Chapter One

Introduction

This is a book about econometrics and the philosophy of economics, two topics that seem worlds apart. Econometrics is a study of good and bad ways to measure economic relations. Philosophy is a study of the nature of things and the principles governing human behavior. And the philosophy of economics is a study that searches for truth and knowledge about the part of reality that pertains to economic matters. This book will show that in economic theory-data confrontations the two topics are inextricably conjoined. Meaningful applied econometrics requires proper understanding of the purport of economic theory, and empirically relevant economic theorizing requires knowledge of the power of applied econometrics.

1.1 THEORY-DATA CONFRONTATIONS IN ECONOMICS

A *theory-data confrontation* is an empirical analysis in which theoretical arguments play an essential role. Economic theory-data confrontations occur in many different situations. In some of these confrontations economists try to establish the empirical relevance of a theory. In others econometricians search for theoretical explanations for observed regularities in their data. In still others economic researchers produce evaluations of performance and forecasts for business executives and government policy-makers.

1.1.1 A Unifying Framework

There is a unifying framework within which we can view the different activities in economic theory-data confrontations. All have a core structure consisting of three parts: two disjoint universes, one for theory and one for data, and a bridge between them. The theory universe is populated by theoretical objects that have all the features that the theory ascribes to them. The elements in the data universe are observations from which we create data for the theory-data confrontation. The bridge is built of assertions that describe the way that elements in the two universes are related to one another.

I think of an economic theory T as one of two abstract ideas. In one case T is a pair (ST, M), where ST denotes a finite set of assertions concerning some economic situation and M is a family of models of these assertions that delineates the relevant characteristics of the situation in question. The other T is a formal theory AT that is developed by the axiomatic method. It consists of the axioms and all the theorems that can be derived from them with the help of logical rules of inference. The intended interpretation of AT delineates the characteristics that its originator considered sufficient to describe the kind of economic situation about which he or she was theorizing.[1]

In economic theory-data confrontations the "theory" (IT) is either the M of a pertinent ST or an interpretation of some AT. For example, let T denote the theory of consumer choice under certainty. In one theory-data confrontation of T, IT might present the way Franco Modigliani and Richard Brumberg's (1955) life-cycle hypothesis views consumer allocation of resources over time. In another confrontation, IT might delineate Kenneth Arrow's (1965) ideas of how consumers allocate their net worth to safe and risky assets. There are many possibilities. Note, therefore, that regardless of what IT supposedly describes, the elements in the corresponding theory universe remain theoretical objects.

The characteristics of theoretical objects are not interesting per se. Hence in a theory-data confrontation, econometricians do not question the validity of IT in its own universe. Instead they ask whether, when the objects in the theory universe have been interpreted by certain bridge principles, they can use IT to deduce true assertions about elements in the data universe. These principles constitute the bridge between the theory universe and the data universe.

In a given theory-data confrontation, the data universe consists of a collection of observations and data. The nature of the observations depends both on IT and on the original purposes for which those observations were collected. The purpose of an empirical analysis need not be the same as the purposes for which the observations were collected. The observations are used to create data for the theory-data confrontation. The makeup of the data depends on IT and the design of the particular empirical analysis.

The theory-data confrontation that I have described above is pictured in Fig. 1.1. On the right-hand side of the figure we see at the bottom the sample population on whose characteristics observations are based. Econometricians use their observations to create data that they feed into the data universe on top. On the left-hand side of the figure we find at the bottom the theory, that is, (ST, M) or AT as the case may be. Thereupon follows IT, the relevant parts of which are fed into the theory universe on top. The bridge between the two universes contains all the bridge principles and nothing else, and the arrows describe the flow of information in the system. Finally, the numbered ellipses are nodes in which researchers receive and send information and decide on what to feed into the pertinent boxes.

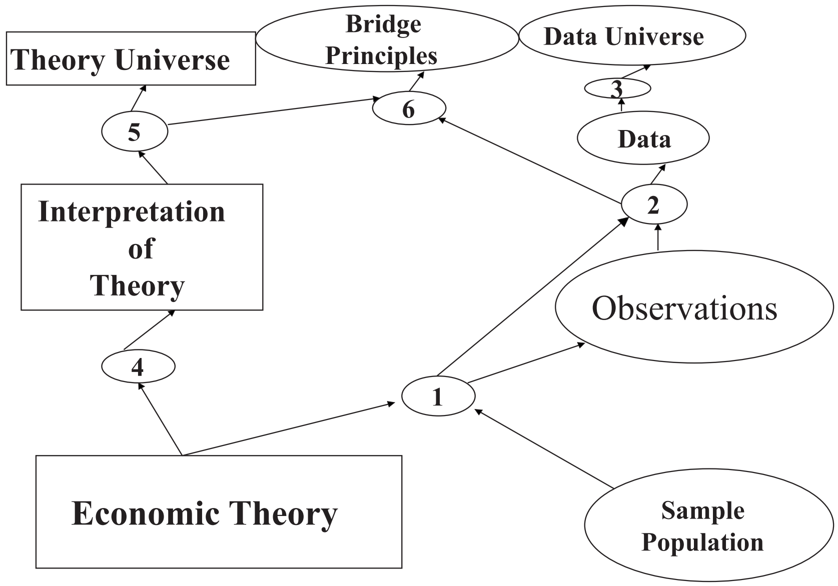

Fig. 1.1 A theory-data confrontation.

1.1.2 A Disturbing Riddle

Economic theory is developed and econometrics is used in the theory-data confrontation to obtain knowledge concerning relations that exist in the social reality. Generating such knowledge is problematic. We have seen that the references of the variables in the theory universe are theoretical objects, for example, toys in a toy economy.[2] It appears, therefore, that meaningful econometric work stands and falls with the references of observations and data in the data universe belonging to the social reality. In Chapter 3, I demonstrate that the references of most variables in contemporary econometric data universes live and function in a socially constructed world of ideas. This world of ideas has little in common with the true social reality. That fact raises a serious question concerning the relevance of contemporary econometrics: How is it possible to gain insight into the social reality with data concerning a socially constructed world of ideas?

Figure 1.2 illustrates the gravity of the situation in which econometricians find themselves. At the top of that figure we discover the top of Fig. 1.1, that is, the theory and data universes and the bridge between them. Below them on the left-hand side is the toy economy in which reside the references of all the variables in the theory universe. On the right-hand side we observe the socially constructed world of ideas that contains the references of all the variables in

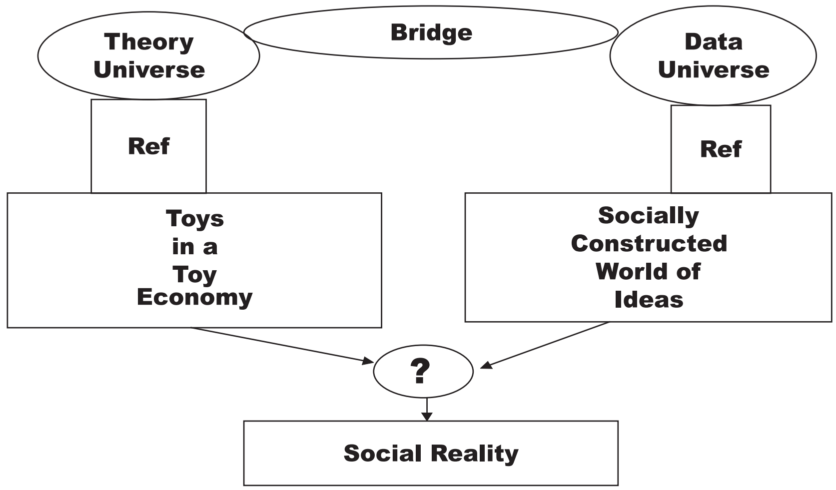

Fig. 1.2 A disturbing riddle.

the data universe. Finally, at the bottom we find the elements that constitute the social reality. The arrows and the question mark underscore the fact that it is uncertain how combining elements from a toy economy with elements from a socially constructed world of ideas enables econometricians to learn interesting things about the social reality.

1.1.3 The Resolution of the Riddle

The question I posed above amounts to asking, *how is a science of economics possible*? A long time ago, Immanuel Kant (1781, 1787) asked a similar question: "Wie ist reine Naturwissenschaft möglich?" ["How is pure natural science possible?"] The answer he gave to his question differs from the answer that I give to mine. Since both the differences and the similarities are interesting to us, I shall recount Kant's answer and the reasons he posited in support of that answer.[3]

Kant's (1787) book, *Kritik der reinen Vernunft*, which in F. Max Müller's translation (Kant, 1966) became *Critique of Pure Reason*, is an analysis of the powers of human reason in gaining knowledge about the world independently of all experience. He argued that knowledge begins with experience but insisted that all knowledge need not arise from experience. Experience suffices to establish that "snow is melting in the streets today." A priori reasoning is needed to ascertain that "every change has its cause." Knowledge gained from experience alone Kant called *empirical knowledge.* All other knowledge he referred to as *knowledge a priori.* Some knowledge a priori can be obtained independently of experience. For example, it can be known from a priori reasoning alone that, "all instances of seven added to five result in twelve." Such knowledge Kant referred to as *pure knowledge* (p. 3).

