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Things Political:
A Study of the Object of Politics

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Introduction

As an academic discipline, politics is an exceptionally self-reflexive activity. Much political reflection has been devoted to defining its object, so much so that it is customary to cite definitions of politics that date as far back as Plato and Aristotle. References to Aristotle's definition of the human being as a "political animal" are not uncommon to this day, even though Aristotle intended for it to be a definition of the *human being* rather than of *politics*. Similarly, Max Weber's definition of the state as claiming "the monopoly of the legitimate use of the physical force within a given territory" is often accepted as a working hypothesis to circumscribe the political domain, even though this definition was meant to characterize *the relation between modern states and violence*, and not *politics*. In fact, despite its ubiquitousness the abstract concept of "politics" remains rather vague and liberally defined, even though it is meant to capture very mundane aspects of social living. Indeed, debate in political theory has often hinged on disagreement about the nature and boundaries of politics: for some time before the full academization of the discipline, political science was largely identified with political theory, and self-reflection on the scope of political science has been centre stage for decades. The fundamental essence of "politics" still remains an open question and a point of departure both for empirical investigation of political phenomena and for theorizing about the nature of the social world we live in.

Generally speaking, any definition of politics attempts at describing a sphere of human interaction. The problem, of course, lies in convincingly identifying the boundaries of such sphere. *Prima facie*, such indefiniteness might appear surprising: after all, the sphere of politics cannot describe but concrete activities that take place in the social world, and therefore no ambiguities should arise from ostensively pointing to the phenomena that the word refers to. Yet the meaning of the word is not co-extensive with any one of the families of phenomena it designates; "politics" is a common noun, and therefore it denotes a class of objects rather than a particular phenomenon. The vagueness of the concept of politics results from the different ways in which social phenomena can be grouped together into a more or less homogeneous class. If we believe that hostile confrontations between fighting factions are a recurring and striking enough feature of social interactions, for instance, we may agree with Carl Schmitt's definition of "the political" as "the most intense and extreme antagonism" (Schmitt, [1932] 2007; 29). If, on the other hand, we consider state politics to be separate enough from any other social activities, we may agree with the more traditional equation between politics and ruling (or influencing the decisions of those who rule). Why,

then, is the theoretical concept of politics necessary? After all, our vocabulary is rich enough that we might as well rest content with the common names that are used to define the concept of politics or, in more complex cases, the expressions that we use as definitions. To refer to the two examples cited above, if "state" and "intense antagonism" are believed to be clear enough notions that can be used to clarify the meaning of an abstract concept like "politics", the latter seems to be unnecessary addition to our vocabulary.

Implicit in any definition of politics seems to be the idea that the sphere of human activity that politics refers to is an object in its own right rather than a mere collection of heterogeneous social objects. I believe that the best way to make sense of this aspect is to consider politics as a *scientific* object. There is an immediate link between the concept of politics and the activity of science. As a descriptive concept, "politics" refers to an ensemble of empirical phenomena—however they are conceptually assembled—the causal explanation of which is a primary concern of political science. Moreover, as a theoretical abstraction the political sphere points to the more or less recurrent way in which those phenomena present themselves in their general structure, and it is a widely praised feature of science that it is able to identify such generalities. Much of the recognized authority of science in fact lies in its capacity to explain not just individual occurrences of phenomena, but the semi-general causal processes that generate them. As a common noun, politics also refers to a class of objects, and classificatory activity is the bread and butter of the descriptive task of empirical sciences, from biological taxonomies to concept formation in comparative political analysis. Finally, politics in general defines the scope of political science, and therefore to specify the object of "politics" is to define the object of political science. It is in virtue of all this that the political sphere can be seen as a scientific object.

Yet not all such theoretical concepts can be straightforwardly considered to be *scientific* objects. We consider different animals to be all instances of the category of "pets" in just the same way as we consider different social activities to be instances of politics, yet it would be counterintuitive, to say the least, to consider the category of "pets" as a scientific term, and the class of objects it designates as a scientific object. The fact that class-concepts refer to empirical objects that are also studied by the empirical sciences seems to be an insufficient condition for the class of those objects to qualify as a scientific object in its own right. Scientific objects are such because they are constructed for specifically scientific purposes. I have mentioned earlier that whatever the concept of "politics" may mean, anything that falls under this rubric also falls within the scope of political science: the concept of politics defines the scope of political science. It is for this reason that the theoretical notion of a "political sphere" can be considered to be a *scientific object*. In fact, on closer inspection the relationship between the identification of a political sphere and the demarcation of the scope of the science that studies it must be reversed. It is political science that defines the concept of politics, and not the other way round. The reason for this will be clear in a moment.

Instead of providing a general theory of scientific objects, I will take a different route. In the next chapter, I will argue that the idea of politics as a separate sphere of societal interaction emerged as a consequence of the rise of political science as an institutionalized

discipline. In its pre-disciplinary stage, social science still conceived of its field of investigation as a unified domain of phenomena which allowed for no distinctions between the political sphere and other spheres of human activity. At that stage, politics had no identity as a thing in its own right, and the word was loosely employed to identify the whole and indistinct body of society in its current ordering. The emergence of politics proper began in the middle of the eighteenth century, and it was historically and sociologically determined by the need to differentiate political science from the other social sciences. In the aftermath of the French Revolution, the rise of sociology as a scientific discipline gave rise to the idea that "society" identifies a class of *sui generis* phenomena characterized by an autonomous functioning quite independent of state regulation. Contextually, the development of economics determined the identification of the economy as governed by its own laws of motion that could be studied by means of specifically economic methods. For a relatively long time, politics remained an inchoate disciplinary field whose object of study was determined *ex negativo* by the object of sociology and the object of economics. The institutionalization of U.S. political science after WWII gradually led to a clearer definition of its scope and, consequently, of politics as a scientific object. The expansion of comparative research that accompanied the "behavioural turn" in political science in the 1950s laid the groundwork for identifying distinctively political phenomena across different national and temporal settings; it is at this stage that the concept of "the political system" was developed to denote the ensemble of such phenomena. The idea of politics as a separate sphere of society thus emerged as a consequence of the methodological development of political science, and it was characterized since its inception as the distinctive scientific field that political science sought to investigate. It is in this sense that politics can be thought of as a quintessentially *scientific object*.

The relationship between the development of academic political science and the construction of "politics" as a scientific object is, in fact, common knowledge in political theory. In an influential article, aptly titled *What is "Politics"* (1973), Giovanni Sartori explicitly related the autonomy of politics to the "discovery of society" on the part of eighteenth-century economists. While it was once common to see Machiavelli has having posited such autonomy, Sartori argues that his thought implies the independence of politics from morality and religion, but not the establishment of politics as a *thing*—i.e. an object—in its own right. Machiavelli may well have shown that ethics is different from politics (although it is instrumental to it); yet this does not make politics a separate sphere within the social landscape.[1] This separation is instead the product of the conceptual separation between the hierarchical and coercive elements of coexistence on the one hand, and the spontaneous organization of all other human transactions on the other. This view of society as regulated not by political coercion but by a spontaneous automatism was, of course, an intellectual product of the laissez-faire orientation of classical economics, and therefore the construction of the political object can be traced to the development of the social sciences. Despite recognizing the dependence

1. This "a-moral" reading of Machiavelli's political theory, spearheaded by Benedetto Croce ([1925] 1945) and Leo Strauss (1958), has been contested by Isaiah Berlin ([1972] 2013) as well as by "Republican" interpreters of Machiavelli's thought (Skinner, 1978; Viroli, 1990; Pettit, 1997).

of the identity of politics from the historical dimension through which the different social science disciplines have differentiated themselves from one another, however, Sartori does not give up on the possibility of a purely theoretical definition of politics. As he writes in the conclusion of his article: "Political decisions do deal with the most diverse matters: political economy, social policy, the politics of law, of religion, of education, and so forth. If all these decisions are essentially political, it is because they are made by personnel located in political sites. This is their political 'nature'" (Sartori, 1973; 21).

On the other hand, the existence of competing conceptions of what politics is an established fact in political science. Adrian Leftwich ([1984a] 2004), for instance, writes that "any experienced academic in this field will know that the conception of politics one adopts directly influences not only the questions one asks but also the framework of analysis one uses" (Leftwich, [1984a] 2004; vii). He thus provides a survey of the different meanings that practising political scientists attach to the "word" politics understood as the subject matter of political science. For Leftwich ([1984b] 2004), the debate on the definition and conceptualization of politics is dominated by two approaches, which he calls the "arena" or "site" approach and the "process" approach: the basic distinction is between politics as an activity found only in the institutional sites or processes within certain kinds of societies (normally, those with states), and politics as an intrinsic activity of the human species in all social settings (including stateless societies: Leftwich, [1984b] 2004). More recently, Bartolini (2016) has argued that the "dismembering" of political societies into different sub-fields of activity has led political scientists to recompose those sub-fields into unified conceptions of politics according to their own schools and traditions of research. He thus proceeds to regroup those conceptions into six "families" on the basis of the themes that different scholars emphasize as the quintessence of politics. These are: *politics as activity*, *politics as institutional locus*, *politics as conflict*, *politics as a (coercive) means to an end*, *politics as allocation*, and *politics as aggregation*.

Bartolini acknowledges the importance of such theoretical frameworks for political analysis; in uncovering their scientific roots, he even attributes their construction to the intrinsic tendency towards generalization that pervades the methodology of empirical research. Yet the plurality of definitions of politics is seldom acknowledged by political scientists to be an inherent feature of their object of study *qua a scientific object*. Most political scientists seem to imply that the conceptualization of politics as a scientific object must be unequivocal and not open to a pluralism of definitions. Those who have defended the concept of "the political system", for instance, sought to discard other definitions of politics—most notably the identification of politics with the state—as based on a flawed conceptual approach. Gabriel Almond ([1960] 2015) wrote for instance that the dichotomy between state and non-state is to be rejected in political science because it unduly restricts the scope of comparative research solely to those social structures that visibly perform a political function, while Easton (1953) advocated banning the term "state" altogether in that its meaning is open to exceeding vagueness. Albeit recognizing the interdependence between the variety of definitions of "politics" and the different directions of political science research, even

Leftwich ([1984b] 2004) concludes that the disciplinary unity of political science is secured by the convergence of the competing approaches to the understanding of politics on a unified focus of research—which he equates with the analysis of the provenance, forms, distribution, use, control, and consequences of *power*.

I will argue instead that taking seriously the idea of politics as a scientific object implies accounting for the scientific function that the variety of definitions of the scope of political science fulfils. Rather than attempting at reducing the complexity of conceptual approaches to the object of politics, I will argue that its "essentially contested" nature is the necessary outcome of the construction of scientific objects on the basis of the methodological assumptions of political science. To show this, I will resort to the notion of *scientific model* that refers to the theoretical devices commonly used in the empirical sciences to represent real-world objects. In particular, I will argue that the different conceptions of politics are best seen as representational models of the aspects of the social world that are considered to fall within the scope of political science, and that doing so can provide us with the tools for making sense of the apparent vagueness of the concept of politics. In the empirical sciences, models are theoretical objects that are defined in such a way as to selectively represent some aspects of reality; the salient characteristics of reality that a model represents are selected and isolated in order for the model to be applicable in forthcoming contexts. The main thesis of this work is that the political sphere is a representational object similar to the empirical models of science. As a concept that denotes the entire field of political science, "politics" can be defined on the basis of some widespread but local characteristics of social reality so that the phenomena that possess those characteristics can be included in a unified domain of investigation. The fact that reality can be scientifically modelled in potentially innumerable ways, which results in the indefiniteness of the concept of politics, is an inherent feature of the modelling activity of science, and one that has distinctively scientific grounds at that. All scientific models are necessarily purpose-relative, and therefore they involve a stipulation about the purpose of the model on the part of the inquirer. The different conceptualizations of politics cannot result but from disagreement about their explanatory purposes and, ultimately, about the nature of explanation.

By pointing to the purpose-relative nature of representational models, I will argue that the seemingly competing views on what politics is in fact correspond to the different conceptualizations of the scope of political science that necessarily result from different approaches to political explanation, and indeed to different views of the nature of scientific explanation altogether. Since the object of politics is defined by its role in demarcating the boundaries of what political science can explain, its "purpose" ought be related to the explanatory outreach of political science methodology. The model of explanation implicit in any definition of politics thus provides the testing ground for assessing its adequacy to explaining political phenomena. In a sense, the present work can thus be seen as an exercise in applied philosophy of science. By considering the political sphere as a scientific object, I will use the tools of the philosophy of science to impose some conceptual order on a debate that traditionally belonged in the province of political theory. Specifically, I will contend that

the one-sidedness through which many existing conceptualizations of politics purport to exhaustively and unequivocally model the object of political science does not adequately capture the model-based nature of scientific objects. This is because, as I shall argue, they rely on a highly controversial model of explanation.

Traditional political science was based on the equation between explanation and the search for "covering laws". This now rather outdated model of explanation adopted the view that the "science" part of political science can only by defined by logical criteria of inferential validity, and therefore explanation can be described in terms of the logical deduction from general statements which express some uniform regularities in the empirical world and can thus serve as "covering laws" under which particular phenomena can be subsumed. For some time after the publication of Hempel and Oppenheim's (1948) extremely influential account of explanation in terms of deduction from covering laws, this view has qualified as the received view of explanation, and political science was no exception. The comparative expansion of political science in the 1950s was premised on the idea that covering laws are necessary to infer empirical explanations, and therefore cross-area comparison required to identify the invariant features of social systems that could feature in general laws that are valid across time and space. This is not to say that the development of modern political science can be straightforwardly seen as the mere application of the logical-empiricist view of explanation—as codified e.g. by Hempel in his "deductive-nomological" (D-N) model (Hempel, 1942; see below, section 2.1). After all, some prime instances of covering-law political science like Almond's ([1960] 2015) "functionalism" or the "structuralism" implicit in Easton's (1953) "systemic" approach would be anathema to many logical empiricists—including Hempel ([1959] 1965, 1969)—and hardly be amenable to a strictly deductive-nomological form. In fact, Hempel's D-N model, like his "inductive-statistical" (I-N) model (Hempel, 1965a) are but instances of the overall notion that scientific explanation ought be conceived in inferential terms as the subsumption of explanation-statements under statements that express empirical regularities.

It is this *inferential* rather than *causal* view of explanation—of which the covering-law model is a corollary—that underwrites the search for regularities in empirical political science and that will be the target of the present work. The very idea of "the political system" as overcoming the traditional dichotomy between state and non-state was seen by its proponents as a necessary condition in order for political science to be oriented to finding regularities, since the identification of politics and state would limit the scope of research to the institutional arena of state societies. On the contrary, identifying the subject matter of political science with the abstract concept of "the political system" opened up the possibility of identifying similar functions in institutional as well as non-institutional settings, including stateless societies and the traditionally non-political structures of state societies such as kinship networks.

In the first part of this work I will argue that the covering-law model of explanation is inadequate. I will defend a *causal* instead of an *inferential* understanding of scientific explanation, and I will show how this view is implicit in much empirical social science, and

more explicit (if unacknowledged) in recent proposals for a "mechanism-based" approach to social explanation. In light of the fact that this position stands opposed to the model of explanation on which some influential conceptions of politics as a scientific object have been based, in the second part of this work I will show how the causal approach to scientific explanation also provides the criteria for assessing the explanatory adequacy of those definitions as representational models. Specifically, chapter 1 will reconstruct the emergence of politics as a scientific object out of the methodological development of political science in the twentieth century. I will show how existing conceptualizations of the scope of political science depend on the adoption of a covering-law model of explanation, which I will criticize in chapter 2. I consider a realist tradition in political science that has recently been formulated in the form of the "mechanism-based" approach to political explanation. The link between mechanism-based explanation and realism about causal explanation is a well-established topic in the philosophy of physics, biology, and cognitive science, where the concept of explanatory mechanism is a well-established notion, but its discussion in the social sciences has essentially reproduced the existing tropes of debate in general philosophy of science. More often than not, disputes concerning causal explanation in the social sciences are internal to the debate among theoretically-minded practitioners of the various social science disciplines, and therefore their philosophical implications remain inevitably underappreciated. Even though proponents of the mechanism-based approach in the social and political sciences have self-consciously drawn on the life sciences as mechanism-based disciplines, mechanisms are often presented by its advocates in quite prescriptive terms, e.g. as models for constructing "middle-range" explanatory theories as an alternative to mere correlational analysis. I will argue instead that that the idea of mechanism-based explanation did not emerge with the purpose of remedying the inadequacies of the covering-law model of explanations that correlational analysis instantiates. The contrast between mechanisms and covering laws in fact mirrors the fundamental opposition between causal realism and causal eliminativism. The increasing favour for mechanism-based explanation in the social sciences, therefore, will be understood as a case for causal realism in social explanation.

Having linked rival methodologies for social science explanation to their different assumptions regarding the nature of causation, I will then consider the literature on the metaphysics of causality. Different theories will be examined and assessed on the basis of their compatibility with the perspective of causal realism. In order to impose some order on the varieties of causal theories, I will introduce a distinction between "passivist" and "productive" theories of causation. Some of the most prominent theories of causation—INUS theory, manipulative interventionism, nomological realism, and the counterfactual approach—will be shown to be inherently passivist insofar as they are taken to be theories about the nature of causation. For this reason, I shall argue, they are unable to substantiate causal realism. The mechanism-based model of explanation, on the other hand, demands that causality be understood as an inherently productive process. In chapter 3 I will thus focus on *dispositionalism* as a metaphysical theory that fulfils the realist demand for a productive account of social causation. As I characterize it, dispositionalism is a metaphysical approach associated with the "causal

powers" discussed in some realist strands in the philosophy of science. The idea of "causal powers" was a staple of classical Aristotelian metaphysics, yet anti-metaphysical approaches to the philosophy of science—particularly the received view—have cast a veil of suspicion on the meaningfulness of such concept, let alone its explanatory relevance. Contemporary social ontology, on the other hand, has focused almost exclusively on questions concerning the role of language in constructing the different "levels" of reality, without relating them to the metaphysical standing of individual social objects.

Beginning in the 1970s, talk about "powers" and "dispositions" has gradually garnered new respectability in analytic metaphysics. For the present purposes, I will focus on the way different metaphysical theories of properties underpinning dispositionalism can lead to a theory of objects. Specifically, I will take into account those theories of properties that are not conducive to causal eliminativism, and briefly consider their implications for the fundamental nature of objects. I take dispositionalism to be a theory about the causal powers that entities have in virtue of the dispositional character of the properties of which they are endowed, and therefore I take dispositionalism to be an approach that requires for causation to be analysed in terms of the fundamental properties of entities. For the sake of the present work, dispositionalism allows us to recast scientific realism as a theory about objects, and therefore it lays the groundwork for analyzing the nature of scientific objects from a realist point of view. Dispositionalism, I will argue, entails a theory of entities as *individual* objects, and therefore it provides no metaphysical foundation to the construction of scientific objects as classes of objects.

For this reason, in chapter 4 I will move from metaphysics to the philosophy of classification in order to clarify the place that scientific objects have in the actual practice of science. The philosophy of classification is one of the most recurring topics in the philosophy of biology, but it is an exceptionally underappreciated issue in the social sciences in spite of the fact that typology-building is part and parcel of much social science, and political science in particular. The main alternative approaches to understanding the nature of natural science classifications are *essentialism* and *nominalism*; I will argue that concept formation in traditional political science tacitly relied on an essentialist approach—i.e. the idea that classifications of objects into classes mirror real-world differences among different kinds of objects. In the light of dispositionalism, I take *essentialism* to be conflicting with the principles of scientific realism, and therefore I contend that conceptual analysis in political science must necessarily rely on a *nominalist* approach. Class-concepts, I will argue, are the products of stipulations about the "family resemblances" between the properties of individual objects.

