Experiments as Mediators in the Non-Laboratory Sciences

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1. Introduction

Let me start by explaining what I mean by '*non-laboratory sciences*'. I am building here on Ian Hacking's characterisation of 'laboratory sciences',"those whose claims to truth answer primarily to work done in the laboratory" (1992: 33). *Non*-laboratory sciences, then, are those whose claims to truth do *not* answer primarily to work done in the laboratory. More importantly,they can*not* answer primarily to work done in the laboratory, because the primary aim of such sciences is to explain and control *non*-laboratory phenomena. But non-laboratory sciences use experimentation too, albeit in a different way than laboratory sciences do.

In this paper I shall argue that experiments in non-laboratory sciences are just an intermediate step on the ladder leading to scientific knowledge "of a general or generalisable sort" (Hacking, *ibid.*). I shall rely on an example from economics, and try to show that experiments in sciences like economics play the role of '*epistemic mediators*'. They help bridging the gap between a theory and its target domain of application, but not in the straightforward way imagined by the proponents of standard models of testing such as the hypothetico-deductive one. Experiments are just one part of a rather complicated engine for testing scientific theories. The role of experiments will be explicated by analogy: I shall try to show that experiments are used in many respects like models, relying on a view of modelling recently defended in a volume edited by Margaret Morrison and Mary Morgan (the 'models as mediators' view).

The issue will be examined from two distinct points of view. First, I shall focus on the nature of laboratory experiments in the non-laboratory sciences. Secondly, I shall turn to epistemology and show what role

experiments play in the process of confirming a scientific hypothesis. Despite their being conceptually distinct, the two aspects of the problem are clearly connected. What role experiments can play depends of course on what experiments are, and conversely, it is mainly by looking at how experiments are used that we can tell what they are.

2. (Re)producing the winner's curse phenomenon

When you run an economic experiment, you are bound to ask two independent questions: *Does any theory provide an adequate explanation of what is going on in the experiment?* And secondly, *Does the experiment correctly reproduce 'real' economic situations, properties, or phenomena?* In order to discuss these issues with a concrete case in mind, it is useful to pick up an example of experimentation where the two are kept neatly separate. The case of experiments on the 'winner's curse' phenomenon is a good example from this point of view, and throughout the paper I shall go back and forth from methodological analysis to the case study. In this section I shall just introduce the example and show how the first question above ('internal' validity) was addressed by experimental economists. Then, in sections 5 and 6, I shall come back to the winner's curse focusing on 'external' validity.

In 1971 the Atlantic Richfield Company claimed that the constantly low profits derived from the exploitation of oil leases in the Gulf of Mexico were due to their being the victims of a 'winner's curse' (Capen et al., 1971).[1] Oil leases are auctioned by a federal agency, the Outer Continental Shelf (OCS). Auctions of this kind are, technically speaking, 'common value auctions' - auctions in which the value of the auctioned item is the same for all participants, but initially unknown to all. A crucial part of the bidding game, then, consists in trying to estimate the true value of the lease in the light of the other bidders' offers. When the participants fail in this estimation, the winning bid is likely to turn out being overoptimistic and the exploitation of the lease not profitable.

The claims of the Atlantic Richfield Company were suspect: they had

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[1] For an introductory survey of the literature on the winner's curse phenomenon, see Thaler (1988).

an interest in convincing other companies to be more cautious in their valuations, and their move may have been a disguised invitation to act as a cartel rather than competitively by bidding less on the licences. On the other hand, a winner's curse phenomenon may have *really* been hidden below the data. How can we decide? The problem is that field data do not help very much to settle the dispute, not being able to convey information about crucial variables such as agents' private valuations or the real profitability of an oil lease in the long run.

