

## **Laboratory Experimentation in Economics under the Microscope**

**Nikos P. Siakantaris**

Recent years have witnessed a growth of interest in experimental economics.<sup>1</sup> Indeed, it has become a rather influential project. Somewhat surprisingly, although it is a project unquestionably concerned with method, it appears to have generated very little sustained methodological discussion (Mirowski 1994a: 66-7). Moreover, as Smith (1991d: 800) observes the reactions towards the experimental project often depend on particular results, giving rise to mixed reactions by its proponents and its skeptics. The objective of this paper is to clarify and illuminate issues regarding experimental economics and hence dissolve some of the perplexity economists often exhibit.

Specifically, this paper aims at a better understanding of some central questions:

i) What are the distinctive characteristics of the experimental method in economics? ii) To what extent are the results obtained in the laboratory relevant for non-laboratory situations? iii) What are the limits of the experimental method in the inquiry of economics? iv) Why is it that experimentation, which is so successful in the natural sciences, remains controversial in the social sciences including economics?

In order to address these issues I outline key features of the method of experimental economics as it has been put forward by some of its distinguished proponents. This is the object of the next section. The following four sections seek to illuminate aspects of the experimental economics methodology and hence attempt to answer, among other things, questions i)-iii) above. In this endeavour no alternative method or theory will be presupposed. Rather I aim towards an understanding of the experimental economics methodology from an immanent point of view. The sixth section introduces some recent developments in post-positivist philosophy of science that are directly linked with the

---

<sup>1</sup> Reference to experimental economics in this paper implies exclusively the branch of laboratory experiments.

analysis of the natural science experimental activity. Hence by shifting from an internal to an external point of view my intention is to situate the results obtained in the previous sections in a broader framework and to throw some light on issues surrounding question iv) mentioned above. In short, my strategy relies on a pincer movement: the results of the immanent approach are situated, explained and in a sense anticipated by the external approach. Some concluding remarks complete the argument.

### **I. The method and aim of experimental economics.**

Experimental economists give a multiplicity of reasons for conducting experiments in economics. These reasons range from typically epistemic considerations, like the need to test hypotheses and theories, to more pragmatic undertakings, like their usefulness in teaching economics or persuading decision makers. Mostly, though, it is accepted that the basic functions of experiments in economics are the generation and testing of hypotheses and theories. If the generation and testing of hypotheses and theories were not possible, there would not be much point in doing experiments. It is from the possibility of these two functions that other more detailed ones (e.g. exploring the causes of a theory's failure) logically emerge. Similarly, the more pragmatic reasons for conducting experiments presuppose the feasibility of the two more fundamental activities. So, in the rest of this paper the focus will be on the two basic functions of experiments.

The work of Wilde (1981) and especially Smith (1982) has been particularly influential both in defining the methodological procedure of conducting experiments and in unifying the efforts of experimental economists. Moreover, the methodology of experimental economics outlined by Smith (1982) is significantly more sophisticated than that which many experimental economists actually employ (Smith 1994:129; Roth 1994). According to Smith, the framework of every laboratory experiment consists of three ingredients: environment, institution and behavior. The environment specifies for each agent the initial commodity endowments and thus aggregate resources, the initial endowment of technology which can be also interpreted as knowledge, as well as the agents' preferences. This specification of initial

circumstances does not rule out learning since preferences and endowments can be defined with reference to learning activities. The institution defines the language which can be used in the laboratory (i.e. the type of messages in the form of bids, offers, etc. ), the rules which govern the exchanges of messages as well as the rules under which messages can lead to transactions. These rules then specify what kind of messages are acceptable, the acceptable sequences in which messages can be sent and the conditions under which the experimental procedure is terminated. The elements of the environment and institution are the control variables. Thus, the behavior observed in the laboratory is interpreted as a function of the environment and the institution. Certain conditions must be satisfied in order to gain control over these variables. Smith (1982) lists five conditions crucial to gaining control and thus to the possibility of generating and testing hypotheses and theories.

The first requirement is for non-satiation of preferences, implying the familiar concept that more of a "good"(e.g money) is strictly preferable. Saliency, the second condition, refers to the linking of subjects' rewards with the experimental outcome and thus indirectly with their action taken in the form of messages. These two conditions guarantee the creation of the environment and the institution. However, two more are needed for achieving control over the respective variables. The third condition, dominance, aims to render the subjective costs of participation in the laboratory experiment negligible by offering to the subjects payoffs which are perceived to be significant. The fourth condition, privacy, requires that each participant receives information only about his/her own reward schedule to avoid interpersonal influences. Smith (1982: 935-6) argues that these four conditions are sufficient for a controlled experiment and as long as we are interested in testing hypotheses nothing more is required. The argument continues by claiming that there is no doubt about the richer circumstances implied by the laboratory in comparison with those of the theory, since it involves real people who have been financially motivated with real money in order to make real decisions (Smith 1982: 936).