While this results in a metaphysically arbitrary account of the way social objects are constructed through the classification of individual entities into typologies, in chapter 5 I will attempt at a rationalization of such arbitrariness by exploring its function for the purpose of providing scientific explanation. As mentioned, the concept of a "scientific model" can explain this function by pointing to the representational aspect of scientific practice. Empirical models can be considered to be scientific objects that are constructed in order to

represent some partial aspects of the world; the way those aspects are selected corresponds to the purpose for which the model has been designed, and therefore different purposes can produce different models that represent a portion of reality in equally adequate ways. I will compare the idea of the political sphere, *qua* a scientific object, to an empirical model: the concept of politics has been developed in order to define the scope of political science, and therefore the scientific object it refers to is constructed for the purpose of explaining political phenomena. In this way, the adequacy of the model can be judged by assessing the adequacy of the model of explanation that it implies. As a theory about the nature of explanation, scientific realism can thus provide the criteria for an adequate model of explanation, thereby reducing the arbitrariness of model-building.

Chapter 1

The object of politics

> The definition of a science has almost invariably not preceded, but followed, the creation of the science itself. Like the wall of a city, it has usually been erected, not to be a receptacle for such edifices as might afterwards spring up, but to circumscribe an aggregation already in existence. Mankind did not measure out the ground for intellectual cultivation before they began to plant it; they did not divide the field of human investigation into regular compartments first, and then begin to collect truths for the purpose of being therein deposited; they proceeded in a less systematic manner. As discoveries were gathered in, either one by one, or in groups resulting from the continued prosecution of some uniform course of inquiry, the truths which were successively brought into store cohered and became agglomerated according to their individual affinities. Without any intentional classification, the facts classed themselves. They became associated in the mind, according to their general and obvious resemblances; and the aggregates thus formed, having to be frequently spoken of as aggregates, came to be denoted by a common name. Any body of truths which had thus acquired a collective denomination, was called a science.
>
> John Stuart Mill (1844)

Introduction

In this chapter I shall address the age-old question, "What is politics?", from a scientific point of view, i.e. as a question about the boundaries of politics as a field of scientific

investigation. The reasons for making this move derive first and foremost from historical considerations about the development of the political field within the larger framework of the rise of the social sciences in their modern disciplinary arrangement. With the increasing institutionalization and professionalization of social science knowledge, what was once conceived of as a unitary world became apportioned into sectoral areas of competence. The identification of a "political sphere" with its own specificities, it will be argued, followed the previous emergence of "society" and "economy" as distinctive scientific objects: the definition of politics as an autonomous system of relations within the broader social order was determined by the quest for the disciplinary autonomy of political science from sociology and economics.

Thus, although the emergence of the idea of the autonomy of politics is traced back to various stages in the intellectual history of the West, its shaping as a distinctive scientific object followed rather than set the conditions for its scientific investigation. Definitions of the scope of a science, wrote Mill, are like the walls of a city: they are built to delimit the borders of an aggregation that has already been erected rather than to carve out a construction area on a virgin soil. The purportedly unitary character of such aggregations, thus, is not pre-determined: on the contrary, it is determined exogenously by the characteristics of science itself. In the following sections, the emergence of politics as a field of scientific investigation will be traced to the construction of politics as a scientific object in the foundational stage of modern political science. Post-WWII generations of political scientists came to define their object of study in formal terms as constituted by specific "functions"—typically associated with authoritative decision-making—which different social structures perform across time and space.

Not surprisingly, this notion was based on a particular understanding of political science explanation. The "behavioural turn" that accompanied the rise of contemporary political science and the consequent definition of its scope was grounded in a "covering-law" understanding of political explanation. The purpose of political science, it was generally argued, was to discover regularities in political processes that could serve as causal laws under which individual events could be subsumed. Political explanation was thus conceived in terms of the deduction of particular phenomena from correlations between recurring events whose well-established empirical confirmation could warrant causal inferences. The expansion of the field of comparative politics in the post-WWII was motivated precisely by the search of such regularities, and the assumption that their identification is an exhaustive goal for political explanation determined the definition of the scope of science in terms of certain "functions" that are operative in different national contexts irrespective of their specificities.

Modern-day answers to the question about the distinctiveness of the political sphere as a scientific domain will thus be shown to be dependent on a covering-law approach to political science. The next chapters will then examine critically the epistemological presuppositions of covering-law explanation and explore the alternative models of explanation that have been proposed in order to bring them to bear on the status of politics as a scientific objects.

1. A pre-disciplinary history

The idea that politics can be thought of as a distinct sphere of social life has a history of its own. Like that of all concepts, the meaning of the word "politics" has shifted considerably over time. Those who have pointed to the etymology of the word typically emphasize that the Greek term from which it derives (*polis*) had a variety of meanings, and although this word is most distinctively used to the refer to the city-state as the basic territorial entity of Ancient Greece, in the Archaic and Classical Ages it could also be used in a much broader sense including as synonymous with *politai*, the collection of citizens (see e.g. Aristotle, *Pol.* III, 1274b41; see also Hansen, 1998).

The indistinctiveness of the Ancient Greek conception of politics is expressed in Aristotle's characterization of man as "*politikón zoon*", which Thomas Aquinas authoritatively rendered with the Latin phrase "*animal sociale et politicum*" as if to attach a broader meaning to Aristotle's definition than the Latin adjective *politicum* alone would allow. The specificity of politics in the European culture of the late Middle Ages, however, should not be overemphasized. As Rubinstein (1987) has noted, the word *politicus* has been introduced into the language of Western political thought through William of Moerbeke's translation of Aristotle's *Politics*, where it is used as synonymous with the pre-existing Latin word *civilis* and where "*politikón zoon*" is translated as "*animal civile*". It would seem, therefore, that the *civil* conception of the Romans translated the *political* conception of the Greeks rather straightforwardly, at least linguistically.

Sheldon Wolin (2004; 118) tempered this view by pointing out that in the thought of the Church Fathers "the belief persisted that society represented a spontaneous and natural grouping, while the political stood for the coercive, the involuntary". Augustine of Hippo's concept of a "City of God" (*civitas dei*), he observes, is a more perfectly political order than the "Earthly City" (*civitas terrena*) primarily because it is more perfectly "social" in binding together its members into a harmonious fellowship, while the Earthly City is internally divided by the multiplicity of private goods and interests. This, in Wolin's view, implies the superiority of the social dimension over the political in Late Medieval thought and, a fortiori, the discernibility of the two. This does not exclude, however, that the Latin idea of *civitas* was one of "a diluted body politic", as Sartori (1973) put it. For Sartori, this is the case not because of the similarities between the Greek *polis* and the Roman *civitas*, but rather because of their differences: the Romans, he argues, "were absorbing Greek culture at a time when their city had long since surpassed the dimensions that permitted a 'political' life as understood by the Greeks" (Sartori, 1973; 8). This made the *civitas* a large ensemble of citizens whose only political organization is that of the juridical order: rather than contrasting the social with the political dimension, the idea of *civitas* depoliticizes altogether the ordering of the community by substituting the juridical for the political.

Be this as it may, it is true that the word *civitas* indicated the well-ordered community of men living together under the rule of law and therefore left no room for further political qualification of sparse aspects of the social order. A decoupling of politics from society on

the whole, therefore, seems to be absent from Western intellectual history since Ancient Greece and throughout the entire Latin-speaking culture of the Late Middle Ages. Even where the conflict between a "social animal" and a "political animal" is most explicit, Sartori argues, it ought not be interpreted as a conflict between two distinct dimensions of the social life of human beings: when early authors such as Seneca wrote about man as a *sociale animal* in Stoic terms—i.e. as a citizen who has withdrawn from the *polis* to live in a virtual *cosmopolis*—they did not present it as a mode of social life that coexisted alongside life within the *polis*; rather, the *sociale animal* is one who has lost the *polis*—which in the Hellenistic period had largely become dominated by other city-states or by the kingdom of Persia or had become part of a federal state—and has replaced the erstwhile *politicum animal* as the new anthropological model of life in the community.

For Sartori, the autonomy of politics from the overall social realm is associated with a "hierarchic" or "vertical" perception of politics, i.e. "an altimetric projection which associates the idea of politics with the idea of power, of command, and, in the final analysis, with a state superimposed upon society" (Sartori, 1973; 8). As he notes, the autonomy of politics has less to do with its independence from ethics than it has to do with its separation from *society*. The identity of politics as a *thing* in its own right, in other words, is premissed on "the discovery of society" as an autonomous reality and on the subsequent differentiation of the political sphere from it. Correctly, Sartori points out that this separation of society and politics occurred through a prior outgrowth of the economic sphere from the body politic. It is liberal-constitutional political theorists, he observes, who first postulated that economic transactions could better prosper in the absence of state intervention, and it is on this idea that the eighteenth- and nineteenth-century conception of society as an independent reality with a self-regulating functioning was based. Eventually, the notion of a self-sustaining social arrangement would lead in the nineteenth-century to a form of "pan-sociologism" that conceived of the social body as encompassing even the juridical, thereby severing the links between society and the political system. As a result, the modern notion of the political system came about as associated with—and in the last instance confined to—what Sartori calls the "vertical dimension" of social life, i.e. the hierarchical structure implied in the exercise of power and command, typically—though by no means exclusively—on the part of governmental institutions such as the state.

2. The object(s) of social science

It is worth stressing that these independent spheres of the social world—the economy, society, and the political system—emerged in intellectual discourse first and foremost as objects of inquiry. To be sure, "political economy" had already distanced itself from statecraft with the rise of physiocracy, mainly out of liberal political preoccupations against protectionism, and in the writings of Cantillon and Quesnay, the idea of the market as a "natural" order independent of that of the state served as a warning against intrusive regulation on the part of governments. However, this contention was based on the idea that the market as a societal

object had its own independent "laws of motion" that could be investigated and detailed. A physicist by training, Quesnay based his economic theory on knowledge of the laws of the "natural order" of associative life:

> In order to understand the order of time and space, and to control navigation and safeguard trade, it has been necessary to observe and calculate precisely the laws of the movement of celestial bodies. Similarly, in order to understand the extent of the natural right of men joined together in society, it is necessary to settle upon the natural laws which form the basis of the best government possible … For without an understanding of the natural laws which ought to serve as the foundation of man-made legislation and as sovereign rules for the conduct of men, there is no self-evident knowledge of just and unjust, natural right, and physical and moral order; there is no self-evident knowledge of the essential distinction between general and individual interests, and of the real nature of the causes of the prosperity and decline of nations; and there is no self-evident knowledge of the essence of moral good and evil, and of the sacred rights of those who command and the duties of those to whom the social order prescribes obedience. (Quesnay, [1768] 1962; 53–54)

The economic model of a commercial society and its use for liberal political prescriptions also fit in the process that led to the emergence of the idea of society. The intellectual development that accompanied the rise of physiocracy in France and the separation of economics from state politics was in many respects paralleled in the Scottish Enlightenment, which would act as a main driver of the process of "discovery of society". For John Pocock, the very birth of Scottish political economy is shaped by the emergence of a "transactional" vision of the conduct of individuals within society. In particular, the new model of commercial society replaced older political paradigms of community, and "the locus of virtue shifted decisively from the civic to the civil, from the political and military to that blend of economic, cultural and moral which we call the social for short" (Pocock, 1983; 240).

2.1. Society

Through Adam Smith's influence on Say and on Auguste Comte, the "transactional" model of society laid the foundations for the conceptualization of "society" as a separate domain of inquiry for a "social science". Condorcet, for instance, outlined the prospects of a "social mathematics" concerned with "people and things"—i.e. individuals and resources—implying that they could be thought of as relatively independent of state regulation and therefore as the building blocks of the separate domain of "society" (Heilbron, [1990a] 1995; 170–171). Indeed, it is within post-revolutionary groups of social reformers—particularly the *Society of 1789*, in which Condorcet played a prominent role—that the very idea of a "social science" developed, and it is through Condorcet's work that the term was introduced into the English language.

Indeed, one may go so far as to view the apportioning of the overall social realm into different domains of investigation as a by-product of the process of compartmentalization of social knowledge that accompanied the rise of the different social sciences between the nineteenth and twentieth century. With the increasing disciplinarization and institutionalization of the social sciences, the boundaries between different domains of inquiry within the broader landscape of social reality sharpened into qualitative differences between scientific objects that constituted the subject matter of the various social science disciplines. Johan

Heilbron ([1990a] 1995) indicated the period between around 1600 and the middle of the nineteenth century as the "predisciplinary stage" in the development of the social sciences. Crucially, what marked the organization of knowledge in this pre-disciplinary stage was that "[i]deas were informed by general conceptions in which such terms as 'reason', 'nature', and 'philosophy' were key concepts" (Heilbron, [1990a] 1995; 3). One should have no difficulty in locating the intellectual history of politics summarized in the opening pages of the present chapter within this framework: as Heilbron argues elsewhere, before the "disciplinary stage" and modern concepts of the social world became an established framework, human matters were conceived of in an essentially unitary fashion, often under the banner of "practical philosophy" inherited from the Aristotelian tradition (Heilbron, 1990b; [1990a] 1995; 76). A number of factors contributed to a reshaping of this state of affairs; ultimately, the societal changes that influenced the framing of intellectual discourse through the modern concepts of the social world became institutionalized into distinct disciplines, each with a subject matter of its own. The separation of "things political" from other aspects of the overall ordering of society can be interpreted as a by-product of this compartmentalization of knowledge characteristic of the disciplinary stage in the development of the social sciences.

For Heilbron, the disciplinary stage proper began under the reign of Louis XVI, when Turgot was appointed minister of finances (1774). Turgot's extensive reforms were implemented with prominent Newtonian scientist Lavoisier acting as an advisor, and it was during Turgot's term as a minister that scientific experts acquired unprecedented social prestige and the Newtonian sciences flourished in newly founded scholarly institutions. It is only through the work of Condorcet, however, that the significance of the Newtonian model of science was applied to moral reflections on the emerging idea of "society". A disciple of Turgot's, Condorcet built on Laplace's work in mathematics, particularly probability theory, as a tool that could be utilized to explain empirical phenomena. What Laplace had envisioned with respect to the natural sciences, Condorcet extended to the social world through the development of the idea of a "social mathematics".

By the breakout of the French Revolution, Condorcet had been already appointed secretary of the Academy of Sciences. Actively involved in Turgot's programme of reforms as well as in the activities of the National Assembly during the Revolutionary period, Condorcet advocated tackling social issues by means of mathematical tools, which could be used for the statistical management of quantitative demographic data. Eventually the idea of a "social science" developed by the *Society of 1789* and shared by other *idéologues* of the era such as Cabanis and Saint-Simon would be influentially employed by Charles Fourier in the early decades of the nineteenth century and, more critically, by Auguste Comte in his foundational writings on what would become the discipline of "sociology" (Baker, 1975; Head, 1982; Shapiro, 1984).

Despite Comte's foundational attempts at establishing a positive social science, however, it is only in the last quarter of the century that sociology began to acquire its status as an autonomous discipline. Unsurprisingly, this process occurred through scientific efforts at differentiation from economics, and in some relevant sense classical sociological approaches

as expressed in the writings of Durkheim, Pareto, and Weber originate from dissatisfaction with the partial and one-sided way in which mainstream political economy was considered to account for social phenomena in individualist-utilitarian terms (Therborn, 1976). Weber's position was more properly methodological in nature and more strictly situated within the context of the Austro-German *Methodenstreit* concerning the possibility of a deductivist science of economics, which Weber largely rejected. For Pareto, the tendency to deductive approaches in political economy directly resulted in a self-limiting of its scope: specifically, political economy proved inadequate as a comprehensive social science in that it could not exhaustively account for "non-logical" social action and therefore left entire families of social phenomena unaccounted for. Not too dissimilarly, Durkheim—whose approach to sociology and to Comte's work had been mediated by his knowledge of the German and English literature in the field—notably defended the irreducibility of "social facts" vis-à-vis the methodological individualism of political economy (Wagner, 1990). Durkheim ([1897] 1951, [1895] 1964), in particular, conceived of society as causally autonomous from individuals, and although he admitted that "social facts" ultimately emerge out of individual interactions, they result in social structures that are autonomous and external to them so that society becomes an independent object in its own right (Durkheim, [1893] 1984, [1895] 1964). This idea, together with the corollary postulate that one can discover *sui generis* "sociological laws", was adapted by Talcott Parsons and his associates into the concept of "social system" that would have an almost unrivalled impact on twentieth-century sociology (Wagner, 2000; 147).

2.2. Economy

Economics, too, did not become a separate science until the "marginal revolution" that emerged in response to the 1860s–1870s crisis of classical political economy both in policy and in theory. It is true that the liberal ideology of *laissez-faire* economists encapsulated in the mainstream of classical economics had been to a great extent successful in establishing economics as a separate science. Yet the Ricardo-Mill paradigm that dominated classical economics until the late 1860s still served primarily as a guide for policy and blurred to a certain extent the distinction between economics and policy. When the detrimental effects of the repeal of the Corn Laws on British agriculture broke out, however, the crisis in intellectual hegemony of the Ricardo-Mill paradigm—whose defenders had supported the repeal of the Corn Laws—became irreversible (Hutchison, 1978). In the attempt to rescue economics as a purely theoretical discipline, W.S. Jevons—like Walras in Lausanne and Menger in Austria—advocated a restructuring of economics as a purely theoretical discipline and one concerned almost exclusively with the value problem. Jevons and Walras, in particular, conceived of economics as a strictly deductive and, ultimately, purely mathematical science that ought to be kept distinguished from other applied social sciences (Winch, 1972).

The ultimate affirmation of neoclassical economics and the establishment of economic science as an academic discipline occurred through the work and institutional role of Alfred

Marshall in the 1880s and 1890s. Jevons had in fact already thought through the conditions for public recognition of the autonomy of pure economics when he wrote: "Among minor alterations, I may mention the substitution for the name political economy of the single convenient term economics. I cannot help thinking that it would be well to discard, as quickly as possible, the old troublesome double-worded name of our science" (Jevons, [1911] 2013; xxxv). Nonetheless, before Alfred Marshall no economist had systematically adopted the term "Economics" instead of "Political Economy" in their major works, and although Marshall was more concerned with defending the formal generality of economic science than its circumscribed scope, he codified an "economic organon" based on the principle of marginal utility and on mathematization of the science which would serve as the basis of neoclassical economics teaching at Cambridge (Collini, Winch, and Burrow, 1983).