There are two sources of human knowledge in Kant's theory: sensibility and understanding. The first is the faculty by which objects are received as data. The other is the faculty of judging. Knowledge of objects requires the cooperation of both faculties. "Without sensibility objects would not be given to us; without understanding they would not be thought by us" (p. 45).

Kant distinguished between two kinds of judgments: the *analytic* and the *synthetic*. A judgment is an operation of thought that connects a subject and a predicate. In an *analytic judgment* the concept of the subject contains the idea of the predicate. Examples are "all bachelors are unmarried" and "all bodies are extended." In both cases the idea of the predicate is contained in the idea of the subject. The validity of an analytic judgment can be established by a priori arguments that appeal to the logical relation of subject and predicate.

A *synthetic judgment* is one in which the idea of the subject does not contain the idea of the predicate. Examples are "the object in my hands is heavy" and "seven plus five equals twelve." In either case the predicate adds something to the concept of the subject. Most synthetic judgments are judgments a posteriori in the sense that they arise after an experience. There are also synthetic judgments that are judgments a priori, and their validity can be established by a priori reasoning alone. Of the two examples above, the first is a synthetic judgment a posteriori and the second is a synthetic judgment a priori (pp. 7–10).

Kant believed that necessity and strict universality were characteristic features of synthetical judgments a priori (p. 3). He also insisted that such judgments permeated the sciences. For example, mathematical propositions, such as $5 + 7 = 12$, "are always synthetic judgments a priori, and not empirical, because they carry along with them necessity, which can never be deduced from experience" (p. 10). Similarly, in geometry a proposition such as "between any two points the straight line is the shortest line that connects them" is a synthetic judgment. It is also a priori because it carries with it the notion of universality (p. 11). Finally, natural science (Physica) contains synthetical judgments a priori as principles. Examples are "in all changes of the material world the quantity of matter always remains unchanged" and "in all communication of motion, action and reaction must always be the same." Both judgments are obviously synthetical. They are also a priori since they carry with them the idea of necessity (pp. 10–12).

To solve his original problem Kant had to figure out how synthetical judgments a priori were possible. The best way to do that was to study the functioning of the human mind, and he thought of that functioning as occurring in three stages. In the first, the mind places in *space* and *time* the manifold of experiences that humans receive through their senses. Space and time are not empirical concepts. They are pure forms of sensuous intuition (*Anschauung*) that are necessary representations a priori of all intuitions (pp. 23–35). In the second stage, the mind organizes the manifold matter of sensuous intuitions in forms of sensibility that constitute concepts of the understanding that humans use to judge the meaning of their experiences. Kant believed that the forms exist in the mind a priori, and he attributed the synthesizing act of arranging different representations in forms to twelve basic categories of thought (pp. 54–66). In the third stage, the mind combines categories of thought and established concepts to give a unifying account of the manifold world of sense impressions that humans face. He ascribed the mind's ability to accomplish that to what he deemed the highest principle of cognition: the existence of an a priori synthetical unity of apperception that is constitutive of the synthetical unity of consciousness and hence the *self* (pp. 76–82).

In this context there are two especially interesting aspects of Kant's view of the functioning of the human mind. First, he distinguished between two kinds of reality: the world of phenomena that human beings experience and the world of things as they are in themselves independently of human observation. One can have knowledge about phenomena but not about things-in-themselves (*Dinge-an-sich*). Secondly, knowledge of phenomena is limited by the way human faculties of perception and understanding synthesize experiences. In doing that, the two faculties with application of the ideas of space and time and the twelve categories of thought structure the world of phenomena in accordance with their own way of knowing.

It is remarkable that two abstract ideas, space and time, and just twelve a priori categories of thought should enable humans to relate meaningfully to the world of phenomena and to make synthetic judgments about its constituents that have the marks of necessity and universality. To understand how that is possible requires a closer look at the categories of thought. Kant arranged the categories in four groups with names, *quantity*, *quality*, *relation*, and *modality*. In the quantity group one finds three categories: *unity*, *plurality*, and *totality*. In the quality group reside three other categories: *reality*, *negation*, and *limitation*. In the relation group one finds the categories *inherence and subsistence*, *causality and dependence*, and *community*. Finally, in the modality group resides a fourth triple of categories: *possibility*, *existence*, and *necessity* (pp. 62–66). Kant believed that the categories enabled the mind to delineate characteristic features of objects in the world of phenomena. Examples of how the mind accomplishes that can be found in E 1.1. In reading the examples, note that even the simplest judgments make use of a combination of several categories.[4]

E 1.1 *Consider the collection of four balls on my desk. To say something about all of them, one must perceive what the balls have in common, for example, that "they are all billiard balls." To insist that "only one ball is red," one must be able to comprehend the totality of balls. In such judgments one uses the categories in the quantity group. To judge that "there is a black ball and no green ball" and that "the balls are not soft," one applies the categories in the* *quality group. To judge that "one ball is smaller than the others," that "if one ball is given a push, it will start rolling," and that "the balls are either black or red or blue," one employs the categories in the relation group. Finally, to judge that "it is possible that the balls, if pushed, will roll at uneven speeds," that "one ball has a hole in it," and that "the balls are billiard balls and billiard balls are round," one uses the categories in the modality group.*

Kant believed that the ideas of space and time and the twelve categories enable humans to gain a unified account of the world of phenomena. He also believed that humans are able to use their imagination to develop theories about relations that exist among elements in the world of phenomena. These beliefs and the a priori existence in the human mind of the notion of space and time and the twelve categories provided him with the ideas he needed to show how pure natural sciences such as Euclid's geometry and Newton's dynamics are possible.

The arguments that I shall advance to substantiate my claim that a science of economics is possible differ in many ways from those that Kant used in support of his claim. In my case social reality appears in place of Kant's world of phenomena and the "things-in-themselves" are not in sight. Further, the theories are about toys in a toy economy instead of phenomena and the a priori notion of space and time does not appear. Finally, I make no use of Kant's twelve categories. Even so there is one significant similarity. The unified account of the world of phenomena that Kant's humans gain with the help of categories and an a priori notion of space and time is an account of characteristic features of the objects in the world they experience. Consequently, if he is right in insisting that humans structure the world in accordance with their way of knowing phenomena, the theories that humans develop must also be about characteristic features of objects and relations in the world of phenomena. I believe that economic theories, be they about toys or abstract ideas, attempt to delineate characteristic features of behavior and events that the originators of the theories have observed in the social reality in which they live. I also believe that econometricians can assess the likelihood of the empirical relevance of the characteristic features about which economic theories talk. This book will show that my belief is right, and in doing that it will establish the possibility of a science of economics.

1.1.4 A Final Remark on the Riddle

Wassily W. Leontief (1982), Lawrence H. Summers (1991), Tony Lawson (1997), and many others worry about the dire straits of econometrics and economic theory, and all of them give weighty arguments for their concern. Be that as it may, I have tried to write a book that gives an econometrician the idea that he or she should "get on with the job" while keeping Fig. 1.2 clearly in mind. Cooperating economic theorists and econometricians, the references of their theory and data variables notwithstanding, can learn about characteristic features of the workings of the social reality. Such a happy ending, however, depends crucially in each case on the cooperating scientists delineating the purport of the particular economic theory and giving details of all the relevant bridge principles.

1.2 THE ORGANIZATION OF THIS BOOK

The book deals with social reality and the means economists use to learn about those of its characteristics that they find interesting. That involves saying what social reality means, discussing what the purport of an economic theory is, delineating formal aspects of economic theory-data confrontations, and exhibiting salient features of contemporary applied econometrics. To accomplish that my collaborators and I have written twenty-six chapters that are arranged in seven parts with meaningful titles. In this section I describe briefly the contents of the various parts.

1.2.1 Facts and Fiction in Econometrics

Part I concerns facts and fiction in econometrics. I begin by discussing the facts. To me that means describing how human beings create social reality. I then discuss the fiction, that is, the social construction of reality in economics and econometrics. Finally, I show how, in a fictional reality, cooperating economic theorists and econometricians can gain insights into essential characteristics of the social reality in which we live.

This part covers topics that have occupied the minds of philosophers of social science ever since the publication of Peter L. Berger and Thomas Luckmann's (1966) *The Social Construction of Reality*. The philosophers' discussions centered on two basic problems. How do human beings go about creating the social reality in which they live? What is the "reality" philosophers have in mind when they discuss the "social construction of reality?" Judging from Finn Collin's (1997) authoritative account of the subject matter, there does not seem to be a consensus as to the right answer to either of these two questions.

John R. Searle (1995) gives a philosopher's answer to the first question in his book on *The Construction of Social Reality*. I give an economist's answer in Chapter 2. Searle identifies social reality with the totality of social facts, and he insists that all social facts are facts involving collective intentionality.[5] There is no room in his social reality for things, personal facts, and personal and social possibilities. In my conception, social reality comprises all the things and facts there are, all the possibilities that one or more persons envisage, and nothing else. The missing elements in Searle's notion of social reality constitute fundamental parts of mine. Without them I would have no chance of analyzing the workings of an economic system.

There are several answers to the second question. To Berger and Luckmann the “reality” in the “social construction of reality” is an ordered world of institutions that through a process of socialization receives a certain stability over time. In her book *The Manufacture of Knowledge*, Karin D. Knorr-Cetina (1981) studies life in a laboratory. To her, the scientific products that such a laboratory engenders constitute the reality in the social construction of reality. My vision of the social construction of reality is not about the construction of institutions and not about the production of scientific artifacts. It is rather about the social construction of “objects of thought and representation” (Sismondo, 1993). Thus, the reality in my vision of the social construction of reality is the socially constructed world of ideas that I describe in Chapter 3.

