques in experimental economics have considerably improved and now it is nearly possible to integrate every factor.

Let's take an example of what can be introduced in experiments: Schotter and Sopher's intergenerational transmissions (Schotter and Sopher, 2001A, 2001B; Chandhuri and al., 2001). First this is a revelator of what is done now by leading researchers in experimental economics. Secondly, the way the authors introduce this parameter refers to the methodology developed in this section. The desire of the authors seems to capture essential features on what happen in a typical situation like the creation of convention in the wild world: "our motivation for studying such games comes from the idea that while much of game theoretical research on convention creation has focused on the problem of how infinitely lived agents inter-act when they repeatedly play the same game with each other over time, *this problem is not the empirically relevant one*" (Schotter and Sopher, 2001A, p.2, my italics) or also "it is our claim in this paper that in the real world games […] are played in a manner that differs from that depicted in previous experimental studies" (Chandhuri and al., 2001, p.2).

So how to integrate in an experiment intergenerational transmissions? The idea is the following. A first generation "the progenitor" play the game - whatever it is Ultimatum Game (Schotter and Sopher, 2001B), Minimum Game (Chandhuri and al., 2001), Battle of sexes game (Scotter and Sopher, 2001A) - without any advice from other people. Then they transmit advice to their successor via free-form message. Following this procedure, each generation t+1 can see the advice of their predecessor, the generation t. To ensure that people will give good advice, the authors create an incentive to do so: the payoff of the subjects depends on what they have played plus what their children will earn. In doing so the experimenter can be sure that the advice will be "real" advice. Such an integration of intergenerational transmission has not the pretension to be perfect but is very useful to illustrate how methodological objections can be introduced in the laboratory. We can see that this intergenerational transmission was non-overlapping. But it can easily be thought of the same procedure with overlapping generations. And so on. It seems that everything can be introduced in the lab.

The second task is to know how to define the domain of application of the theory and so the conditions in which the experiment will be performed. Does Akerlof's (1970) lemon market theory apply only to the market of the old cars? When which parameters are at work, does this theory apply? This task seems more complicated. But the trick is to shift the burden of proof to the theorists. It is to the theorists to fix the characteristic of the situations as to where the theory applies. If they don't delimitate enough the scope of the theory and if an experiment contradicts their conclusion, the conclusions should be that the theory is unsatisfactory: or the theory is false (in non-accordance with empirical result), or the applicable domain of the theory must be clarified whatever it limits the scope of the theory or show the non accordance between the laboratory setting and the applicable domain. In both last cases the integration of new features in the laboratory setting implies an incessant dialogue between experimentalists and theorists.

Before closing this section, let's note that it can be argued that the simple fact to be in the laboratory does not cross any other application domain. In this case, the conclusions of the laboratory should be only valid for the laboratory. But regarding the ontology developed in this section, such an opposition is a methodological one. Indeed, it can be easily imagined that it exists an ideal situation in the laboratory where there is no bias of the experimentation or at least where the experimentation biases are so weak that it permits to find the intersection between the laboratory and the "wild" world. The discussion of the way to limits or avoids the biases are practical and such a discussion is not our matter here. So we have to assume here that the laboratory is a real situation (Smith, 1976, 1982). Contrarily, the next posture studied supposes that this intersection can not exist because of a matter of ontology.

3. Critical realism: the Lawson-Siakantaris critics of (experimental) economics.

The last ontological position I'm going to examine in this paper is Lawson's critical realist one. In fact, the critical realist posture is mainly inspired from the transcendental realist account. Lawson has applied the transcendental ontological argument (Bhaskar, 1978, 1989) to economics and has added a specific social ontology.

Transcendental realism asks what the world must be like before investigated by science and how it must be in order to be investigated by science. Whereas scientific realism presupposes the existence of things prior to their investigation, for Bhaskar, like for Cartwright, the aim of science is to uncover capacities that are responsible of causal mechanisms. But contrary to Cartwright's position that is essentially naturalist, Bhaskar's position is ontological, arguing for the weak interest of experimental practice.