Having apparently secured the possibility of testing theories with experiments, the next step is to consider whether the results of the laboratory are transferable to other situations and in particular to the real world. So, parallelism, the fifth condition suggests that the

propositions established in the laboratory hold in other, especially non-laboratory, real world situations if similar *ceteris paribus* conditions hold (Smith 1982: 936). The identification of the cases in which parallelism holds is perceived to be an empirical question. With regard to economics, parallelism implies that behavior will be qualitatively the same in different contexts such as the laboratory market and a real world market (Smith 1982: 936-7). Such a similarity, Smith argues, is unlikely to hold quantitatively since it would require a closer, more narrow and detailed correspondence of the conditions involved. Parallelism, then, presumes that the results of the laboratory have external validity (Friedman and Sunder 1994: 16). To question parallelism "one must show what is different about the outside world that might change results observed in the laboratory" (Friedman and Sunder 1994:16). Moreover, and probably because of the fragility of their claims, experimental economists are specific about what is the only acceptable way to check if parallelism holds. This way is empirical pointing towards further experiments or econometric tests. (Friedman and Sunder 1994: 16). The external validity, relevance and the interest of the laboratory experiments in economics are defended in the standard fashion by appealing to the reality of the people, rewards, etc. The laboratory process, despite its simplicity, is perceived as a real process and this is what grants status to the precept of parallelism and relevance to the laboratory experiments (Plott quoted in Friedman and Sunder: 16). The message is clear: parallelism must be taken seriously.

Having outlined the key issues of the experimental methodology in economics it is possible now to attempt a better understanding of its implications and consequences.

## **II. Obtaining control over variables.**

The inability of econometricians to control variables is the starting point of the experimental economists in outlining the comparative advantages of their method. The economics laboratory enables the experimentalist to design and specify a particular situation of interest. Clearly, the multiplicity and subtlety of variables incorporated under the headings

of environment and institution imply that obtaining control over these variables is a difficult and demanding enterprise. Effective control over these variables facilitates accurately and well specified laboratory situations. When these variables are not adequately controlled they may interfere in various ways and influence the outcome, rendering the experimental results questionable or ambiguous and hence implying a misspecified laboratory situation. The control of variables necessitates sufficient isolation from, or neutralization of all these external factors which may distort the specified situation. As Smith (1982) explains setting up an economics laboratory does not guarantee the achievement of control over the variables of the environment and institution. The conditions of dominance and privacy are of crucial importance in order to avoid undesirable and probably pernicious influences in the conduct of experiment. The condition of privacy implies that in order to gain control over the variables, possible influences among the subjects in the laboratory must also be ruled out. This condition illustrates that in order to gain control over the variables, subjects must be treated atomistically. By atomistic treatment of the subjects I mean that on top of their necessary isolation from external factors and influences, they must also be sufficiently isolated from each other inside the laboratory. The requirement of privacy aims precisely to ensure that conditions of atomism hold inside the laboratory. Moreover, one could also add that the influence of factors internal to the organism of each one of the subjects has also to be minimized. The influence of mood, age, gender, occupation, cultural aspects and characteristics and numerous other more detailed factors may interfere with the experimental results. Careful and precise experimental design, pilot studies, randomization of factors that cannot adequately be controlled and replication of results are some of the usual ways experimental economists seek to achieve control over variables in practice.

To sum up, achieving control over variables implies the precise and accurate specification of the laboratory situation. The more successful the experimental design in obtaining conditions of isolation and atomism, the more precise is the specification of the laboratory situation, the more effective is the gaining of control over variables and the less ambiguous the implications of the experimental results. These issues will be further examined after a close look at the condition of parallelism.

### **III. Some issues regarding parallelism .**

Some economists uncritically dismiss the experimental economics project provoking the justifiable reaction of the experimentalists. Parallelism is indeed an empirical matter, as experimental economists argue. Nevertheless, there are some thorny issues surrounding the condition of parallelism<sup>2</sup>. My first aim in this section is to examine the sense in which parallelism is an empirical matter and illustrate how we come to be confident about its validity.

Experimental economists argue that parallelism can be tested empirically by means of another experiment or by econometric tests. This is problematic. Consider first the case in which parallelism is to be tested by means of another experiment. There are two possibilities: either parallelism is found to hold or not. If parallelism is found to hold then the honest skeptic question is to ask whether the second experiment which is used as a test also exhibits parallelism. Clearly there is a situation of infinite regress involved in this case known as the experimentalist infinite regress (Collins 1985). It follows that it is logically impossible to test parallelism by means of another experiment. Take now the case where parallelism is to be tested econometrically. This is also problematic since it is due to the inability of econometrics to provide conditions of control over variables that the experimental approach has been put forward in the first place. Econometric testing involves qualitatively different conditions with regard to control over variables than experiments. Hence, econometric testing is quite unable to test parallelism. Consider now the case where parallelism is found not to hold by means of another experiment. But then, if parallelism is tested negative for the first experiment it may also not hold for the second experiment which could then mean that parallelism may hold for the first experiment. This implies the familiar paradox of switching conclusions and thus indeterminacy of the testing situation. Of course in the case that parallelism would be found not to hold by means of an econometric test, experimental economists would rightly object to the absence of conditions of control over variables inherent in the econometric testing. In any of the above cases and in all the possible combinations parallelism cannot be tested by further experiments or econometrics. In what sense, then, is parallelism an empirical question? It is an empirical question in terms of the

---

<sup>2</sup> The way parallelism is conceived by experimental economists will remain unchallenged until the sixth section since it requires the adoption of a different point of view.

overall success of the experimental project as a research program. It is for this reason that over the centuries fruitful experimentation has become unquestionable beyond reasonable doubt in the natural sciences, meanwhile it faces profound and justifiable skepticism in the social sciences including economics<sup>3</sup>.