Revealingly, Marshall's *Principles of Economics* (1890) open with one of the best-known definitions of economics:

> *Political Economy* or *Economics* is the study of mankind in the ordinary business of life; it examines that part of individual and social action which is most closely connected with the attainment and with the use of the material requisites of wellbeing. (Marshall, [1890] 2013; 1)

As for John Neville Keynes after him, the definition of economics is for Marshall of paramount importance in defending the theoretical consistency of economics against empiricist approaches such as William Cunningham's (Marshall, 1892; Whitaker, 2005; 250–252). In his *Politics and Economics* (1885), Cunningham had written:

> Economic science is wholly practical, it has no *raison d'être* except as directing conduct towards a given end: it studies the means leading towards that end not merely for the sake of knowledge, but in the hope of guiding men so that they may pursue that end in the most appropriate way: it is not content to describe the principles that have actuated human conduct, but desires to look at these principles in the light of after events, and thus to put forward the means that are best adapted for attaining the end in view. (Cunningham, 1885; 12)

As an inherently practical discipline, Cunningham argued elsewhere, political economy is not amenable to unifying laws like those that characterized the science of physics at the time. Instead, political economy should seek to remain an entirely empirical science, and to advance this cause its intellectual task should be not to explain but first and foremost "to *observe* and *classify* and *describe* and *name*" (Cunningham, 1887; 8 emphasis in the original).

Rejecting this idea, Keynes adopted what he deemed a useful distinction between "economics" and "political economy", favourably reporting the increasing popularity of the former term over the latter. Even though he deliberately used both terms interchangeably in that "the name *political economy* … is too firmly established to be altogether discarded" (Keynes, [1890] 1955; 53 n.1, emphasis in the original), he condemned "the intrusion of ethics into economics" as a source of undue controversy. Economics, he contended, is *a positive science*, and conceptual clarity is helpful in maintaining its independence from questions of justice: *economics* should be preferred over *political economy* in that it is less subject to theoretical ambiguity. As a positive science, Keynes argued, economics is characterized by a scope as well as by a method. Accordingly, he defines economics in a

deliberately recursive fashion as "the science which treats of the phenomena arising out of the economic activities of mankind in society", whereby economic activities are those human activities directed towards the production and appropriation of *wealth* understood as the potentially exchangeable means of satisfying human needs (Keynes, [1890] 1955; 95–101).

2.3. Politics

With the scientization of social knowledge into different disciplines, therefore, came the need for demarcating the scope of each particular social science. In this way, the subject matter of the various social sciences became entrenched in the scientific development of the various social science disciplines, so much so that scientific objects came to be defined in terms of the very method of a science. So, for instance, the economy was identified with market interactions characterized by general equilibrium as defined by the differential calculus of neoclassical economics. Likewise, "statistical and demographic research was said to reveal some solid and lawlike features in the characteristics and movements of a population and thus to underpin the idea of the existence of 'society'" (Wagner, 2000). Even when authors as influential as Weber (or Simmel) tended to regard society less as an entity in its own right than as a bundle of processes and relations, these are typically presented as analyzable only in terms of a distinctively sociological method. As section 3. will try to show, the discovery—or, more appropriately, the establishment—of the "autonomy of politics" is but one further step along this path and, as Sartori has argued, one that is parasitic upon the former two.

Interestingly, Wallerstein et al. (1996) describe the disciplinarization of social science as a process of professionalization that supervenes on the academic institutionalization of knowledge in a compartmentalized fashion. So, for instance, Alfred Marshall's 1885 appointment as professor of political economy at Cambridge provided the conditions for the institutional recognition of the discipline he substantially contributed to reform. During his Cambridge years, Marshall's role was pivotal in constituting the British Economic Association (1890) as well as its organ, the *Economic Journal* (1891), which Marshall directed and which provided an early vehicle for dissemination of neoclassical ideas in Britain (Coats, 1968). Yet Marshall was first and foremost instrumental in recruiting new economists at Cambridge, like his successor Pigou, and in introducing the first examination in economics in the form of the traditional Cambridge *tripos* (1903). Favoured by the relative backwardness of Oxford and the financial and political hardships of the newly founded London School of Economics, the "Cambridge School" had thus initiated a process of academic professionalization of economics as a distinct intellectual expertise (Coats, 1967).

Not too dissimilarly, sociology became an institutionalized discipline somewhat independently of Comte's effort at laying out its intellectual foundations. As Wallerstein et al. argue, it is social reformers of the mid-nineteenth century who gave it a decisive impulse by moving their work to a university setting, thus surrendering their role in active lobbying in favour of a more reflexive endeavour (Wallerstein et al., 1996; 19). It is within this context that the British Association for the Promotion of Social Science was organized in 1857, shortly

followed by the creation of the American Social Science Association (ASSA, 1865). Members of the ASSA published the *Journal of Social Science* and had important roles in numerous political committees, all of which attests to the influence of early social science experts; yet this also brings to attention the fact that these intellectuals still thought of themselves as reformers, and that the comprehensive idea of social science that they had in mind was still very much policy-oriented (Silva and Slaughter, 1984). As Wallerstein et al. remark, for sociology to become an established discipline the break with its origins in lobbying organizations had to be consummated through the cultivation of a broadly positivist thrust (Wallerstein et al., 1996; 19).

Although early twentieth-century sociology continued to gain prestige from its practical role in society, its professionalization was in fact perfected through the institutionalization of its distinctively positivistic method. The University of Chicago acted as a key site for this transition. In 1892, Albion Small founded the first department of sociology at the University of Chicago; in 1894 he established and edited the *American Journal of Sociology*, the only professional journal of sociology until 1921; finally, in 1905 he contributed to organize the American Sociological Society (Silva and Slaughter, 1984; Manicas, 1990). When the conflict between the traditional idea of an all-encompassing (and hence imperialistic) science of society and the division of academic labour among different social science departments became a pressing intellectual issue, the faculty at Small's department in Chicago began to explore the alternatives. Gradually, in textbooks and other influential publications the discipline of sociology and its experts came to be characterized by their use of qualitative and quantitative (demographic and statistical) methods to describe social *groups* (Manicas, 1990). Yet even aside from the Chicago case, the model of an empirical-descriptive and neo-positivistically guided social science developed across different countries—including European ones—as a response to the intellectual crisis of the institutional legitimacy of classical sociological paradigms that resulted from their failure to meet the organizational standards of the academia (Wagner, 1990; 233–240). Eventually, this transformation was perfected through a new methodological—mainly statistical—treatment of individual behaviour which led to a reconceptualization of the relation between individual and society and, ultimately, of the very concept of "society" as a scientific object (Wagner, 1990; 240–241).

Not surprisingly, the development of political science and the definition of its object as a separate sphere of society would essentially reiterate this pattern of institutionalization, which culminated with the definition of the "political system" in terms of the behavioural model of explanation promoted by the post-WWII generation of political scientists at the University of Chicago.

Interlude: The issue of scope as a philosophical problem

At this point, however, some clarification is in order. The proposed idea that social science objects arose out of the methodological demarcation of the scope of the various social science disciplines should not suggest that these became "normal sciences"—as per Kuhn's ([1962]

2012) famous phrase—upon reaching undisputed consensus regarding their method. Not only would this presuppose a Whiggish interpretation of the rise of the social sciences that provides a rational reconstruction of their development as teleologically oriented towards the achievement of a "mature stage". An exclusively "internalist" picture that focuses on scientific rationality as the only interpretive criterion of scientific development would also miss important institutional features that have nonetheless been crucial in shaping the modern arrangement of the social science disciplines. This institutionalizing pattern, however, should not be seen as a disruptive perversion of the otherwise autonomous development of the social sciences. On the contrary, scientific development is in some sense inherently characterized by "external" features as much as by the "internal" ones that respond to the criteria of scientific rationality. This should not appear as mere historiographic pedantry. In fact, the very separability of "internal" and "external" features of science is in itself a philosophical issue, and an especially controversial one.

Much historiography and sociology of science in the twentieth century has been characterized by its relative position in the debate between external and internal approaches (see Kuhn, [1968] 1977). The externalist pole of the debate was typically centered on the idea of "social production of knowledge": by emphasizing that scientific production is an utterly social activity, externalist accounts stressed social and political factors as decisive—and, in the strongest variants of the paradigm, exhaustive—factors for explaining scientific development. One classic example in externalist historiography of science, for instance, situated Newton's work within the context of seventeenth-century societal development. In his long essay on *The Social and Economic Roots of Newton's "Principia"* (1931), Soviet historian Boris Hessen saw Newton as a self-conscious exponent of his social class, and interpreted his scientific work as the purported solution to the technical problems of his time related to ballistics, hydrostatics, magnetism, optics and mechanics (Hessen, [1931] 1971).

As Kuhn ([1968] 1977) remarks, utterly externalist approaches developed out of dissatisfaction with the supposed one-sidedness of more traditionally internalist ones. As a consequence, the latter do not add up to a coherent intellectual movement nor do they share any common programmatic assumptions. According to Kuhn, internalist historiographers traditionally set as their goal the investigation of the actual thinking of the scientists involved in the scientific debates under consideration, without relying on the science that they know. This characterization, however, is partial at best, since it is entirely based on methodological principles concerning the historiography of science rather than on theoretical considerations on what is the driving force of knowledge production. Perhaps more accurately, what is typically referred to as the "internal"—or "internalist"—approach to the historiography and sociology of science seems to imply a commitment to the idea that non-intellectual—e.g. social or economic—factors are entirely contingent and therefore rationally irrelevant to making sense of scientific development. *Contra* externalism, science is taken to be a purely intellectual and fundamentally rational enterprise whose development can be fully accounted for in terms of the cogency (however defined) of the arguments provided in support of the accepted scientific theses. According to internalists, the theoretical elements of knowl-

edge production are, in the words of Karl Mannheim ([1929] 1936), "immanent" to the intellectual history of science; by denying the theoretical importance of extra-intellectual factors, internalists seem to imply that the process of scientific knowing is driven by an "inner dialectic" and therefore follows only from the "nature of things" or from "pure logical possibilities" (Mannheim, [1929] 1936; 240). Among internal historians of astronomy and mechanics to Newton, Kuhn cites the work of Alexandre Koyré, presumably because of his understanding of Newtonian physics as originating, among other things, from a purely theoretical "synthesis" between the curpuscular philosophy of Boyle and the mathematical and geometrical theory of Galileo and Descartes (Koyré, [1948] 1965; see Kuhn, [1968] 1977). The image of scientific development that results from internalism is thus one that sees science as proceeding through progressive rectification of theories, and therefore as governed by a purely internal "logic of discovery". Accordingly, the appropriate methodology to account for it is that of "rational reconstructions"—as Lakatos ([1970] 1978) termed them.

In the last decades of twentieth century, the externalism-internalism debate largely cooled down, primarily because of the extinction of hard-line externalist programmes. As Shapin (1992) reports, as early as 1963 "extreme" externalism could be pronounced dead, and what could be seen by some as moderate externalist approaches in fact presented themselves as explicitly eclectic with respect to the externalism-internalism dichotomy. In hindsight, the very opposition between externalism and internalism could be seen as by and large unwarranted. After all, Hessen himself had clairified with respect to his account of the origins of Newton's *Principia*:

> The economic position is the foundation. But the development of theories and the individual work of a scientist are affected by various superstructures, such as political forms of class war and the results, the reflection of these wars on the minds of the participants—political, juridical, philosophic theories, religious beliefs and their subsequent development into dogmatic systems. (Hessen, [1931] 1971; 177)

At the other end of the spectrum, even Koyré's work was seen by a generation of post-WWII Anglo-American historians of science as providing a source of inspiration for adopting an intermediate position between "vigorously self-conscious internalism and temperate eclecticism" (Shapin, 1992; 341). As a result, the externalism-internalism controversy came to be increasingly seen as a fake dichotomy.

In fact, the very distinction between "internal" and "external" aspects of science is a philosophically laden notion, and sidestepping such distinction implies a rejection of the philosophical assumption on which it rests. The idea that purely logical features are internal to science while contextual ones are best characterized as external to it overlaps to a great extent with the epistemological distinction between a "context of discovery" and a "context of justification". First introduced by Hans Reichenbach, this pair of notions pointed to the inherent difference between questions of the origins of scientific theories and questions of their validity. "If a more convenient determination of this concept of rational reconstruction is wanted", Reichenbach wrote, "we might say that it corresponds to the form in which thinking processes are communicated to other persons instead of the form in which they are

subjectively performed. … I shall introduce the terms *context of discovery* and *context of justification* to mark this distinction" (Reichenbach, 1938; 6–7, emphasis in the original). Drawing on this influential distinction, logical empiricists were able to describe the scope of philosophy of science in terms of justification. "Epistemology", Reichenbach stated, "is only occupied in constructing the context of justification" (Reichenbach, 1938; 7). Specifically, criteria of justification were used by philosophers of science to demarcate science from "non-science" in terms of the ability of scientific theories to withstand justification. "The logical analysis of scientific knowledge", wrote Popper, is concerned "only with questions of *justification or validity* … Its questions are of the following kind. Can a statement be justified? And if so, how? Is it testable? Is it logically dependent on certain other statements? Or does it perhaps contradict them?" (Popper, [1934] 2002; 7). Accordingly, the task of philosophy of science—or, in Popper's parlance, of the logic of knowledge—consists "solely in investigating the methods employed in those systematic tests to which every new idea must be subjected if it is to be seriously entertained" (Popper, [1934] 2002; 8). Through the enduring influence of demarcationist philosophies of science such as Popper's ([1934] 2002) or Lakatos's ([1974] 1978), philosophy of science would thus assume the problem of demarcation as its central question and limit its scope to theories of justification.

As Giere (1999a) points out, however, this historically circumscribed assumption was to be challenged again by the immense and variegated historically-oriented scholarship of philosophy of science that followed the publication of Kuhn's *Structure of Scientific Revolutions* (1962). Most notably, he argues, historical approaches to the philosophy of science as well as societal implications of scientific discoveries were being explicitly debated both in the United States and in Germany in the 1920s and 1930s, and they were often considered as more relevant questions than the ones concerning the validity of scientific conclusions. It is only in the heyday of Logical Empiricism in post-WWII United States—ironically, a major influence on the scientization of political studies—that Reichenbach's distinction between a context of discovery and a context of justification came to be accepted almost universally as a cornerstone of philosophy of science. As Giere notes, therefore, questions of disciplinary history had long been included into epistemological considerations. As Mill suggested in the passage quoted in the epigraph to this chapter, scientific development inevitably reinforces the ascription of the phenomena under consideration to a homegeneous but specific class, lest science lose its scope and therefore its identity. As far as social science is concerned, one might see this process in action in the conceptual differentiation of social reality into different and separate domains—the economy, society, and the polity—as a mirror of the branching out of the social sciences between the nineteenth and the twentieth century. For the present purposes, it is important to stress that the way in which the idea of politics as an autonomous sphere of the social world became institutionalized in the guise of the notion of a "political system" and its cognate concepts is inextricably intertwined with the distinguishing features of political science as an academic discipline. The issue of scope, therefore, is a philosophical problem, and one that counts among the distinctive foci of traditional philosophy of science. Indeed, it is by focusing on the institutionalization of

the logic of inquiry of a particular science that the philosophical relevance of its scope can be unraveled.

3. The rise of the "noble science" of politics

By the time economics and sociology had become separate academic disciplines, political science had already been turned into an established university curriculum with its own chairs, graduate schools, departments, associations, journals, textbooks, libraries, and even institutions of its own. Before British universities like Oxford, The LSE, and Cambridge each introduced a chair in politics (in 1912, 1914, and 1928, respectively: see e.g. King, 1977; xi), as early as 1856 a short-lived chair at Columbia College in New York had been designated as "History and Political Science" at the request of Francis Lieber, its first incumbent and one of the founders of U.S. political studies (Gunnell, 1993). At Yale, a Chair of Political and Social Science was created in 1871 to carry on the Lieberian tradition after the retirement of Theodore Dwight Woolsey, Yale's president and a disciple of Lieber's; in 1860, the new Cornell University founded a College of History and Political Science.

Lieber's *Manual of Political Ethics* (1838–1839) and *Civil Liberty and Self-Government* (1853) were adopted as textbooks in the mushrooming politics courses at U.S. universities, as was one generation later *Political Science and Constitutional Law* (1890) by John W. Burgess, who inherited Lieber's chair at Columbia and founded the first graduate training in political studies in the form of the Columbia School of Political Science (1880). Aside from creating a library and organizing the Academy of Political Sciences, The School of Political Science at Columbia also published its own journal—the *Political Science Quarterly*—since 1886, as did the American Academy of Political and Social Science (established in 1889) with its *Annals*, and soon after the creation of the American Political Science Association (APSA) in 1903 an *American Political Science Review* appeared (Gunnell, 1990; Farr, 1991; Somit and Tanenhaus, [1967] 1982; Stein, 1995; Seidelman, [1985] 2015; Gunnell, 2006).

Yet still in the 1930s many practitioners of the discipline lamented the stagnation of what Collini, Winch, and Burrow (1983), following Cockburn ([1856] 1910), called the "noble science" of politics. Speaking of the "nebulous province" of early-twentieth-century political science (especially British), Collini, Winch, and Burrow write:

> The absence of any distinctive and agreed-upon 'science' was certainly evident in the familiar heterogeneity of the syllabuses, a mixture usually made up of classical political philosophy and the history of political theory on the one hand, British constitutional history and a selective taxonomy of political institutions on the other. (Collini, Winch, and Burrow, 1983; 375)

By now stripped of much of its traditional scope by the successful institutionalization of the other social sciences, British political science had to abandon its pretension of constituting a comprehensive science of associated life and undertook a readjustment of its boundaries, yet it generally remained "a kind of intellectual refugee-camp" for those who did not easily fit in other areas of social scientific expertise (Collini, Winch, and Burrow, 1983; 374). Still at the end of the 1940s, only four British journals of political science existed—*International Affairs*

(started in 1922), *Public Administration* (started in 1923), the *Political Quarterly* (started in 1930), and *Parliamentary Affairs* (started in 1947)—and when, in 1949, the UNESCO Conference proposed that an International Political Science Association be formed, a British Political Science Association had yet to be created (Chester, 1975; see also Stein, 1995).

Despite having garnered earlier institutional legitimacy, the status of political science in the United States at the end of the nineteenth century was still essentially contested, as was its disciplinary autonomy. At that time, the influence of the German *Staatswissenschaften* ("sciences of the state") was still predominant on the pioneering work of first-generation political scientists. Francis Lieber (born Franz Lieber) was a Berlin-educated, American-naturalized Prussian. Although born in America, John Burgess was also trained in Germany, and so were other founding figures of U.S. political science like Theodore Woolsey, Andrew D. White (the founder of Cornell University and its president at the time the College of History and Political Science was created), Herbert Adams (who created the John Hopkins University Studies in Historical and Political Science), and Julius Seelye (Adams and Burgess's tutor; see Farr, 1991; Crick, [1959] 1998; Gunnell, 1990). Indeed, Herbert Adams described the first political science association in the United States, the Hopkins Historical and Political Science Association (1877), as "a kind of *Staatswissenschaftlicher Verein*, or Political Science Union like that in Heidelberg University" (Adams, 1887; 194, italics added).

Drawing on the academic organization of political science curricula at German universities, Lieber saw political science as a systematic and abstract activity, to be kept separate from practical politics and history. Accordingly, he identified the organizing concept of political science in the notion of "state", which he saw as a theoretical abstraction quite distinct from the government and the legislative. As Gunnell (1990) points out, the vast adoption of the German academic model certainly spurred the professionalization of U.S. political science, and the identification of the state as the central focus of political studies met the quest for autonomy on the part of political science. Under the influence of traditional *Staatslehre* ("doctrine of the state"), Lieber—like Woolsey, White, and Burgess—came to see the state in a radically different way than contract theory would prescribe, namely, as the ultimate source of rights and the crowning accomplishment of the historical development of the political community (Gunnell, 1990). Such abstract and metaphysically-infused notion of "state" adapted from the German idealistic tradition could match "the search for a community that stood behind an expanding and sometimes intrusive government and the conflictual pluralism of region and interest seemed necessary to give substance to the idea of democracy" (Gunnell, 1990; 156).