3.1 Transcendental realism

According to transcendental realism, the objects of the world are structured – irreducible to the events of experience - and intransitive – they exist and act independently of their identification. It exists then another level of reality that governs states of affairs. Thus, there are three levels of reality: the empirical (experience and impression), the actual (actual events and state of affairs) and the real (structures, powers and mechanisms)[14]: "not only does the autumn leaf pass to the ground and not only we do experience it as falling but, according to the perspective in question, underlying such movement and governing it are real structures and mechanisms such as gravity (or curved space)" (Lawson, 1997, pp.21-22). These three ontologically distinct domains are out of phase with each other. The empirical is different from the actual. For example, people watching a football match experience differently a same event (goal or foul). In the same way, the real is different from the actual. For example, the autumn leaves are not in phase with the action of gravity because they are also subject to other factors like aerodynamics (Lawson, 1997, p.22).

The third level of reality, the real, says that complex things such as underlying structures, powers and tendencies, compose the world. Underlying structures, powers and tendencies are not directly observable but nevertheless they exist and govern the actual events and so govern what we experience: "structured things, then, possess causal powers which, when triggered or released, act as generative mechanisms to determine the actual phenomena of the world" (Lawson, 1997, p.21). Furthermore, even if these causal powers always exist and are present in things they are not always exercised[15]. And even when they are exercised they can be unfulfilled (not fully manifested) because of others countervailing powers. Tendency denotes this continuing exercised causal power. Here as thinks Cartwright as well, tendencies have always effects whatever the situation and even if the causal powers haven't been manifested. And it's because tendency generally acts in conjunction with other tendencies in such a way that causal powers do not always manifest, that regularities of the empirical realist type cannot be found.

Indeed, for transcendental realism, the empirical realist regularities of the form "whenever event x then event y" arise in special circumstances: they are "extremely rare, spatio-temporally restricted and usually artificially produced [...] Most of the accepted results of science are not of the form "whenever event x then event y always follows" after all but of the form "whenever event x then event y as long as conditions e hold" where conditions e typically amount to a specification of the experimental situation" (Lawson, 1997, pp.27-28). Indeed, social systems are open; the closure of the laboratory does not exist outside the laboratory. In the real world such event regularities do not occur (in the exception of astronomy). What happens in the laboratory vacuum is just a product of human intervention. So results are not universal but precisely they are just the result of a manipulation by the experimentalist without any valid justification of its appliance to the real world or the possible success of the application to the real world; those two questions staying unanswered. Experimentation can make event regularities apparent but they do not determinate which mechanism is behind (responsible for) this regularity. Event regularities find in the laboratory are not the scientific object but the product of the human who have permit to show the consequence of an underlying causal power. The error of standard scientists is to abduct this result obtained in very special circumstances to more general contexts.

3.2 The experimental trade-off

As experiments are products of human intervention and as constant conjunctions are rare, experimental activities cannot say to us what the laws that govern the world outside the laboratory are. The causal regularities that are found in the laboratory apply only to the conditions of the laboratory. But this problem of experimentation increases particularly in economics.

Indeed a major problem of experimentation in economics is the emphasis on the internal validity. Experimental economists try to control as many variables as possible. This implies a more narrowly specification of the laboratory conditions, and so more restrictive conditions for the application of the theory. That's what Siakantaris calls " the experimental trade-off ": the more the experimentalist achieves control over the variables (better the experiment is), the less the results they discover are applicable (Siakantaris, 2000)[16].

3.3 A critics of economics

We have seen that for transcendental realists, experiments are not of a great utility in the social science to understand the *way the world works*. It's seems like an insurmountable task to show that this position is wrong just because this assertion is the basis for this ontological position. Criticizing this ontology is not my matter here[17]. So I agree that the researchers adhering to this philosophical position cannot admit results of experiments. But I just want to add two remarks regarding this position.