Experimental economists admit that lack of parallelism is a possibility. I now consider the implications for the experimental project in economics for the case that parallelism does not hold. A straightforward consequence which Smith (1982) acknowledges is that experiments would be of no systematic use in the generation of hypotheses and theories. Nevertheless, he argues that this does not jeopardize the second basic function of the experimental project which is the testing of theories and hypotheses. The presumption which allows this claim is that the experimental circumstances are richer than those implied by the theory. It seems, though, that the possibility of testing is dubious in the absence of parallelism. As the experimental economists recognize, the two main parts in a theory which can be tested are the assumptions and the consequences; the connection of assumptions with consequences follows the rules of deductive logic. Indeed, if the main concern were about assumption testing the experimental project could be of some value in assessing whether the assumptions of hyper-rationality and hyper-computability have universal validity. But only very unsophisticated economists would believe that this type of assumptions describe how people actually act and behave. As is well known economic theorists feel reluctant to defend the realism of their assumptions, shifting the issue into the grounds of methodology following Milton Friedman. Despite the use of such assumptions, the consequences of the theory often do claim relevance for the real world, as if there were a magic stick. This is something that D. Friedman and Sunder (1994) recognize and thus the focus of the experimental testing of economic theories is placed rather on the consequences. The focus on consequences rather than assumptions can be explained from a different point of view. Establishing in the laboratory that people possess certain capacities related to rationality or computability has little, if any, significance for theory testing. Even if the focus is placed on this rudimentary level rather than the consequences of theories, the important feature is not whether people can for instance calculate probabilities according to Bayes rule in general, but in what circumstances, in what situations they can or cannot, or do and do not. This is why experimental economists define parallelism at the level of the experimental results. Even if

---

<sup>3</sup> This difference between the natural and social sciences will be further examined in the sixth section.

the emphasis were placed on assumption testing, in the absence of parallelism, the experiment is interesting only to the extent that it provides evidence against claims of universal rationality or computability. Vindicating evidence would also require the specification of the circumstances in which people possess and/or exercise these capacities in non-laboratory situations, that is parallelism. Note that the importance of the well reported laboratory result of preference reversals, which indicate violations of rational choice theory, has been questioned on whether it is also a type of phenomenon which occurs in economic environments as markets (Roth 1988:1018). The question then becomes in what sense the consequences of a theory can be tested by means of an experiment when parallelism does not hold? Consider first the case where the consequences of a theory are validated by means of an experiment which lacks parallelism. Clearly as long as we are interested in the relevance of the theory for real world situations, the lack of parallelism is prohibitive for actually gaining more confidence in the theory tested. Since one side of the testing operation has dissolved, the lack of parallelism already implies severe consequences for the second basic function of the experimental project as well. But let us now take the case where a theory does not pass the experimental test for completeness. As long as the "as if" argument of the theoretician's magic stick claims relevance for real world circumstances it serves simultaneously the purpose of eschewing the consequences of the test, since the experimental test lacks parallelism. But even if mainstream economists do not appeal to the magic stick *Deus ex Machina*, the argument of Smith that the experimental situation entails richer conditions than the theory cannot do the trick. The most that this argument can suggest in the absence of parallelism is less irrelevance for the experiment in comparison with the theory. It is incoherent to argue that a procedure which is, like the experiment in this case, irrelevant for the outside world can actually perform the testing function because the theory is more irrelevant. In the absence of parallelism, it becomes nonsensical to talk in these terms, unless one has a yardstick to measure irrelevance. In fact, the argument of Smith presupposes some unqualified, ill-formed notion of parallelism by shifting its definition from the similarity of conditions holding inside and outside the laboratory, to the bold and tautological claim of the reality of people and rewards involved in the experiment. If parallelism is an empirical matter and not an article of faith for the experimental economists, something more than the people and rewards must be similar to the outside world.

Hence, if parallelism does not hold, the two fundamental functions of experimentation in economics do not hold either. An important and straightforward implication is that animal



experimentation is unlikely to have anything to offer in economics. But if, as it has been argued, the lack of parallelism has such undesirable consequences for the experimental project, it becomes pertinent to examine the circumstances in which parallelism is more likely or unlikely to hold with human subjects. Again, I would like to emphasize that, as with my arguments so far, the attempt to clarify this issue will be from an immanent point of view.

A necessary condition for parallelism to hold is that the situation set up in the laboratory should be analogous to a real world one. If the situation is irrelevant, as appears often to be the case with mainstream theory, then clearly parallelism does not hold and there is no sense in which you can test the consequences of the theory by means of an experiment in this instance, as it has been already explained. The existence of an analogous situation broadly defined, is not enough for parallelism. This situation must also exist in relative conditions of isolation in the economies outside the laboratory, which in turn implies that it is a stable and identifiable situation. In order to illustrate this point I will make use of a strong result of the experimental economics project which is highly plausible and in accordance with a large amount of economic theory, both mainstream and heterodox, as well as the insights of other social sciences. This is the importance of institutions and rules for human behavior. In such circumstances, behavior is influenced by a multiplicity of rules and institutions from many different directions. The result of such a multiplicity of influences can affect behavior in many ways in comparison with the laboratory: it can change merely its intensity, as Smith requires for parallelism, but it can also change it qualitatively due to the complexity and multiplicity of rules and institutions. Note, also, that even if the behavior does not change qualitatively at the first place, the changes in its intensity can alter the overall outcomes considerably and this in turn can have effects on behavior since the situation is no longer the same. We should expect that both the differences in the outcomes and the presence of continuous feedback implied, can change the similarity of conditions holding inside and outside the laboratory with regard to the types of behavior and situation of interest. The implication is that parallelism will be more likely to hold for real world situations which exhibit considerable stability and are not influenced significantly by some other. I think that this is an insight of considerable importance since it explains why one of the most successful and probably least controversial areas of research of experimental economics refers to the theory of auctions. An auction is a typical case of a situation which exists in relative conditions of isolation outside the laboratory. Situations which can be found in conditions of

