ONE

Introduction

On October 9, 2002, the news began to circulate that Daniel Kahneman and Vernon Smith had been awarded the Bank of Sweden Prize in Economic Sciences in Memory of Alfred Nobel. The prize was not entirely unexpected. For a few years, the names of prominent experimental economists had been in the list of plausible Nobel candidates, and everybody agreed that it was just a matter of time. Yet, for the community of experimenters, the event was epoch making: laboratory work was recognized officially as one of the most important advancements in the last half century of social science.

This wasn't the first time that the Nobel Prize had been assigned to the proponents of doctrines or approaches that do not enjoy universal acceptance within the profession. And economists like Maurice Allais, Herbert Simon, and Rheinhard Selten, who had contributed in many ways to the birth and development of experimental economics, already figured among the laureates. But Simon, Allais, and Selten had been prized for their work in other areas of economic theory, and the 2002 award constituted an innovation in at least two major respects. First, it recognized the work of a scholar who according to the conventions of contemporary academia should not be labeled as an "economist." Daniel Kahnemann was prized "for having integrated insights from psychological research into economic science, especially concerning human judgment and decision making under uncertainty" (Nobel Press Release 2002). This was a contribution "from without," by a prominent psychologist who challenged many key ideas in the mainstream economic tradition.

But secondly, and more importantly perhaps, the other half of the prize was devoted to recognizing a *methodological innovation*, rather than a

contribution to the body of economic theory. Vernon Smith was prized "for having established laboratory experiments as a tool in empirical economic analysis, especially in the study of alternative market mechanisms" (ibid.). Of course methodological innovations carry important novel theoretical insights with them: if you look at the world with different instruments, you are likely to notice different things. And in fact the work of Vernon Smith includes important theoretical contributions as well. But the Nobel committee was keen to stress that

> Economics has been widely considered a non-experimental science, relying on observation of real-world economies rather than controlled laboratory experiments. Nowadays, however, a growing body of research is devoted to modifying and testing basic economic assumptions; moreover, economic research relies increasingly on data collected in the lab rather than in the field. (ibid.)

It was this change in the nature of economic science that was primarily recognized by means of the 2002 award.

Why experiment in economics?

Until fairly recently, most economists believed that controlled experimentation had little to offer economic science. These beliefs are voiced in some of the most influential methodological writings of the last couple of centuries; despite their different views about what constituted "good" methodological practice, everybody seemed to agree that economic research was bound to take place mostly outside the laboratory. In 1836, John Stuart Mill claimed that "there is a property common to almost all the moral sciences, and by which they are distinguished from many of the physical; that is, that it is seldom in our power to make experiments in them." About a century later, Lionel Robbins wrote that "our belief [in economic generalizations] does not rest on the results of controlled experiments." And Milton Friedman in his influential essay on the methodology of positive economics also states that "we can seldom test particular predictions in the social sciences by experiments explicitly designed to eliminate what are judged to be the most important disturbing influences." Such statements – partly because of the prestige of their authors, partly because they reflected the views of the average economist – migrated in the most popular economics textbooks, upon which generations of economists have been trained. "Economics must be a non-laboratory science," wrote Richard Lipsey, given that "it is rarely, if ever, possible to conduct controlled experiments with the economy."

Samuelson and Nordhaus similarly claim that "economists [. . .] cannot perform the controlled experiments of chemists or biologists because they cannot easily control other important factors," and even in the *Encyclopaedia Britannica* one reads that "there is no laboratory in which economists can test their hypotheses."[1]

Nowadays these claims have become obsolete. Economists perform hundreds of laboratory experiments every year, and routinely test their theories in the laboratory. But *why* do they do such things? Why is experimentation highly considered today, but was not half a century ago? An obvious answer is that the success of experimental economics was made possible by several profound changes in the discipline of economics as a whole. In order to write a proper history of experimental economics (something that still has to be done), one would certainly have to look back at the birth of expected utility and game theory in the forties and fifties, examine the rise and fall of general equilibrium analysis in the sixties and seventies, discuss the high expectations and frustrations that accompanied the development of econometrics, and probably much else.[2]

However, this is not supposed to be a book of history, and I shall leave it to someone else to do a proper historical job. When I ask, Why experiment in economics? I am not concerned with the reasons why experimental economics is more popular today than, say, one hundred years ago. That is an interesting question indeed, but it is not the question of this book. This book asks, Why experiment in economics? *in general*, or as a matter of principle. I take the latter to be an ahistorical question, in the sense that it asks what sort of knowledge social scientists can collect in the laboratory, regardless of time, place, and context. It is, in other words, an *epistemological* question about the capacity of laboratory experimentation to produce knowledge about economic matters. As such, it does not investigate the

[1] Cf. Mill (1836, p. 124) and Robbins (1932, p. 74). The Friedman and Lipsey quotes are taken from Starmer (1999, p. 1), Samuelson and Nordhaus' from Friedman and Sunder (1994, p. 1), the *Encyclopaedia Britannica* from Davis and Holt (1993, p. 4, n. 2).