John Kagel and Dan Levin (1986) tried to give an answer by reproducing the winner's curse phenomenon in the laboratory. A game-theoretic account of auction mechanisms is available since the sixties thanks to the pioneering work of William Vickrey (1961). Vickrey devised a model known as the 'independent private values model', where each bidder is supposed to know exactly the value of the auctioned item to himself, but does not know the value to other bidders. Such an assumption seems to be satisfied in auctions of, e.g., antiques, which will be privately enjoyed by buyers who do not intend to resell them. Oil leases do not seem to be that kind of good: their value, as already said, is unknown but approximately the same for all bidders. Wilson (1977), and then Milgrom and Weber (1982), proposed a generalised theory of auctions able to account for the private-value and the common-value models as special cases. The auction is modeled as a non-cooperative game played by expected-utility maximising bidders. The players are assumed to adopt equilibrium strategies - in the standard sense of a Bayes-Nash equilibrium in which, given everyone else's strategy, no agent can do better than he is presently doing by changing his strategy.[2]

The solution of the standard bidding model is known as 'non-cooperative equilibrium with risk-neutral bidders' (or RNNE for short), and predicts that the agent with the highest private signal (denoted x_1) will generally win the auction. If bidders are rational maximisers, as the RNNE models assumes, then each of them is supposed to revise the expected value of the item as if his private information signal the highest. In technical terms, the need for this revision is known as the 'adverse-selection problem'. Denoting the expected value conditional on having the

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[2] For an introduction to auction theory, cf. Milgrom (1989); for a more comprehensive survey, see McAfee and McMillan (1987).

highest information signal as $E[\,x_0\,|\,X_i\,x_1]$, a winner's curse occurs every time the actual estimate of value exceeds the latter, i.e. whenever

(WC) $E[x_0\,|\,x_i] > E[\,x_0\,|\,X_i = x_1]$.

In this case, in fact, the winner failing to take into account the adverse-selection problem will experience on average negative profits. The inequality above is better characterised as 'the winner's curse *hypothesis*': unlike the RNNE model, it does not provide a full explanation of the bidding process. It is mostly defined as a contrast case: it conjectures that real bidders are not fully rational and fail to revise their expected values correctly. If the RNNE model is right, and bidders really are rational maximisers, the winner's curse should not occur, and evidence such as that presented by the Atlantic Richfield Company should be explained in a different way. The aim of the experiment devised by Kagel and Levin was to show how the data may result from a certain mechanism by producing it in the laboratory. The experiment had a very precise, predetermined *target*.

Kagel and Levin (1986) constructed their argument for the existence of the winner's curse by controlling for the number of subjects and public information. To begin with, (i) they ran experiments with a 'large' number (6-7) and experiments with a 'small' number of bidders (3-4). When the number of competitors is large, a rational maximiser is supposed to take into account two opposite considerations: one should bid more aggressively because the signal values are more congested, but less aggressively because the adverse selection problem becomes more severe. A RNNE bid function taking into account these considerations requires the bids to remain constant or to decrease when there is a growing number of competitors.[3] If the winner's curse explanation is right, in contrast, higher bids should be observed as the number of competitors increases. Varying the number of bidders thus provides a mean to discriminate between the two rival hypotheses.

(ii) Some experiments, moreover, involved only private information signals, whereas others involved public information: bidders were asked to provide a first evaluation under knowledge of x_i only, and then a

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[3] See Kagel and Levin (1986) for the quantitative analysis behind such a hypothesis.

second one after having been given some additional public information signal x_p (the lowest of the private signals formerly distributed, x_L, is particularly convenient for analytical reasons). The public information control is useful for providing insights into the bidding mechanism. In RNNE, in fact, public information is supposed to raise the bids of all the subjects who have not had the highest private signal; this should put pressure on the x_1 bidder and therefore diminish his profits by almost one half.[4]

Kagel and Levin (1986) observed two results: (i) in 'small group' experiments, the winners bought the items at a profitable price, but the profits were considerably lower than those predicted by the RNNE model (65.1% of the latter). In 'large group' experiments, the winners experienced losses on average. (ii) In auctions with a small number of bidders, the injection of public information raises prices; when the number of bidders is large, in contrast, prices fall contrary to the RNNE prediction. Both results are consistent with a winner's curse explanation. Winners, ex hypothesis, overestimate values; public information tends to reduce uncertainty about the true value of the item, so that bidders with the highest private information can revise their evaluations.

