Chapter 1
Towards the Methodological Turn in the Philosophy of Science

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Abstract This chapter provides an introduction to the study of the philosophical notions of mechanisms and causality in biology and economics. This chapter sets the stage for this volume in three ways. First, it gives a broad review of the recent changes and current state of the study of mechanisms and causality in the philosophy of science. Second, consistent with a recent trend in the philosophy of science to focus on scientific practices, it in turn implies the importance of studying the scientific methods employed by researchers. Finally, by way of providing an overview of each chapter in the volume, this chapter demonstrates that biology and economics are two fertile fields for the philosophy of science and shows how biological and economic mechanisms and causality can be synthesized.

1 Introduction

In the philosophy of science, interest has recently shifted from scientific concepts to scientific practices. That means what really matters to philosophers of science, and what philosophical discussions should be based on, is what scientists actually do and how they do it rather than philosophers' visage of what science is and how

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scientists should do it. This application of Hume's guillotine is one of the prevailing trends in the late twentieth century and is sometimes considered as a kind of *naturalism*. Philosophical naturalism is received in various ways. Despite the opposition of supernaturalistic or a priori explanations, as the name suggests, the main theme of naturalism is to align philosophy of science with science and to pay special attention to scientific methods. A sophisticated investigation of the naturalization of philosophy of science requires addressing the questions of how philosophy is naturalized to a specific science and in what respects particular sciences and philosophies of those sciences are similar to one another (Giere 1999, 2008). In contrast, a broadly defined naturalism, which is widely shared by philosophers of science (even by those who do not identify themselves as naturalistic philosophers of science), suggests a two-way study: It on the one hand focuses on scientific practices that matter to philosophical investigations and on the other hand examines philosophical concepts in terms of scientists' work and the devices they employ.[1]

More importantly, as Ronald Giere (2008) points out, philosophical naturalism in turn implies a *methodological stance*. What Giere means is to characterize naturalism as a method that seeks a naturalistic explanation (Giere 2008, p. 214). However, it can be easily extended to a more general naturalistic program that stresses scientific methods. William Bechtel (2008, p. 8) well describes this type of position by stating that the naturalistic philosophy of science attempts to understand science by addressing the following questions: What are the objectives of scientific inquiry? What methods are used to obtain the results? How are the methods and results of science evaluated? How do value issues impinge on the conduct of science? Since the answers to Bechtel's questions crucially require examining scientific methods, the philosophical perspective offered by naturalism necessarily turns to methodology. This edited volume contributes to such a *methodological turn* in the philosophy of science.

In this edited volume, we specifically investigate mechanism and causality in biology and economics. Why do we target mechanism and causality? Despite the fact that they both stand long as important conceptions in the philosophy of science, causality and mechanism are two main guiding ideas that underlie scientists' practices of making explanations. To identify the characteristics of a scientific explanation, we need first to explore what causality and mechanism are and how scientists infer their existence, then conjoin the discussion of causality with that of mechanism for a comparative study.

We particularly focus on the context of biology and economics for three reasons. First, recent developments in the philosophy of science have shown that the philosophy of biology and economics are two of the most fertile fields. The findings in these subdisciplines not only posit serious challenges to but also provide novel ideas for traditional accounts in the philosophy of science that are based mainly on the physical sciences. Second, the current trend of investigating biological or

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[1] This point is also suggested in Bechtel (2008, pp. 8–9). For philosophical investigations of scientific devices, examples are experimental and observational instruments by Ian Hacking (1983), models by Mary Morgan and Margret Morrison (1999), and by the semantic or model-based view philosophers such as Ronald Giere (1988, 1999) and Bas van Fraassen (1980, 1989).