Because of its vague and open meaning, however, this idea of state did not serve to pinpoint the object of political science once and for all, and "the search for what it represented continued in other forms" (Gunnell, 1990; 157). With the so-called "behavioural revolution" of the post-WWII period, focus on the state came to be viewed as an unwarranted conceptual limitation to the scientific study of politics, which could be best accounted for by referring to its "function" or through a "systemic" approach to the political sphere. In introducing his 1960 landmark comparative study on the "developing areas", Almond programmatically

wrote:

> [The] rejection of the "state and non-state" classification, which is found throughout the anthropological, sociological, and political science literature is not merely a verbal quibble. It is a matter of theoretical and operational importance. Such a dichotomous classification could come only from an approach to politics which identifies the political with the existence of a specialized, visible structure, and which tends to restrict the political process to those functions performed by the specialized structure … The articulative, aggregative, and communicative functions may be performed diffusely within the society, or intermittently through the kinship or lineage structure. An adequate analysis of a political system must locate and characterize all of these functions, and not simply those performed by the specialized political structure …The rule to follow which we suggest here is: If the functions are there, then the structures must be, even though we may find them tucked away, so to speak, in nooks and crannies of other social systems. (Almond, [1960] 2015; 12)

In his seminal book *The Political System* (1953), David Easton had suggested that the word "state" "should be abandoned entirely", and that if the notion is scrupolously avoided, "no severe hardship in expression will result. In fact", he argued, "clarity of expression demands this abstinence" (Easton, 1953; 108). As a substitute, Easton famously introduced his notion of a "political system" which he identifies as "a set of interactions, abstracted from the totality of social behaviour, through which values are authoritatively allocated for a society" (Easton, 1965; 57). Not too dissimilarly, Dahl (1976; 10) wrote that "A political system is any persistent pattern of human relationships that involve to a significant extent, control, influence, power, or authority". Crucially, the notion of the political system thus understood is used precisely to characterize the scope of political science. In the same way as Almond characterized political structures as "political systems", "What distinguishes political interactions from all other kinds of social interactions", writes Easton, "is that they are predominantly oriented toward the authoritative allocation of values for a society" (Easton, 1965; 50).

Unsurprisingly, this redefinition of the object of political science was premissed upon the general conception of the science of politics prescribed by the behavioural model. As Almond and Genco (1977) characterized it:

> The … "behavioral" tradition in political science tends to rest on three epistemological and methodological assumptions which it has taken from the hard sciences: (1) that the purpose of science is the discovery of regularities in, and ultimately laws of, social and political processes; (2) that scientific explanation means the deductive subsumption of individual events under "covering laws"; and (3) that the only scientifically relevant relationships between events in the world are those which correspond to a physicalistic conception of causal connections. These assumptions are highly interrelated, and each carries important substantive implications for the study of politics. (Almond and Genco, 1977; 497–498)

So, for instance, Easton argued in *The Political System* (1953) that "knowledge becomes critical and reliable when it increases in generality and internally consistent organization, when, in short, it is cast in the form of systematic generalized statements applicable to large numbers of particular cases" (Easton, 1953; 55). Similarly, Robert Dahl claimed that "[i]f we wish to explain an event, E, in a strictly causal manner, we consider E as an effect and bring it under some generalization of the form: 'Every event C is accompanied later by an event E'" (Dahl, 1965; 87). Tellingly, what Dahl identified as the features of the political

system—control, influence, power, and authority—are for him amenable, *qua* behavioural relations, to scientific investigation in terms of causal analysis:

> when we single out influence from all other aspects of human interaction in order to give it special attention, what interests us and what we focus attention on is that one or more of the persons in this interaction get what they want, or at least get closer to what they want, by causing other people to act in some particular way. We want to call attention to a *causal relationship* between what *A* wants and what *B* does. (Dahl, 1976; 30, emphasis in the original)

For P. A. Hall (2003), the emergence of the concept of the "political system" and the view of the political world as governed by laws that establish causal relations constitute two "ontological shifts" that jointly encouraged the methodological innovations that began with the "comparative revolution" of the 1950s and 1960s and continued to exert an enduring influence on the development of political science. By attaching an overwhelming importance to the *function* of political systems irrespective of the kinds of institutions in which the system is instantiated—states or non-states—the search for law-like regularities could be best performed through a comparative inquiry that sought to identify stable causal patterns across different national contexts, while at the same time increasing the general knowledge of the characters of the political system itself.

At a deeper level, one might see how the idea of a political system and the covering-law model of political explanation are in fact interrelated. As Sartori (1970) remarked, an unresolved problem of the structural-functional approach is that the category of "structure" largely collapses into that of "function". Despite Almond's claim that "What we have done is to separate political function from political structure" (Almond, [1960] 2015; 59), Sartori argues, the notion of structure is seldom adequately defined in its own terms—i.e. *qua* structure; on the contrary, structures typically bear a functional definition. So, for instance, the political system is for Easton a means—a structure—*for* the authoritative allocation of values, while for Dahl it is defined by its function of exerting control, influence, power, or authority. For Sartori, this functional definition of the central political concepts means that these are applicable based on the presence or absence of a given property, as per the logic of classification building. Scientific taxonomies, he argues, follow an either-or type of logic by constructing class concepts that represent characteristics which the objects must either have or lack. Understood as defining the scope of political science, the concept of the political system can be seen precisely as such a class concept, or a meta-concept that encompasses all objects that bear similar properties—or more precisely, functions—so as to qualify as *political*.

What Sartori calls *conceptual stretching*—the increasing vagueness and amorphousness of class concepts with indefinite extensional coverage, the ultimate instance of which cannot be but the overarching concept of the "political system" as co-extensive with the scope of political science—is required precisely to accommodate the comparative expansion of political science as a discipline. Comparative inquiry, he argues, ultimately needs "'universal' categories—concepts that are applicable to any time and space" (Sartori, 1970; 1035), and therefore their meaning must be just as broad. Functionally defined concepts, then, provide

sufficiently universal categories that underpin comparative research. Taking one step back, however, one may ask why it is necessary or at least desirable to compare at all. Revealingly, in an influential article Arend Lijphart defined the comparative method as "a *method of discovering empirical relationships among variables*" (Lijphart, 1971; 683) and described its logic as corresponding to the general standard of the experimental method expounded by Ernst Nagel, i.e. a method of discovering empirical correlations between variables in order to arrive at general laws (Nagel, 1961; 452; Lijphart, 1971; 684). For Lijphart, "It was the promise of discovering universal laws through global and longitudinal comparisons that made Edward A. Freeman enthusiastically espouse the comparative method" in his 1873 *Comparative Politics* (Freeman, 1873; Lijphart, 1971; 686). Similarly, in agreeing with Lijphart Sartori concludes that the distinctiveness of comparative politics consists "of a systematic testing, against as many cases as possible, of sets of hypotheses, generalizations and laws of the 'if … then' type" (Sartori, 1970; 1035).

Conclusion

The structural-functionalist definition of "the political system" thus rests necessarily on a law-based understanding of political explanation which requires and indeed is exhausted by the search for stable correlations. More generally, the demarcation of the scope of political science and the subsequent definition of its object of investigation in methodological terms rests upon a foundationalist view of science which underpins comparative methodology understood as the search for invariant regularities. It is from this foundationalist standpoint that the scope of science could be seen as the product of the methodological differentiation between the different scientific disciplines: by assuming logical criteria of justification as the demarcation line between science and non-science, traditional philosophy of science focused solely on the context of justification as instantiated in the methodology of science. In this way, methodology could be seen as the very site of demarcation between science and non-science, and therefore as the distinguishing feature of a science. It is on the basis of this assumption that the boundaries of scientific disciplines—i.e. their scope—could be defined in terms of the scientific objects as seen through purely methodological lenses. Given its demarcationist and foundationalist presuppositions, the view that scientific objects are defined by the methodology of science—which provides the context of justification—can only be defended as long as the underlying image of science is accepted.

The next chapters will show how criticism to covering laws in social explanation impinges substantially on the issue of scope of science. The idea that causal relations can be fully accounted for in terms of relations of logical dependence from law-like regularities and that causal explanation can be defined as a logical deductive argument rests on the foundationalist philosophy of science of the logical empiricists, according to which the nature of science is entirely defined on the basis philosophical—more specifically, logical—criteria. Chapter 2 will consider mechanism-based criticism to covering laws in terms of a realist criticism to the causal eliminativism and logical foundationalism implicit in the covering-law model of

explanation. While the explanatory power of covering laws is defended on a non-causal, logical basis, mechanism-based critics of covering law point to a causal-realist view according to which causal explanation is about detailing the causal process that actually occur in the world.

Once such eliminativist reductionism about causation is rejected in favour of causal realism, a different account of scientific objects is required. For such realism entails a commitment to the existence of mind-independent causal processes, and therefore it assumes that the role of scientific explanation is to provide true theories about such causal processes. Chapter 3 will thus proceed to examine the scientific presuppositions of causal realism. The idea that causal relations are real-world causal processes to be reflected in scientific explanation will be linked to a productive view of causation, according to which the social world be ontologically defined in terms of causal powers. The metaphysics of science that underwrites causal realism, it will be argued, is one that sees causal processes as contextually produced by the particular properties of individual entities.

A definition of the object of politics in realist terms must thus depend on the causal properties of entities that generate what are described as political phenomena. The demarcation of the scope of political science depends on the classification of causal properties into political and non-political ones. In chapter 4, such classification will be presented as the result of a stipulation. Because phenomena are individually generated by the causal properties of particular social entities, which of them count as political must depend on the recognition of "family resemblances" between different properties. Such family resemblances, however, are only established by the inquirers *a posteriori*, and therefore different stipulations about what family resemblances are politically relevant can lead to very different conceptualizations of politics as a scientific objects.

Chapter 5 will address this problem by considering the definition of scientific objects that result from the classifications of particular causal properties as representational models. Like all scientific models, representational models are neither true nor false; in fact, they are all equally valid to the extent that they all represent reality in some way. Like the experimental models of science, however, representational models can be judged on the basis of the models of reality they assume. From a realist perspective, definitions of the object of politics that result from a covering-law understanding of political processes can thus discarded because of the inadequacy of the explanatory model of causation they depend on.

Chapter 2

Productive causation and political explanation

> It may seem extraordinary that anyone should ever have held the view that there is definitely no "because" in nature—that there is definitely nothing about the world in virtue of which it is regular. It is … one of the most baroque metaphysical suggestions ever put forward—principally by people who pride themselves on dispensing with metaphysical extravagance.
>
> Galen Strawson (1989)

Introduction

This chapter will consider the enduring influence of the positivistic image of science in political explanation as well as criticism to it. The positivistic foundationalism that justified the adoption of a covering-law approach to political explanation, in particular, will be shown to be the ultimate target of the long-lasting debates on political methodology that have accompanied the development of the discipline. Critics of the use of covering laws in political explanation as well as in social explanation broadly understood typically defend what has come to be known as "the mechanism approach". Social explanation, the argument goes, requires detailing the "causal mechanism" that produced the phenomenon to be explained rather than deductively subsuming it under covering laws.

This contention in turn presupposes that all explanation is causal. As Jon Elster (2007) put it,

> The basic type of explanandum is an *event*. To explain it is to give an account of why it happened, by citing an *earlier event* as its cause. Thus we may explain Ronald Reagan's victory in the 1980 presidential elections by Jimmy Carter's failed attempt to rescue the Americans held hostage in Iran. Or we might explain the outbreak of World War II by citing any number of earlier events,

> from the Munich agreement to the signing of the Versailles treaty. (Elster, 2007; 3, emphasis in the original)

Although some authors have openly equated explanation and *causal* explanation (Salmon, 1984; Lewis, 1986b), most scholars now tend to leave open the possibility of non-causal explanation, if only by not excluding it explicitly. In this latter case, however, it is not entirely clear what a suitable candidate for a non-causal explanation would be (Skow, 2014). Therefore, throughout this work I will simply assume the (realist) equation between explanation and causal explanation.

What appears to be a purely methodological debate between different models of explanation—the covering-law model and the mechanism-based model—will in fact be shown to be a metaphysical debate about the nature of social causation. The explanatory power of covering laws can only derive from an eliminativist view about causation according to which the regularities which those laws describe are seen as more fundamental than causal relations themselves. It is on the basis of such causal eliminativism that regularities can constitute the very focus of scientific explanation. On what is often referred to as "the Humean view of causation", causal links are nothing but shorthand for regular co-occurrences of otherwise unrelated events: it is because of such eliminativism about causal links that the validity of scientific explanation can only derive from the empirical confirmation of such regularities and on the deductions they accordingly yield. The logically foundationalist approach to science that supports the covering-law model of explanation thus rests in its turn on an anti-realist metaphysics of causation.

Covering-law explanations will thus be criticized as unwarranted generalizations of particularistic causal claims. According to the above view of *causal explanation*, generalizations of causal claims have no explanatory value per se. In fact, I take it that causal generalizations are usually valued because of an implicit commitment to an anti-realist empiricism according to which causal claims are heuristic devices rather than real-world processes. In contrast, the use of mechanisms for political explanation and social explanation in general are often advocated on the basis of a realist view of causation. The explanatory power of social mechanisms, in particular, is defended comparatively to covering laws in that mechanisms are deemed capable to "open up the black-box" between the relata of causal laws and show the causal "cogs and wheels" that make up causal processes. Mechanism-based explanations are thus considered to be true descriptions of real-world causal processes, while the phenomena to be explained are considered to be the outcomes of genuinely productive and metaphysically real causal processes.

Despite the unquestionable theoretical influence of the covering-law model of explanation, the idea of explanatory mechanisms is as old as the idea of scientific laws, both of which originated during the Scientific Revolution of the 17th century. But while explanation in the social and political sciences have often been presented in law-like terms in accordance with the precepts of the positivistic image of science, the actual explanatory relevance of laws in empirical analysis is far from indisputable. In fact, the idea that scientific explanation amounts to inferring particular phenomena from general causal laws arguably played no

serious role in the social sciences despite having been the dominant view in philosophy for decades. Explanation of statistical associations have nearly always been provided in terms of causal mechanisms. Causal realism thus seems to be a tacit assumption of much social and political science in the form of mechanism-based explanation. Causal explanation, the argument goes, must necessarily rest on the assumption that causal mechanisms are real processes, and that mechanism-based explanation is capable of capturing them.

Since the main goal of science is to provide causal explanations of particular events, one can take political science to be a set of statements that provide answers to why-questions, i.e. a set of causal statements. Coupled with a realist view on the nature of causal relations, this assumption posits the reconstruction of those causal relations as the main goal of political science, to which other goals are subordinate. On the variant of causal realism consider here, for instance, causal relations are considered to be the materialization of the particular properties of individual entities. It is in order to explore the conception of scientific objects that derives from this perspective that the latter part of this chapter will detail the metaphysical implications of the realist approach to causation. The reality of causal processes, it will be argued, implies the notion that the phenomena at stake in scientific explanation have been actively generated by the productive activities that make up the social world. Causal realism, therefore, is conducive to a productive view of causation according to which causal activities are primitive relations that cannot be further reduced to logical relations of inferential dependence. This will provide the guidelines for a social ontology of scientific objects to be outlined in the next chapter.

1. Political science and covering laws

As P. A. Hall (2003) pointed out, the covering-law view of the political system that arose with the comparative revolution continued to exert a heavy influence in the discipline of political science. Writing in 2001, the late Charles Tilly wrote that "approved political science doctrine generally favors some combination of propensity and covering law explanations" (Tilly, 2001; 25). Citing studies on regime transitions as a case in point, Tilly remarked that in the existing political science literature democratization processes are often accounted for by singling out the necessary and sufficient conditions for those processes to occur, or by invoking well-established empirical correlations between variables such as e.g. the creation of representative institutions and the democratization of a polity. Within this framework, political outcomes are explained by reconstructing the intentional states of relevant agents (i.e. their "propensity" to perform a certain action) within the process under examination, and by subsuming the actions determined by such propensity under a "covering law" concerning human behaviour.

The notion of a "covering-law explanation" was first introduced by Dray (1957; see Hempel, [1963] 2001), even though an earlier sketch of this explanatory model can be found in Karl Popper's early work (see e.g. Popper, [1934] 2002; 38). Its textbook version, however, is most often associated with Carl G. Hempel's "deductive-nomological" model. Drawing

on the logical-empiricist image of science as based in large part on deductive inferences from true universal generalizations, Hempel set forth a formal model of explanation in which the *explanandum*—i.e. what is to be explained—features as an individual statement logically deducible from the *explanans*—the explanatory theory, formed by a logical conjunction of individual statements specifying some initial conditions and some universal statement(s) in a law-like form, whence the name "deductive-nomological". Following this idea, a physical phenomenon like a swift rise in the mercury column of a thermometer rapidly immersed in hot water is explained by deduction from some initial conditions—such as the fact that the thermometer consists of a glass tube filled with mercury and that it is immersed into hot water—together with some law-like statement about the thermic expansion of mercury (Hempel and Oppenheim, 1948).

In a very much logical-empiricist vein, this model is intended to provide a rational (and logical) reconstruction of actual scientific practice, assuming the natural sciences—and the physical sciences in particular—as the ideal of science. Interestingly, however, Hempel first presented his covering-law model with reference to historical explanation. Through it, Hempel intended to show that general laws have quite analogous functions in history and in the natural sciences, that they form an indispensable instrument of historical research, and that they even constitute the common basis of various procedures which are often considered as characteristic of the social in contradistinction to the natural sciences (Hempel, 1942; 35). To exemplify this point he considers the migration of farmers from the Great Plains to California during the Great Depression. Not unlike physical phenomena, he argues, particular historical events of this kind are explained by specifying some initial conditions—e.g. continual drought and sandstorms rendering the farmers' existence increasingly precarious, California offering better living conditions than the Great Plains—and by subsuming them under some "covering law" concerning human behaviour, such as the universal hypothesis that populations tend to migrate to regions offering better living conditions. This model, for Hempel, can be extended rather straightforwardly to explain revolutions as well as to "historical explanations in terms of class struggle, economic or geographic conditions, vested interests of certain groups, tendency to conspicuous consumption", and indeed to virtually all social explanations (Hempel, 1942; 42).

While in the late 1950s Hempel's deductive-nomological model began to lose momentum in the face of devastating philosophical criticism (Salmon, 1989), and although the consensus around the covering-law model of explanation seems to have had little impact on other areas of empirical social research (Hedström and Bearman, 2009), many authors identified the main stream in political science methodology as tied to a covering-law account of explanation (see e.g. Moon, 1975). According to Tilly (2001), political science manuals, courses, and presidential addresses tend to endorse a model of explanation that is in large part based on covering laws. For him, this approach is epitomized in traditional studies on regime transitions insofar as they provide explanatory theories centred on the democratizing action of crucial agents (most typically the ruling elite, but also collective actors such as unionized workers) and on the overarching assumption that such action conforms to some

law that makes it a necessary and sufficient condition for transition to democracy to occur. Indeed, in a seminal book-length study on contentious politics jointly authored with fellow social movements scholars Doug McAdam and Sidney Tarrow, Tilly detected covering-law explanations in vast areas of empirical political research, including not only the rational-actor analyses predominant in the fields of industrial conflict and electoral politics, but also, and more broadly, what the authors identify as the classic agenda of North American and European social movement scholarship (McAdam, Tarrow, and Tilly, 2001).