Kagel and Levin, thus, tried first to run experiments that could teach something about the functioning of laboratory common value auctions. I shall return to their experiments later (sections 5-6), to show how they were designed so as to support an inference from the 'internal' to the 'external validity' of the winner's curse explanation. Before then, some more philosophical weaponry must be introduced.

3. The problem of parallelism

Kagel and Levin demonstrated that a winner's curse phenomenon can be created in a laboratory economy - but what about the 'real world', the phenomenon that originally stimulated their investigations? Is a winner's curse interpretation of the OCS data legitimate? A further step is needed

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[4] From $E[\Pi \mid W] = 2\epsilon\_ / (N + 1) - Y$ (where N is the number of bidders in the auction and Y is a negative exponential becoming rapidly negligible as the value of x_i departs from extremely low values), to $E[\Pi \mid W, X_L] = \epsilon\_/(N + 1)$. See Kagel and Levin (1986) for the details of such a prediction.

in order to claim that the same phenomenon observed in the laboratory lies hidden behind real-world empirical data. Two well known and distinct methodological problems arise with any laboratory experiment: the problem of underdetermination of theory by data and the problem of causal underdetermination.

The problem of *underdetermination of theories by data* consists in the impossibility of logically determining the validity or inadequacy of a given theory on the basis of evidence and deductive logic alone.[5] Logically speaking, in fact, a potentially infinite number of theories can account for a (no matter how great) body of evidence. And conversely, when faced with a falsifying observation, there always is the logical possibility of revising a peripheric assumption so as to save a given theoretical claim. The problem cannot be solved, but at least reduced in the laboratory. In practice, in fact, in any historically given controversy only a finite number of competing theories are at stake, and one can discriminate between them by means of 'quasi-crucial' tests such as those devised by Kagel and Levin.

Experimental testing can help reduce the problem of underdetermination of theory by data and thus confirm that a certain explanation is able to account for laboratory evidence. But it cannot eliminate nor even reduce the problem of *causal under-determination*: different causal processes may generate similar patterns of data in different situations. In order to generalise a laboratory result, a further step has to be made: one has to show that the system constructed in the laboratory is the same as the one at work in the real world (the 'target', from now on), and that the similarity between 'artificial' results and 'real' phenomena is not illusory. Economists have always been rather sceptical of laboratory results. Experimenters, therefore, have been confronted since the early days with the problem of justifying their inferences from the laboratory to the real world. They have named it the problem of 'parallelism' (Smith, 1982): what does the behaviour of laboratory economic systems tell us about phenomena observed in other, sometimes more complicated or very different, situations?

My proposed answer will be that experiments act as 'mediators'. The idea of 'mediating entities' has been used by philosophers of science to

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[5] The *loci classici* are of course Duhem (1906) and Quine (1953).

characterise the notion and role of scientific models. I shall build on this idea and speak of 'epistemic' mediators: experiments, according to this account, constitute an intermediate step in a more general procedure aiming at supporting a given theoretical explanation of field data. In the next section the notion of models as mediators will be presented and briefly discussed. Then, I shall turn to experiments, and argue that a common denominator of all mediating entities is their being simpler, more manageable, and more controllable systems which can be manipulated with the aim of understanding the functioning of a complex, little manageable, and partially or totally uncontrollable system.

4. Mediators

Philosophers have learnt from meta-mathematicians to think of models as *objects*. According to the Semantic View of Theories, indeed, models are structures (i.e. sets of object and relationships) of which some theoretical sentences are true. Such a formal approach, however, fails in many respects to capture modelling practice in all its various aspects. In real science one finds models of different kinds, and several taxonomies have been put forward in order to classify them.[6] Instead of engaging in a review of the literature, I shall focus on one particular kind of models, which have been called, according to their function, 'mediating models'.