The first remark is a naturalist argument: the way to choose between two philosophies is determined by the manner the scientists practice the discipline. Concerning critical realism, the way to develop science is retroduction says Lawson: "a move from knowledge of some phenomenon existing at any one level of reality, to a knowledge of mechanisms, at a deeper level or strata of reality, which contributed to the generation of the original phenomenon of interest " (Lawson, 1997, p.26). But this process is not really explained: "Not much can be said about this process of retroduction independent of context other than it is likely to operate under a logic of analogy or metaphor and to draw heavily on the investigator's perspective, beliefs and experience" (Lawson, 1997, p.212)[18]. Lawson add that through this logic of analogy and metaphor economics should refer to evolutionnary biology (Lawson, 2003). Despite the fact that critical realism has been applied by Lawson himself to several movements like Post-Keynesianism, institutional economics (Lawson, 2003) or Hayek (Lawson, 1994), the way science is developed does not correspond at all to the critical realist view (Baert, 1996).

Unlike the method advocate by critical realists, the experimental tool is more and more successfully used in economics to test and improve theories. The integration of fairness and reciprocity considerations in theories is representative. Since the first experiment on the Ultimatum Game (Güth and al., 1982), hundreds of experiments on Ultimatum Game, Trust

Game (Berg and al., 1995), Gift Exchange Game (Fehr and al., 1993) have shown that the standard hypothesis of homo oeconomicus' egoism is not observable in the laboratory. These games have revealed that people take care about each other. After a collection of experimental evidence, the first models explaining experimental data have emerged: Rabin (1993) and Dufwenberg and Kirschsteiger (1998) referring to reciprocity, Bolton and Ockenfels (1999) and Fehr and Schmidt (2000) integrating inequality aversion. Now in turn these models are tested and should lead to second-generation models [19].

The implication concerning the way to collect knowledge after this position constitutes the second remark. For that, let's go deeper in Lawson's work. One of his major contributions to the philosophy of science is to have applied the transcendental argument to the methodology of social science and more particularly to the methodology of economics. In this way, he has developed his vision of social ontology according to transcendental realism. His social ontology lies on the notion of human choice [20]. Lawson argues that in standard neoclassical economics, choice is not involved at all: "in the formal models found in the mainstream journals and books, human choice is ultimately denied. For if real choice means anything it is that any individual could always have acted otherwise. And this is precisely what contemporary "theorists" are unable to allow in their formalistic modeling" (Lawson, 1997, p.9). In these models choices are reduced to specific behavior as maximizing in order to find some humean notion of law of the type "whenever event x then event y". Standard economists are then wrong. They use methods that are inappropriate to understand the socio-economics realm. According to critical realists, the method to obtain the knowledge of underlying structures and mechanisms is retroduction. Retroduction sharply demarcate with standard methods of induction or deduction. In inferring respectively the general from the particular and the particular from the general, both techniques stay at the surface phenomena, and consider causal relation as events relations. In contrast, retroduction moves from surface phenomena to deeper mechanisms and considers causal relation as a relation between an event and a governing mechanism. *Deductivism* – logical deduction from a set of hypothesis plus the humean account of scientific law – the method that is practiced by standard economists[21], is then the wrong approach to practice economics.

Let's make a detour by what is important for experimental economists in order to test theories: the continued dialogue between theorists and experimentalists to improve the theory[22]. Critical realists can reject experiments but in the same packet they are rejecting the way economics is developed nowadays. But it's far from being big a trouble to support critics about experiments in one owns discipline. Even physics, where experimenting seems to be natural, endorses some reserves (Galison, 1987). The main point for experimentalists is that standard economists accept experiments. It’s non-sense to reject the experimental results of different models because if these critics are valid, they must reject the models too. So it seems inconsistent that a theorist doesn’t take into account experimental inconsistencies of his model in regard to this philosophical posture because if he does so, he rejects also the way he practices his discipline.