By the 1980s, McAdam, Tarrow, and Tilly argue, most social movement investigators came to pursue a unified explanatory schema that, aside from the varying degrees of emphasis placed on its different components, referred to some invariant factors that jointly characterize the emergence of contentious episodes—typically: *social change* (however defined); the presence of *mobilizing structures* (such as existing social movement organizations or the less institutionalized networks of everyday life) and political *opportunities*; *repertoires* of political means for contention; and the *frames* that orient participation in collective action. For McAdam, Tarrow, and Tilly, however, compressing the complexity of the different episodes of contention under investigation into these pre-ordered boxes inevitably led social movement scholars to single out the relevant actors in each of the contexts expressed by the boxes, in the same way as traditional propensity-based approaches to political explanation focused on the actions of key social agents in order to nomologically explain the dispositions that allegedly resulted in those actions. To show this, McAdam, Tarrow, and Tilly focus on what they believe to be a paradigmatic case, namely, traditional accounts of the rise of the U.S. Civil Rights Movement of the 1950s–1960s. Classic scholarship, they argue, typically framed this phenomenon against the background of large *societal changes*—the collapse of the cotton economy and the resulting migratory flows—that triggered the other factors in the scheme: migratory flows to the North increased the importance of the "black vote", opening up *political opportunities* for African Americans, while migration from rural to urban areas favoured the development of black churches, black colleges, and civil rights organizations as *mobilizing structures*. This resulted in a different and more politically-oriented *framing* of long-existing grievances, such as those against segregation on city buses, and in a broadening of the *repertoire of contention* to marches, sit-ins, and other forms of protest.

For McAdam, Tarrow, and Tilly, however, in focusing exclusively on such single, invariant pattern, the classic social movement agenda obliterated the causal pathways that led, for instance, Montgomery protesters to attribute an opportunity to the bus boycott and movement leaders to appropriate the formerly conservative black church, both engaging in an interpretive framing effort that extended to the adoption of innovative collective action. By leaving unspecified the inner causal workings of the process at stake in favour of subsuming it under a pre-ordered schema, that is, the classic social movement agenda presupposed a model of explanation that is as unsatisfactory as is subsuming political phenomena under general laws, and for the same reasons: in both instances, causal relationships are black-boxed into stable links connecting the variables of an invariant model rather than explained.

Criticism of covering laws of the kind advanced by McAdam, Tarrow, and Tilly underlies

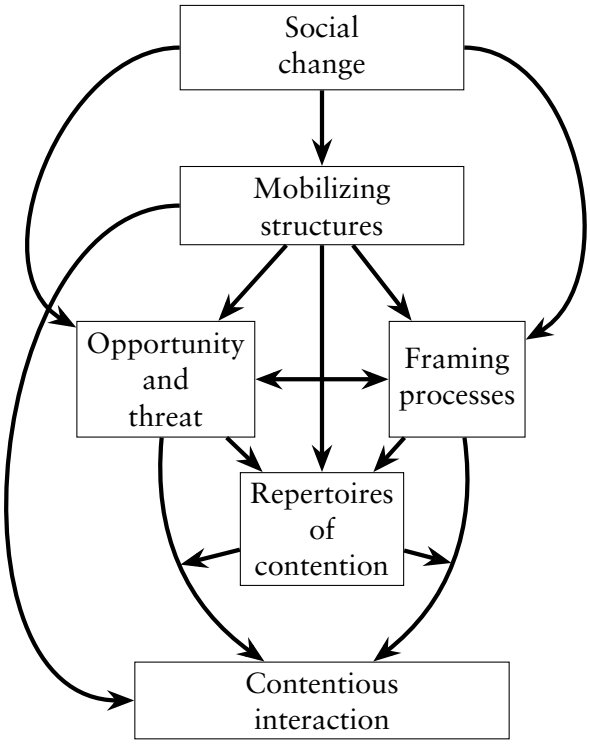

Figure 2.1: The Classic Social Movement Agenda for Explaining Contentious Politics, as presented in McAdam, Tarrow, and Tilly (2001; 17).

much recent methodological debate in the social sciences, especially the debate on social mechanisms. More specifically, it will be shown, those who advocate the use of mechanisms instead of covering laws for the purpose of social explanation typically question the adequacy of covering laws for *causal explanation* altogether. Indeed, some point out that empirical phenomena can hardly be explained in a law-like form, and that aside from theoretical advocacy of the logical-empiricist model of deductive science epitomized in Hempel's deductive-nomological scheme, mechanism-based explanation has been a staple in social science for decades (albeit perhaps in a covert form, see Hedström and Bearman, 2009; 5, see also f. 3; Bechtel, 2006; 3–4). Supposedly, such presumed explanatory inadequacy is a consequence of the anti-realism about causation built in into the covering-law approach. By defining explanation entirely in terms of logical dependence, as per Hempel's deductive-nomological model, advocates of the covering-law approach identify explanatory power with the capacity to yield logical deductions. In this way, causal explanation altogether evaporates in logical deduction, and causation itself ceases to be an irreducible feature of the (social) world to become analyzable in terms of more primitive relations of logical dependence. Such reductionism about causation, it will be argued, is the implicit target of criticism of covering laws. Advocates of the mechanism-based approach typically defend the greater explanatory power of mechanisms vis-à-vis laws by virtue of their capacity to unravel the actual, real-world causal processes that bring about the phenomena at stake in scientific explanation.

2. Political mechanisms

Critics of the covering-law approach to social explanation typically see it as unsatisfactory in that law-like links between events obliterate rather than detail causal relations. In criticizing the "classic social movements agenda", for instance, McAdam, Tarrow, and Tilly (2001) contend that the pursuit of a covering-law explanatory model led to the formulation of a coarse-grained and highly static explanatory scheme, which obfuscated the dynamical complexity of single episodes of contention by evaporating specific causal pathways into purportedly universal causal links. For this reason, they advocate replacing covering-law accounts of political processes with mechanism-based ones. In their understanding, the mechanism-based approach consists precisely of placing "under microscopes" the law-like links that featured in the traditional explanatory agenda in order to "observe what goes on inside them" (McAdam, Tarrow, and Tilly, 2001; 189). Rejecting the view that explanations should proceed by subjecting whole categories of phenomena to general models, they argue that contentious episodes are best analyzed by locating recurrent *mechanisms* within them, which they define as follows:

> *Mechanisms* are a delimited class of events that alter relations among specified sets of elements in identical or closely similar ways over a variety of situations. (McAdam, Tarrow, and Tilly, 2001; 24, emphasis in the original)

Accordingly, they propose a different account of the outbreak of contentious episodes within the U.S. Civil Rights struggle. Mechanisms of *competition for power*, McAdam, Tarrow, and Tilly argue, were active when the moderate leadership of the civil rights movement was being contested by the more radical currents of the movement, resulting in a further radicalization of the latter. Mechanisms of *diffusion* of forms of contention can be found in the spread of "sit-ins" and "freedom marches" across geographically, organizationally, and ideologically different civil right groups. Mechanisms of *repression* operated in the broader civil rights movement's marginalization of ghetto militants, who organized into paramilitary groups such as the Black Panthers. Finally, mechanisms of *radicalization* account for the younger civil right activists' search for new themes and new forms of action to revivify the movement after the exhaustion of the equal opportunity agenda. It is the combination of such mechanisms (together with other, more context-specific ones) and their coalescence into a process of polarization of the polity that lie at the basis of contentious episodes such as the Montgomery bus boycott.

McAdam, Tarrow, and Tilly's definition of mechanisms in terms of classes of "events" is not unproblematic, especially since they contrast mechanisms with *processes* and *episodes*. In their view, *processes* are "regular sequences of … mechanisms that produce similar (generally more complex and contingent) transformations" of the elements of mechanisms, while *episodes* are "continuous streams of contention including collective claims making that bears on other parties' interests" (McAdam, Tarrow, and Tilly, 2001; 24). Yet this tripartite distinction seems to be arbitrary, and deliberately so, since its proponents state that "Mechanisms and processes form a continuum. It is arbitrary, for example, whether we call

brokerage a mechanism, a family of mechanisms, or a process. In this book, we generally call it a mechanism to emphasize its recurring features" (McAdam, Tarrow, and Tilly, 2001; 27). Episodes, in turn, are somewhat counter-intuitively described as entanglements of two or more processes (McAdam, Tarrow, and Tilly, 2001; 28), but the introduction of this nested taxonomy of political phenomena leaves open the question as to what are the ultimate *explananda* of political mechanisms—and indeed, whether political mechanisms presuppose any specific *explanandum* at all.

Yet McAdam, Tarrow, and Tilly are by no means the only authors advocating mechanism-based explanations of political phenomena, nor are they the only ones to contrast covering-law explanations with mechanism-based ones. Studies on democratization, as well as historical inquiries on macropolitical outcomes such as revolutions, modernization, regime change, and the like, make extensive use of mechanism-based explanations in accounting for large-scale historical processes (P. A. Hall, 2003; Falleti and Lynch, 2009). Paul Pierson (2003) argued for instance that the gradual *replacement* of political generations is the principal causal mechanism involved in slow-moving, cumulative political outcomes such as the changing fortunes of political currents within the U.S. Congress. Thelen (2003) identifies cases in which gradual but nevertheless transformative institutional change (including transition to democracy in Eastern Europe) occurs through what Schickler (2001; 15, emphasis added) described as a "tense *layering* of new arrangements on top of preexisting structures"—a mechanism through which "new coalitions may design novel institutional arrangements but lack the support, or perhaps the inclination, to replace preexisting institutions established to pursue other ends". And so on.

At a more abstract level, Mahoney (2001) identified twenty-four different definitions of "mechanism" as put forward by various authors—including social and political scientists. Aside from McAdam, Tarrow, and Tilly, even authors otherwise very unsympathetic to the epistemological underpinnings of mechanism-based explanation like King, Keohane, and Verba (1994; 85) conceded that it "makes intuitive sense" to account for causation in terms of mechanisms linking causes and effects. In fact, many commentators pointed to the lack of consensus on an unequivocal definition that could serve as a shared explanatory paradigm (Mahoney, 2001; Hedström and Bearman, 2009; Hedström, 2005; Hedström and Ylikoski, 2010). McAdam, Tarrow, and Tilly, for instance, emphasize that mechanisms operate at different levels to bring about complex outcomes: in the civil rights movement example, *environmental mechanisms* responsible for urbanization and *relational mechanisms* such as the civil rights activists' brokerage of aggrieved people combine with *cognitive mechanisms* (e.g. the attribution of an opportunity for voicing grievances to the injustice of bus segregation) into a process that led to the bus boycott in Montgomery. Not dissimilarly, Mayntz (2004; 250–251) stresses that relational mechanisms are irreducible to individual-level ones and nevertheless indispensable to political explanations in that "relational constellations that may, but need not, be institutionally based are integral parts of the processes generating social macro-phenomena". A political science example of this would be Ostrom's ([1990] 2015, 1999) work on common pool resource problems, the occurrence and solution of which

Ostrom ascribes to institutional rules and to structural properties of social groups more than to the individual behaviour of group members. On the other hand, Hedström and Swedberg (1998; 11-12) contend that mechanism-based explanations in the social sciences necessarily invoke causal agents of some form which cannot be but individual actors; as a consequence, "intelligible social science explanations should always include references to the causes and consequences of their actions". Others (Schelling, 1998; Cowen, 1998; Gambetta, 1998) tie the very idea of a mechanisms to the decision-making processes constantly undertaken by social actors, thus subscribing to a principle of methodological individualism similar to that invoked by Hedström and Swedberg.

Yet other crucial differences divide the camp of mechanism advocates. Followers of Hedström and Swedberg's account of mechanism-based explanation typically contrast it with the deductive-nomological model of explanation, while early defenders of the mechanism approach seem to adopt the jargon—if not the conceptual apparatus—of the covering-law approach. Little (1991; 15), for instance, defines mechanisms as "series of events governed by law-like regularities that lead from the explanans to the explanandum", while Glennan (1996; 52) writes that "A mechanism underlying a behavior is a complex system which produces that behavior by of the interaction of a number of parts according to direct causal laws."

In their extensive review of mechanism approaches in philosophy and in the social sciences, Hedström and Ylikoski (2010) tried to impose some order on this diversity by listing the features shared by most definitions of "mechanism", namely, that mechanisms are *effect-specific*, *causal*, *structured*, and *hierarchically nested*. Virtually all of these characteristics, however, are contradicted by some well-established theory about mechanisms. The idea that mechanisms are identified by the kind of effect they produce, for instance, is seemingly at odds with a widely praised and supposed advantage of mechanism-based explanation, namely, their (semi-)generality. Hedström and Bearman, for instance, favourably cite Merton's famous characterization of *self-fulfilling prophecies* as a quintessential epitome of a mechanism on grounds that it is capable of explaining phenomena as diverse as the placebo effect and bank runs (see also Biggs, 2009). And while Hedström and Bearman carefully specify that these phenomena are all of the same *type* in that they exhibit the same structure linking the types of entities—irrespective of the particular actors, beliefs, or actions that occur in the concrete phenomena—it is not clear whether such a degree of generality still allows to talk of effects as a defining feature of mechanisms in any meaningful sense.

As Hedström and Ylikoski (2010) acknowledge, even the condition of causality has been violated by a pioneer in the social mechanism approach such as Jon Elster. Although in his earlier work Elster openly subscribes to Donald Davidson's view that intentional states such as beliefs and desires can be bona fide causes of actions (Elster, 1983; 20–24, emphasis added), his early definition of "mechanism" as "a continuous and contiguous chain of *causal or intentional* links" seems to allow for non-causal interpretations of mechanisms.

The idea that mechanisms have a structure is actually encapsulated in many arguments making reference to the metaphor of "black-box" explanations. The :osition between non-

explanatory "black-boxism" and the explanatory conversion of black boxes into "translucid boxes" through the disclosure of the mechanisms that make up its internal structure is typically associated with the thought of Mario Bunge (for an early statement of this theory see Bunge, 1963). In Hedström and Ylikoski's understanding, this opposition can be translated into one between variables and components of actual causal processes:

> When a mechanism-based explanation opens the black box, it discloses [its] structure. It turns the black box into a transparent box and makes visible how the participating entities and their properties, activities, and relations produce the effect of interest. For this reason, the suggestion (e.g., Opp, 2005) that a mechanism is just an intervening variable misses an important point. The focus on mechanisms breaks up the original explanation-seeking why question into a series of smaller questions about the causal process: What are the participating entities, and what are their relevant properties? How are the interactions of these entities organized (both spatially and temporally)? What factors could prevent or modify the outcome? And so on. (Hedström and Ylikoski, 2010; 51–52)

However, Mahoney (2001; 578) points out how a large family of definitions of mechanism understand this notion precisely as "an intervening variable or set of intervening variables that explain why a correlation exists between an independent and dependent variable", citing seven references in support of this claim which interestingly include also the seminal work that Hedström himself co-authored with Swedberg.

Finally, the claim that mechanisms form a hierarchy is justified by Hedström and Ylikoski in the light of an epistemologically reductionist position—albeit a weak one: "While a mechanism at one level presupposes or takes for granted the existence of certain entities with characteristic properties and activities", they argue, "it is expected that there are lower-level mechanisms that explain them". Yet it is not entirely clear how "lower-level" mechanisms can explain causally the properties and activities of entities that figure in "higher-level" ones. Elsewhere, Ylikoski (2013) has drawn on Salmon's (1984) distinction between "etiological" (i.e. causal) and constitutive explanation and argued that mechanism-based explanations of the (causal) capacities of entities are of the latter kind. However, the application of this distinction to the levels of mechanism-based analysis is problematic, since it would licence, *contra* Hedström and Ylikoski, a non-causal interpretation of (mechanism-based) explanation.

3. Mechanisms versus covering laws?

Admittedly, Hedström and Ylikoski's review serves a critical rather than descriptive purpose, its stated goal being one of detailing "some general ideas about what good social science is all about" (Hedström and Ylikoski, 2010; 58) as articulated by the self-named "Analytical Sociology" movement within the social sciences (see Hedström and Bearman, 2009; Hedström, 2005). What is perhaps most distinctive about the mechanism approach, at least in the self-understanding of its advocates, is that it can be viewed as a plausible response to a general dissatisfaction with the covering-law model of explanations guiding correlational

and variable-oriented analyses in empirical research.[1] Followers of Hedström and Swedberg's (1998) approach, for instance, draw on the Mertonian tradition of "middle-range" theorizing—later revivified by authors like Boudon (1991)—the main thrust of which they find in Merton's ([1949] 1968; 39) opposition to "the all-inclusive systematic efforts to develop a unified theory that will explain all the observed uniformities of social behavior, social organization and social change". The "middle-range" alternative to such unifying theories of explanation Merton identifies precisely in the task of detecting "social mechanisms", which he defines as "the social processes having designated consequences for designated parts of the social structure" (Merton, [1949] 1968; 44).

More broadly, as the previous section sought to suggest, defenders of mechanism-based explanations in the social and political sciences tend to conceive of them in quite prescriptive terms as a research programme capable of overcoming the flaws of the more well-established methodologies at use in actual social research, which they identify with the covering-law model of explanation. In his review of the volume on *Social Mechanisms* edited by Hedström and Swedberg, Mahoney (2001) reinforces this view by framing recent contributions to the literature in the field of social mechanisms within a broader trend of methodological innovation. Specifically, he draws a parallel between mechanism-based explanations and Ragin's (2000) "fuzzy-set" methodology, though he understands mechanisms in quite specific terms as "posited entities or processes capable of generating certain outcomes". One example of the latter would be the *power relations* postulated by Rueschemeyer et al. (1992), which are hypothetical but nonetheless capable of producing political equality and hence democracy when they are balanced among classes, between state and society, and between states in the international arena. Both the search for causal mechanisms thus understood and the fuzzy-set approach, Mahoney argues, focus on necessary and sufficient causes for given outcomes, favour case-oriented research, and unequivocally stand in sharp contrast to the search for bivariate correlations prescribed in traditional social and political analysis.

Even more ambitious in generality of scope, though more selective in extent of coverage, Little's (2009) review spans two decades of methodological debates within U.S. social science and arrives at similar conclusions regarding the critical relationship between mechanism-based explanations and its methodological antecedents. Little summarizes the ontological insights he draws from these debates as follows:

> doubt about the availability of strong universal laws among social phenomena; attention to the multiple pathways and structural alternatives that exist in large-scale historical development; awareness of the deep heterogeneity of social processes and influences—in time and place; attention to the substantial degree of contingency that exists in historical change; attention to the plasticity of social organizations and institutions; and attempts at bridging between micro- and macro-level social processes. Almost all of these points amount to a fundamental challenge to positivist social science. The skepticism that is now well established about social regularities is a deep blow to

1. Notable exceptions to this widespread attitude include Opp (2005) and King, Keohane, and Verba (1994). Opp (2005; 174–177) argues for instance that Hempel and Oppenheim's deductive-nomological model neither prescribes nor prohibits mechanism-based explanations; therefore, covering laws and mechanisms are mutually compatible. Indeed, for Opp they complement one another: as King, Keohane, and Verba (1994; 75–87) argue, identifying causal mechanisms presupposes rather than provides causal inferences, which can only be established using the variable-oriented methods of correlational analysis.