The idea of 'mediating models' has been at the centre of a research project on 'Modelling in Physics and Economics', carried on at the Centre for Philosophy of Natural and Social Science of the London School of Economics. The idea has been applied to a number of case studies, collected in a forthcoming volume edited by Mary Morgan and Margaret Morrison (eds. 1999), and has also been developed in a few journal articles (Hughes 1997; Morrison, 1998[7]). In this section, therefore, I shall simply summarise some of the main ideas of the 'mediators' approach and show they can be used for my purposes.

Mediating models have three main characteristics: *(a)* they are *partly*

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[6] Cf. e.g. Giere (1979) and Redhead (1980).

[7] See also the paper presented by Mauricio Suarez at MBR98, "The Role of Models in the Application of Scientific Theories: Epistemological Implications".

independent both from high theory and from the systems they are sup-
posed to help to explain (which I call 'target systems'); *(b)* they '*stand
for*' some real-world systems of interest; and *(c)* they can be *manipulated*
in order to learn something about the real world.

(a) Independence.

The know-how to build models comes from different sources. Scientific
models are rarely entirely theoretical or empirical in character. They are
usually hybrid objects, and for this reason they can function like tools or
instruments. "It is precisely because models are partially independent of
both theories and the world, that they have this autonomous component
and so can be used as instruments of exploration in both domains" (Mo-
rgan and Morrison, 1999: 10).

Experiments too are autonomous from theory and the systems they
are intended to represent. They are obviously autonomous from theory
from an ontic point of view (which is not so clear in the case of abstract
models), but are also *partially* autonomous from theory from an epistemic
viewpoint: a lot more than theoretical knowledge is needed in order to
perform and interpret an experiment correctly (a well-known fact since
at least Duhem, 1906).

Experiments are designed in part according to theoretical constraints.
The path from the target system to models and experiments can be repre-
sented as in Figure 1. Let us discuss the diagram using the example of
Kagel and Levin's winner's curse experiments. The target systems to be
represented consist of the OCS auctions, and oil companies' low profits
is the phenomenon to be explained. The first move is to choose a theore-
tical model providing an explanation of this phenomenon. There are in
our case two competitors, the RNNE model and the winner's curse
model. The decision to represent the target system by means of either of
the two models involves a number of assumptions; some of them are
common to both models: for instance that the rights to exploit a lease are
worth the same for all bidders, or in other words that OCS auctions are
common value auctions; that each bidding firm obtains an estimate from
its experts; and that the estimates are unbiased, so that their mean cor-
responds to the common value of the track. Let us call these assumptions
common to both models 'neutral' assumptions. Other assumptions, like
for instance the one put forward by the RNNE model that bidders are

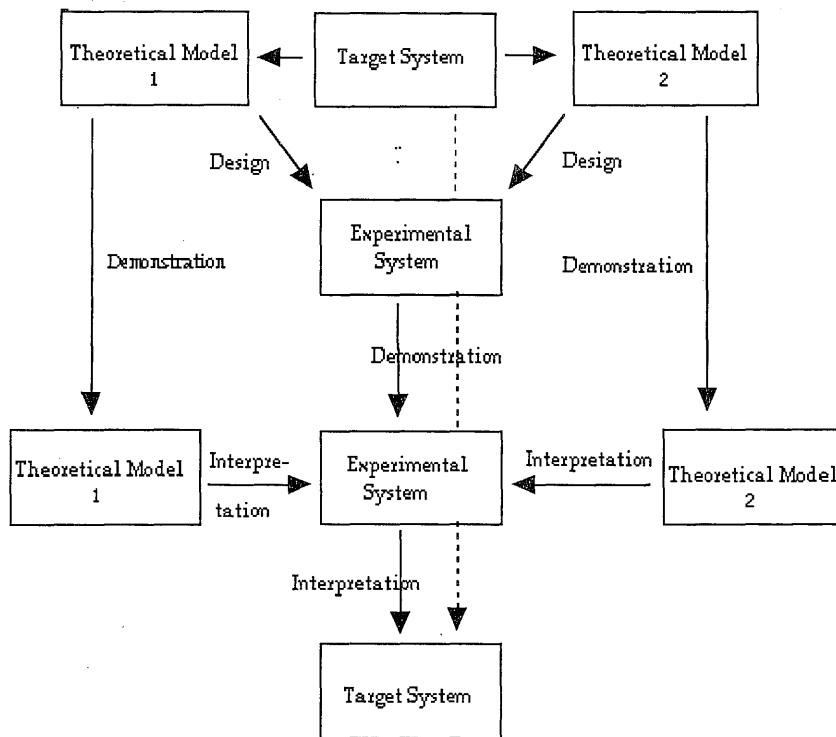perfectly rational, are specific to either one or the other of the two models.