[2] Brief reconstructions of the early history of experimental economics can be found in Smith (1991a; 1992), Davis and Holt (1993), Friedman and Sunder (1994), Roth (1995), and Hargreaves Heap and Varoufakis (1995). Leonard (1994) is the only contribution by a professional historian, but focuses on bargaining experiments only. Mirowski (2002) vividly reconstructs the milieu of mid-twentieth-century economics, in which the conditions for the birth of experimental economics were created, and devotes a short section to Vernon Smith's research program (pp. 545–51). Two Ph.D. dissertations at Notre Dame are beginning to explore the history of experimental economics in more depth (Lee unpublished, Nik-Kah unpublished).

contingent factors that as a matter of fact have prompted economists to turn to the laboratory. It investigates the reasons why economists *should* (or should not) endorse the experimental approach, by articulating what experiments can (and cannot) do for them.

Why experiment?

The experimental method is as old as science itself, and became the hallmark of the most successful science – physics – during the scientific revolution of the Renaissance. One may think, therefore, that the basic elements of the experimental method must be well understood by now. Surprisingly, this is not the case. Of course the literature on experiments is large. All great scientists since Galileo have put forward their own views about the proper use of experiments, and professional philosophers have added more thoughts on this topic. Philosophers' views, however, were for a long time detached from experimental science as it is practiced in real-world laboratories. They typically followed from fairly abstract speculations about the nature and sources of knowledge in general, as if laboratory experimentation had no peculiar features of its own that justified a separate analysis. And curiously, scientists tended to follow philosophers on this track, by privileging philosophical speculation instead of reflecting on their *real* practice. (Perhaps this is partly because many great scientists of the seventeenth and eighteenth centuries were also great philosophers, with very precise views on abstract epistemological matters.)

A fundamental assumption of this book is that in contrast, philosophy of science must look closely at the messy business of science in the making. There has been a movement in the past two decades toward a philosophy of science that is more sensitive to the details of scientific practice. This body of work has provided an impressive amount of data about the methods actually followed by scientists in their everyday work, and has been an important source of inspiration for what is to follow.[3] There are two good reasons, however, not to start from an account of "*the* experimental method" as it emerges from these studies. First of all, there is no such account. The picture emerging from such studies is patchy, and to try to distill a unified story out of this material would be an unlikely task. Secondly, and related to this, this material is diverse because scientific

[3] For a survey of the so-called new experimentalist movement in the philosophy of science, cf. Ackerman (1989), Franklin (1998), or Morrison (1998a).

practice probably *is* diverse. Experimental physics or biochemistry (which have been studied extensively) may follow in part different procedures from experimental economics. We should not expect these disciplines to be *entirely* different, of course, but neither should we presuppose that they are identical. An account of experimental science based on physics or biology may not fit the bill of the experimental economist.

Why economics?

Experimental economics seems a pretty odd topic for a philosopher of science. Most philosophers interested in normative questions about science (How should genuine scientific knowledge be generated? How do scientists avoid falling into error? What exactly is scientific knowledge *knowledge of*? and so on) tend to look at the natural sciences, because these are supposed to be the most advanced disciplines with respect to both their results and their methodology. However, as I said, we should not assume that what works for physics or biochemistry should work for economics too. After all, the methods of discovery and validation that scientists use must be right for the particular domain or sort of thing they are studying.

So one reason to look at economics is that it might teach philosophers of science something new. Indeed, in the second part of this book, I shall argue that natural science-based methodology tends to neglect an important problem of scientific inference: the problem of external validity, or how to generalize experimental results to nonlaboratory settings. I shall suggest that in this case, natural scientists have something to learn from social scientists, rather than the other way around. Another reason to focus on economics is that laboratory experimentation is a fairly new methodology there, and the field has not crystallized yet on a set of rules of "good" scientific practice. As opposed to physics or chemistry, in which methodological discussion has arguably had little effect in changing scientists' habits, the social sciences seem to be more permeable to philosophical arguments. Within experimental economics, in particular, methodological discussion is alive and well, and also potentially influential in the way in which the discipline is taking shape.

Many philosophers of science suffer from a sense of guilt of being useless, and every now and then make an attempt to write something aimed at helping scientists in their everyday work. Unfortunately, their way of engaging scientists is to start with a painfully analytical discussion of abstract issues that are only very indirectly of practical relevance. In

this book, I have tried to avoid that approach and to stick close to the real concerns of experimental scientists. I have tried to keep the detours in abstract philosophical arguments under control, and put philosophy at use in understanding the rationale of down-to-earth methodological principles.

What is in this book?

So this is a book of methodology, devoted to a relatively small but growing field of the social sciences called experimental economics. It discusses the techniques used by experimenters in order to investigate economic phenomena, evaluates them, and occasionally puts forward some advice about how to revise our thinking about laboratory experimentation (its goals, its role, and its tools). It is divided in two parts. In the first part (Inferences within the Experiment), I discuss how the experimental method allows the drawing of tight inferences from data to phenomena, and from phenomena to their causes within a given experimental setting. In the second part (Inferences from the Experiment), I show how, and under which circumstances, it is sometimes possible to generalize an experimental result from laboratory circumstances to some real-world situation.