> the positivist program. The emphasis on causal mechanisms suggests a post-positivist, realist approach to scientific explanation. The point about heterogeneity of social processes suggests the importance of theoretical pluralism rather than unified grand theories of social phenomena. (Little, 2009; 161–162)

Taken together, these reconstructions of the emergence of mechanism-based thinking seem to portray it as a reaction to the near-predominance of law-like explanation in empirical social research under the influence of the positivistic philosophy of logical empiricism. On closer inspection, however, this picture proves to be in need of some adjustments. As Hedström and Bearman (2009; 5) admit, the covering-law model devised by logical empiricism in philosophy "never played any serious role in sociology … Sociological explanation is all about mechanisms and statistical associations and has been so for decades". In support of this claim they cite a sample survey run on the two leading journals in the field of sociology—the *American Journal of Sociology* and the *American Sociological Review*—over the period spanning from their date of establishment (1895 and 1936, respectively) to the turn of the last millennium (2001). Throughout this publication history, only 40 articles prove to refer to "'scientific law(s)', 'social law(s)', 'covering law(s)', and the like" in their titles or abstracts, and only 8 of them were published after 1950. Analogous conclusions can be drawn if one considers the philosophy of the natural sciences, and the philosophy of biology in particular. In the social mechanism narrative, it is rather commonplace to justify the use of mechanisms vis-à-vis law-like regularities by making reference to an epistemological contrast between physical and biological explanation, which are taken to be the natural-science epitomes of the covering-law and the mechanism-based model, respectively. This contrast is based on the idea that social phenomena are inherently different from physical phenomena in a way that makes them hardly amenable to deterministic laws governing them, while biological mechanisms provide a more suitable explanatory model to draw on in explaining social phenomena (see e.g. Hedström and Swedberg, 1998; 2, see also f. 4; McAdam, Tarrow, and Tilly, 2001; 23). However, a search of titles of *Science* articles undertaken over the first 118 years of the journal's history would reveal 656 articles including "mechanism", "mechanisms", and "mechanistic" in their titles—the earliest of which concerning biological phenomena appeared in 1904 (earlier titles referred to psychological mechanisms)—almost four times as many as the articles including "law" or "laws" in their title (165 between 1880 and 1998: Bechtel, 2006; 3–4).

In fact, even though modern-day approaches to mechanism-based explanation in the social sciences developed by and large autonomously from antecedent philosophical debate, it is worth noting that both the idea of a *mechanical philosophy* of nature and that of *laws of nature* emerged during the Scientific Revolution of the 17th century and did not become rival explanatory paradigms until the twentieth century. Descartes, for instance, is alternatively regarded as one of the first philosophers having made use of the concept of "scientific laws" and as a leading figure in the mechanistic philosophy of nature. So is Newton, whose laws are one of the main influences on the logical empiricist movement in philosophy despite the fact that in his work he refers to *forces* as "mechanical principles" and that his

attempt to administer mathematical treatment to physical phenomena aims at supplementing rather than replacing the principles of mechanical philosophy (Boas and Hall, 1959). It was Wesley C. Salmon (1984) who, insisting on some well-known problems besetting the deductive-nomological account, came to the conclusion that deductive laws stood in need of being themselves explained in mechanism-based terms. On the deductive-nomological account, he argued, an observation to the effect of a sharp drop in the reading of a properly functioning barometer licences one to infer not only that there will be a storm, but that it is the barometer drop that causes it. This is because the deductive-nomological model allows no room for causation, which is replaced by logical deduction, and hence provides no criteria for telling causal relations from non-casual ones. After all, Hempel ([1959] 1965; 300) himself went so far as to claim that "causal explanation is a special type of deductive nomological explanation", suggesting that logical deduction is a more fundamental and more comprehensive relation than causation itself. For this reason, Salmon concluded that the only possible solution to the barometer puzzle is to consider deductive laws as in themselves non-causal—the true cause of the rain being the mechanisms of falling atmospheric pressure.

No philosophical urge to replace laws with mechanisms, however, has been perceived even by early proponents of a mechanism-based solution to the barometer problem with Hempel and Oppenheim's model, who were prone to interpret it as a complement to rather than as a replacement for the theory of deductive-nomological explanations (see e.g. Jeffrey, 1969; Railton, 1978). The relationship between mechanisms and covering laws, therefore, does not seem to be one between different, subsequent stages of a cumulative path towards explanatory adequacy. Nor do the mechanism-based and covering-law model amount to incommensurable, self-enclosed and full-blown explanatory paradigms. In fact, the reason why mechanisms are most often opposed to covering laws does not seem to lie in any formal aspect of the mechanism model itself, but rather in its underlying understanding of *causation* and its role in (social) explanation.

In this respect, it is worth noting that while the mechanism approach is, at different degrees, critical of the received view of social explanation, it does not target uniquely covering laws. Recall from the previous chapter that the "behavioural revolution" in the study of politics led to the characterization of the "political system" in terms of "structures" and "functions". While such *structural-functionalism* laid the foundations for a "systemic" approach to politics that allowed for political functions to be subsumed under covering laws, philosophers of science typically regard functional explanations to pursue a *sui generis* explanatory logic that overlaps only partially with that of the covering-law model. G. A. Cohen ([1978] 2000) provided an influential account of functional explanation in his reconstruction of *Karl Marx's Theory of History*. In his 1859 *Preface* to *A Contribution to the Critique of Political Economy*, Marx had written:

> In the social production of their existence, men inevitably enter into definite relations, which are independent of their will, namely relations of production appropriate to a given stage in the development of their material forces of production. The totality of these relations of production constitutes the economic structure of society, the real foundation, on which arises a legal and political superstructure and to which correspond definite forms of social consciousness. (Marx,

[1859] 1987; 263)

Taking this passage as "the clearest statement of the theory of historical materialism" (Cohen, 1983; 112), Cohen contends that the relations expressed by such formulas as "appropriate to …" or "the real foundation, on which arises …" constitute a particular kind of explanatory relations, namely *functional* relations. In the attempt to provide epistemological foundations for historical materialism thus understood as an explanatory programme, Cohen suggests that functional explanation can be seen as a special case of *consequence explanation*, of which he aims to provide a logical reconstruction. Indeed, Cohen straightforwardly argues that "central Marxian explanations are functional, which means, *very roughly*, that the character of what is explained is determined by its effect on what explains it" (Cohen, [1978] 2000; 278). To establish a functional relation of the kind expressed by statements of the form "The function of x is to ϕ", Cohen argues, is to subsume such statements under a "consequence law", which states that whenever the occurrence of an event x would have a functional effect ϕ, the event x will occur. Functional explanations, therefore, are but deductions from law-like schemes having the following form:

> If it is the case that if an event or fact of type E were to occur at t_1, then it would bring about an event or fact of type F at t_2,
> Then an event or fact of type E occurs at t_3. (Cohen, [1978] 2000; 260)

As one can readily see, Cohen's account of functional explanation makes it nothing but a variant of the covering-law model of explanation, whereby covering laws are instanced by what Cohen calls "consequence laws" that correlate the occurrence of an event with its consequences.

As Kincaid (1996) has argued, Cohen's account of functional explanation is inadequate for the same reasons that the covering-law model is, namely, that it replaces causation with other kinds of relations such as the correlation of events and their consequences. For Kincaid, what makes such substitution problematic is that it does not make functional explanation robust with respect to spurious correlations, since correlation alone is not enough to establish causation, as per the well-known dictum. Accordingly, Kincaid puts forward a different account of functional explanation that takes the cue from L. Wright's (1973, 1976) "consequence-etiological" model. On this account, functional explanation is a conjunction of three components that must be jointly satisfied for the resulting explanation to be a valid one:

(1) A causes B;
(2) A persists because it causes B;
(3) A is causally prior to B.

Within this scheme, condition (2) is introduced for the purpose of excluding spurious correlation through an explicitly *causal* statement of the relationship between a fact's persistence and its functional effects. In a similar fashion, Little (1991) has argued that functional explanation can be valid on the condition that the causal history of functions be available,

lest the explanation prove defective. Accordingly, Little provided a model of functional explanation that is logically equivalent to Kincaid's. Let P be a social practice in a society S and B the benefit it confers to S. Then, a functional explanation for the persistence of P in S would correspond to the following scheme:

> (1) P persists in S;
> (2) P has the disposition to produce B in the circumstances of S;
> (3) P persists in S because it has the disposition to produce B. (Little, 1991; 94)

Like Kincaid's condition (2) above, Little's requirement (3) that "P persists in S because it has the disposition to produce B" must be satisfied for the explanation to be a causal—and therefore acceptable—one. Much like Kincaid acknowledges that the causal condition "does much of the work" (Kincaid, 1996; 111), Little characterizes it as an outright "causal explanation establishing the postulated functional relationship" (Little, 1991; 91). Such remark is indeed a crucial one, in that it suggests that functional explanation per se is no explanation after all, the entire explanatory work lying with the lower-level causal relation that in turn explains functional relations.

Most relevantly for the present purposes, the requirement of causal relations is tantamount with the requirement that a *causal mechanism* be detailed in order for a causal explanation to be valid. As Little writes:

> The causal history requirement above is often the most difficult to satisfy in defending a functional explanation for it is almost always possible to come up with *some* beneficial consequences of a given social feature. Therefore to justify the claim that the feature exists *because* of its beneficial consequences we must account for the mechanisms that created and reproduced the feature in a way that shows how the needs of the system as a whole influenced the development of the institution. (Little, 1991; 98–99)

The same point had effectively been already raised by Jon Elster in his critique of Cohen, and of functionalism in the social sciences in general (Elster, [1979] 1984, 1980, 1982, 1983). Like Little, Elster derives the requirement for a causal mechanism from functional explanation in biology. Functional explanation, Elster argues, may succeed in biology because it can demonstrate that a "feedback loop" operates from the consequence of a phenomenon to the occurrence of the phenomenon itself, as per criterion (5) below:

> An institution or a behavioural pattern X is explained by its function Z for group Z if and only if:
>
> (1) Y is an effect of Z;
> (2) Y is beneficial for Z;
> (3) Y is unintended by the actors producing X;
> (4) Y—or at least the causal relation between X and Y—is unrecognized by the actors in Z;
> (5) Y maintains X by a causal feedback loop passing through Z. (Elster, 1983; 57)

The functional theory of evolution through natural selection adapting a species to its environment, for instance, is a scientifically acceptable one in that "natural selection" provides such a feedback loop: natural selection, for instance, is capable of *causally* explaining the the survival of an asexually reproducing species in terms of the random emergence of a mutation within a population of that species that yields higher reproductive capacity, thus allowing that mutation to spread within the population and therefore to be preserved (Elster, 1983; 49–55). Lacking an analogous to such *mechanisms*, Elster argues, Cohen's defence of historical materialism and Cohen-style functional explanation in the social sciences are in a "pre-Darwinian" or "Lamarckian" stage, a fact that Cohen himself acknowledges (Elster, 1980, 1983; Cohen, 1982b, 1982a).

The point is that, for Elster, the lack of such causal mechanisms leaves open the possibility that an external causal factor explains *both the occurrence of a phenomenon and its function*, just like Darwin was able to explain—*contra* Lamarck—that natural selection caused giraffes to develop a long neck *and* why giraffes feed predominantly on leaves from tall acacia trees. The Lamarckian explanation that the species giraffe developed a long neck *because* of the utility of that feature in relation to its diet is, therefore, a non-causal explanation, and indeed an invalid one that proves to be a pseudo-explanation. As Elster categorically concludes: "I am simply at a loss to see why functional explanation should be of interest over and above the particular mechanisms that may justify it in any given case" (Elster, 1980; 126).

Revealingly, Elster's critique of functional explanation in the social sciences was one of the earlier advocacies of mechanism-based explanation. As mentioned above, the philosophy of biology provided a prominent source of inspiration for defending the explanatory power of mechanisms in the social sciences, and the idea that the mechanism is required to fulfill a *causal* requirement served just the purpose of ruling out consequence explanation (i.e. functional explanation in Cohen's form) and covering-law explanation alike as altogether non-explanatory. Whatever the formal similarities between covering-law explanation and consequence explanation, the reason why they are demanding from a mechanism-based point of view is not that they share some puzzling logical feature that makes them inadequate to capture a commonsensical or operational understanding of explanation. Rather, the argument from mechanisms holds that whatever practical or formal inadequacies one may identify in such models of explanation points to a deeper-level lack of a causal account of the *explanandum*, which then proves to be the ultimate defining feature of any proper explanation. The general attitude of mechanism advocates towards covering laws can be summarized through Bunge's (1997) contention that while covering laws and mechanisms are formally compatible with one another, the former make for incomplete explanations. In order for them to be "truly explanatory", they must refer to the causal mechanism that produces the regularities they describe; this implies the view that regularities *per se* are non-explanatory in a causal sense. The whole point of mechanism-based explanation, in short, seems to be based on an understanding of scientific explanation in fundamentally *causal* terms.

4. Causal realism versus causal eliminativism

Although Hedström and Ylikoski (2010) consider it one of the advantages of the mechanism-based account of explanation that it is not tied to any specific theory about the nature of causation, they nonetheless acknowledge that some constraints can be put as to what theory is acceptable within the mechanism framework. For instance, most critics of the use of covering laws in social explanation seem to agree that law-like accounts presuppose a *regularity view* of causation that is incompatible with the causal underpinnings of the mechanism approach. This view is so tightly associated with the thought of David Hume that it is often referred to as simply the *Humean* view of causation. The reason for this lies in a number of statements in which Hume defines causation in epistemic terms as the result of experiencing the repeated co-occurrence of two events. In his *Treatise*, for instance, Hume writes:

> A *cause* is an object precedent and contiguous to another, and so united with it, that the idea of the one determines the mind to form the idea of the other, and the impression of the one to form a more lively idea of the other. Hume ([1738] 1978; 170, emphasis in the original)

What is especially distinctive about this view across the many variants of "the Humean view" is its *eliminativism* about causation. The choice between *realism* and *eliminativism* is a fundamental one for theories of causation. Interestingly, eliminativist and realist positions may be further distinguished into realist or eliminativist positions *about causal laws* and realist or eliminativist positions *about causal relations* altogether, depending on the extent to which causation is accorded or denied existence. On the realist view, causal relations (or even causal laws) are taken to be analytically basic and not susceptible of being further analyzed in terms of more ontologically fundamental relations (such as, say, accidental uniformities, or counterfactual dependence); conversely, eliminativists tend to conceive of causal relations and/or laws as supervenient on more primitive relations between events or states of affairs (Tooley, 1990a). The Humean view clearly falls within the most radical wing of the latter camp. In the regularity view, causation is not regarded as a real feature of the world. For the world is made up solely of atomistic, discrete, and unrelated events that (jointly) occur with a certain degree of regularity: no underlying relation can be invoked to further explain those regularities. Quite on the contrary, those regularities are taken to be as ontologically fundamental as the very events they involve—or, in Hume's parlance, the "objects". Causation, therefore, is at best an epistemological device which nonetheless is formed, as Hume put it, only in the mind of the observer.

Whether or not the regularity view can be said to be altogether anti-realist is a debated issue, and even more so is Hume's supposed anti-realism. Hume's own positive use of expressions such as "necessary connexion", "secret power", and "inviolable connexion" has led some to see it as problematic to assimilate him in the anti-realist camp, and indeed to question whether "the Humean view" actually corresponds to the Humean view (see e.g. Kemp Smith, [1941] 2005; J. P. Wright, 1983; Buckle, 2001; Strawson, [1989] 2014). These authors generally regard Hume as a *sceptical* realist, i.e. one for which any causally

productive relations underlying the experienced regularities can be postulated, or at least presupposed, even though their true nature remains hopelessly hidden from us. Arguing along the same lines, the regularity view's seeming anti-realism can be contested, too. It is safe to assume, for a start, that regularity theorists would be unembarrassed to commit to realism about entities, phenomena or, for that matter, regularities. Yet whether such realism requires postulating something more primitive than regularities which underwrites them is an entirely different story (see Table 5.). While Strawson ([1989] 2014; 23) has labelled the idea that there is nothing about the world in virtue of which it is regular "absurd" and "dogmatically anti-realist", he nonetheless acknowledged that there is no "conclusive evidence" for taking any further ontological step beyond a metaphysics of mere regularities (Strawson, 1987; 264), nor is it conducive to metaphysical inconsistency (see e.g. Beebee, 2006).

	(Humean) causal eliminativism	Causal realism
Events	✓	✓
Regularities	✓	✓
Causal processes		✓

Table 2.1: Realism vs. eliminativism about causation.

It is also true, however, that the price of such "epistemological humility" (Strawson, 1987; 264) includes a considerable degree of scepticism about causation itself, which has led regularity theorists to be cavalier about the notion of causation: even before the logical empiricists attempted to replace the concept of causation with that of logical deduction or probability for explanatory purposes, Bertrand Russell (1912–1913; 1) famously dismissed causality as "a relic of a bygone age, surviving, like the monarchy, only because it is erroneously supposed to do no harm". Thus, even setting aside the question as to whether the regularity view is altogether anti-realist, it is safe to consider it a plainly eliminativist view *about causation*.

The link between covering laws and causal eliminativism is not a necessary one. Genuinely realist arguments for the existence of causal laws can certainly be provided; David Armstrong (1983), among others, put forth an influential account of laws of nature as relations of necessitation between whole kinds of properties or relations, or *universals*. If universals can be granted real existence, the argument goes, then nomic relations can be assumed to actually exist between two universals F and G to the effect that some particular a being F necessitates that particular a being also G. Within this framework, causation can be described as the instantiation of laws between particular states of affairs: given a nomic relation between F and G, a acquiring the property F can be said to properly cause a to acquire the property G. Note, however, that this realist approach is strongly opposed to the ontology of regularities considered so far, according to which nothing exists beyond states of affairs and hence no ontological difference can be posited between accidental regularities and nomic relations instantiated in causation. For one thing, Armstrong's nomological realism entails that laws can be ascertained directly and non-inferentially, since laws are fully present

in singular causal instances and, by definition, no causation proper can occur without a covering law. On the other hand, the way in which a number of research traditions are often regarded as exemplifying the covering-law model in social and political analysis clearly suggest their leaning towards a regularity view of causation rather than a necessitarian one: allegedly covering-law explanations in social and political research typically make use of variable-oriented methods to establish probabilistic correlations rather than causal laws.