FIGURE 1: Experiments as mediators

The role of the experiment is to discriminate between the two representations. The theoretical models provide thus the necessary information to design an experiment such that, ideally, (*a*) the neutral assumptions are satisfied, and (*b*) the experimental system can produce a result discriminating between the two rivals, i.e. a result that can follow from only one of the two models. If so, the experiment will provide a crucial test between the two competing explanations.

(b) Manipulability.

Three similar processes are therefore carried on in parallel, by *manipulating* and letting the three machines (the two theoretical models and the experimental system) run. Models display the important property of having internal mechanisms, which determine their evolution under certain circumstances. This is a property of both theoretical (mathematical) and material models. R.I.G. Hughes (1997) calls the process of producing certain consequences by manipulating the model, '*demonstration*'. With models, whether theoretical or material, we demonstrate. The basic idea implicit in demonstrating is that of triggering a mechanism and observing what its consequences (a theorem, a physical effect, an allocation of goods, etc.) will be. This idea can straightforwardly be applied to experiments. In the picture above, actually, *four* demonstrations are depicted: two demonstrations from models, one from an experimental system, and one (in the background) from the target system. The latter is of course a 'spontaneous' one - the target cannot be 'triggered' nor manipulated at will - producing the field data that were to be explained in the first place.

(c) Representativeness.

I adopt the word 'demonstration' also because it is evocative of the fact that the experiment is neither the target system nor a representation of it - it just *stands for* it. In this sense the experimental auction does a similar job as models do. Following R.I.G. Hughes (1999), One may even say that it 'represents' the real OCS auction, but in the sense of being a surrogate for it (as a mediating model, it is a 'representative', rather than a 'representation'). A first process of interpretation is needed to link the theoretical models' predictions to the outcome of the experiment. But this

is not the end of the story: the experimental system is not the target system, and thus there is a further step, from the experimental auction to the real OCS auction, to be made. We shall see (sections 5-6) that this step is far from trivial.

In the laboratory sciences experimenters 'play' with the target system itself; in the non-laboratory sciences it is sometimes possible to manipulate the target system in a non-laboratory environment, but this is more often difficult, costly, dangerous, even impossible, and the inferences drawn from uncontrolled experiments are hardly reliable anyway. Thus, *laboratory experiments in the non-laboratory sciences demonstrate with experimental systems that 'stand for' the target systems of interest*. Such is my main claim concerning the nature of experiments in the non-laboratory sciences.

Any mediating entity is linked either to the real world or to another mediator by means of some particular relationship. According to Ronald Giere (1979), theoretical models are linked to some real-world entity by a 'theoretical hypothesis', stating what relationship holds between the model and the real world. In the case of experiments, similarly, a '*parallelism hypothesis*' has to be put forward saying that the laboratory system stands in some particular relationship with the target phenomenon at stake. The two steps leading from a given economic model to the real-world phenomenon it intends to explain can therefore be represented as follows:
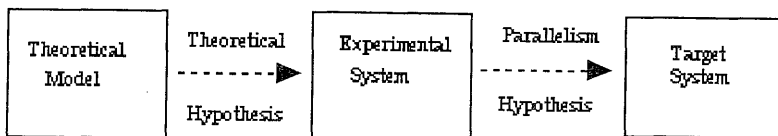

FIGURE 2: The path from theortical models to the real world

In the next section I shall show how one can deal with parallelism by turning back to the OCS case. I shall therefore focus on epistemological matters more closely: what kinds of data are needed to confirm or refute a parallelism hypothesis? And how are they used?