There is one aspect of the production of artifacts in Knorr-Cetina’s laboratory that is particularly interesting. The artifacts are produced in a preconstructed artificial reality with purified chemicals, and specially grown and selectively bred plant and assay rats that are equally preconstructed. Such products cannot be part of “nature” as I understand the term. Nevertheless scientists use such products to further their understanding of processes that are active in nature. For me, the interesting aspect is that it illustrates how knowledge of relations in one world, the laboratory, can be used to gain insight about relations that exist in another world, nature. Analogous problems arise each time an econometrician attempts to use relations in the data universe to establish properties of relations in social reality. I devote Chapter 4 to sorting out the latter problems as they relate to individual choice, market characteristics, and macroeconomic policies.

1.2.2 Theorizing in Economics

Economics is a branch of knowledge concerned with the production and consumption of goods and services and with the commercial activities of a society. In Part II, I discuss the art of theorizing in economics and the purport of an economic theory. My aim is to determine what attitude one ought to adopt toward such a theory.

I begin in Chapter 5 by detailing salient features of the axiomatic method. Greek scientists knew of and used this method more than 2,000 years ago. It was only introduced to economics in William N. Senior’s (1836) treatise, *An Outline of the Science of Political Economy*. Today the axiomatic method constitutes the primary way of theorizing in economics. It features a finite number of assertions, the axioms, and a few rules of inference. The axioms delineate pertinent properties of certain undefined terms, and the rules of inference tell the user how to pass from axioms to theorems and from axioms and theorems to new theorems. The totality of theorems that can be derived from the axioms makes up the searched-for theory. For this book it is important to keep in mind that the theory is about undefined terms, not about anything specific in the world. In different words, it is a theory about symbols and nothing else.

The axiomatic method is powerful and provides intriguing possibilities for theorizing in science as well as in mathematics. Examples of its use in economics and econometrics can be found in Stigum (1990). However, the method is also treacherous. Beautiful theoretical structures have collapsed because the axioms harbored surprising contradictory statements. A well-known example of that is George Cantor's theory of sets (1895, 1897).

A theory is said to be consistent if no contradictory statements can be derived from its axioms. A consistent theory can be made to talk about objects of interest by giving the undefined terms an interpretation that renders all the axioms simultaneously true. Such an interpretation is called a *model* of the axioms and the theory derived from them. Different models of one and the same theory may describe very different matters. For example, one model of the standard theory of consumer choice under certainty may be about the allocation of an individual's income among various commodity groups. Another model may be about the allocation of an individual's net worth among safe and risky assets. In either case the individual may, for all I know, be a family, a rat, or a pigeon.

There are philosophers of science, for example, Wolfgang Balzer, C. Ulises Moulines, and Joseph Sneed (1987), who believe that the best way to think of a scientific theory is to picture it as a set-theoretic predicate that prescribes the conditions that the models of a theory must satisfy. Some of these conditions determine the conceptual framework within which all the models of the theory must lie. Others describe lawlike properties of the entities about which the theory speaks. If one adopts this view of scientific theories, one can think of the scientist as formulating his theory in two steps. He begins by writing down the assertions that characterize the conceptual framework of the theory. He then makes simplifying assumptions about the various elements that play essential roles in its development. The latter assumptions determine a family of models of the original assertions that I take to constitute the searched-for theory. I refer to this way of constructing a scientific theory as *model-theoretic* or *semantic* (cf. Balzer et al., 1987).

I consider that an economic theory of choice or development delineates the positive analogies that the originator of the theory considered sufficient to describe the kind of situation that he had in mind. For example, in an economic theory of choice, a decision-maker may choose among uncertain prospects according to their expected utility. This characterization describes succinctly a particular feature of behavior in the intended reference group of individuals in social reality. Similarly, in an economic theory concerning a given kind of financial market the theory may insist that the family of equilibrium yields on the pertinent instruments are cointegrated ARIMA processes. This characterization describes a characteristic feature of the probability distributions that govern the behavior over time of equilibrium yields in such a market. Whether the positive analogies in question have empirical relevance can only be determined by confronting the given theories with appropriate data.[6]

In reading the preceding observations on the purport of an economic theory it is important to keep the following points in mind: (1) To say that expected utility maximization is a positive analogy of individual choice among uncertain prospects is very different from saying that an individual chooses among such prospects as if he were maximizing expected utility. An individual in the theory's intended reference group, by hypothesis, does choose among uncertain prospects according to their expected utility. (2) Even though an accurate description of the behavior of a decision-maker in a given reference group would exhibit many negative analogies of individual behavior, the positive analogies that the theory identifies must not be taken to provide an approximate description of individual behavior. (3) One's understanding of the theory and the data one possesses determine what kind of questions about social reality one can answer in a theory-data confrontation. With the first and third point I want to rule out of court Milton Friedman's (1953) instrumentalistic view of economic theories. With the second and the third point I want to distance my view from the idea that economic theorems are tendency laws in the sense that John Stuart Mill (1836) gave to such laws.[7]

I confront my view of the purport of an economic theory with the views of leading economic theorists in Chapter 6. It differs both from Max Weber's (1949) and from the classical English economists' view of economic theories. However, I believe that it is very much like the way in which John Maynard Keynes (1936) and Robert M. Solow (1956) think of the import of their theories, and Chapter 6 gives ample evidence of that.

I believe that "rationality" is one of the most misused terms in economics. The concept is ill-defined, and it often gets in the way of sound reasoning. Moreover, we can do without the term and we would be better off if we did. In Chapter 7, I advance arguments to show that my opinion is well taken. Three of these arguments are as follows: (1) Theories of choice for consumers and firms can be formulated and discussed without ever mentioning rationality. (2) The positive analogies that two models of one and the same theory identify may be very different. (3) In a given situation, the optimal choice that two different theories prescribe need not be alike. These and other arguments suggest to me that we ought to substitute for "rational agent" and "rational choice and judgment" less loaded terms such as "rational animal" and "good choice and judgment," the meaning of which I explicate in Chapter 9.[8]

It is interesting to note that the adoption of my view of rationality has an important consequence for applied econometrics. Econometricians cannot use economic theories and data to test the rationality of members of a given population. This is true of human populations since their members are "rational animals" and hence rational by definition. It is also true of other kinds of populations, such as those of rats or pigeons. Their members are not rational. Whether a given population possesses the positive analogies on which an economic theory insists is a matter to be settled in a relevant theory-data confrontation.

Much of economic theorizing today is carried out by mathematical economists at a level of sophistication that is way beyond the comprehension of a large segment of contemporary economic theorists. This work has resulted in beautiful theorems, which have provided important insights in both economics and mathematics. The economic insights come with the interpretations that the mathematical economists give their theories. The importance of such insights, therefore, depends on the relevance of the associated interpretations. In Chapter 8, I discuss two of the most beautiful theorems in mathematical economics. One is by Gerard Debreu and Herbert Scarf (1963) and the other is by Robert J. Aumann (1964, 1966). Both theorems concern the relationship between core allocations and competitive equilibrium allocations in large economies. My results demonstrate that the insight that these theorems provide depends as much on the appropriateness of the topologies that the authors have adopted as on the number of agents in the economy. These results are not meant to detract from the unquestioned importance of the two theorems to mathematical economics. Instead, they provide evidence that it is not sufficient to assign names to the undefined terms and to check the mutual consistency of the axioms when interpreting an economic theory. The originator of a theory owes his readers a description of at least one situation in which the empirical relevance of the theory can be tested.[9]

1.2.3 Theory-Data Confrontations in Economics

The chapters in Part II concern subject matter that belongs in the economic-theory and the interpretation-of-theory boxes in Fig. 1.1. In Part III I discuss topics that concern the contents of the remaining boxes in the same figure.

I begin in Chapter 9 with a discussion of what characteristics an econometrician can, in good faith, expect rational members of a sample population to possess. The characteristics I end up with have no definite meaning. Rather, they are like undefined terms in mathematics that can be interpreted in ways that suit the purposes of the research and that seem appropriate for the population being studied. My main sources here are translations of and philosophical commentaries on Aristotle's treatises, *De Anima* and the *Nicomachean Ethics*. In their spirit I designate a "rational individual" by the term rational animal and identify "rational" choices and judgments with good choices and judgments.

In Chapters 10–12, I search for good ways to formalize the core of an economic theory-data confrontation. In such a formalization the theory universe is a pair (Ω_T, Γ_t), where Ω_T is a subset of a vector space and Γ_t is a family of assertions that the vectors in Ω_T must satisfy. The axioms of the theory universe, Γ_t, need not be the axioms of the pertinent IT in the interpreted-theory box in Fig. 1.1. In the intended interpretation of Γ_t, the members of Γ_t delineate just the characteristics of IT that are at stake in a given theory-data confrontation. Moreover, Γ_t need not constitute a complete axiomatic system. Chapter 10 contains examples of theory universes from consumer choice, the neoclassical theory of the firm, and international trade in which I describe the theory-data confrontation that I have in mind, delineate the salient characteristics of IT, and formulate the axioms in Γ_t.