Eight themes recur in the chapters that follow:

1. Experimental and theoretical knowledge often grow independently from each other.
2. The growth of experimental knowledge is slow and piecemeal: experimental scientists learn little by little rather than attempting great leaps forward.
3. "Local" knowledge of the experimental systems and the background conditions in which hypotheses are tested is crucial for the reliability of scientific inference.
4. Experimental inference is based on "eliminative induction," a process aimed at eliminating alternative interpretations of empirical evidence.
5. Social practices and conventions play a key role in determining when a given phenomenon or hypothesis is accepted as established by experimental means.
6. Experiments act as "mediators" between the real world and the theories, models, and hypotheses we devise to explain its functioning.

7. Real-world applicability is nonetheless the ultimate aim of science, which conveys knowledge of causal relations for intervention and policy making.
8. It is difficult to extend experimental results to real-world circumstances unless we are able to shape the experiment and the real world so as to resemble each other.

Some of these ideas are not new and have been defended before by philosophers working on experimental methodology. Others are philosophically less conventional and spring from the observation of the concrete problems economic experimenters face in their day-to-day work. They should sound familiar to experimenters, however, because they try to capture the concerns that drive their research. The accent on applicability and policy making, for instance, is a common feature of many recent overviews and discussions of experimental economics.[4] However, experimenters often make use of a *rhetoric* of scientific method that is far removed from the reality of their work. Partly this has to do with the fact that methodological norms sometimes serve the purpose of marking political alliances and contrapositions (e.g., "economics vs. psychology") independently of the similarity or dissimilarity of the methods that are effectively used. And partly, it is an effect of the fact that scientific method (like science itself) often evolves by imitation, and paradigmatic experiments often are much more effective in shaping the practices of a discipline than are explicit methodological pronouncements.[5] So, like Vernon Smith, I believe that by and large "if you look at what experimental economists do, not what they say, you get the right picture of science learning" (1994, p. 129). Ideally, one would like to capture all the "good" methodological practice implicit in the work of experimental economists, purged from the "bad" rhetoric that obfuscates experimenters' achievements and, sometimes, diverts them onto dangerous or dead-end trails. By doing this, one can do a service to philosophers and practitioners alike.

It may be useful also to clarify right from the start what this book is *not* about. This is not a handbook of experimental economics. There are already some excellent surveys of the main results in the field (Kagel and

[4] Cf. e.g., Plott (1987), Roth (1991, 2002), Ross Miller (2002), Smith (2002).

[5] A reader indicated Cox, Robertson, and Smith (1982); Isaac, McCue, and Plott (1985); and Grether and Plott (1979) as examples of seminal experiments on (respectively) auctions, public goods, and decision making that were also extremely influential in setting the methodological standards of the discipline. I will return to the issue of the economics–psychology divide again in Chapter 11.

Roth, eds., 1995; Plott and Smith, eds., in press) and it is not my purpose to add to these resources. In some chapters, I shall of course illustrate some experiments, but my aim is neither to be exhaustive nor to present the state of the art in the discipline. I also have no intention to add anything new to the experimental literature – I am interested in philosophy and methodology, and the experiments described in the book are always used as examples of methodological principles, never as novel contributions to experimental research.

This is not a textbook of experimental methodology either, nor a book that will teach you how to design experiments. If you are interested in that, you should consult Davis and Holt (1993), Friedman and Sunder (1994), Bergstrom and Miller (1997), or Friedman and Cassar (2004). This book falls somewhere between the concrete instructions of a textbook and the abstract analysis of classic philosophy monographs. The main reason to pitch the discussion at such a level is that concrete techniques of experimental design must fulfill the sort of higher-level requirements that are customarily discussed in the philosophy of science literature. However, unfortunately, it is difficult to reach any firm conclusion about higher-level methodological principles or requirements unless one keeps in mind the subject-specific problems experimental scientists have to deal with in their daily work. One purpose of this book is to try to fill the gap between abstract philosophy and concrete scientific practice.

Despite my efforts to simplify as much as possible, the level of detail may be at times a bit demanding for the noneconomist. However, I am afraid it is difficult to say anything meaningful about the method of science by sticking to totally unrealistic examples of scientific reasoning like "there are black swans in Australia, therefore it is not true that all swans are white."[6] To help the novice, I have provided a lengthy description of a "normal" economic experiment (Chapter 2) and, for the nonphilosophers, an introduction to basic notions of testing and confirmation in Chapters 3 and 4. Hopefully, both economists and philosophers of science will find something useful in this book.

As far as I'm concerned, I have certainly learned a lot by studying experimental economics. I have been surprised by the ingenious

[6] For the nonphilosophers: "all swans are white" is an alleged example of law-of-nature or scientific theory that is widely abused in the philosophical literature. The first to use the example was, as far as I know, John Stuart Mill (a great philosopher-economist, incidentally).

techniques invented by experimenters, I have been amazed by the robustness of certain results, and not the least, I have realized how addictive experimental work can be. It would be nice if I could transmit only part of this fascination to my nonscientist readers. And at the same time I hope the economists will be convinced that interesting and useful things are taught in (some) philosophy classes.

PART ONE

INFERENCES WITHIN THE EXPERIMENT