economic issues by employing the concepts and tools developed in the other field (e.g., evolutionary game theory, behavioral economics) has drawn substantial attention among scientists and philosophers of science alike. A study that juxtaposes biology with economics and explores a deeper understanding of various philosophical and methodological issues would prove meaningful. Daniel Steel's (2007) highly acclaimed book has demonstrated this. Finally, recent accounts of mechanism and causality in the philosophy of science are often associated with biology and economics. Whereas the philosophy of mechanism has been developed mainly by philosophers of biology (e.g., Machamer et al. 2000; Glennan 1996, 2002; Bechtel and Abrahamsen 2005), philosophical discussions of causality have been inspired by the practices of economists (e.g., Cartwright 1999, 2007; Woodward 2003). Recent works on causality in economics (e.g., Hoover 2001) have also made significant contributions to current and future research on the methodology of causal structure in science in general. However, even though mechanism and causality occupy the main stage of research in both the philosophy of biology and that of economics, only few studies have been done that bring the accounts in one discipline to the other. This edited volume can be seen as a result of collaborative interaction and mutual understanding among philosophers from different disciplines.

2 Mechanism and Causality in the Philosophy of Science

Although causal inquiry has long been regarded as one of the core elements of science, the focus of the philosophical investigation of causality has changed over time since at least the modern era. Traditionally, the discussion tended to pay much more attention to inquiring about the *metaphysical* aspect of causality. This tendency reached its climax in Hume's famous inquiry about the secret connection between any two events—cause and effect. Then in the first half of the twentieth century, influenced by the positivist philosophy of science and the Humean regularity view of the laws of nature, the discussion shifted to a concern about the *epistemological* aspects of the subject. In particular, attempts have been made to delineate the characteristics of causality by using conditional analysis, that is, by analyzing causality in terms of necessary or sufficient conditions, or both. By temporarily leaving aside the question of the existence and characteristics of causality, the new generation of philosophers tries to construct down-to-earth accounts of causality, especially by referring to practicing scientists' achievements in finding patterns in the empirical data of targeted variables that they collect from experiments or field studies. In other words, contemporary philosophers of causality, recognizing that we human beings are agents of our own knowledge, tend to use their restricted *methodological* lever to tease out indications of the answers of what previously were thought to be questions about metaphysics and epistemology.

Similarly, the conception and application of mechanisms are nothing new in science and philosophy. From the seventeenth century onward, we observe the development of "mechanical philosophy," represented by the achievements of the giants of science such as Galileo, Descartes, Huygens, Boyle, and Newton. Marie Boas's (1952) seminal article on the establishment of the mechanical philosophy identifies the rise of the mechanical philosophy as due to the development of new the science of mechanism that replaced Aristotelian physics and thus concludes that explanations for the properties of bodies should be based on it (Boas 1952, p. 414). In the contemporary philosophy of science, the first half of the twentieth century also witnessed mechanistic explanations developed in philosophy of science when the discussion was centered on the mechanics and physics (e.g., Nagel 1961). The resurgence of the importance of mechanisms in recent studies in the philosophy of science, however, is not because its application would reduce explanations in other sciences to mechanics and physics (e.g., Nagel's 1961 attempt to reduce biology), but because of its involvement in how scientists actually explain. A number of philosophical characterizations of mechanisms have been recently put forward (Tabery 2004; Skipper and Millstein 2005). Most of them are inspired by biology. Among them, the two most salient accounts are developed by Peter Machamer, Lindley Darden, and Carl Craver (2000)—hereafter, MDC—and by Stuart Glennan (1996, 2002). Glennan's *interactionist* account evolves hand in hand with the literature of causality. His recent definition adopts James Woodward's (2003) interventionist account of causality. In contrast, MDC endeavor to give up causal language entirely.

Briefly, Woodward's view is that "X is a total cause of Y if and only if under an intervention that changes the value of X (with no other intervention occurring) there is an associated change in the value of Y" (Woodward 2007, p. 73). More specifically, Woodward clarifies the relationship among the concepts of manipulation, the change-relating property of a relation, and invariance. Inspired by Douglas Gasking's idea that a causal relation is a "means-end" or "producing-by-means-of" relation (Gasking 1955), Woodward refines this causal idea by adding a condition of invariance. According to Gasking, C causes E in cases in which we can, with the aid of a certain kind of general manipulative technique, produce an antecedent occurrence of kind C as a means to bring about a subsequent occurrence of kind E. As with Gasking, Woodward agrees that a relation, if it is to be regarded as having causal and explanatory import, must be explicated in terms of manipulation. What is new in Woodward's account is that he further suggests that, for a relation R between C and E to count as being causal and explanatory, relation R must be invariant under the manipulation of C. That is, the manipulated change in C should bring about the change in E in the way stated in R; otherwise, C does not cause E in the way stated in R and perhaps does not cause E at all. Clearly, for Woodward, a causal relation should be a relation that is exploitable by manipulation for the purposes of control. Woodward's account seems to imply that a relation R will express a causal relation only if R is invariant over a range of interventions.