In Chapter 11, I discuss the contents in the observations and data boxes of Fig. 1.1 and describe ways to construct a data universe to go with a given theory universe. In my formalization of a theory-data confrontation the data universe is a pair (Ω_P, Γ_p), where Ω_P is a subset of a vector space and Γ_p is a family of assertions that the vectors in Ω_P must satisfy. The assertions in Γ_p describe salient characteristics of the observations on which the theory-data confrontation is based and delineate the way the pertinent data are constructed from the observations. Like the axioms of the theory universe, the members of Γ_p need not constitute a complete axiomatic system. In economic theory-data confrontations the data universe is usually part of a triple $[(\Omega_P, \Gamma_p), \Im_p, P_p(\cdot)]$, where $\Im_p$ is a σ-field of subsets of Ω_P, and $P_p(\cdot) : \Im_p \to [0, 1]$ is a probability measure. The probability distribution of the data that $P_p(\cdot)$ generates plays the role of the true probability distribution of the vectors in Ω_P. I call this distribution FP.

The bridge in Fig. 1.1 is composed of assertions $\Gamma_{t,p}$ that, in a subset of $\Omega_T \times \Omega_P$ named the *sample space* and denoted Ω, relate variables in the theory universe to variables in the data universe. Bridges and their principles seem to be objects of thought that econometricians avoid at all costs. Hence, in order not to scare friendly readers I try in Chapter 12 to introduce the topic in a logical way. I begin by discussing theoretical terms and interpretative systems in the philosophy of science. I then give the reasons why bridge principles are needed in applied econometrics and describe ways to formulate such principles in empirical analyses of consumer and entrepreneurial choice. Finally, I present an example that exhibits the functioning of bridge principles in a formal theory-data confrontation.

Traditionally, in philosophy as well as in economics, researchers have learned that the bridge between the two universes in Fig. 1.1 is to be traversed from the theory universe to the data universe and that the empirical analysis is to be carried out in the latter. The last section of Chapter 12 shows that the bridge can be traversed equally well from the data universe to the theory universe and that an interesting part of the empirical analysis can be carried out in the theory universe. That sounds strange, but it is not. In the example E 12.5, I contrast the process of testing a theory in the data universe with testing the same theory in the theory universe. The example shows that the formulation of the bridge principles and the econometrician's inclinations determine in which universe he ought to try his theory. That insight throws new light on the import of exploratory data analysis and also suggests new ways for theorists and econometricians to cooperate in their pursuit of economic knowledge.

In theory-data confrontations in which the data-generating process is random, there are three probability distributions of the variables in the data universe for which the econometrician in charge must account. One is the true probability distribution FP. Another, the so-called *MPD*, is the probability distribution of the data variables that is induced by $\Gamma_{t,p}$ and the joint probability distribution of the variables in the theory universe. The third is the probability distribution that, in David Hendry's (1995) terminology, is a minimal congruent model of the data, a model that mimics the data-generation process and encompasses all rival models.[10] The *MPD* plays a pivotal role in the empirical analysis when the econometrician traverses the bridge from the theory universe to the data universe. The probability distribution of the vectors in Ω_T that one of the minimal congruent models of the data and the bridge principles induce play an equally pivotal role in theory-data confrontations in which the econometrician traverses the bridge from the data universe to the theory universe. Example E 12.5 demonstrates this idea.

When an econometrician traverses the bridge from the theory universe to the data universe, the bridge principles enter his empirical analysis in two ways. First, since the *MPD* depends on the pertinent bridge principles, those become essential parts of the econometrician's characterization of the empirical context in which his theory is being tried. Vide, for example, the errors-in-variables and qualitative-response models in econometrics. The errors in the former and the relationship between true and observed variables in the latter help determine the characteristics of the associated data-generating processes. Second, when confronting his theory with data, the econometrician uses the bridge principles to dress up his theory in terms that can be understood in the given empirical context. The empirical relevance of a theory depends on the extent to which the dressed-up version can make valid assertions about elements in the empirical context that it faces. Since the *MPD* is different from the FP, the double role of the bridge principles in theory-data confrontations raises interesting philosophical problems about the status of bridge principles in empirical analyses. Some of these problems are discussed in Chapters 11 and 17 and others in Chapter 22.

1.2.4 Data Analyses

As a graduate student at Harvard, I learned that it was a sin to "look at the data" before confronting a theory with data. Since then, I have come to believe that the alleged sin need not be a sin at all. In fact, such exploratory data analyses play an important role in my methodological scheme.

The axioms in the theory universe have many models. Thus, in a theory-data confrontation econometricians are usually confronting data with a family of models rather than a single model of the theory. From this it follows that a complete analysis of the empirical relevance of a theory should delineate the contours of a pair of families of models: one for the theory universe and one for the data universe and the *MPD*. The latter family is to characterize the empirical context within which the theory is tested or applied. If this context does not allow us to reject the theory's empirical relevance, the former family of models will delimit the models of the theory that might be empirically relevant in the given context.

In Chapter 13, the first chapter in Part IV, Tore Schweder and Nils Lid Hjort develop a statistical method by which outside information and a novel frequentist prior and posterior analysis can be used to delimit the family of models of the data universe. Briefly, they argue as follows.

In 1930 R. A. Fisher introduced fiducial probability as a means of presenting in distributional terms what has been learned from the data given the chosen parametric model. In clear-cut cases, J. Neyman (1941) found that the fiducial distribution corresponds to his confidence intervals in the sense that fiducial quantiles span confidence intervals with coverage probability equal to fiducial mass between the quantiles. B. Efron (1998) and others picked up the fiducial thread, but used the term confidence distribution because the confidence interpretation is less controversial than the fiducial probability interpretation.[11] Chapter 13 discusses the basic theory of confidence distributions, including a new version of the Neyman-Pearson lemma, which states that confidence distributions based on the optimal statistic have stochastically less dispersion than all other confidence distributions, regardless of how that dispersion is measured.[12] A version of Efron's (1982) abc-method of converting a bootstrap distribution to an approximate confidence distribution is also presented.

To allow data summarized in distributional terms as input to a frequentist statistical analysis, the authors identify the likelihood function related to the prior confidence distribution when the prior is based on a pivotal quantity.[13] This likelihood is termed the reduced likelihood, since it represents the appropriate data reduction, and nuisance parameters are reduced out of the likelihood. The reduced likelihood is a proper one and is often a marginal or a conditional one. It is argued that meta-analyses are facilitated when sufficient information is reported to recover the reduced likelihood from the confidence distribution of the parameter of primary interest. The theory is illustrated by many examples, among them one concerning the Fieller method of estimating the ratio of two regression parameters in a study of monetary condition indexes where no proper confidence distribution exists and one example concerning the assessment of the Alaskan stock of bowhead whales.

In Chapter 14 Harald Goldstein shows how the *corrected ordinary least squares* (COLS) method can be combined with sophisticated moment analyses to determine a statistically adequate characterization of the error-term distribution in stochastic frontier production models. In such models the structure of the error term constitutes an essential element in the description of the empirical context in which the pertinent theory is to be tried. Often the error-term distribution is specified in an ad hoc manner. The diagnostic that Harald develops can help an econometrician make better assumptions about the error term and, in that way, lend increased credibility to an efficient estimation procedure, such as maximum likelihood, at a later stage in his data analysis. In the univariate case he discusses the characteristics of several common specifications of the error-term distribution as well as a semiparametric generalization of the normal-gamma model.

To the best of my knowledge, there is no generally accepted statistical method for analyzing multivariate stochastic frontier models. Harald shows how to analyze one kind of multivariate case by combining the moment method on the residuals with the generalized method of moments (GMM) estimation of the system. He applies his method to studying the model of the transportation industry in Norway that I present in Chapter 12. His results are not quite what I wanted them to be, but such is the life of an applied econometrician. The exciting thing to note is the extraordinary diagnostic opportunities that his method affords econometricians when they search for an adequate characterization of the empirical context in which they are to try their multivariate stochastic frontier models.

In Chapter 15 Christophe Bontemps and Grayham E. Mizon discuss the importance of *congruence* and *encompassing* in empirical modeling and delineate the relationship between the two concepts. They give a formal definition of congruence and discuss its relationship with previous informal definitions. A model is congruent if it fully exploits all the information implicitly available once an investigator has chosen a set of variables to be used in modeling the phenomenon of interest. Though congruence is not testable directly, it can be tested indirectly via tests of misspecification, but as a result more than one model can appear to be congruent empirically. A model is encompassing if it can account for the results obtained from rival models and, in that sense, makes the rivals inferentially redundant. Thus a congruent and encompassing model has no sign of misspecification and is inferentially dominant. A feature of empirically congruent models is that they mimic the properties of the data-generation process: They can accurately predict the misspecifications of noncongruent models; they can encompass models nested within them; and they provide a valid statistical framework for testing alternative simplifications of themselves. These results are consistent with a general-to-simple modeling strategy that begins from a congruent general unrestricted model being successful in practice. An empirical example illustrates these points.

Finally, in Chapter 16 David Hendry and Hans-Martin Krolzig describe a general method that computes a congruent model of the data-generating process that does not nest a simpler encompassing model. Their results constitute a giant step toward the goal of automating the process of model selection in time-series analyses of nonstationary random processes. They argue as follows.