relative isolation in non-laboratory economies, can be characterized by varying degrees of internal complexity. The greater the ability of the experimentalist to construct a broad enough design, the greater the chance to capture the elements of complexity in the experiment and the greater the subset of this type of situations for which parallelism may hold. Similarly, the smaller the above mentioned ability, the smaller the subset of this type of situations for which parallelism is likely to hold.

#### **IV. The experimental trade-off**

I am now in a position to synthesize the results obtained so far. In the second section it has been argued that experimental economists try to obtain control over many variables in order to isolate the effects of undesirable factors. The better they achieve control over these factors or variables, the better the experimental design and the less ambiguous the evidence. But the better they achieve control the more narrowly defined and precise is the situation in the laboratory. As we have seen in the previous section, the more precisely defined the situation in the laboratory, the less likely it is that parallelism will hold, even if such a situation can be found in relative conditions of isolation outside the laboratory. This is what I call the experimental economics trade-off which implies and to a certain extent also explains, the severe and unavoidable limitations for the experimental economics project. The better experimental economists do their job in controlling variables, the more they are threatened by the lack of parallelism and hence the insignificance for their project. The consequences of the experimental economics trade-off become devastating if we recognize that situations of relative isolation like those of auctions are the exception rather than the norm in economic life. Before I shift from the internal to the external critique, a few comments on the issue of replication seem necessary.

#### **V. Replication**

I think that the following experience from animal ethology entails considerable merits. For some period of time animal behavior was studied in the zoo. It is noteworthy that observing animal behavior in conditions of captivity has led to systematic mistakes which

were corrected by observing animals directly or with cameras in their natural conditions of life (Collier 1994). The first lesson from animal ethology is that if the conditions of captivity significantly distort our picture of animal behavior, to experiment with animals is hopeless for learning anything about humans. But there is also a second, less trivial lesson. This of how careful and well replicated research can be misleading. Let us now look at the issue of experimental replication in its more general setting.

It has become something of a slogan to argue that the scientific enterprise is an inherently social activity. Replication of experimental results is no exception as the work of Collins (1985) and Latour and Woolgar (1986) illustrates. Undoubtedly, replication has crucial significance and is correctly perceived as a necessary condition for establishing valid experimental results (Popper 1959 :45, 49). But the experimental repetitions are rarely exact, they are at best more or less similar to each other (Popper 1959 :420, 422). Differences in the experimental design are important in increasing our confidence in the results provided. Nevertheless, these differences have to be within some acceptable bounds from the relevant scientific community. Moreover, when experimental results are well comprehended, repetition is no longer seen as increasing the confirming power as in the case of experiments taught in schools (Collins 1985 :35). The ideal balance between difference and similarity of the experimental design is not clear; it depends on many circumstances. As a consequence, replication, even though a necessary condition, cannot be perceived as a formal operation which by itself entails proof of results even in the highly developed experimental natural sciences. After all, competing, mutually exclusive hypotheses often claim support from well replicated experimental results.

With regard to experimental economics in particular, replication of results at times seems to be proposed as a strong intuitive support, if not as a proof of the validity of the project. Nevertheless, it is also the case that important proponents of the project are often apologetic for the insufficient replication of some results, often pointing to, among other things, the restrictions journals impose and their requirement for differences between papers. In addition, the practical problems involved with the multiplicity of human factors which must be controlled under different designs, raise doubts even among experimental economists (Roth 1994) about the power of the conclusions reached. Indeed, with regard to the importance of these issues nothing more could be added here than Roth's (1994) paper and his suggestive title "Let's take the con out of experimental economics". Some of these problems have been long

ago identified in experimental psychology<sup>4</sup> and they relate, among other things, with the systematic influence of the experimentalist's prior beliefs<sup>5</sup> on the results and even the expectations of the subjects. The aim so far has been to illustrate that appeal to the replication of results is not enough to establish the validity of the experimental economics project.

Moreover, it seems that we can further illustrate the lack of parallelism despite the replication of results. This implies the existence of systematic influences due to the laboratory setting, influences which go beyond the levels of practical difficulties and the inevitability of human bias. As Harre and Secord (1972) and Danziger (1990) argue, the experimentalist engages in a temporary social relationship with the subjects. The laboratory is not a socially neutral context. The laboratory is itself an institution which entails its own rules. The work of Danziger (1990) shows how the development of the particular institutionalization of the laboratory came about over the years as a historical process of an increasing division of tasks in the laboratory between the experimentalist and the subjects. Harre and Secord (1972) emphasize that the relationship among the subjects and between them and the experimentalists are these of strangers gathered together. Interactions among strangers exhibit different and characteristic patterns of behavior (Harre and Secord 1972). This implies that the laboratory is a special kind of society significantly distorted. This distortion does not depend on the particular experimental design or the intentions of the experimentalist, but on the idea of experimenting with humans itself. As in the case of animal ethology, the works of Harre and Secord (1972) and Danziger (1990) illustrate how well replicated results could lead to invalid conclusion, irrelevant for non-laboratory social contexts. In the language of experimental economics, it is precisely because institutions and rules matter that the laboratory matters as well, since it is itself an institution implying its own rules. This is an additional and systematic factor for the lack of parallelism.