5. Tightening the bridge

Demonstrating with models and experiments is an activity with a clear epistemic goal. The main question one has to face when dealing with a conjectural (theoretical or experimental) 'demonstration' of a certain phenomenon is: What is the relationship between the model or the experiment and the target system? How can one prove that the model or experiment is a good representative of the target system? Here lies the epistemic gap to be filled by parallelism arguments.

Kagel and Levin say they have produced the winner's curse phenomenon in the laboratory, and their claim is clearly intended to bear some weight on the real-world issue at hands: *was the winner's curse the cause of average low profits in the OCS auctions?* We had two possible explanations of the data, and the experiment was designed so as to increase the plausibility of the winner's curse explanation. That is, the experiment was performed in order to confirm either the standard RNNE model, or the alternative winner's curse model (or possibly neither one nor the other) as explanations of the target phenomenon at stake.

According to the experimental economist Vernon Smith, "which kinds of behaviour exhibit parallelism and which not can only be determined empirically by comparison studies" (1982: 267; see also 1989: 152). The proof, in other words, must be *empirical*.

The case of the OCS auctions required one step beyond the reproduction of the winner's curse in the laboratory. Real-world evidence did not play any role in the arguments presented in section 2 (except of course as a motivation for the experiments). Thus, the strength of the winner's curse explanation had to be somehow increased. Kagel and Levin focused on an interesting parallelism between laboratory results and a real-world phenomenon. Some data existed about different profits achieved by oil companies on so-called 'wildcat' as opposed to 'drainage' leases (Mead et al.,1983). The former are on tracts for which no productivity data are available, whereas the latter are on tracts lying adjacent to some hydrocarbon reservoir. The developers of the adjacent tract (the 'neighbours') have more private information on the profitability of the drainage tract, but all bidders ('non-neighbours') know that something is likely to be found. Mead et al. (1983) noticed that in the Gulf of Mexico from 1954 to 1969 both neighbours and non-neighbours have had on average higher

rates of returns on drainage than on wildcat leases, a fact which is not compatible with the RNNE explanation. In RNNE, in fact, depending on whether the information available is (a) purely public, (b) purely private, or (c) both private and public, we should expect rates of return: (a) lower for all; (b-c) higher for neighbours than non-neighbours, with the latter earning less than they would in absence of insider information. If a winner's curse effect is present, however, the data can be easily explained: the increase in insiders' information helps to reduce the winners' overestimation of wildcat tracts, and thus raises the returns of both neighbours and non-neighbours. Kagel and Levin (1986) show that a phenomenon of the above sort can be replicated in the laboratory, where one can control for public information at will (the strategy has been outlined in section 2 above).

From a methodological point of view, the logic of the procedure can be analysed as follows. Let us call the evidence in need of explanation, i.e. the fact that oil companies in the Gulf of Mexico experience on average low returns from drainage leases, e. The goal of the experiment is to discriminate between two alternative theoretical hypotheses H_1 and H_2 - the RNNE explanation and the winner's curse explanation respectively. The construction of an artificial common value auction system enables us to test (new) predictions from H_1 and H_2. Kagel and Levin, by varying initial conditions such as public/private information and the number of bidders, construct an independent test which is moreover a *quasi*-crucial experiment relative to H_1 and H_2, i.e. such that $H_2 \Rightarrow e'$ but $H_1 \Rightarrow \sim e'$. The new evidence e' collected in the lab confirms that a winner's curse phenomenon is likely to be hidden behind experimental bidding. Indeed, Kagel and Levin produced two *quasi*-crucial experiments (their conclusion depends in fact on the decision to limit the analysis to H_1 and H_2), by varying the number of bidders and the nature of the information provided. The results of both tests were consistent with the winner's curse hypothesis, which was therefore highly confirmed.