Scientific disciplines advance by an intricate interplay of theory and evidence, although precisely how these should be linked remains controversial. The counterpart in "observational" (as against experimental) subjects concerns empirical modeling, which raises the important methodological issue of how to select models from data evidence. The correct specification of any economic relationship is always unknown, so data evidence is essential to separate the relevant from the irrelevant variables. David and Hans-Martin show that simplification from a congruent general unrestricted model (GUM)—known as general-to-specific (*Gets*) modeling—provides the best approach to doing so.

Gets can be implemented in a computer program for automatic selection of models, commencing from a congruent GUM. A minimal representation is chosen consistent with the desired selection criteria and the data evidence. David and Hans-Martin explain the analytic foundation for their program (*PcGets*) and show, on the basis of simulation studies, that it performs almost as well as could be hoped. With false acceptances at any preset level the correct rejections are close to the attainable upper bound. Yet the study of automatic selection procedures has barely begun—early chess-playing programs were easily defeated by amateurs, but later ones could systematically beat grandmasters. David and Hans-Martin anticipate computer-automated model selection software developing well beyond the capabilities of the most expert modelers: "Deep Blue" may be just around the corner.

1.2.5 Empirical Relevance

The way in which the formal structure of a theory-data confrontation is applied depends on the purpose of the particular empirical analysis. Harald Goldstein and I used it in Chapters 10, 12, and 14 to evaluate the performance of firms in the Norwegian transportation sector. In Part V I use it to determine the empirical relevance of an economic theory. We can check the empirical relevance of one theory at a time. It is also possible to confront two theories with each other and check their empirical relevance in some given situation. I demonstrate that the formal structure is equally applicable in both cases.

In Chapter 17, I begin by discussing how my views of the purport of a theory-data confrontation differs from Karl Popper's (1972) ideas. Then I give a formal characterization of what it means to say that a conjecture or a theory has empirical relevance. Finally, I construct formal tests of the empirical relevance of expected utility theory, of Eli Heckscher (1919) and Bertil Ohlin's (1933) conjecture concerning factor endowments and trade flows, and of Milton Friedman's (1957) permanent-income hypothesis.

The tests in Chapter 17 differ in interesting ways. In the trial of expected utility theory there is only one model of the theory universe and many models of the data universe. The bridge principles come with many models, all of which are independent of the models of the two universes. In the trial of Heckscher and Ohlin's conjecture the data universe has only one model. The theory universe has many models with controversial import. Moreover, it is hard to delineate the subfamily that is relevant in the given empirical analysis. The bridge principles come with many models. Their formulation depends on the models that are chosen for the two universes. Finally, in the trial of the permanent-income hypothesis both universes and the bridge principles come with many models. Further, the bridge principles relate variables in one universe to variables and pertinent parameters in the other universe. In the trials of expected utility theory and of Heckscher and Ohlin's conjecture there is no sample population and no probability measure on the subsets of the sample space. In the trial of the permanent-income hypothesis there is a sample population, a probability measure on the subsets of the sample space, and a sampling scheme. The trial of Friedman's hypothesis has many interesting features, one of which concerns Duhem's trap. I believe that the trial describes a way in which judicious use of my methodology can help circumvent the difficulties about which Pierre Duhem (1954) warned.

In Chapter 18, I confront two theories with the same data. The theories in question concern choice in uncertain situations: the Bayesian theory and a formal version of Maurice Allais's (1988) (U, θ) theory in which uncertain options are ordered according to the values of a Choquet integral rather than their expected utility. I check whether neither, one, or both of them are empirically relevant in a given laboratory situation. In this case the formal structure of the theory-data confrontation contains two disjoint theory universes, one data universe, and two sets of bridge principles. Each theory has its own theory universe and its own set of bridge principles that relate the theory's variables to the variables in the data universe. One interesting aspect of the test is that the set of sentences about the data on which the empirical relevance of one theory hinges is different from the set of sentences that determine the empirical relevance of the other theory. Thus, econometricians may find that they reject the relevance of one theory on the basis of sentences that are irrelevant as far as the empirical relevance of the other theory is concerned. In the chapter I formulate the axioms for a test of the two theories, derive theorems for the test, and present results of a trial test that Rajiv Sarin and I carried out with undergraduate economics majors at Texas A&M University.

There are distinguished econometricians who believe that econometrics puts too much emphasis on testing hypotheses and gives too little weight to evaluating the performance of theories. Clive Granger is one of them. Part V contains a very interesting chapter, "Evaluation of Theories and Models," in which Clive elaborates on ideas that he presented in his 1998 Marshall Lecture at the University of Cambridge. He presents several examples that ought to give food for much afterthought on this matter.

1.2.6 Diagnostics and Scientific Explanation

Scientific explanation is a multifaceted topic of interest to scholars, government policy-makers, and men and women in charge of business operations. Whatever the call for an explanation might be, for example, a faulty economic forecast or a test of a hypothesis that failed, researchers in artificial intelligence (AI) have developed ideas for both the design and the computation of such explanations. The ideas for design delineate criteria that good explanations must satisfy. Those for computation describe efficient ways of calculating the explanation whenever such calculations make sense. I believe that economic and econometric methodology can benefit from adopting ideas that the researchers in AI have generated. Therefore, I begin Part VII by discussing some of these ideas as they appeared in Raymond Reiter's (1980, 1987) seminal articles on diagnostics and default logic.[14]

In Chapter 20, I present Reiter's interesting logic for diagnostic reasoning and discuss its salient characteristics. His diagnostic arguments delineate ways to search for cures of ailing systems, for example, a car that refuses to start, and they provide researchers with means to find reasons for faulty predictions. It is also the case that Reiter's logic can be used to rationalize reasoning that appears in economic journals and books. A good example is Maarten Janssen and Yao-Hua Tan's (1992) use of Reiter's logic to rationalize the arguments that Milton Friedman (1957) advances in support of the permanent-income hypothesis. In the context of this book it is particularly interesting to contrast Reiter's arguments with the kind of diagnostic reasoning that is exemplified in Heather Anderson's extraordinary econometric analysis of the rational expectations hypothesis in Chapter 21.

Most diagnostic reasoning in econometrics is carried out entirely in the realm of a data universe.[15] In the last half of Chapter 20, I use Reiter's default logic as a vehicle to see what happens to such analyses in the broader context of a theory-data confrontation in which bridge principles play an essential role. Formally, the bridge principles are similar to the axioms in the theory and data universes. However, there should be a difference in the attitude that an econometrician ought to adopt toward them. The axioms in the theory universe concern objects in a toy economy. Those in the data universe concern observations and data that the econometrician has created himself. Hence, there is no reason why he should doubt the validity of the axioms in the two universes. In contrast, he usually has no firm evidence as to whether the bridge principles are valid. In the vernacular of logicians, the most he can claim is that he is sure that there are worlds in which they are valid.

When abstracting from my formalism, the role I assign to bridge principles in theory-data confrontations is the one factor that sets my methodology apart from Trygve Haavelmo's (1944) methodology. For that reason it is important that I be able to explicate the logical status of bridge principles in economic theory-data confrontations. I do that in Section 20.3 with the help of a multisorted modal language for science that I developed in Stigum (1990). In this language I postulate that the logical representatives of Γ_t and Γ_p are valid in all possible worlds and that the logical representative of $\Gamma_{t,p}$ is valid in at least one world, but that that world need not be the Real World (RW). From this it follows, in the given language, that a logical consequence A of Γ_t, Γ_p, and $\Gamma_{t,p}$ in the proof of which a member of $\Gamma_{t,p}$ plays an essential role need not be valid in RW. If $\Gamma_{t,p}$ is valid in a world H, then A must be valid in H. A may be valid in many other worlds as well, but I cannot be sure that one of them is RW. Hence, I cannot have more confidence in the validity of A than I have in the validity of $\Gamma_{t,p}$. If I find that A is not valid in RW, I have not run across a contradiction in the ordinary sense of this term. The only thing of which I can be certain is that one of the members of $\Gamma_{t,p}$ that I used in the proof of A is not valid in RW. In the context of a theory-data confrontation, finding that A is not valid in RW calls for a diagnosis in Reiter's sense of the term. I give an example of such a diagnosis at the end of the chapter.

In Herman Ruge Jervell's appendix to Section 20.3, he establishes interesting properties of my language, which he describes in three theorems: a completeness theorem, a cut elimination theorem, and an interpolation theorem, the last of which provides a new grounding for the logical status of bridge principles in theory-data confrontations. Equally important for me, the appendix shows that my arguments have a firm basis.

In Chapter 21 Heather Anderson illustrates the interplay between theory development and data analysis by considering the ability of the rational expectations hypothesis to explain the empirical cointegration structure found in the term structure. She finds that although a standard no-arbitrage theory that incorporates rational expectations can explain some of the properties of Treasury bill yields, this theoretical explanation is incomplete. A broader-based explanation that accounts for government debt and time-varying risk premia can improve predictions of yield movements relative to those predictions based solely on a bill yield spread.

In Chapter 22, I give a formal characterization of the meaning of scientific explanations in economics and econometrics. An explanation is an answer to a why question. It makes something that is not known or understood by the person asking the question clear and intelligible. A scientific explanation is one in which the ideas of a scientific theory play an essential role. In economics this scientific theory is an economic theory, and its ideas are used to provide scientific explanations of regularities that applied economists and econometricians have observed in their data.