---

<sup>4</sup> When economists speak of psychology they usually refer to the tradition associated with the experimental psychology. Recently the domain of psychology experienced drastic changes. For a comprehensive representation of the new directions in psychology see Smith, Harre, Lagenhove (1995). For criticisms of the experimental psychology Harre and Secord (1972) and Danziger (1990) are essential readings.

<sup>5</sup> If the beliefs projected to the subjects are correct this could lead to some success for the experimental economics which will hardly be due to the method.

## **VI. Realism, experimentation, the natural and the social.**

The analyses of the constructivists (Collins 1985; Latour and Woolgar 1986) are of great value regarding the sociological dimensions of scientific knowledge including the practice of experimentation. However, in order to understand why experimentation has been so much more successful as a research project in the natural domain<sup>6</sup> than in the social, a different point of view seems more promising. I think that scientific realism<sup>7</sup> provides the best available explanation for such an inquiry. Starting from the success of experimentation as a research project in the natural sciences, scientific realism attempts to explain this phenomenon by arguing that it is due to the reality of the mechanisms isolated under the experimental conditions. The following is a very brief sketch of the argument.

Scientists try to achieve in their laboratories conditions of control over numerous factors in order to produce a situation of measurable and well defined event regularities of interest of the form: when event x then event y. These event regularities, which can also take a well defined probabilistic form, do not prevail outside the laboratory; they take at least a partial, not well defined form. With the exception of astronomy it is only by means of the experimental activity and the establishment of closure conditions that significant event regularities come about. What is then the common element between the laboratory conditions and the natural world we are interested in? The common element is the causal factors which can explain the event regularity in the laboratory. In other words, the similarity between the laboratory and the natural world is established at the level of generative mechanisms, structures and entities which can account for the event regularities. But then the question

---

<sup>6</sup> Not all natural sciences or subfields of them enjoy directly the fruits of experimentation. Practical difficulties regarding the cost of necessary equipment, the magnitude of the relevant structures or entities (too big or too small), or even the state of our pre-experimental theoretical knowledge and numerous other factors can be a serious and often long lasting constraint. Nevertheless, these sciences or subfields do benefit from the experimental results in their scientific neighborhoods.

<sup>7</sup> Scientific realism does not necessarily contradict with the constructivist insights of the sociology of knowledge. They are engaging in the explanation of phenomena regarding science of different scale: constructivists focus more on the implications of the daily activities and practices of the scientific community and the way these influence results, etc., meanwhile realists focus on more macroscopic phenomena like the one considered here. The insight of scientific realism suggested here follows closer Bhaskar's (1978), (1989a), (1989b) transcendental realism. Similar results regarding experimentation are also put forward by Hacking (1983), the voluminous work of Harre and to an extent by Cartwright (1989). For differences between Bhaskar and Cartwright see Lawson (1997).

becomes why the event regularities do not also prevail outside the laboratory? This is due to the multiplicity of mechanisms simultaneously operating at different directions, overlapping, opposing and/or re-enforcing each other. What the experiment achieves is to isolate one of them in the laboratory conditions. Note that contrary to the case in experimental economics parallelism, the similarity between the laboratory and the natural world, is not argued at the level of events or the similarity of results. It rather refers to the activity of structures which may or may not be manifested in the events. This activity is described as a tendency to distinguish it from the actuality realized at the level of events. "A statement of a tendency is a transfactual statement about a structure or thing and its activity...It is not a statement of logical necessity subject to *ceteris paribus* restrictions, but a statement of natural necessity without qualifications attached" (Lawson 1997: 23). Indeed, if the realist analysis of the natural science experimental activity is correct then the similarities with the experimental economics project is rather superficial as the radically different notion of parallelism implies.

We become more confident that the particular causal mechanisms, natural structures and/or entities isolated and identified in the laboratory are real in the sense that they also exist in the outside natural world, by our ability to control them, manipulate them, use them for practical purposes and predictions inside and outside the laboratory. Of course all the particular results are fallible subject to further investigation and scientific inquiry. What we perceive today as a particular real mechanism may turn out, as it often does, to be an illusion. Further scientific inquiry may go deeper or even change the whole picture of what we perceive at the moment as a particular real mechanism, natural structure or entity. But if the reality of mechanisms, natural structures etc., is rejected in general then the success of the natural science experimental project becomes a mystery. I think this is extremely important. The common failure to distinguish between the possible reality of particular mechanisms and the reality of mechanisms in general has led to unnecessary confusion with regard to the scientific realist insights<sup>8</sup>. Scientific realism does not stand or fall with the reality of quarks or electrons. Scientific realism though is itself fallible. If a better explanation of the success of experimentation in natural sciences in general as a research project comes about then scientific realism may lose its appeal. But for the moment I think scientific realism is the best explanation<sup>9</sup> we have and we can carry on with its implications.