The experimenters were aware that such evidence (e') could not settle the dispute about e, the target data at stake. Therefore, they showed that in the real world there are cases of variation of public/private information analytically analogous to those reproduced in the laboratory. In the OCS case, such evidence was provided by Mead, Moseidjord and Sorensen's study. The crucial argument for parallelism consists in arguing that (i) in some cases the initial conditions of the real systems under study are

(qualitatively) similar to those of the laboratory systems; and thus (ii) some data in the real world, let us call them e^*, can really play the same role as e', so that $H_2 \Rightarrow e^*$ and $H_1 \Rightarrow \sim e^*$.

It is really e^* that provides confirmation for H_2 as an explanans of e. It can do so because a final process of interpretation has taken place: wildcat and drainage leases are assumed to provide information of a different quality (public vs. private), so that both the results 'demonstrated' from the theoretical models and those 'demonstrated' from the experiment can denote a phenomenon in the target system. It is crucial that such a process of interpretation, like the initial one of denotation, be 'neutral' with respect to the two theories at stake, so that the parallelism inference can be accepted by both parties. No assumption incompatible with any one of the two hypotheses can be used in order to interpret e^* as e'.

The moral is that experiments can help just at an intermediate stage of confirmation. They cannot fill the gap between the target phenomenon and the theoretical model completely.[8] The need of an argument for parallelism is clear: experiments cannot, on their own, prove much about the real world. They can increase the plausibility of an explanation, but only up to a certain point. The reason is not only that a pattern of data can be explained by different theories, but that it may also be the result of different causal processes. The problem of underdetermination of theory by data can be reduced in the laboratory by controlled testing, but establishing that a certain explanation is the right one in the ('artificial') domain X does not prove that the same process lies at the origins of a similar pattern of data in the target domain Y. In order to establish this, one needs some further independent evidence from the target domain of application of the theory at stake. By presenting such evidence, Kagel and Levin made the first move and sent the ball into the opponents' camp. It was their turn, then, to discredit Kagel and Levin's results by challenging the parallelism argument just examined.

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[8] Experimenters are aware of this: see Kagel and Levin (1986: 914).

6. Parallelism as analogy

With the OCS example in mind, I can now try to define more precisely what kind of reasoning is involved in making the parallelism step from experimental systems to the real world of phenomena the theory aims at explaining. I have argued earlier that the demonstrative capacity common to models and experiments is due to their having some internal mechanism which can be 'triggered' and let run. The procedure of experimental confirmation in the non-laboratory sciences can therefore be thought of as a demonstration carried on in parallel, on three systems at the same time: a real-world system, a theoretical model, and an experimental system.

In order to argue for parallelism, a number of moves have to be made. The first one consists in associating the initial conditions stated by the theoretical model with both the initial conditions of the experimental system and those of the target system. The first step, as I have argued in section 4, is automatically fulfilled by designing the experiment so as to mirror (some of) the model's assumptions. The second step is more problematic, and amounts to finding some features of the real-world systems that correspond to the model's *and* to the experiment's initial conditions. The same operation must then be carried on at the level of the outcomes of the demonstrations: the model's predictions must be associated with the experimental outcome and with some real-world observed data.

The next (big) step consists in arguing that *since* a correspondence has been established both at the level of the initial conditions *and* at the level of the outcomes, *then* there also exists a correspondence at the level of the internal processes. Imagine two sets X and X^* such that every element of the first set can be associated to an element of the other by means of a function f. Nothing guarantees that all the relations holding between the elements of the first set will also hold between the elements of the second set. In order for a relation R on the first set to translate into a relation R^* on the second set, we must impose the further requirement on the function f (taking us from the set X to the set X^*) that $x^*R^*y^*$ if and only if xRy (where x, y and x^*, y^* are elements of X and X^* respectively, of course).