Accordingly, Glennan (2002, p. S344) recently offered the following definition: "A mechanism for a behavior is a complex system that produces that behavior by the interaction of a number of parts, where the interactions between parts can be characterized by direct, invariant, change-relating generalizations" (Glennan 2002,

p. S344). He thus avoids the notion of laws that was employed in his early studies (e.g., Glennan 1996). In contrast, MDC think such causal language is too vague to characterize the actual specific activities within a mechanism, such as pulling, scraping, or binding. In their *dualist* account, mechanisms are constituted of *entities* and *activities:* "Mechanisms are entities and activities organized such that they are productive of regular changes from start or set-up to finish or termination conditions" (Machamer et al. 2000, p. 3). Other definitions of mechanisms include one by Bechtel and Abrahamsen (2005) who stress mechanisms as *structures*. They argue that "[a] mechanism is a structure performing a function in virtue of its component parts, component operations, and their organization. The orchestrated functioning of the mechanism is responsible for one or more phenomena" (Bechtel and Abrahamsen 2005, p. 423). Some philosophers think the differences between these accounts are highly significant, whereas others think they are minor.

Notice that, prior to the emergence of the mechanist approach in philosophy of biology, mechanisms have been investigated by philosophers of social sciences such as Mario Bunge (2004) and Jon Elster (1983, 1998, 2007) and been advocated by economic sociologists Peter Hedström and Richard Swedberg (1998). Like MDC, the advocates of social mechanisms share Francis Crick's view that biologists prefer to think in terms of mechanisms rather than laws (Hedström and Swedberg 1998, p. 3). But it should be noted that, as successfully argued by Carl Craver and Marie Kaiser in their chapter, mechanist philosophers do not deny the epistemic virtue of regularities, as they help scientists search for mechanisms, even as mechanisms in turn help us to understand how regularities and generalizations provide the basis for scientific activities such as explanations, predictions, and control.

A general notion of social mechanisms is aptly characterized by Thomas Schelling, a Nobel Laureate in economics, who defines social mechanisms in contrast with laws, theories, correlations, and black boxes, conceiving them as plausible hypotheses that explain social phenomena, where the explanation is offered in terms of interactions between individuals or between individuals and social aggregates (Schelling 1998). Similarly, Elster (1998) contrasts mechanisms with black boxes (which could provide no explanations) and laws (which provide only deterministic explanations). He defines social mechanisms as "frequently occurring and easily recognizable causal patterns that are triggered under generally unknown conditions or with intermediate consequences" (Elster 1998, p. 45) and regards them intermediates between laws and descriptions (ibid.). Elster's mechanism-based explanations would consist of the form "if conditions C_1, C_2, $\ldots C_n$ obtain, then sometimes E" (Elster 1998, p. 48). At present, the investigations of social mechanisms seem to converge on the accounts developed by philosophers of biology by reevaluating social mechanisms in terms of mechanist philosophy of science (e.g., Hedström and Ylikoski 2010), implying an attempt to reconcile social mechanisms in a broader conception of scientific mechanisms.