---

<sup>8</sup>This seems to be the case with Boylan and O'Gorman (1995).

<sup>9</sup> Lipton (1996) argues that inference to the best explanation does not presuppose realism.

The perception of experimentation put forward by realism has breathtaking consequences for our notion of science. The scientific enterprise is not about control, prediction or measurement. It is about uncovering causal factors, mechanisms, structures and entities which lie behind the appearances of phenomena which are governed by them. It is not experimentation which makes a discipline scientific. It is what experimentation is about: the uncovering of mechanisms. Successful science means successful uncovering of mechanisms, structures, etc. Prediction<sup>10</sup>, control and measurement may or may not be possible, may or may not be needed depending on the nature of the mechanisms, the phenomena of interest and the particular context in which questions are being posed.

It is now possible to move from the domain of natural sciences to the domain of economics and social sciences<sup>11</sup>. In what sense is, or can, economics be a science? This depends on whether mechanisms and structures exist at the socio-economic level. If there are mechanisms of this sort then economics could be as scientific as any other discipline, trying to uncover and illuminate the mechanisms and causal factors which generate the economic phenomena of interest. But do mechanisms and structures of this sort exist? Realism has good news for economists and other social scientists. It is noteworthy that the same reasoning which establishes the reality of mechanisms at the socio-economic level also explains why they cannot be isolated and hence explain and anticipate the limited success of experimentation in this domain. Under this perspective the conclusions obtained from an immanent point of view with regard to the experimental economics project can be situated in a broader setting. This is I hope a case where immanent and external critique successfully meet.

The existence of every particular individual takes place within a certain socio-economic context which pre-exists him/her. This pre-existence of socio-economic structures, of organized forms of life, which any single individual faces to a large extent as given,

a significant degree of autonomy for the existence, reproduction and transformation of many of these structures from the particular individual. As long as these structures have a causal effect in their own reproduction and transformation, it allows us to ascribe causal powers to them and hence enable us to talk about mechanisms at the socio-economic level. This of

---

<sup>10</sup> Some of the most successful explanatory biological theories lack these features.

course does not deny that agents also possess the capacity of purposeful action and thus the ascription of causal powers. There is clearly here the need for a mediating concept between the ascription of causal powers to holistic structures and to powerful particulars (agents). The reconciliatory concept of these two levels can be found by noticing the importance of positions in the socio-economic life. Positions like those of teacher, student, production manager, minister of finance, investment banker and numerous others, imply and carry with them certain rules, tasks, obligations and privileges. These sets of rules, obligations, etc., exist to a large extent independently from the particular individuals which undertake them. Most socio-economic positions of interest are internally related to others and take their place within certain contextual systems. Two positions are perceived to be internally related when they exist by virtue of one another, like these of manager and worker, landlord and tenant etc, Furthermore, the existence of internally related positions takes place within certain social and economic systems and structures such as the legal system, the banking system, technological conditions of production and many others. Indeed, these systems presuppose internally related positions, existing within them and by virtue of them. Hence, the internal relationality of social reality does not stop at the level of positions. Of particular interest are the ways systems are related with certain positions as well as the interrelationship between systems and structures. So, human agency takes place within a structured socio-economic world, in the sense of internally related positions and systems. Clearly, even though structures pre-exist the particular individual, they only manifest themselves by virtue of human actions and they do not exist independently of the human race. If a nuclear war eliminates human life from the planet, there will be no more socio-economic structures. These structures do not only constrain human activity by the imposition of rules and obligations attached to the relevant positions, since this would imply a passive reproduction of these structures, but also enable and facilitate forms of action which can lead to the conscious or unconscious transformation of these structures. Structures pre-exist every particular individual but they only exist, reproduce and transform by virtue of our every day life activities. Both reproduction and transformation can be conscious or unconscious, intended or not. The relational element of socio-economic life has crucial importance in understanding these interdependencies.

It is the causally efficacious character of internally related positions, structures and systems manifested in the reproductional and transformational elements of our activities, that

---

<sup>11</sup> Here again I follow the insights of the realist social theory of Bhaskar (1989a), (1989b) known as critical realism and the work of Lawson (1997) for economics in particular towards which the intrigued reader is encouraged for a thorough



allows us to talk about mechanisms at the socio-economic level. Since positions and structures exist in an internally related way, economy and society can be perceived as totalities. Hence the causal mechanisms at this level exist relationally by virtue of the totalities they presuppose and comprise. Without the relational dimension of social and economic reality it makes no sense to speak about mechanisms at this level. But if relationality is the precondition for the existence of mechanisms at the socio-economic level it also entails a crucial qualitative difference with mechanisms at the natural level. It is precisely because socio-economic mechanisms exist by virtue of their relationality that it is not possible to isolate them as in the natural science laboratories. Since isolation of socio-economic mechanisms is not possible, experimentation in economics can only be of limited value, typically relevant for situations which exist in relative conditions of isolation, like auctions. This is how the two different perspectives adopted here, the internal and the external, merge.