In principle, Armstrong's account can be extended to accommodate probabilistic laws by allowing for relations of "nomic probabilification" (Armstrong, 1978a; 158–159). Other proponents of a similar theory of nomological realism, like Tooley, have argued that causation proper occurs only between singular events; laws between universals merely retain the "direction" of causation, which Tooley understands as a "transmission of probabilities" from causes to effects (Tooley, 1987, 1990b). It would seem, however, that to the extent that social and political explanations make reference to covering laws, these are best interpreted as regularities rather than as laws of nature that really exist and govern the relations between universals. Lijphart's (1971) and Smelser's ([1976] 2013) seminal advancements in the statistical method for comparative research, for instance, are explicitly presented by their proponents as an approximation to the "Joint Method of Agreement and Difference" that Mill ([1843] 1974) prescribed to natural scientists. Like his other "Methods of Experimental Inquiry", Mill's own statement of the "Joint Method" prescribes that causes be identified in terms of their regular occurrence in conjunction with the phenomenon identified as their effect:

> If two or more instances in which the phenomenon occurs have only one circumstance in common, while two or more instances in which it does not occur have nothing in common save the absence of that circumstance; the circumstance in which alone the two sets of instances differ, is the effect, or the cause, or an indispensable part of the cause, of the phenomenon. (Mill, [1843] 1974; 396)

Mill's choice of wording strongly suggests that some form or another of the regularity view of causation is presupposed in his "Joint Method". It is also true, however, that all of Mill's "Methods of Experimental Inquiry" are intended as guidelines for ascertaining causes empirically, not as metaphysical definitions of them. A committed empiricist, Mill himself is highly sceptical about the claim that causation implies "a mysterious and most powerful tie, such as cannot, or at least does not, exist between any physical fact and that other physical fact on which it is invariably consequent" (Mill, [1843] 1974; 326). Yet no such metaphysical scepticism is strictly required or logically necessary for the purposes of his methods—nor indeed is any metaphysical position at all, as it has been argued above. Indeed, the vast majority of methods for social and political analysis whose critics identify with the covering-law model generally focus on issues of explanation rather than causation, and therefore their proponents tend to be theoretically parsimonious when it comes to grounding their epistemological inclinations onto a particular causal metaphysics. Yet precisely such purported metaphysical innocence is the implicit target of the mechanism-based criticism of covering laws.

As Salmon realized, the claim that laws fail to explain causally rests upon the idea that the

observed regularities—however well confirmed—cannot be the whole story about causation, which has led many to dismiss causation talk altogether in addressing explanatory issues. Insisting on this point, Mahoney (2001; 575) argued that the problems with the covering-law model embodied in correlational analyses, as identified by the social mechanism approach, "are not simply familiar themes such as a correlation fails to represent causation because it might be spurious or because the time order of correlated variables may not be clear". Rather, contributors to the social mechanism tradition generally go so far as to suggest "that even nonspurious correlations in which the time order of variables is well-established may be inherently limited representations of causal processes". What is being implied here is that there is more to causation than the covering-law account would allow. This is perhaps most explicit in the claim that mechanisms explain by opening up the "black box" of statistical correlations and showing the underlying causal wheel-work—the "cogs and wheels" that make up the causal chain of events leading up to their outcome. Here, the explanatory focus is directed to the cogs and wheels of the mechanisms in that they are considered to be the relevant factors capable of causally producing the outcome to be explained. Implicit in this idea is a commitment to a *realist* view of causation according to which causes are, at least in principle, clearly discernible and, *a fortiori*, real; conversely, law-like explanations would fail to acknowledge the reality of causes and hence to adequately portray causation as it occurs.

As Salmon reconstructed in his *Four Decades of Scientific Explanation* (1989), in the first decade after the publication of Hempel and Oppenheim's (1948) article, the covering-law model of explanation "was widely accepted by scientifically minded philosophers; indeed, it qualified handily as the received view" (Salmon, 1989; 4). During the second decade, however, criticism of Hempel's comprehensive account of scientific explanation became increasingly vocal through counterexamples to particular instantiations of the Hempel-Oppenheim model. Salmon (1989; 46–50; 1992; 20–29) groups them into a number of different families according to what have long become standard counterarguments to the Hempel-Opennheim model and which ultimately led to the demise of the covering-law model as the definitive account of scientific explanation. Revealingly, some of the earliest lines of criticism to the covering-law model were explicitly expressed in causal terms, and targeted the inadequacies of law-like explanation by pointing to the presupposed anti-realism about causation from which they derived. Michael Scriven, for instance, objected to the use of covering-law explanation of particular events through his famous "ink stain" example:

> If you reach for a cigarette and in doing so knock over an ink bottle which then spills onto the floor, you are in an excellent position to explain to your wife how that stain appeared on the carpet, that is, why the carpet is stained (if you cannot clean it off fast enough). You knocked the ink bottle over. This is the explanation of the state of affairs in question, and there is no nonsense about it being in doubt because you cannot quote the laws that are involved, Newton's and all the others; in fact, it appears one cannot here quote any unambiguous true general statements, such as would meet the requirements of the deductive model. (Scriven, 1962; 198)

For Scriven, what this example shows is that one may have reasons to make a causal claim without thereby being able to quote laws. This suggests that explanation is not necessarily coextensive with subsumption under covering-laws, since one may legitimately explain

singular events by virtue of singular causal claims. Still other counterexamples aimed to show that covering-law model was explanatorily defective on accounts of its neglect of causation. Such is the case, for instance, of *explanatory asymmetries*. Going along with the covering-law model, Salmon argues, it is possible to deduce the occurrence of a total lunar eclipse from the relative positions of the earth, sun, and moon at some time *after* the eclipse in conjunction with the laws of celestial mechanics that govern their motion. The reason why this does not seem to qualify as an explanation even though it satisfy the requirements of the Hempel-Oppenheim model, Salmon concludes, is that some temporal constraints must be put on explanation—which, incidentally, Hempel (1965a; 353) refuses to do. Yet the reason why this counts as an objection to anti-realism about causation is that causal relations can count as asymmetrical temporal relations (Salmon, 1984; 171), and therefore the appeal to causality does impose the required temporal constraints that prevent the occurrence of explanatory asymmetries. Intriguingly, Salmon also invokes causal relations to explain the appearance of *explanatory irrelevancies* in covering-law explanation, such as the well-known case in which a man explains his failure to become pregnant by invoking his regularly taking birth control pills. While such explanation would satisfy the requirement of covering-law explanations—it is perfectly deducible from a true law stating that "All man who regularly take birth control pills will not get pregnant"—a requirement of relevance must be met for such explanation to be a genuine answer to the question: "Why did the man fail to become pregnant?"—i.e. for it to be an explanation at all. This is because defining explanation in terms of truth and validity instead of causality allows one to cite as a contributing cause something which is not (Salmon, 1989; 102–103, 300-301). As in Scriven's ink stain example, the singular causes may be cited—indeed, in this case *must* be cited—to fully explain a given phenomenon while dispensing with law-like inferences.

Crucially, Salmon concludes from the above that covering laws have no explanatory role, explanation being best characterized in *causal* rather than *inferential* terms. More importantly, Salmon commits to a *realist* understanding of causation, which is required to make sense of temporal asymmetry and relevance in explanation: causes, in other words, must be conceived of as singular and contributing process that *produce* a certain outcome. Such ineliminability of causal relations is required for causal explanation not to be further analyzable in purely inferential terms and for causality not to be reducible to some underlying relation of logical dependence. Salmon even makes his causal realism vividly clear in calling his account of scientific explanation "the ontic conception" as opposed to "the epistemic conception", so as to suggest that causal explanation is best characterized in terms of causality as an ontological feature of the world rather than of merely inferential requirements (Salmon, 1984). Most notably, however, Salmon's realist theory of causation is also cited as one of the early defences of explanatory mechanisms. This is because *causal interactions* are for Salmon characterized by (spatio-temporal) *contiguity* and by their ability to transmit a *mark*, which are required to make sense of causal explanation to be sensitive to temporal asymmetry and to relevance. Accordingly, an explanation can be characterized as valid insofar as it traces the *causal process* that leads up to the *explanandum* (Salmon, 1984). Such model of explanation

Salmon calls the "causal-mechanical", and in fact, the idea of explanation as process-tracing is in many relevant respects equivalent to the requirement that a causal mechanism be made explicit.

5. Social mechanisms and causal realism

While Salmon's "causal-mechanical" model is explicitly premissed on a realist understanding of causation, still other authors in the mechanism tradition have explicitly invoked causal realism in defence of the use of mechanisms for social explanation. Mahoney (2001; 580) argues for instance that going beyond correlational analysis requires a realist interpretation of mechanisms to the effect that "the activation of a mechanism actually generates an outcome", since only such an account is robust enough as to rule out explanations in terms of variables increasing or decreasing the probability of having higher or lower values on an outcome. From a different angle, Hedström (2005; 28) places an explanatory premium on individual-level mechanisms on grounds that actors and actions are the cogs and wheels of social mechanisms. This, he argues, follows from a "realist principle" underlying the scientific rule that "theories should be formulated in terms of the processes that are believed to have generated the phenomena being studied". How such realist approach to causation is to be translated into a specific theory about the nature of causation, however, is once again the source of substantive disagreement among mechanism advocates. Salmon, for instance, introduced his own version of a "causal process theory", which he once summarized through the following quote by John Venn:

> Substitute for the time-honoured "chain of causation", so often introduced into discussions upon this subject, the phrase a "rope of causation", and see what a very different aspect the question will wear. (Venn, 1966; 320; cited in Salmon, 1980; 67)

The idea that the ultimate nature of causal relations is one of continuous flows or processes rather than, as Venn (1966; 320) put it, one "of determinate distinct links following one another in succession, of stages marked off from one another" has however been put into question with respect to its ability to provide adequate foundations to mechanism-based explanation. Hedström and Ylikoski (2010; 53) point out for instance that very few scientific disciplines outside of some specific sub-fields of physics actually deal with spatio-temporally continuous processes, and therefore Salmon's process-centred theory of causation proves ill-suited to ground the use of mechanisms for social or even biological explanation. Chakravartty (2007) has recently defended a process-based view of causation on grounds that only continuous causal processes as opposed to the discrete links of a causal chain of events can overcome the "contiguity objection" to causal realism. First formulated by Russell (1912–1913), the objection from contiguity alleges that no two events C and E can be contiguous because temporal contiguity is incompatible with the baseline notion that time is dense and compact. For Chakravartty, this objection requires that a coherently realist theory of causation disposes of proximate causes and conceives of causation as a continuum rather than as a chain of discrete causal links.

Hedström and Ylikoski (2010; 53) state quite clearly that "the mechanism-based account of explanation is not wedded to a specific theory of causation". Yet they, too, seem to allow for some constraints to be put on acceptable causal ontologies. Building on their critique to Salmon, they write that "a more natural complement to the mechanism-based approach" would seem to be a manipulative-interventionist theory according to which causation can be described as a relation of counterfactual dependence in terms of its being invariant under manipulative intervention (such as, e.g., that developed by Woodward, 2002, 2003; Hedström and Ylikoski, 2010). Such is, for instance, the proportional relation between the frictional force of a block sliding down an inclined plane and the normal force exerted by the surface of the plane perpendicular to the direction of motion of the block: such relation can be considered as causal on grounds that it supports counterfactuals, viz. it will hold without variations even if one modifies the inclination of the plane or the materials of which the block and the plane are made (Woodward, 2002).

Hedström and Ylikoski seem to suggest that Woodward's account alone is insufficient to provide a causal basis for the mechanism approach. The latter, they write, "differs from Woodward's approach by its emphasis on the importance of opening up black boxes and making explicit the causal cogs and wheels through which effects are brought about" (Hedström and Ylikoski, 2010; 54). This may sound misleading in the light of the authors' clear-cut distinction between the goal of the mechanism approach and the definition of causation, since this passage seems to suggest that the mechanism approach has something to say about how causation operates. Yet this is entirely consistent with what has been argued in the preceding sections. If the adoption of the mechanism approach is to be preferred over covering-law accounts on the basis of a realist position about causal explanation, some non-eliminativist theory of causation is required to replace the regularity view. It is not entirely clear, however, whether a manipulative-interventionist approach alone could fulfil this task. Manipulative intervention is taken to be explanatory on accounts of its ability to support counterfactuals. In this way, however, causation is reduced to what is believed to be a stronger relation, namely, counterfactual dependence.

Not too dissimilarly, Mahoney defends mechanism-based explanations on the basis of a theory of causes as relations between necessary or sufficient conditions, as detailed in Ragin's ([1987] 2014, 2000) work. As comparative studies in political research show, and contrary to what the use of regression analysis in comparative research presupposes, Ragin points out that causation hardly occurs homogeneously across different instances. For the same phenomenon (e.g. land hunger, sometimes treated as a causing variable) may or may not concur in bringing about a certain outcome (e.g. peasants rebellion) depending on the factors it occurs in conjunction with (e.g. commercialization of agriculture, or lack thereof). In fact, the same outcome may happen in different settings in the absence of any recurring condition, suggesting that a whole combination of conditions is causally required for the outcome, and not any of the independent individual variables correlating with a change in outcome (Ragin, [1987] 2014). In Ragin's version, therefore, causes are combinations of conditions that are sufficient for an effect to be brought about. Of course, this begs the question of

how sufficiency is defined. One may be tempted, for instance, to interpret sufficiency in counterfactual terms as in: a certain set of conditions would regularly produce a given outcome in all possible settings. In this case, however, we would fall back to the sophisticated regularity theory detailed in the previous paragraph. Mackie ([1974] 1980), the main source for Ragin's theory, also seems to have in mind a refined version of the regularity view in providing his account of causation in terms of necessity and sufficiency. Famously, Mackie (1965, [1974] 1980) describes causes as *at least INUS* conditions, i.e. as *I*nsufficient but *N*on-redundant (viz. necessary) parts of an *U*nnecessary but *S*ufficient set of conditions for the effect. Importantly, the fact that causes must be *at least* INUS conditions implies that they can be sufficient conditions, necessary conditions, or both. Yet for Mackie such conditions are still to be characterized in terms of regularities: the regularity view, he argues, has the merit of not relying on "the mystery of a special sort of regularity, a 'nomic universal', to account for the ability of causal laws to sustain counterfactual conditionals" (Mackie, [1974] 1980; 60). Accordingly, he describes necessity and sufficiency in terms of co-occurrence:

> "X is a necessary condition for T" will mean that whenever an event of type T occurs, an event of type X also occurs, and "X is a sufficient condition for T" will mean that whenever an event of type X occurs, so does an event of type T. (Mackie, [1974] 1980; 62)

Following Mill, Ragin insists that the same event may be brought about by a wide range of combinations of particular conditions, none of which recurs in more than one instance, and the capacity to account for this he presents as the main advantage of a multicausal approach. Also drawing on Mill, Mackie ([1974] 1980; 64) argues that in these cases where one effect is caused by non-overlapping conjunctions of conditions—say, ABC, DGH, or JKL—it is the whole disjunction of such conjunctions—(ABC or DGH or JKL)—that does the causing. It is in this sense that Mackie speaks of such disjunctions as "complex regularities" and to the extent that these are arguably the most distinctive contribution of his theory of causation, this can in turn be characterized as a refined version of the regularity view.

6. Causal realism and productive causation

Luckily, the appeal to regularities—whether in a counterfactual or complex form—is by no means the only possible complement either to Hedström and Ylikoski's account of mechanisms or to Mahoney's appeal to Ragin's multicausality. At this point, however, one might wonder how a properly realist causal ontology is to be qualified. In this respect, it is perhaps worth referring to a fundamental distinction between two varieties of causation, namely, causation as *dependence* and causation as *production*. As N. Hall (2004; 225) has it, this distinction is essentially one between causation understood as a relation of *counterfactual dependence* and a theory of causes as factors that help to *generate* or *bring about* or *produce* an effect. Hall (2004; 256, 265) acknowledges that production seems to be the more "central" causal notion, yet he believes this on the basis of a deliberately restricted account of production, which he defines as "the ancentral of proximate causation". He further acknowledges that counterfactual dependence and productive causation often

coincide in practice, yet he takes this to reinforce rather than undermine the autonomy of counterfactual causation. Accordingly, he takes the two variants to be distinct and mutually irreducible kinds of causation.

As with manipulative-interventionist theories, however, counterfactual dependence theories of causation tend to come down to the regularity view, since some degree of uniformity between actual and counterfactual states of affairs is required for the latter to be meaningful. This is the route followed by David Lewis in his classic treatment of causation as counterfactual dependence (Lewis, 1973a). Revealingly, in his paper Lewis draws on Hume's ([1777] 1975) counterfactual definition of causation, whereby Hume posits a cause to be "an object, followed by another, and … where, if the first object had not been, the second never had existed" (Hume, [1777] 1975; 76). Such definition Hume overtly meant to be a mere rephrasing of his regularity-based definition of a cause as "*an object, followed by another, and where all the objects similar to the first are followed by objects similar to the second*" (Hume, [1777] 1975; 76). Unpersuaded by this equation, Lewis opens his essay with a quote of Hume's two-fold definition, and interprets causal relations to be of a counterfactual nature in that "[w]e think of a cause as something that makes a difference, and the difference it makes must be a difference from what would have happened without it. Had it been absent, its effects—some of them, at least, and usually all—would have been absent as well". It is to provide an analysis of causation thus defined that he resorts to the semantics of possible worlds. In a seminal paper, Stalnaker (1968) had defended the thesis that counter-to-fact conditional statements are false in the actual world but have a positive truth value in a possible world that otherwise "differs minimally" from the actual one (Stalnaker, 1968; 102). Building on this, Lewis provided a now standard analysis of causal statements as counterfactual conditionals: C causes E if, in the world *closest to actuality* where C does not occur, E does not occur either.

Lewis' "closest to actuality" clause, like Stalnaker's "minimal difference" clause, is of course required for the truth of conditional—and therefore causal—statements to be meaningful in the actual world. Suppose we are to interpret a causal relation of the form "Striking the match caused it to light" in counterfactual terms as equivalent to the subjunctive conditional "Had the match not been struck, then it would not have been lit". We might well imagine a possible world in which the match did light despite its not having been struck, e.g. because a lightning struck it. This would make the conditional statement "Had the match not been struck, then it would not have been lit" false, and therefore we may be led to reject also the causal statement "Striking the match caused it to light". Plainly, this is not a satisfactory analysis of the causal relation as occurred in the actual world. To overcome this, a constraint can be put on possible worlds to the effect that the only difference between the actual world and the possible world under consideration must be the striking of the match. Such is Lewis' *closest-to-actuality clause*. To be sure, such ranking of possible worlds rests, in Lewis' words, on a primitive relation of *comparative over-all similarity* which makes alternative worlds mutually comparable. The overall interpretation of causation as a relation of counterfactual dependence, therefore, depends entirely on the existence of such primitive

relation of similarity. In a later article, Lewis (1979) provides a definition of similarity suited to his analysis of causation in terms of counterfactual dependence that is based on "laws". Two worlds, the argument goes, are more similar to one another the less violations of laws occur between them. Or, in other words, two worlds are similar insofar as they are governed by the same laws. In what has come to be known as the Mill-Ramsey-Lewis account of laws, Lewis (1973b, 1986e) adopts Ramsey's ([1928] 1990) view (originating in Mill ([1843] 1974; Book III, Chapter IV, Section 1)—that laws are "consequences of those propositions which we should take as axioms if we knew everything and organized it as simply as possible in a deductive system" (Ramsey, [1929] 1978; 138). For Lewis, laws are generalizations that are *simple* and *strong*—i.e. *informative*—enough to serve as axioms for deductive system. *Qua* generalizations, therefore, laws are regularities, and the whole Mill-Ramsey-Lewis view of laws is, in Lewis' words, "a regularity theory of lawhood" (Lewis, 1986e; 122).