3 Biological Causality and Mechanism

Until recently, philosophical accounts of causation and mechanism in the philosophy of biology used a very limited set of philosophical accounts of causation, assuming they used any account of causation at all (e.g., Rosenberg 1985 and Hodge 1987 discuss causation in biology without appeal to any particular account). Perhaps the most common invocation was of Wesley Salmon's "screening off" condition (see, e.g., Brandon 1988; Lloyd 1988; Sober 1984 articulates his own account of causation as an alternative). This is not to say that biologists and philosophers of biology did not appeal to causes—far from it. It seems rather that, until recently, most philosophers of biology did not find accounts of causation such as Lewis's counterfactual account or Salmon's Mark Transmission/Conserved Quantity account particularly useful for illuminating phenomena in biology. Indeed, some recent discussions of causation in the philosophy of biology still do not cite causation literature (e.g., Mitchell and Dietrich 2006); this is not meant as a criticism, but simply to point out, again, that the philosophical literature on causation is sometimes not seen as helpful or necessary for illuminating philosophical issues concerning causation in biology.

However, the recent development of the above-mentioned mechanist and interventionist philosophies has begun to change that. It would be tedious to list all of the philosophers of biology who have drawn on these works, so here is a short sampling: Fehr (2004), Tabery (2004), Reisman and Forber (2005), Waters (2007), Steel (2007), and Illari and Williamson (2010). In trying to understand the explosion of literature on interventionist accounts of causation and mechanist accounts, it is surely no coincidence that whereas accounts such as Salmon's and Lewis's were derived from physics or from traditional philosophical analysis of our everyday language, accounts such as Woodward's and MDC's were derived from the social sciences, economics in particular, and from biology.

With the interventionist account of causation and accounts of mechanisms in ascendancy in the philosophy of biology, an obvious question arises as to the relationship between causation and mechanism in biology. The possible answers to this question, however, are varied and complex. As Roberta Millstein's chapter in this volume notes, philosophers such as Glennan (2009) have argued that there are *two* types of causation, contra the decades of argument over what constitutes *the* account of causation: causal relevance (or causal dependence) and causal production. Woodward's account, falling into the broad category of counterfactual accounts, is supposed to be a causal relevance account, whereas Glennan's, MDC's, and even Salmon's accounts are seen as causal production accounts. If Glennan and others (e.g., Cartwright 2004; Hall 2004) who have argued that there are two types of causation are right, then different philosophers use different accounts of causation because they pick out different phenomena in the world (or different aspects of the same phenomenon, though Millstein argues that Glennan has failed to make his case for natural selection). However, aside from reservations that one might have about there being two accounts of causation, the relationship between the two accounts is unclear, because both Craver and Glennan incorporate Woodward's views into their accounts of mechanism (Glennan 2002; Craver 2007) and because Woodward himself has described how interventionist accounts of causation can be used as an account of mechanisms. Moreover, Glennan has argued (1996, 2010) that mechanisms can serve as the basis for a theory of causation. So, perhaps causal relevance and causal production (including mechanisms) are tightly linked. Lindley Darden's chapter usefully explores some of the ways in which causes might manifest themselves in mechanisms: activities of entities, stages of mechanisms, or as start or setup conditions. On Darden's view, then, analyses of "mechanism produces phenomenon" are much more detailed and specific than "*C* causes *E*," as the former incorporates many of the latter, plus other aspects such as the ways in which entities and activities are organized.

A second sort of question arises as to which biological phenomena can be profitably illuminated by accounts of causation and/or mechanisms; each of the biology papers in this volume contributes partial answers to this question by exploring causation and mechanism in different areas of biology. Once again, however, we quickly realize that for every illumination, new questions are uncovered. Several of the papers deal with causation and/or mechanisms in evolutionary biology. Millstein, who has elsewhere (2006) argued that natural selection is a population-level causal process, argues (contra Glennan) that the causation at the population level exhibits causal production (in Salmon's sense) as well as causal relevance (a point on which she and Glennan agree). But she does not take a stand on whether natural selection should be understood as a mechanism, having elsewhere (Skipper and Millstein 2005) raised concerns for such a claim. However, Rong-Lin Wang offers some criticisms of Millstein's claim that natural selection is a population-level causal process. For example, he argues that Millstein's account of natural selection does not handle cases of what Elliott Sober has called "selection of" (as distinguished from "selection for" and random drift). Moreover, he suggests that prospects of the view that natural selection is a population-level causal process depend on a satisfactory solution to each of the three problems: the redundant cause problem, the overdetermination problem, and the epiphenomenon problem. Thus, according to Wang, we need to pay attention to the work of metaphysicians in order to understand the nature of selection. The other philosophy of biology papers, discussed elsewhere in this Introduction, explore causation and mechanisms in other aspects of evolutionary biology as well as other areas of biology such as genetics, plant breeding, and biomedicine.