Realism has been found powerful in explaining successful experimental activity in natural sciences. It can also illustrate the reasons why experimentation is facing skepticism in the domain of economics and other social sciences<sup>12</sup>. A detailed account of the ways research in economics can be compatible with the realist insights is beyond the scope of this paper<sup>13</sup>. Nevertheless, I would like to briefly discuss what is perceived as the closest, albeit imperfect, analogue of experiments in the social sciences and economics. Bhaskar (1989a: 48) argues that such a similarity can be found during transitional historical periods or periods of crisis when the operation of certain mechanisms becomes more clear and direct. Giddens (1984; 61-3) also suggests something along these lines by focusing on what he calls "critical situations". The particular "critical situation" he discusses following Bettelheim, is that of the concentration camps and the insights it can offer in the understanding of routinization. I think that one can undoubtedly learn a lot from this kind of change but should not wait for revolutions, the collapse of existing socialist economies or Nazi type atrocities for the identification of mechanisms at the social or economic level. Consider the following amusing example in contrast with the big scale and often depressing ones suggested above. During the

---

analysis.

<sup>12</sup> As it has been mentioned experimental psychology is experiencing severe criticisms and new approaches are being developed.

<sup>13</sup> But see Lawson (1997).

last few years, Greece has moved under the pressure of the monetary union from a period of high inflation, high nominal interest rates and low real interest rates to a period of considerably lower inflation, much lower nominal interest rates and higher real interest rates. Businessmen, economic advisors etc., celebrated this desirable change. But it is also the case that a considerable amount of people, mainly pensioners and others who live from the interest of their bank accounts, became angry with an economic policy which they perceived to be disastrous for their interests. The minister was at pains trying to explain even to his mother why this is a beneficial change as he says in a recent interview in a daily Greek newspaper. I think as economists we cannot find better conditions for arguing that money illusion is a real possibility among some groups of people. In this sense money illusion can be a real mechanism and can have causal efficacy, if for example influences consumption or election behavior<sup>14</sup>. It is in this sense also that history in its detail and its wealth can become our closest, albeit imperfect analogue, to experimentation in natural sciences.

A positive development in which experimental economics has contributed is in familiarizing economists with the idea that the opinions, decisions, motivations and reasons of agents for acting matter. Realism can save these valuable insights by encouraging hermeneutic types of research. Interviewing and observing people within the relations they possess in the economy and trying to understand why for instance the business executives of a particular firm decided in a certain way and not another can also have beneficial results. Hermeneutic types of research have had considerable success in other scientific disciplines and are compatible with realism<sup>15</sup>. Hermeneutic types of research are not panacea and they have their own problems and limitations. But it seems that their problems pale to insignificance, especially when they become incorporated in a realist framework, in comparison with these considered in the first five sections of this paper with regard to experimental economics.

## **VII. Conclusion**

---

<sup>14</sup> Again note that the reality of money illusion can be illusory if for example these people have to pay back large mortgages in predetermined nominal interest rates, but then of course the possibility of financial ruin is not illusory!

<sup>15</sup> For a detailed account of the relationship between realism and hermeneutics and how realism can go beyond hermeneutics see Bhaskar (1989a) and Outhwaite (1987).

The arguments developed here imply that the value of the experimental research project in economics is rather limited. Economics does not need to imitate the natural sciences (scientism) in order to be scientific (naturalism). On the contrary if the realist insights are correct it is necessary for economics to develop methods of research that better suit the needs for mechanism identification at the economic level. Indeed, realism urges a reconsideration of economics which goes well beyond the sphere of its experimental branch. The bells of a great philosopher in his attempt to shake the foundations of Humean empiricism regarding the possibility of science, toll for economics: "It is never too late to become wise; but if the change comes too late there is always more difficulty in starting a reform"( Kant 1996.30).

## BIBLIOGRAPHY

- Buckhouse, R.E.** (1994) (ed) *New Directions in Economic Methodology*, London: Routledge.
- Bhaskar, R.** (1978) *A Realist Theory of Science*, Hemel Hempstead: Harvester Press, (2nd edn).
- Bhaskar, R.** (1989a) *The Possibility of Naturalism*, Hemel Hempstead: Harvester Press, (2nd edn).
- Bhaskar, R.** (1989b) *Reclaiming Reality*, London: Verso.
- Boulter, B and Coldfarb, R.** (1991) "Pisces Economicus", *Economics and Philosophy*, 7:83-6
- Boylan, T.A., and O'Gorman P.F.**, (1995) *"Beyond Rhetoric and Realism in Economics"*, London: Routledge.
- Cartwright, N.** (1989) *Nature's Capacities and Their Measurement*, Oxford: Clarendon Press.
- Collier, A.** (1994) *Critical Realism: An Introduction to Roy Bhaskar's Philosophy*, London: Verso.
- Collins, H.** (1985) *Changing Order*, London: Sage.
- Cross, J.** (1980) "Some Comments on the Papers by Kagel and Battalio and by Smith" in Kmenta J. and Ramsey J., edn.
- Danziger, K.** (1990) *Constructing the Subject*, New York: Cambridge University Press
- Davis, D. and Holt C.** (1993) *Experimental Economics*, Princeton, New Jersey: Princeton University Press.
- Friedman, D. and Sunder, S.** (1994) *Experimental Methods a Primer for Economists*, Cambridge: Cambridge University Press.
- Giddens, A.** (1984) *The Constitution of Society. Outline of the Theory of Structuration*, Oxford: Polity Press.
- Hacking, I.** (1983) *Representing and Intervening: Introductory Topics in the Philosophy of Natural Science*, Cambridge: Cambridge University Press.
- Harre R., and Secord P.F.**, (1972) *The Explanation of Social Behaviour*, Oxford: Basil Blackwell.
- Hausman, D.M.** (1992) *The Inexact and Separate Science of Economics*, Cambridge: Cambridge University Press.