The form in which the causes of events and the reasons for observed phenomena are listed and used in scientific explanations differ among scientists, even within the same discipline. There is, therefore, a need for formal criteria by which one can distinguish the good from bad. These criteria must list the necessary elements of a scientific explanation and explicate the ideas of a logically and an empirically adequate scientific explanation. I provide such criteria for scientific explanations in economics and econometrics.

My formal account of scientific explanations in Chapter 22 differs in many ways from Carl G. Hempel's (1965, pp. 245–251) deductive-nomological scheme (DNS) for scientific explanations. According to Hempel, a scientific explanation of an event or a phenomenon must have four elements: (1) A sentence E that describes the event or phenomenon in question; (2) a list of sentences $C_1, \ldots, C_n$ that describes relevant antecedent conditions; (3) a list of general laws $L_1, \ldots, L_k$; and (4) arguments that demonstrate that E is a logical consequence of the C's and the L's. In my account, E describes salient features of a data universe, the C's are axioms of the data universe, the L's are axioms of a theory universe, and the logical arguments demonstrate that E is a logical consequence of the C's, the L's, and bridge principles that, in a pertinent sample space, relate variables in the two universes to one another. The explanation is logically adequate if E is not a logical consequence of the C's. It is empirically adequate if the L's are relevant in the empirical context in which the explanation takes place.

The important differences between the DNS and my account of scientific explanation are twofold: (1) The L's of the DNS concern matters of facts in a data universe. My L's concern life in a toy economy. (2) The L's of the DNS are laws that are valid irrespective of time and place. My L's are theoretical claims of limited empirical relevance. The differences notwithstanding, Hempel's fundamental symmetry thesis concerning explanation and prediction is valid for a logically and empirically adequate explanation in my account of scientific explanation as well as for an adequate explanation in the DNS.

Since the L's are about life in a toy economy and the E concerns matters of facts in a data universe, the relevant bridge principles play a pivotal role in my account of scientific explanations in economics. To make sure that the role of the bridge principles is understood I formulate two equivalent explications of such explanations, SE 1 and SEM 1, the first with the means of real analysis and the second with the help of the modal-logical language that I developed in Chapter 20. I exemplify the use of SE 1 and SEM 1 by giving a real-valued and a modal-logical scientific explanation of a characteristic of individual choice that Maurice Allais and his followers have observed in their tests of the expected utility hypothesis.

The situations that call for scientific explanations in econometrics differ from the situations already envisaged in one fundamental way. In SE 1, E is a family of sentences each one of which has a truth value in every model of (Ω_P, Γ_p). In econometrics E is often a family of statistical relations. For example, in econometrics an E may insist that "the prices of soybean oil and cottonseed oil vary over time as two cointegrated ARIMA processes." This E has no truth value in a model of (Ω_P, Γ_p). To provide scientific explanations of such E's statistical arguments are required.

At the end of Chapter 22, I list the elements and the requirements of a logically and empirically adequate scientific explanation in econometrics. In the situations that econometricians face, the E is a pair (H_1, H_2), where H_1 describes conditions that the vectors in (Ω_P, Γ_p) satisfy and H_2 delineates salient characteristics of the FP. As in economics, the antecedent conditions are members of Γ_p, the L's are axioms in a theory universe (Ω_T, Γ_t) and the bridge principles $\Gamma_{t,p}$ describe how variables in the theory universe are related to variables in the data universe. In addition, there is a probability measure $P(\cdot)$ on subsets of the sample space Ω, and logical arguments that show that H_1 is a consequence of Γ_t, Γ_p, and $\Gamma_{t,p}$ and that the *MPD* that Γ_t, Γ_p, and $\Gamma_{t,p}$ and the axioms of $P(\cdot)$ determine has the characteristics that H_2 imposes on FP. The explanation is logically adequate if (H_1, H_2) is not a logical consequence of Γ_p and the axioms of the probability measure on subsets of Ω_P that generates FP. It is empirically adequate if the theory is relevant in the empirical context in which the explanation is taking place. Chapter 23 contains an example of such an explanation.

In Chapter 23 Heather Anderson, Geir Storvik, and I give a logically and empirically adequate scientific explanation of a characteristic of the Treasury bill market that Anthony Hall, Heather Anderson, and Clive Granger observed in Hall et al. (1992). The economic-theoretic formalism and the statistical analysis of the data that our explanation comprises differ in interesting ways from the arguments that Hall et al. advanced in explaining their findings. Our arguments also differ from the diagnostic arguments that Heather uses in Chapter 21 to assess the ability of the rational expectations hypothesis to account for the stochastic properties of the Treasury bill market. In the context of the book the interplay between economic theory and statistical arguments that the chapter exhibits and the new insight that it offers concerning the relationship between the *MPD* and the FP are particularly exciting.

1.2.7 Contemporary Econometric Analyses

In theory-data confrontations econometricians test hypotheses and estimate parameters of interest. When testing hypotheses, they base their procedures on several fundamental ideas. One of them originated in Neyman and Pearson (1928, 1933) and concerns the relative importance of the chance of rejecting a hypothesis when it is true versus the chance of accepting it when it is false. Another is the notion of *optimum confidence sets* as set forth in Neyman (1937). A third, of more recent origin, is the idea of *an encompassing data-generating process* as formalized in Mizon (1984). When estimating parameters, econometricians also base their procedures on several fundamental ideas. One of them is *the principle of maximum likelihood*, which originated in Fisher (1922). Another is L. Hansen and K. Singleton's (1982) idea of a *generalized method of moments estimator*). A third is a cluster of ideas concerning the conditions that an equation's independent variables must satisfy, for example, exogeneity (Koopmans, 1950) and Granger causality (Granger, 1969).

Hypothesis testing and parameter estimation can be viewed as characteristic aspects of games that statisticians play against Nature. Abraham Wald (1947, 1950) used this idea to develop a formal unifying account of hypothesis testing and estimation in his theory of decision procedures. In Wald's theory a game is a quadruple consisting of a sample space S; a set of pure strategies for Nature, Φ; a space of randomized strategies for the econometrician, Ψ; and a risk function $\rho(\cdot)$. Each part of the quadruple varies with the statistical problem on hand. Here it suffices to identify a strategy of Nature in tests with the true hypothesis and in estimation problems with the true parameter values. Moreover, the sample space is a triple $[\mathrm{O}, \Phi, q(\cdot|\cdot)]$, where $\mathrm{O} \subset R^n$ is the space of observations, $q(\cdot|\cdot) : \mathrm{O} \times \Phi \to R_+$ is a function, and $q(\cdot|\theta)$ is the probability density of the econometrician's observations for each choice of strategy by nature, θ. The space of randomized strategies is a triple $[\mathrm{A}, \mathrm{O}, \psi(\cdot|\cdot)]$, where A is a set of acts, $\psi(\cdot|\cdot) : \{\text{subsets of A}\} \times \mathrm{O} \to [0, 1]$ is a function, and $\psi(B|r)$ measures the probability that the econometrician will choose an act in B for each $B \subset A$ and each observed r. In tests A may be a pair {accept, reject} and in estimation problems it may be a subset of R^k, where k is the number of relevant parameters. Finally, the risk function $\rho(\cdot)$ is defined by the equation

$$\rho(\theta, \psi) = \int_{\mathrm{O}} \int_A L(\theta, a) d\psi(a|r) q(r|\theta) dr,$$

where $L(\cdot) : \Phi \times A \to R$ is the econometrician's loss function. In tests of hypotheses the range of $L(\cdot)$ may be a triple $\{0, b, 1\}$, and in estimation problems $L(\theta, a)$ may simply equal $\|\theta - a)\|^2$, where $\| \cdot \|$ is a suitable norm.

The econometrician's optimal strategies depend on the assumptions that he makes about nature's choice of strategies. I consider three possibilities: (1) Nature has chosen a pure strategy and the econometrician must guess which one it is. (2) Nature has chosen a mixed strategy, which I take to be the econometrician's *prior* on Φ. (3) Nature is out to do the econometrician in and has chosen a strategy that maximizes the econometrician's expected risk.

Corresponding to Nature's choice of strategies, there are three classes of the econometrician's optimal strategies to consider: the admissible strategies, the Bayes strategies, and the minimax strategies. These are defined as follows.

1. A given $\psi \in \Psi$ is *inadmissible* if and only if there is a $\psi^* \in \Psi$ such that for all $\theta \in \Phi$, $\rho(\theta, \psi^*) \leq \rho(\theta, \psi)$ with inequality for some θ. Otherwise ψ is *admissible.*
2. For a given prior $\xi(\cdot)$ on the subsets of Φ, ψ^* is a *Bayes strategy* if

$$\int_\Phi \rho(\theta, \psi^*) d\xi(\theta) \leq \int_\Phi \rho(\theta, \psi) d\xi(\theta) \text{ for all } \psi \in \Psi.$$

The value of the left-hand integral is called the *Bayes risk* of $\xi(\cdot)$.

3. A pair (ξ^*, ψ^*) constitutes a pair of "good" strategies for Nature and the econometrician if, for all priors $\xi(\cdot)$ on the subsets of Φ and for all $\psi \in \Psi$,

$$\int_\Phi \rho(\theta, \psi^*) d\xi(\theta) \leq \int_\Phi \rho(\theta, \psi^*) d\xi^*(\theta) \leq \int_\Phi \rho(\theta, \psi) d\xi^*(\theta).$$

Here it is interesting to note that the econometrician's "good" strategy minimizes his maximum risk, and Nature's "good" strategy maximizes the econometrician's minimum risk. Hence, in the vernacular of game theorists, the econometrician's "good" strategy is a *minimax* strategy, and Nature's "good" strategy is a *maximin* strategy.