4 Economic Mechanism and Causality

A mechanism is often conceived as a machine, which is on the top of Craver and Darden's (2005) list of ideas associated with the term mechanism. It is so because machines provide *models* of intelligibility that have contributed to our understanding of the mechanisms in the natural world. This understanding of mechanism in

$$\text{Input} \rightarrow \boxed{\text{Mechanism}} \rightarrow \text{Output}$$

Fig. 1.1 The Bungean input-output mechanism (Adopted from Bunge 2004)

terms of such an artifact is commonplace in both natural and social sciences, since such visualization helps understand the various aspects of mechanisms and how they are constituted. A mousetrap, for instance, is used by Craver and Bechtel (2006) to illustrate the philosophical notion of mechanism. In social science, underneath different definitions of social mechanisms we previously discussed is a strong perception of the society as a machine. An example is Elster's assertion that an explanation in social science requires of specifying *social cogs and wheels.* According to Elster (1983, p. 24), "To explain is to provide a mechanism, to open up the black box and show the nuts and bolts, the cogs and wheels of the internal machinery." For Bunge, a mechanism is specifically perceived as an input-output machine, which is contrasted with black boxes by which inputs and output are connected without knowing the inner machinery (Bunge 2004; Hedström and Swedberg 1998), and can be understood in terms of the diagram in Fig. 1.1.

In economics, economists have also been using and adopting the concept of mechanism for centuries. As Harro Maas (2005) demonstrates, the practices of mechanical reasoning among the political economists had been observed in Victorian Britain, where the economic world—perhaps analogous to the physical world—was envisaged as a machine. More specifically, the type of machine is an *input-output* machine, coinciding with the Bungean input-output mechanism. There are two salient cases in the economic literature. First, the "market mechanism," which is perhaps the most fundamental concept indicating the structure and capacity of the market for allocating the resources among economic units, is commonly understood as such. To study economic mechanisms, a subfield *mechanism design* emerged in the 1960s to apply proper mathematical tools to construct and analyze how economic units and activities are coordinated and guided through the information they receive from a fictitious "information center." A mechanism is thus also viewed as a communication or a dialogue between the information center and economic units or "periphery" (Hurwicz 1973, pp. 6–7). In this regard, economic mechanisms bear some similarity to mental mechanisms as they both are information-processing mechanisms (c.f., Bechtel 2008). Second, the *input-output analysis*, which was established in the 1930 by the economist Wassily Leontief, explicitly treats the structure of an economy as constituted by input-output relationships. With its ambition to quantitatively deal with all components of the economy, the input-output analysis requires its models to be computable and statistically measurable so that it can describe and interpret the economic operations in terms of "directly observable basic structural relationships" (Leontief 1987, p. 860).

However, as of now, it seems causality rather than mechanism is economists' primary concern. Mechanism in general is understood in the context of "causal mechanism," whose structure—causal structure—needs to be identified. Kevin Hoover (2001, p. 24) offers one definition of causal structure as "a network of counterfactual relations that maps out the underlying mechanisms through which one thing is used to control or manipulate another." While mechanism is defined freely in this definition, the general idea of mechanisms developed in various mechanist accounts can surely apply to it.