- Hayek, FA.** (1942-4) "Scientism and the Study of Society", *Economica* (reprinted in *The Counter-Revolution of Science*, Indianapolis: Liberty Press).
- Heiner, RA.** (1985) "Experimental economics: Comment", *American Economic Review*, 75(1):260-4.
- Hogarth, R.M and Reder, M.W** (1987) (ed) *Rational Choice: The contrast between Economics and Psychology*, Chicago: University of Chicago Press.
- Kagel, J. H.** (1987) "Economics According to the Rats (and Pigeons too) : What Have We Learned and What Can We Hope to Learn?" in Roth, A.E. (ed) *Laboratory Experimentation in Economics: Six Points of View*, Cambridge: Cambridge University Press.
- Kagel, J.H. and Battalio, R.C.** (1980) 'Token Economy and Animal Models for the Experimental Analysis of Economic Behavior", in Kmenta, J. and Ramsey, J. *Evaluation of Econometric Models*, New York: Academic Press.
- Kagel, J.H. and Roth, A.E.** (1995) (ed) *The Handbook of Experimental Economics*, Princeton, New Jersey: Princeton University Press.
- Kant, I.** (1996) *Prolegomena to Any Future Metaphysics*, edited by Logan B., London: Routledge
- Latour B., and Woolgar S.,** (1986) *"Laboratory Life: The Construction of Scientific Facts"*, Princeton: Princeton University Press.
- Lawson, T.** (1997) *Economics and Reality*, London: Routledge
- Lewin, S.B.** (1996) "Economics and psychology: Lessons for our own Day from the Early Twentieth Century" *Journal of Economic Literature*, 36:1293-1323.
- Lipton P.,** (1996) "Is the Best Good Enough", In Papineau D., ed. *Philosophy of Science*, Oxford: Oxford University Press
- McCloskey, D.N.** (1983) "The Rhetorics of Economics", *Journal of Economic Literature*, xxi (June): 481-517.
- Mirowski, P.** (1994a) "What Are The Questions?", in Backhouse R.E.(ed) *New Directions in Economic Methodology*, London: Routledge.
- Mirowski, P.** (1994b) "The realms of the Natural", in Mirowski, P.(ed.) *Natural Images in Economics Markets Read in Tooth and Claw*, New York: Cambridge University Press.
- Outhwaite, W.** (1987) *"New Philosophies of Social Science: Realism, Hermeneutics and Critical Theory"*, London: Macmillan Education Ltd.

- Plott, C.** (1987) "Some Policy Applications of Experimental Methods", in Roth, A.E. (ed) *Laboratory Experimentation in Economics: Six Points of View*, Cambridge: Cambridge University Press.
- Popper, K.** (1959) *The Logic of the Scientific Discovery*, London: Routledge.
- Roth, A.E.** (1986) "Laboratory Experimentation in Economics", *Economics and Philosophy*, 2: 245-273.
- Roth, A.E.** (1987) (ed) *Laboratory Experimentation in Economics: Six Points of View*, Cambridge: Cambridge University Press.
- Roth, A.E.** (1988) "Laboratory Experimentation in Economics: A Methodological Overview", *Economic Journal*, 98: 974-1031.
- Roth, A.E.** (1994) "Let's Take the Con Out of Experimental Economics: A Methodological Note", *Empirical Economics*, 19, 279-89.
- Runde, J.** (1996) "On Popper, Probabilities and Propensities", *Review of Social Economy*, 465-85.
- Smith J.A, Harré R., and Langenhove L.V.,** (1995) (ed) *Rethinking Psychology*, London:Sage
- Smith, V.L.** (1980) "Relevance of Laboratory Experiments to Testing Resource Allocation Theory", in Kmenta, J. and Ramsey, J. *Evaluation of Econometric Models*, New York: Academic Press.
- Smith, V.L.** (1982) "Microeconomic Systems as Experimental Science", *American Economic Review*, 72(5): 923-55
- Smith, V.L.** (1991a) *Papers in Experimental Economics*, New York: Cambridge University Press
- Smith, V.L.** (1991b) "Experimental Economics At Purdue", in Smith, V.L. *Papers in Experimental Economics*, New York: Cambridge University Press.
- Smith, V.L.** (1991c) "Experimental Economics: Reply", (to R.Heiner), in Smith V.L, *Papers in Experimental Economics*, New York: Cambridge University Press.
- Smith, V.L.** (1991d) "Theory, Experiment and Economics", in Smith, V.L. *Papers in Experimental Economics*, New York: Cambridge University Press.
- Smith, V.L.** (1991e) "Experimental Economics: Behavioural Lessons for Microeconomic Theory and Policy", in Smith, V.L *Papers in Experimental Economics*, New York: Cambridge University Press.

- Smith, V.L.** (1991 f) "Rational choice: The contrast Between Economics and Psychology", *Journal of Political Economy*, 99(4): 877-97.
- Smith V.L.** (1994) "Economics in the Laboratory", *Journal of Economic Perspectives*, 8(1): 113-31.
- Wilde, L.L.** (1981) "On the Use of Laboratory Experiments in Economics", in Pitt, J. (ed) *The Philosophy of Economics*, Dordrecht: Reidel.