By associating some systems in such a way - claiming that the stated initial conditions of a theoretical model correspond to the initial con-

ditions of an experimental system, and that both correspond to the initial
conditions of a real-world system - we are thus drawing a function from
the properties of one kind of entity to the properties of another. The
relationships exploited in parallelism arguments can be seen as functions
from models to experiments to target-systems. The parallelism step is
however complete only after a correspondence has been established at the
level of the internal processes - the relations between a system's initial
conditions and its successive states. These relations are syntactical rules
in the case of a syntactical entity, abstract relations in the case of a
theoretical model, causal processes in the case of an experimental and a
real-world system.

A parallelism argument seems to be an analogical argument, both in
the vague sense of the word 'analogy' in everyday language, and in the
much more precise technical sense of the Greek word *analogìa* ('acc-
ording to a ratio'). In general, an analogy is a similarity relationship
between two entities or sets of entities. In the Pythagorean tradition, more
precisely, an analogy was an identity of ratios. This meaning survived in
the mathematical, technical sense of analogy as proportion: $a:b = c:d$.
Whereas an analogy in the everyday sense involves two entities, an
analogy in the original, rigorous sense always involves at least four terms
taken in couples: "As A is to B, so C is to D", according to Aristotle
(*Topics*, i, 17). In the case we are concerned with, the parallelism ar-
gument amounts to an analogy of the following sort: given (a) some
controlled initial conditions in the laboratory and (b) some observed
experimental result on the one hand; and given (c) some observed proper-
ties of the target system and (d) some observed field data on the other,
then (by analogy) c stands in the same (causal) relation to d as a stands
to b.

Analogies have a well-known heuristic value: by postulating an
analogy between two sets of properties, we can infer the existence of a
hidden property in one set by observing the existence of other properties
in the other set. Analogical models sometimes work precisely this way:
by observing the properties of a model we are induced to think that
similar properties are to be found in the real entity modeled. To say it
with Aristotle, once again, "A is in B like C is in D". More rigorously,
in mathematics knowledge of three terms of a postulated proportion like
$1:3 = x:6$ allows to obtain the value of $x = 2$. In our case, however, the
parallelism analogy cannot be simply postulated. It is a hypothesis, one

has to justify it, and the argument takes the form of a generalisation from a number of correspondences between the entities in the two sets to an analogy between the relations holding inside the sets themselves.

Induction is part of the process: it intervenes because, of course, the more initial conditions and outcomes are found to correspond to each other by denotation and interpretation, the more one is entitled to think that the systems' internal mechanisms correspond to one another. This is nothing new: it is one of the confirmation processes inductivists have taught us about since a long time ago.[9] The relationship between models, experiments and real systems is one of analogy; the analogy is established (or just weakly confirmed) by inductive reasoning. A characteristic feature of the parallelism step from experiments to target systems is that it is usually supported by very few established correspondences at the level of initial conditions and outcomes. In the OCS case, as we have seen, the analogy is based on just *one* correspondence - the one between private vs. public information and wildcat vs. drainage leases on the one hand, and the one between high vs. low returns of neighbour vs. non-neighbour tracts on the other. In contrast, the analogical step from the model to the experimental system can be supported by more evidence, and is thus more tightly established thanks to the manipulations and controls allowed by the laboratory. In the laboratory one can control the initial conditions so as to derive ('demonstrate') new phenomena, possibly ones that can discriminate between two alternative explanations. In the field, this is not always possible. For example, in the OCS case, field data provided a variation of public and private information but no control on the number of bidders was possible.

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[9] On the problem of confirming analogies, cf. Mary Hesse's (1963) classic on models and analogies. What Hesse says about models can be easily translated in terms of experimen-
tal systems

apply.

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