It seems beyond dispute that an econometrician's choice of strategy is justifiable only if it is admissible. When Φ is discrete and $\xi(\cdot)$ assigns positive probability to each and every strategy of nature, the econometrician's Bayes strategy against $\xi(\cdot)$ is admissible. Moreover, if there is one and only one minimax strategy ψ^*, then ψ^* is admissible. Finally, if ψ^* is admissible and $\rho(\cdot, \varphi^*)$ is constant on Φ, then ψ^* is a minimax strategy. However, Bayes's strategies and minimax strategies need not be admissible. In fact, in many multivariate estimation problems the standard minimax estimator is inadmissible, as was first noted by Stein (1956).[16]

Viewing statistical problems as games against Nature gave statisticians a new vista that engaged the best minds of statistics and led to a surge of interesting new theoretical results in mathematical statistics. It also brought to the fore philosophical issues, the discussion of which split econometrics into two nonoverlapping parts: *classical* and *Bayesian* econometrics. I believe that this split was fortuitous rather than detrimental. It added a second dimension to econometrics that gave econometricians a deeper understanding of the implications of their basic attitudes. Information concerning the development and achievements of statistical decision theory during the last half of the twentieth century can be found in James O. Berger's (1985) book *Statistical Decision Theory and Bayesian Analysis.*[17] Details of the philosophical issues that concern choice of priors and the characteristics of exchangeable processes on ordinary and conditional probability spaces can be found in Stigum (1990, chs. 17, 18).

Part VII includes four chapters—one "Bayesian" and three "classical"—that illustrate four different ways that econometricians analyze important economic problems today. These chapters are important in the context of this book as they provide an indication of the kind of questions that econometricians dare to ask and how they go about answering them. Moreover, they exemplify contemporary applied econometrics at its best.

In Chapter 24, Erik Biørn analyzes an 8-year panel of Norwegian manufacturing firms to determine how materials and capital inputs respond to output changes. Panel data from microunits are a valuable source of information for theory-data confrontation in econometrics. They give the researcher the opportunity of "controlling for" unobserved individual and/or time-specific heterogeneity, which may be correlated with the included explanatory variables. Moreover, when the distribution of latent regressors and measurement errors satisfy certain weak conditions, it is possible to handle the heterogeneity and the errors-in-variables problems jointly and estimate slope coefficients consistently and efficiently without extraneous information. Finally, they make the errors-in-variables identification problem more manageable than unidimensional data (i.e., pure cross-section or pure time-series data) partly because of the repeated measurement property of panel data and partly because of the larger set of linear data transformations available for estimation. Such transformations are needed to compensate for unidimensional "nuisance variables" such as unobserved heterogeneity. Erik illustrates this in Chapter 24. The estimators considered are standard panel-data estimators operating on period specific means and GMM's. The latter use either equations in differences with level values as instruments or equations in levels with differenced values as instruments. Both difference transformations serve to eliminate unobserved individual heterogeneity. Erik illustrates these approaches by examples relating the input response to output changes of materials and capital inputs from the panel of Norwegian manufacturing firms.

In Chapter 25, Herman van Dijk surveys econometric issues that are considered fundamental in the development of Bayesian structural inference within a simultaneous equation system (SEM).

The difficulty of specifying prior information that is of interest to economists and yields tractable posterior distributions constitutes a formidable problem in Bayesian studies of SEMs. A major issue is the nonstandard shape of the likelihood owing to reduced rank restrictions, which implies that the existence of structural posterior moments under vague prior information is a nontrivial issue. Herman illustrates the problem through simple examples using artificially generated data in a so-called limited information framework, where the connection with the problem of weak instruments in classical economics is also described.

A promising new development is Bayesian structural inference of implied characteristics, in particular dynamic features of an SEM. Herman illustrates the potential of such Bayesian structural inference, using a predictivist approach for prior specification and Monte Carlo simulation techniques for computational purposes, by means of a prior and posterior analysis of the U.S. business cycle in a period of depression. A structural prior is elicited through investigation of the implied predictive features.

Herman argues that Bayesian structural inference is like a phoenix. It was almost a dead topic in the late 1980s and early 1990s. Now, it has new importance in the study of models where reduced rank analysis occurs. These models include structural VARs, APT asset price theory models of finance, dynamic panel models, time-varying parameter models in the structural time-series approach, and consumer and factor demand systems in production theory.

In Chapter 26, Jeffrey Dubin and Daniel McFadden discuss and derive a unified model of the demand for consumer durables and the derived demand for electricity. They point out that within the context of their model it becomes important to test the statistical exogeneity of appliance dummy variables that are included in the demand for electricity equations. If the demand for durables and their use are related decisions by the consumer, specifications that ignore this fact will lead to biased and inconsistent estimates of price and income elasticities.

In their chapter Jeffrey and Dan set out to test the alleged bias using observations on a sample of households that the Washington Center for Metropolitan Studies gathered in 1975.

In Chapter 27, Dale Jorgenson develops new econometric methods for estimating the parameters that describe technology and preferences in economic general equilibrium models. These methods introduce a whole new line of research in which econometrics becomes an essential ingredient in applied general equilibrium analysis. Dale applies his methods to studying the behavior of samples of U.S. firms and consumers. The chapter, as it appears in this book, is an edited version of Chapter 2 in Volume 2 of a monograph on issues of economic growth that MIT Press published for Dale in 1998.

NOTES

1. Here the S in ST is short for "semantic" and (ST, M) is a theory that has been developed by model-theoretic means. In addition, the A in AT is short for "axiomatic" and AT is a formal axiomatic theory, that is, a theory that has been developed by the axiomatic method. Chapter 5 describes characteristic features of these two different ways of developing economic theories.

2. I owe the idea of a toy economy to a lecture that Robert Solow gave in Oslo on his way back from the 1987 Nobel festivities in Stockholm. However, I am not sure that Solow will accept the way I use his ideas here.

3. My main sources of reference are the 1966 Anchor edition of F. Max Müller's (1881) translation of Immanuel Kant's (1781, 1787) *Kritik der reinen Vernunft*, *Critique of Pure Reason*, and relevant chapters in Frederick Coplestone's (1994) *History of Philosophy*, *Filosofi og Vitenskap* by T. Berg Eriksen et al. (1987), and S. E. Stumpf's (1977) *Philosophy: History and Problems*. If nothing else is said, the page numbers in the text refer to pages in the Anchor edition of Müller's translation.

4. I owe the idea of this example to Guttorm Fløistad's marvelous account of Kant's theory of knowledge in Berg Eriksen et al. (1987, pp. 485–503)

5. I explicate the ideas of "intentionality" and "collective intentionality" in Chapter 2. Here it suffices to say that collective intentionality refers to the intentional mental state of a group of individuals who have a sense of doing something together.

6. I explicate the ideas of a positive analogy and a negative analogy in Chapter 6. Here it suffices to say that a positive analogy for a group of individuals (or a family of events)

is a characteristic that the members of the group (family) share. A negative analogy is a characteristic that only some of the members of the group (family) possess. When one searches for the empirical relevance of a given set of positive analogies, one is checking whether there are groups of individuals or families of events, as the case may be, whose members share the characteristics in question.

7. There are interesting observations on *ceteris paribus* clauses and tendency laws in Mark Blaug's (1990, pp. 59–69) discussion of Mill and in Daniel M. Hausmann's (1992, ch. 8) account of inexactness in economic theory. Also, Lawrence Summers' (1991, pp. 140–141) stories of successful pragmatic empirical work provide insight into the way economic theorists learn about the positive analogies that their theories identify.

8. Chapter 1 of Hausmann (1992) has a good account of the average economist's idea of rationality.

9. This view of mathematical economics I share with Trygve Haavelmo. In his treatise Haavelmo (1944, p. 6) observes that "[many] economists consider 'mathematical economics' as a separate branch of economics. The question suggests itself as to what the difference is between 'mathematical economics' and 'mathematics.' Does a system of equations, say, become less mathematical and more economic in character just by calling x 'consumption,' y 'price,' etc.? . . . [Any piece of mathematical economics remains] a formal mathematical scheme, until we add *a design of experiments* that [details] what real phenomena are to be identified with the theoretical [variables, and describes how they are to be measured]."

10. The ideas of congruence and encompassing are discussed in depth in Chapters 15 and 16. For a summary account of these ideas the reader can consult Section 1.2.4.

11. Fisher intended his fiducial probability to be an alternative to the Bayesian posterior distribution of a parameter with a noninformative prior. The corresponding fiducial distribution contains all the information about a parameter that one can obtain with a given statistical model from the data alone without use of an informative prior distribution.

12. A statistic is optimal if it leads to most powerful tests or most discriminating confidence intervals. In the Fisherian likelihood tradition, optimal statistics might also emerge from conditioning on ancillary statistics.

13. A pivotal quantity (a pivot) is a function of the parameter and the data that satisfies two requirements. First, it should be monotone in the parameter for all possible data. Second, it must have a fixed distribution, regardless of the value of the parameter. The t-statistic is the archtypical pivot. All confidence intervals are essentially built on pivots.