CONCLUSION

We have seen in this paper that according to the three realisms developed here, it seems inconsistent to unconditionally reject laboratory results. According to an empirical realism point of view, standard justification of experimental economics sees theories as universal and so assumes implicitly that the laboratory – even with the absence of realisticness - must be a place where it is possible to test them. The other two realist postures regarding experiments that we find in the literature do also not condemn experimental critics of theories. The localist realist approach of Cartwright sees theories valid in a domain of application. Regarding the progress made by experimentalists to integrate various factors, nothing prevents us to find a connection between the laboratory and the applicable domain of the theories in order to test the latter. But it should require the experimentalists to be less focused of internal validity principles that encourage abstraction in detriment of realisticness. As Harrison and List note, even if there is logic behind this, “it may have gone too far” (Harrison and List, 2002). Finally, the Lawson-Siakantaris critical realist’s critic of experimental economics rejects any possibility of theory’s experimental testing because of the specificity of the conditions of the laboratory. But this argument – as good as it is - implies to reject also the way standard economics is practiced and so does not constitute a valid argument for a theorist in order to save his (standard) theory from laboratory data.

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Notes

1. Even if there are several other possible utilizations of experiments (see Smith, 1994), the discussion will focus here on the most problematic role of experiments: theory testing.
2. Smith quotes Popper in a footnote (Smith, 1985, p.265), mentioned him in Smith (2002) and in Smith (1982, p.940) as an example of philosopher of science who has developed a methodology of experiments in physical science. Lakatos appears abundantly in Smith (2002) to deal with the Duhem-Quine problem.
3. In Plott (1991B), this applicability should be "independent of time and location except to the extent to which time and location have an effect on the variables of the fundamental equations" (Plott, 1991B, p.905).
4. In the academic work (e.g. Loewenstein, 1999; Nelson, 1998) and most particularly in the everyday discussions.
5. I will try to avoid the term "*real* world" and prefer "*wild* world" to refer to the outside of the laboratory because the argument is that the laboratory *is* the real world.
6. "Real people pursue real profits within the context of real rules".
7. "Real people motivated by real money make real decisions, real mistakes and suffer real frustrations and delights because of their real talents and real limitations".
8. "Real people earn real money for making real decisions about abstract claims as just as real as a share of General Motors".
9. Cf. Smith (1994, p.128) for a discussion of the replication problem.
10. We find also in Plott (1982) such a reference to the fact that laws should be universal: "Principles of economics apply there as well as elsewhere" (Plott, 1982, p.1520).
11. A contemporary account can be found in Hacking (1983).
12. Note that Cartwright rejects the explanation that we have to add the two force's vectors to have the total force because her argument is that science is better explained with capacities.
13. For a survey on preference reversals, see Tversky and Thaler (1990) or Starmer (2000).
14. Contrary to empirical realism where just exist the actual and the empirical.
15. Critical realists use the term t*ransfactual* to express what is going on in opposition to *counterfactual,* which expresses what would happen under different conditions.
16. Strangely, Siakantaris admits some usefulness of experimentation for situations of relative isolation like auctions.

17. Critics of transcendental realism can be found in Hands (2001).

18. See Fleetwood (1999) for few details about retroduction and more precisely the article of Paul Lewis about the role of metaphor in critical realism.

19. This successful development of experimental economics is found also outside the problematic of theory testing. Courses in experimental economics become more and more widespread at universities, and experiments are now a powerful teaching tool in substitution of the “talk and chalk” method (e.g. Holt, 1998; Holt and Mc Daniel, 1998; Bergstrom and Miller, 1997). From a research point of view, a Nobel Prize has been attributed to Vernon Smith, pioneer in experimental economics. There is a continual increase in experiments published in the different top international economic reviews including the journal Experimental Economics devoted to experiments (http://www.kluweronline.com/issn/1386-4157). Also experimental economics has become a powerful tool for policy makers: what Roth calls “whispering in the ears of the prince” (see for example Rassenti, Smith and Wilson (2002) who have used experiments to analyze the decentralization and privatization movement of the electricity market).

20. A second notion, intentionality is at the foundations of his social ontology but serves essentially to relate his social ontology to his vision of underlying structures that do not consist of our matter here.

21. This assertion has been one of the critical point of Lawson work (Hands, 1999).

22. Note that it is what it begins to happen now. Whereas in the beginning of experimental economics, the results where introduced in theory by experimentalists, theorists take now more and more account on experimental results.

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