With respect to Hoover's description of causal structure, each causal path between any two variables within a causal structure is represented as an invariant counterfactual conditional relation. It is called "counterfactual" because it claims that if there is a "hypothetical" change in (or manipulation of) the supposed causal variable, then the supposed effect variable will have a corresponding degree of change. If we represent the causal relation between two variables p and q as the equation $q = \alpha p + \varepsilon$, where the parameter α represents the degree of change of p in q, then the adjective "invariant" means that, against the background of a complicated network of the causal structure, whatever unit of change in p there is, the corresponding effect of α degree of change of p in q will "remain unchanged." In that case, the fact of invariance can be used as a criterion, as was pointed out by Herbert A. Simon in his 1953 article, that would permit us to discriminate among competing structural representations that are consistent with the same set of data. Based on this view, it is no wonder that Hoover remarks that "causal structure is characterized by a parameterization that governs the manner in which variables are related to each other... The patterns of relative independence, dependence, and interdependence among variables—the causal structure—are dictated by the parameterization" (Hoover 2001, p. 59). Hoover's structural account of causality can be regarded as a classic metatheoretical account that aims to characterize scientists' attempt to use their limited methodological lever—such as the available statistical techniques—to tease out, from the probabilistic distribution of those relevant variables, the indications of the answers of causal inquiries. Hoover's chapter in this volume goes further to explicate the structural approach by contrasting with Woodward's manipulability account, arguing that *modularity*—a critical characteristic of Woodward's account indicating that each equation in a system of causal relations corresponds to a distinct causal mechanism—fails in certain cases, because in reality individual equations in a causal structure do not necessarily correspond to distinct mechanisms. Furthermore, Hoover argues that, unlike Woodward's manipulability account in which the notion of causality is defined in token level (causal relations hold among particular events), the structural account explains causal notion in type level (causal relations hold among variables) and could be more explanatory for causal relationships in a practical sense.

5 Representing Causal Structures and Mechanisms

Given the importance of understanding both causal structures and mechanisms, there is a need for inquiring into the possibilities of providing epistemological access and representation to them. One pivotal question concerns whether we can completely know causal structures and mechanisms. Recall that the notion of mechanisms is employed by philosophers and scientists in a sense to contrast with black boxes. But whether mechanisms can be completely known remains under debate. For instance, in the above-mentioned economic approaches of mechanism design and input-output analysis, the mechanism of the economy is regarded as being perceivable, given suitable tools—mathematical methods for mechanism design and statistical analysis for the input-output analysis—exist. As Leontief put it, to understand an economy requires nothing but a "direct structural analysis," like a mechanic looking under the hood (Leontief 1954). Leontief thought not only is such a direct observation possible, but it is the only promising way of understanding the operational characteristics of the economy (Leontief 1954, p. 230). By contrast, Trygve Haavelmo, the pioneer of the probabilistic approach to econometrics, used his famous mechanical analogy to illustrate the methodology of econometric models (Haavelmo 1944, pp. 27–8): The empirical relationship between the amount of throttle and the speed of a car, under uniform circumstances, is regular. Such a relationship is useful for driving a car in a prescribed speed, but is not fundamental. The throttle-speed relationship not only lacks of *autonomy* because it breaks down as the condition changes, but also, the relationship tells us little about how the car works, hence it "leaves the whole inner mechanism of a car in complete mystery" (Haavelmo 1944, 371 p. 27).

Thus, while Haavelmo thought that understanding the inner mechanism is of primary importance, he contrasted with the economists such as Leontief in thinking that a direct observation is impossible. This characterizes the practices of econometricians, who have been trying to use a mix of tools from economics, mathematics, and statistics to analyze empirical data and, in part, concerned whether the *data-generating process*, or *DGP*, that is regarded as being responsible for producing the observed data is real or fictitious and whether it can be fully known.[2] Ontology aside, many have maintained that econometric models do not allow observation of the DGP directly. One can receive only an incomplete image of the underlying structure by inferring from observed data. Because, unlike a mousetrap, scientific mechanisms are usually not available for direct observation, hopes for complete descriptions of mechanisms and/or causal structures would be in vain.