14. For more recent literature on diagnostics the interested reader can refer to W. Marek and M. Truszczynski's (1993) book *Nonmonotonic Logic*.

15. A good example is Anthony Hall and Adrian Pagan's (1983) marvelous account of "Diagnostic Tests as Residual Analysis."

16. The reader can find an interesting discussion and instructive examples of inadmissible Bayes and minimax strategies in Berger (1985, pp. 253–256, 359–361).

17. For a discussion of some of these matters as they concern tests of hypotheses, prediction, and sequential analysis in the econometrics of nonstationary time series the interested reader is referred to Stigum (1967).

REFERENCES

Allais, M., 1988, "The General Theory of Random Choices in Relation to the Invariant Cardinal Utility Function and the Specific Probability Function," in: *Risk, Decision and Rationality*, B. R. Munier (ed.), Dordrecht: Reidel.

Arrow, K. J., 1965, *Aspects of the Theory of Risk Bearing*, Helsinki: Academic Book Store.

Aumann, R., 1964, "Markets with a Continuum of Traders," *Econometrica* 32, 39–50.

Aumann, R., 1966, "Existence of Competitive Equilibria in Markets with a Continuum of Traders," *Econometrica* 34, 1–17.

Balzer, W., C. U. Moulines, and J. Sneed, 1987, *An Architectonic for Science: The Structuralist Program*, Dordrecht: Reidel

Berg Eriksen, T., K. E. Tranøy, and G. Fløistad, 1987, *Filosofi og Vitenskap*, Oslo: Universitetsforlaget AS.

Berger, J. O., 1985, *Statistical Decision Theory and Bayesian Analysis,* 2nd Ed., New York: Springer-Verlag.

Berger, P. L., and T. Luckmann, 1967, *The Social Construction of Reality*, New York: Anchor Books, Doubleday.

Blaug, M., 1990, *The Methodology of Economics*, Cambridge: Cambridge University Press.

Cantor, G., 1895, "Beitrage zur Begrundung der transfiniten Mengenlehre, Part I," *Mathematische Annalen* 46, 481–512.

Cantor, G., 1897, "Beitrage zur Begrundung der transfiniten Mengenlehre, Part II," *Mathematische Annalen* 47, 207–246.

Collin, F., 1997, *Social Reality*, New York: Routledge.

Coplestone, F., 1994, *A History of Philosophy, Vol. 6,* New York: Doubleday.

Debreu, G., and H. Scarf, 1963, "A Limit Theorem on the Core of an Economy," *International Economic Review* 4, 235–246.

Duhem, P., 1954, *The Aim and Structure of Physical Theory*, P. P. Wiener (trans.), Princeton: Princeton University Press.

Efron, B., 1982, *The Jackknife, the Bootstrap and Other Resampling Plans*, Philadelphia: Society for Industrial and Applied Mathematics.

Efron, B., 1998, "R.A. Fisher in the 21st Century (with Discussion)," *Statistical Science* 13, 95–122.

Fisher, R. A., 1922, "On the Mathematical Foundation of Theoretical Statistics," *Philosophical Transactions of the Royal Society of London*, Series A 222, 309–368.

Fisher, R. A., 1930, "Inverse Probability," *Proceedings of the Cambridge Philosophical Society* 26, 528–535.

Friedman, M., 1953, "The Methodology of Positive Economics," in: *Essays in Positive Economics*, Chicago: University of Chicago Press.

Friedman, M., 1957, *A Theory of the Consumption Function*, Princeton: Princeton University Press.

Granger, C. W. J., 1969, "Investigating Causal Relations by Econometric Models and Cross-spectral Methods," *Econometrica* 37, 424–438.

Granger, C. W. J., 1999, *Empirical Modeling in Economics*, Cambridge: Cambridge University Press.

Haavelmo, T., 1944, "The Probability Approach in Econometrics," *Econometrica* 12 (Suppl.), 1–118.

Hall, A. D., and A. R. Pagan, 1983, "Diagnostic Tests as Residual Analysis," *Econometric Reviews* 2, 159–218.

Hall, T., H. Anderson, and C. W. J. Granger, 1992, "A Cointegration Analysis of Treasury Bill Yields," *The Review of Economics and Statistics* 74, 116–126.

Hansen, L. P., and K. J. Singleton, 1982, "Generalized Instrumental Variables Estimators of Nonlinear Rational Expectations Models," *Econometrica* 50, 1269–1286.

Hausman, D. M., 1992, *The Inexact and Separate Science of Economics*, Cambridge: Cambridge University Press.

Heckscher, E., 1919, "The Effect of Foreign Trade on the Distribution of Income," *Ekonomisk Tidsskrift*, 21, 497–512.

Hempel, C. G., 1965, *Aspects of Scientific Explanation and Other Essays in the Philosophy of Science*, New York: The Free Press.

Hendry, D. F., 1995, *Dynamic Econometrics*, Oxford: Oxford University Press.

Janssen, M. C. W., and Yao-Hua Tan, 1992, "Friedman's Permanent Income Hypothesis as an Example of Diagnostic Reasoning," *Economics and Philosophy* 8, 23–49.

Kant, I., 1966, *Critique of Pure Reason*, F. M. Müller (trans.), New York: Doubleday.

Keynes, J. M., 1936, *General Theory of Employment, Interest and Money*, New York: Harcourt Brace.

Knorr-Cetina, K. D., 1981, *The Manufacture of Knowledge*, Oxford: Pergamon Press.

Koopmans, T. C. (ed.), 1950, *Statistical Inference in Dynamic Economic Models*, Cowles Commission Monograph 10, New York: Wiley.

Lawson, T., 1997, *Economics and Reality*, London: Routledge

Leontief, W., 1982, Letter in *Science* 217, 104–107.

Marek, W., and M. Truszczynski, 1993, *Nonmonotonic Logic*, Berlin: Springer-Verlag.

Mill, J. S., 1836, "On the Definition of Political Economy; and on the Method of Investigation Proper to It" reprinted in 1967, in: *Collected Works, Essays on Economy and Society*, *Vol. 4*, J. M. Robson (ed.), Toronto: University of Toronto Press.

Mizon, G. E., 1984, "The Encompassing Approach in Econometrics," in: *Econometrics and Quantitative Economics*, D. Hendry and K. F. Wallis (eds.), London: Blackwell.

Modigliani, F., and R. Brumberg, 1955, "Utility Analysis and the Consumption Function: An Interpretation of Cross-section Data," in *Post-Keynesian Economics*, K. K. Kurihara (ed.), London: Allen and Unwin.

Neyman, J., 1937, "Outline of a Theory of Statistical Estimation Based on the Classical Theory of Probability," *Philosophical Transactions of the Royal Society of London*, Series A 236, 333–380.

Neyman, J., 1941, "Fiducial Argument and the Theory of Confidence Intervals," *Biometrika* 32, 128–150.

Neyman, J., and E. S. Pearson, 1928, "On the Use and Interpretation of Certain Test Criteria for Purposes of Statistical Inference," *Biometrika* 20A, 175–240, 263–294.

Neyman, J., and E. S. Pearson, 1933, "On the Problem of the Most Efficient Test of Statistical Hypotheses," *Philosophical Transactions of the Royal Society of London*, Series A 231, 289–337.

Ohlin, B., 1933, *Interregional and International Trade*, Cambridge: Harvard University Press

Popper, K. R., 1972, *Conjectures and Refutations*, London: Routledge & Kegan Paul.

Reiter, R., 1980, “A Logic for Default Reasoning,” *Artificial Intelligence* 13, 81–132.

Reiter, R., 1987, “A Theory of Diagnosis from First Principles,” *Artificial Intelligence* 32, 57–95.

Searle, J. R, 1995, *The Construction of Social Reality*, New York: Penguin.

Senior, W. N., 1836, *An Outline of the Science of Political Economy*, reprinted in 1965, New York: A. M. Kelley.

Senior, W. N., 1850, *Political Economy*, London: Griffin.

Sismondo, S., 1993, “Some Social Constructions,” *Social Studies of Science* 23, 515–553

Solow, R. M., 1956, “A Contribution to the Theory of Economic Growth,” *Quarterly Journal of Economics* 70, 65–94.

Stein, C. M., 1956, “Inadmissibility of the Usual Estimator for the Mean of a Multivariate Normal Distribution,” in: *Proceedings of the Third Berkeley Symposium on Mathematical Statistics and Probability, Vol. I*, Berkeley: University of California Press.

Stigum, B. P., 1967, “ A Decision Theoretic Approach to Time Series Analysis,” *The Annals of the Institute of Statistical Mathematics* 19, 207–243.

Stigum, B. P., 1990, *Toward a Formal Science of Economics*, Cambridge: MIT Press.

Stumpf, S. E., 1977, *Philosophy: History and Problems*, New York: McGraw-Hill.

Summers, L. H., 1991, “The Scientific Illusion in Empirical Macroeconomics,” *Scandinavian Journal of Economics* 93, 128–148.

Wald, A., 1947, *Sequential Analysis*, New York: Wiley.

Wald, A., 1950, *Statistical Decision Functions*, New York: Wiley.

Weber, M., 1949, *The Methodology of the Social Sciences*, E. Shils and H. Finch (trans.). New York: Free Press.