Even so, the incomplete notions of mechanisms and causal structures are still useful for understanding science. In order to represent the underlying mechanism, scientists use what MDC called "mechanism schemata" or "mechanism sketches" as incomplete description. For them, mechanism sketches are black boxes, serving to indicate required future research work in order to establish mechanism schemata. Mechanism schemata, in contrast, contain more, but still incomplete, information and are usually represented by diagrams. Since neither sketches nor schemata are thorough and detailed, to understand mechanisms via sketches and schemata might be related to the "black box inference" in the philosophy of science. Although the term was made famous by Sober (1998) who discussed particularly the linkage

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[2] See Chao (2009, esp. Ch. 7) for the philosophical discussion on the DGP.

between causes and effects, it is longstanding in the philosophy of science concerning the structure of scientific theories. Hempel (1966), for instance, when discussing the distinction between observables and unobservables, suggests that in an attempt to explain the performance of a black box which "responds to different kinds of input by specific and complex output" (p. 81), the internal structure of the black box is in principle observable, or can be directly inspected, as long as appropriate instruments are available. Hence "any line drawn to divide them into actual physical objects and fictitious entities would be quite arbitrary" (Hempel 1966, p. 82). Similarly, Hanson (1963) illustrates that, as science progresses, our understanding of phenomena switches from the stage of "black box" to that of "grey box" and finally reaches the stage of "glass box" whence the theory and the phenomena are of the same structure and the equations of the theory can actually "mirror" the processes of the nature (Hanson 1963, p. 38). Hanson's account is shared by the mechanists. MDC, for instance, regard the schemata as essential heuristic devices for discovering mechanisms. By reasoning with a schema, scientists are guided to choose known and proper entities or activities to fill the gap. Afterward, when a schema is instantiated, it provides a mechanistic explanation of the phenomena that the mechanism produces (Machamer et al. 2000, p. 29).

It is thus natural to relate mechanism sketches and schemata to scientific models; both serve inferential and representational devices to understanding science. Recent study (e.g., Morgan and Morrison 1999) emphasizes that models are independent of theory and the world and thus have autonomous power for representing each of them. Literature also shows that the distinction between *models of theories* and *models of data* that was earlier made by Patrick Suppes in his influential article "Models of Data" (Suppes 1962) has proven useful for characterizing scientific modeling processes. Following Suppes and the discussion of empirical models in science and philosophy, Ruey-Lin Chen argues in his chapter that scientific discovery in biology can be explained and instantiated through the models of experimental data. In contrast, Till Grüne-Yanoff's chapter in this volume deals with the issue of representing mechanisms at a theoretical level. He examines evolutionary game theory (EGT), arguing that EGT models employed in biology and economics have different interpretations concerning what causal factors and relations they represent, interpretations that are captured by informal mechanism descriptions rather than by the EGT formalism. An abstract model is qualified as MDC's mechanism sketch; it requires an interpretation of the model to represent a specific mechanism in biology or in economics. In other words, biological or economic mechanism descriptions are of a particular kind: They do not describe the composite parts of a system, but they describe in abstract form the stages through which the mechanism runs. Because it does so in a highly abstract way, many different mechanisms can be subsumed under these descriptions, making them general schemata useful for many scientific purposes.

Hoover's and Steel's chapters demonstrate that representational devices such as models can sometimes play a more active role. They both use directed acyclic diagrams (DAGs) to represent causal relations, which has been a popular representational tool employed by philosophers of causality. Hoover points out that results in causal analysis may not be independent of the modes of representation, that is, equations or graphs, and clarifies the relationships between graphical and equational representation of causality. Steel goes a step further to include DAGs as a part of the definitions of philosophical notions of extrapolation and integration, implying that theoretical propositions could be entranced by directly consisting of representational tools.

Furthermore, the variations in methodologies could be represented by the change in representational tools, and vice versa. Marcel Boumans's chapter revisits his (Boumans 1999) "recipe-making" account of models.[3] It suggests that mathematics provides the means of molding different ingredients into a new model. In a sense, as Boumans points out, early econometricians such as Jan Tinbergen regarded mathematical forms as determining the economic movement. However, when the focus switched to identify causal structural relationship among variables, the primary concern for econometricians was to seek the model's property of invariance. Since the econometricians then adopted the strategy of relying on theories to do the job, the role of mathematical molding was lost. These works show how thinking about representation and models provides new insights into mechanisms and causal structures.

6 Mediation and Extrapolation

Let us return to our original aspirations to bring together biological and economic mechanisms and causality. The idea promoted in this volume is that studies in the philosophy of science would be enriched by two ways of research. The first is to start with the concepts that scientists use most in their practices. Such investigations provide concrete grounds of scientific methods and activities on which philosophical notions can (and arguably should) be based. Readers can observe that most of our chapters attempt to address simultaneously the notions of causality and mechanisms. Though the notions defined and employed in their work—and the cases they study—do not belong to one single account, the plural meanings of mechanisms and causality clearly show their importance to understanding science. The second way is to conduct interdisciplinary explorations on how the concepts are understood by different groups of scientists and philosophers. In this volume, in addition to Grüne-Yanoff's chapter studying biological and economic game-theoretic models, we have three chapters dealing with comparisons and contrasts of facts and methods between economics and biomedical science. All three chapters start with specific scientific works in economics and biomedical science, then conduct methodological investigations on the case studied. One central common theme, which has been dealt with by the authors of this volume under various topics, such as Darden's *interfield integration* (Darden 2006) and Steel's

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[3] The term is coined by Mary Morgan (2008).

extrapolation (Steel 2007), is to investigate whether methods, hypotheses, and facts of one field can be applied to another. David Teira and Julian Reiss's chapter compares research methods of randomization in medical and economic sciences: randomized clinical trials (RCTs) and randomized field experiments (RFEs), respectively. Randomized controlled trials have long been regarded as the gold standard for finding causal relations between interventions and experimental outcomes. One reason, as Teira and Reiss point out, is that they provide *mechanical objectivity*, meaning that randomized trials usually follow rigorous and transparent rules so that the results are immune to the bias of subjective expert judgment. They, however, argue that such objectivity is hard to come by. It is because the participants both RCTs and RFEs could act so strategically to obtain their best interest from the trial experiment that the supposed invariance of the controlled environment breaks. Consequently, it is questionable to infer from the evidence the causal connections between treatments and the results.

Both Hsiang-Ke Chao and Szu-Ting Chen's and Steel's chapters deal with the issue of extrapolation that was conceptualized by Steel (2007), and they both deal with the studies that can be categorized as *freakonomics*—using economic principles to (surprisingly) explain a social phenomenon that was first thought to be out of the realm of economics—popularized by the economist Steve Levitt. Steel uses John Donohue and Levitt's (2001) controversial article of the causal relation between legalized abortion and crime rate in the United States. Because there is no direct evidence that can be used to check whether the hypothesis is correct, social scientists support the US case by analyzing results derived from a survey of a similar case that happened in the Scandinavian and Eastern European areas during some periods in the twentieth century. But can we legitimately use evidence obtained in a different time and a different area to support the local case? The problem of extrapolation is analyzed by applying a mechanism-based approach—what Steel calls "comparative process tracing."

Another case where extrapolation could lead to possible explanation is the "missing women" debate discussed by Chao and Chen, in which a biological explanation—hepatitis B virus infection—for the abnormal inequality of sex ratio at birth in Asia is offered by, extrapolated by, and instantiated by the sampling data in the other area. But the biological explanation is claimed to be rejected by economists who used Taiwanese population-level data. They find empirically that cultural factors such as son preference are the cause for the missing women phenomena. Chao and Chen argue that such empirical study does not necessarily deny the existence of the underlying biological causal path, since what is observed is a net causal result. Taking the net causal result as an evidence of ruling out minor causal paths is equal to treating the underlying mechanism as a nontransparent box. In this regard, extrapolations regarding evidence in different time and space can be seen as complementary rather than substitutive.

This echoes our account of the ontology of mechanisms and causal structure: In search of an explanation for a phenomenon, it is adequate to specify the mechanism or identify the causal structure that underlies it. Science progresses thus from black box to grey box and in turn to transparent box, but not the other way around.

7 Conclusion

We have argued in this introductory chapter that thinking about mechanisms and causality enables us to access scientific practices in biology and economics. Methodological investigations centering on mechanisms and causality provide a meaningful way to understand science. The detailed philosophical analysis of these two conceptions, together with specific biological and economic cases given in the chapters of this volume, is our attempt to mediate between mechanisms and causality and between biology and economics. We look forward to seeing more explorations of these topics in future philosophy of science studies.

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