Indeed, Lewis' overall work on causation and counterfactuals can be interpreted, in the author's own words, as "a prolonged campaign on behalf of Humean supervenience" (Lewis, 1986c; ix). The *supervenience* part of this doctrine holds that any two worlds matching in laws, causal connections, and the like must also match in all matters of particular fact. This thesis is warranted by a *Humean* view according to which all there is about a world are particular facts, or combinations thereof (Lewis, 1986a; 111). In this sense, the counterfactual theory of causation can be viewed as a sophisticated version of the regularity view. The fact is that the Mill-Ramsey-Lewis view of laws is the dominant approach to a counterfactual theory of causation, the other main alternative being the nomic realism of the Dretske-Tooley-Armstrong theory briefly discussed above (Beebee, 2000, 2006). On the Dretske-Tooley-Armstrong view, necessitarian relations are real relations between universals rather than merely nominal relations that supervene on regularities. As we have seen, however, hostility towards laws in social science explanation derives from the necessary anti-realism of statistical correlations. Most explanation in empirical science is, to use Hempel's jargon, of an "inductive" rather than "deductive" nature. In other words, the "laws" that feature in covering-law explanations, especially as far as social science is concerned, are usually statistical correlations that arise from well-confirmed and empirically observed *regularities*. This also makes it difficult to interpret such correlations as probabilistic *laws* by virtue of their inherently statistical character. The Dretske-Tooley-Armstrong view allows for relations between universals can be probabilistic as "*universals which are instantiated only in the cases where the probability is realized*" (Armstrong, 1983; 129, emphasis in the original). Nonetheless, this does not allow for probabilistic laws to support causal inference, and in his refined account Tooley (1987, 1990b) explicitly characterizes probabilistic laws as non-causal relations that merely retain a "transmission of probabilities" between universals. Such view is clearly ill-suited to account for covering laws as they are employed in social science explanation. Not only does the statistical nature of covering laws exclude the possibility of a necessitarian as opposed to merely probabilistic interpretation of the link between the variables designating the correlated events; insofar as they aim to capture causal links between particular—if recurring—events, such laws are meant to explain by virtue of their

being regular co-occurrences of particular events rather than probabilistic laws.

If the viability of law-like explanation in the social science is to be discarded on accounts of its anti-realism, the nomic realism of the Dretske-Tooley-Armstrong view is no use. We are thus left with the sophisticated regularity view of the Mill-Ramsey-Lewis theory of laws as the only viable complement to a counterfactual-dependece theory of causation. It would seem, therefore, that identifying causation as a relation of counterfactual dependence inevitably falls back into the regularity-based eliminativism about causation that underwrites the standard Hempelian account. This is indeed the inevitable outcome of any attempt to redescribe causation in terms of a logical relation, a shared feature of the traditional covering-law model of explanation and of Lewis' counterfactual theory of causation. Ruth Groff (2017) seems to have something like this in mind when she evokes a distinction between *productive* and *passivist* theories of causation. The idea that causation is productive, Groff writes, "assumes that activity, or doing, is an irreducible feature of the world"; conversely, the passivist view sees causation as a matter of rational or logical necessitation, empirical regularity, or counterfactual dependence, but *not* activity or doing—at least not as irreducible to other, "passive" relations. On the basis of what has been argued in the preceding paragraphs, this distinction largely overlaps with N. Hall's (2004) between *causation as dependence* and *causation as production*: in discussing counterfactual dependence, Groff too points out that "the appeal to alternative worlds that underwrites counterfactual dependence is simply an appeal to regularity across a larger canvas" (Groff, 2017; 8). In this sense, counterfactual dependence rests on the passive—i.e. non-causally-generative—notion of regularity as a mere collection of given facts.

Crucially, Groff introduces this distinction for the purpose of assessing realism about causal mechanisms. Anti-realists, she argues, entertain an utterly instrumentalist notion of a mechanism as nothing more than a metaphor for variables, while realists take mechanisms to identify actual entities and their properties. Yet insofar as purported realists adopt a passivist view of causation, their position is virtually indistinguishable from instrumentalist anti-realism. This can be seen most clearly if counterfactual theories of causation like Woodward's manipulative interventionism are taken to provide a satisfactory and exhaustive causal basis for the mechanism approach. In this case, Groff (2017; 8) argues, "the realism about the mechanism is negated by the passivism about causation, which leaves the mechanism inert". Hedström and Ylikoski themselves seem to acknowledge that there is more to the causal part of a mechanism than mere counterfactual dependence. As they write:

> A simple causal claim tells us about counterfactual dependency: It tells us what would have happened if the cause had been different. The mechanism tells us why the counterfactual dependency holds and ties the *relata* of the counterfactual to the knowledge about entities and relations underlying it. (Hedström and Ylikoski, 2010; 54)

If the mechanism approach is to be defended on the basis of realism about causal explanation, therefore, some productive account of causation would seem to be indispensable.

Conclusion

Rather than a fully-fledged metaphysical theory, the view of causation as production serves as a general rule about what causation can or cannot be by the standards of scientific realism. What the productive view postulates is, at most, the notion that causal processes ought be seen as metaphysically primitive instead of supervenient on more or less regular patterns of unrelated events. To state the contrary is tantamount to endorsing an eliminativist view of causation which prescribes that causal explanation conforms to the Humean view on which—contrary to much social science—the covering-law model depends.

On this view, manipulative interventionism, fuzzy set methodology, and even the search for statistical correlations may well be operational methods for discovering causal relations, but not theories about the nature of causal relations themselves. In the next chapter, I will try to substantiate the productive view of causation by linking it with the philosophical literature on causal powers as well as with metaphysical theories about dispositional properties. Causal processes, it will be argued, can be thought of as the materialization of the causal properties of individual entities. The ascription of such causal properties is in fact required to make sense of causal processes as concrete relations as well as to account for the contextual, non law-governed way in which events occur. The mechanism-based model of explanation can itself be shown to demand reference to the causal properties that allow entities to interact with one another like the cogs and wheels of a machine.

Pushing scientific realism from causal *processes* to causal *properties* allows us to extrapolate a theory of scientific objects from realism about causation. This will be shown to be a theory about individual and heterogeneous entities which are characterized by their particular and contingent properties. In the remainder of the work, the identification of politics as a domain of scientific objects will thus be characterized as the outcome of the classification of such entities into typologies according to a "family resemblance" mode of analysis: because social entities are metaphysically heterogeneous, grouping them into homogeneous classes must depend on some stipulation about the family resemblances between their properties. Multiple and equally valid definitions of the objects of politics can therefore result depending on what counts as "family resemblance". Providing criteria on which one can assess those definition will be the ultimate goal of this work.

Chapter 3

The nature of social science objects

> Dispositions are as shameful in many eyes as pregnant spinsters used to be—ideally to be explained away, or entitled by a shotgun wedding to take the name of some decently real categorical property. It is time to remove this lingering Victorian prejudice. Dispositions, like unmarried mothers, can manage on their own.
>
> David Hugh Mellor (1974)

Introduction

Ruth Groff (2017) takes the productive theory of causation to be altogether coextensive with a "power-based" approach, i.e. an understanding of the nature of causation as consisting of dispositional properties, or powers, by virtue of which causal production is generated. In what follows, I shall motivate this claim on the basis of the demands of realist models of explanation. The demand for a causal mechanism, for instance, can only be coped with by referring to the causal properties in virtue of which the entities that make up the mechanisms interact to produce a certain outcome.

The link between mechanisms and powers has a long history. As mentioned, the idea of a *mechanical philosophy* dates back to the seventeenth-century debate on natural philosophy. The term itself was popularized by prominent scientist and philosopher Robert Boyle, whose pioneering work also tried to combine the new mechanistic image of the world with a causal ontology that owed much to the Aristotelian notion of "powers". A critic of the concept of natural laws as a moral notion that he deemed ill-suited to describe the clockwork of the natural world, in his work Boyle conceived of bodies—both animated and inanimated—as infused with God-given powers capable of setting other bodies into motion and thus trigger mechanical causation. At the inception of mechanical philosophy, the power-based ontology of causation introduced by the likes of Boyle, Locke, and Thomas Reid rapidly became so popular that Mill ascribed it to "the schools of metaphysics most in vogue at the present

moment", but it gradually fell out of favour under the blows of the empiricists' causal scepticism only to be revived in the second half of the twentieth century.

Aside from the specifically mechanistic understanding of causal production, however, causal realism defines causal explanation as an account of why an event happened as it happened; while causal processes alone can explain change, to explain *how* such change occurs one needs to make reference to the causal properties by means of which an object is disposed to act in a certain way. It is in the attempt to fulfil the demand for causal mechanisms through reference to properties that some realists elliptically invoke the "powers" of ancient metaphysics. In the following sections I aim at showing that such power-based ontology complements the productive view of causation as it is advocated in causal realism. Section 1 will clarify the sense in which the unobservable properties that define the metaphysical structure of entities can be invoked to flesh out the notion that phenomena are produced by real-world causal processes. Section 2 will argue that some reference to the causal structures of entities is required to make sense of causal processes as concrete rather than formal relations (section 2.1), to contrast causal processes with regularities (section 2.2), and to detail the causal mechanisms that most realist models of social explanation require (section 2.3).

Section 3 will then introduce the current metaphysical debate on dispositional properties. Causal properties are often invoked in epistemological theories about causal explanation, and therefore they are often referred to as "causal powers" in order to emphasize their causally productive role. *Qua* properties, however, the structural components of individual entities are more appropriately referred to as "dispositions". Having argued that some properties must have a dispositional character in order to avoid a Humean metaphysics, section 4 will explore the nature of dispositional properties quite apart from their role in causal explanation. Specifically, dispositions will be shown to provide a theory of scientific objects as "powerful particulars", as per Harré and Madden's phrase.

This will provide the ground for the realist theory of scientific objects that will be detailed in chapters 4 and 5.

1. Productive causation and dispositions

The link between a productively realist approach to causation and the commitment to unobservable "causal properties" is not universally accepted. On one side of the argument, Galen Strawson, among others, has provided a defence of a theory of causation (which he calls "Producing Causation") that is clearly of a *productive* type: the only viable alternative to the Regularity view of causation, Strawson argues, is to postulate that part of what it is for *A* to cause *B* is for *A* to *produce B* (Strawson, 1987; 256). For Strawson, this is tantamount to postulating the existence of "objective forces" that govern the productive action of entities, of which the "fundamental forces" postulated by physics are a prime example (Strawson, 1987; 254). For this reason, however, Strawson refuses to talk about "causal powers" altogether: whatever causal powers one may ascribe to entities, his argument

goes, these derive from those objective forces that do the causing, and therefore are redundant for a "Producing Causation" theory (Strawson, 1987; 255). Strawson's allusive reference to dynamic "forces" whose nature deliberately remains unspecified aligns Strawson's position with the *causal process* theory of Wesley C. Salmon, later championed in a revised form by Phil Dowe. As we have seen, Salmon's own brand of causal realism was in considerable part shaped by the idea that genuine causal processes must be able to transmit a mark, i.e. a local modification in the structure of things. Welcoming Dowe's (1992) objections, Salmon later abandoned the "mark criterion" in favour of Dowe's theory of "conserved quantity": on this view, causal interactions are those processes that propagate causal influence via the transmission or exchange of a conserved quantity such as e.g. mass-energy, momentum, or electric charge (Salmon, 1997, 1998; Dowe, 2000). Setting aside for a moment Hedström and Ylikoski's (2010) point that continuous, quality-conserving processes may represent what happens in physical causation but are hardly applicable in the social sciences, one can see how a conserved quality theory of causation is also ill-suited to accommodate a metaphysics of causal powers. Conserved quality may very well be thought as a disposition—indeed, Salmon (1998) speaks of it in terms of *propensity*—but it is the *propagation* of it that does the causing. Like Strawson's objective forces, Salmon's and Dowe's causal processes determine dispositions to produce, but remain irreducible to them and indeed constitute the very nature of causation.

	Humeanism	Causal realism	
		Causal process theory	Dispositionalism
Events	✓	✓	✓
Regularities	✓	✓	✓
Causal processes		✓	✓
Dispositions			✓

Table 3.1: Varieties of causal realism: causal process theory vs. dispositionalism.

On the other side of the argument, power theorists may defend the existence of powers in a strong sense while at the same time denying that they play any serious role in causation. Alexander Bird (2013), for instance, makes the case for maintaining a distinction between *powers* and *dispositions* as two inherently different kinds of properties. While *dispositions* are properties that can be ascribed to objects by virtue of such predicates as "being soluble" or "being fragile", *powers* are merely theoretical concepts that designate the essence of an object implied in it being endowed with dispositions. Such powers are what David Lewis identifies as the "fundamental physical properties" such as the charges or masses of particles (Lewis, 1986d; 60). These merely theoretical entities, Bird specifies, are only relevant in *fundamental* metaphysics: positing their existence serves the purpose of addressing questions concerning "what the fundamental entities of the world are, how they relate, and how they account for the most basic and general features of the world (such as the existence of facts, the fundamental laws of nature)" (Bird, 2013; 30). As such, however, powers are

no use in *non-fundamental* metaphysical projects such as the investigation of the nature of causation. Powers may very well constitute the features that the world must possess in order for there to be causal relations, but they do not illuminate the nature of such causal relations in the slightest. By definition, only non-fundamental properties like dispositions, but not fundamental properties like powers, belong in the province of non-fundamental metaphysics. It is dispositions, not powers that contrast the counterfactual theory of causation. This might sound like a merely terminological quibble, since non-fundamental dispositions may be just what others, less concerned with distinctions between different metaphysical projects, would simply call "causal powers". In fact, however, holding such a distinction leaves the door open to causal Humeanism as the metaphysical complement to a dispositional theory of causation, and conversely, to a power-based metaphysics underwriting a counterfactual analysis of causes. For the veritable oppositions are for Bird that between regularities and powers at the level of fundamental metaphysics, and that between dispositional and counterfactual theories of causation at the level of non-fundamental metaphysics. Accordingly, one might conceive of powers to be "the cement of the universe" without conceding that they account for the mysterious "necessary connexion" to which Hume alludes and which Humeans abhor.

To be sure, Bird's view makes for a non-causal metaphysics of powers which nonetheless can recast the causal focus on dispositions. This, however, imposes some constraints on a theory of dispositions that is suited to provide a metaphysics to the realist approach to social explanation. To see this, let us consider two different conceptions of metaphysics. Jonathan Schaffer (2009) introduces—or more appropriately, re-introduces—an Aristotelian understanding of the scope of metaphysics in order to counter the dominant view of metaphysics originating in Quine's ([1953a] 1961) path-breaking essay *On what there is*. On the standard Quinean view, metaphysics is about "what there is", its task being one of saying what exists. On the Aristotelian view, by contrast, metaphysics is about *substances*, or the fundamental units of being, and therefore its task is to ask what is fundamental, or what "grounds" what. The reason for terming such view "Aristotelian" lies in the fact that Aristotles sets out *substances* (themselves a quintessentially Aristotelian notion) as the object of his *Metaphysics*: "Substance is the subject of our inquiry; for the principles and the causes we are seeking are those of substances" (*Metaph.;* 1069a18–19). To better appreciate this difference, let us consider a *locus classicus* in Quinean metaphysics, namely the existence of numbers *qua* mathematical entities. Questions about the existence of seemingly abstract entities such as numbers, properties, or meaning are what makes the Quinean metaphysical task non-trivial, since it goes beyond a commonsensical commitment to the existence of more ordinary objects like, say, chairs. These questions, however, appear trivial to the Aristotelian metaphysician, who would see a positive commitment to the existence of mathematical entities as unproblematic. As far as numbers are concerned, Aristotelians would be interested in investigating whether numbers are fundamental or derivative, i.e. whether numbers are primitive or grounded in real entities, their existence being beyond question either way. (As a side note, contrary to Plato numbers *are* for Aristotle grounded in concrete substances.)

There is a sense in which the distinction between a (neo-)Aristotelian notion of meta-

physics and the more traditional understanding of metaphysics as "ontological commitment" proves relevant for the present discussion. On one hand, Bird's contention that powers are of exclusive concern to fundamental metaphysics seems to imply the quasi-Aristotelian idea that metaphysics is, at least in part, about *fundamentals* rather than about *existence*. On the other hand, however, dispositions as they have been evoked so far seem to fulfill a properly Quinean task. As Schaffer points out, the two views of metaphysics—the Aristotelian and the Quinean—are characterized by a *method* as well as by a task. Famously, the Quinean method prescribes that we extract existence commitments from our best theories, such as the explanatory theories of science. So, for instance, we commit to the existence of numbers because numbers are postulated in mathematics. Similarly, dispositions are invoked in social theorizing insofar as their existence is required by the supposedly best explanatory theory, namely the productive theory of causation that underwrites scientific realism as it is instanced in social science explanation, e.g. in the form of mechanisms.

Yet the analogy must not be pushed too far so as not to become a source of confusion. In fact, some cautionary words are in order before proceeding any further. To start with, it is worth remarking that Quine's method for extracting an ontological commitment is expressed in purely semantical terms, as per Quine's famous formula "To be is to be the value of a bound variable". Indeed, Quine interprets such variable as bound by an existential quantifier, so that to say that x esists is to say that "there is an x ($\exists x$) such and such". None of these strictures, however, need be added to the present analysis. If dispositions can be said to answer a question raised by a traditional approach to metaphysics, this is so only at a superficial level, namely because one can move from explanatory theories to extrapolate a theory of dispositions. Such move invites no conclusion as to whether the existence of entities needs be expressed in purely semantical terms, let alone in the form of an existential quantification.

Furthermore, theories of dispositions *per se* can fulfill an Aristotelian task as much as a Quinean one, or even neither task (e.g., they could be invoked to account for dispositional talk in ordinary language). Aristotle himself seems to allow for a fully causal—and therefore dispositional—metaphysics of fundamentals when he says that the primary kind of potency is "an originative source of change in another thing or in the thing itself qua other" (*Metaph.;* 1046a10–11). Now what Aristotle calls "potency" (*dýnamis*) is precisely what Bird designates with his use of the word "power", namely, an essential property of substances. It follows that the causal role fulfilled by dispositions can consistently be addressed within a project of fundamental metaphysics. Not only could this reintroduce dispositions as part of a neo-Aristotelian metaphysics; it also limits the extent to which the restrictions that Bird imposes on the scope of dispositions can be said to derive from his adoption of an Aristotelian view of metaphysics, since one can always expand fundamental metaphysics so as to include questions of causation, dispositions, and explanation. To restate the point, therefore, the distinction between Arsitotelian and Quinean views of metaphysics only proves relevant to signal the fact that a metaphysics of dispositions can be seen as the ontological prerequisite of explanatory theories in the social sciences. Having said so much, let us now turn to dispositions as they

are actually invoked to underpin a realist approach to causal explanation.

2. Dispositions for causal explanation

2.1. Dispositions for causal realism

In somewhat direct contrast to Salmon's and Dowe's process-based disavowal of dispositions, Chakravartty (2007) has argued that the latter are a necessary complement of a sophisticated version of scientific realism, which Chakravartty calls "semirealism". A first-approximation definition of realism would be some variant of the claim that mature and non *ad-hoc* scientific theories describe the nature of a mind-independent world with some accuracy that is bound to increase over time. Typically, such claim is defended on the basis of what is often referred to as the "miracle argument" (or "no-miracles argument"), to which Hilary Putnam (1979) gave the following standard formulation:

> The positive argument for realism is that it is the only philosophy that doesn't make the success of science a miracle. That terms in mature scientific theories typically refer (this formulation is due to Richard Boyd), that the theories accepted in a mature science are typically approximately true, that the same term can refer to the same thing even when it occurs in different theories—these statements are viewed by the scientific realist not as necessary truths but as part of the only scientific explanation of the success of science, and hence as part of any adequate description of science and its relation to its objects. (Putnam, 1979; 73)

The naïvely realist position implied by the miracle argument, however, does not make scientific realism robust with respect to anti-realist criticism. Chakravartty focuses on three lines of criticism, namely, the objection to the validity of the inference at the basis of the miracle argument; the possibility that more than one scientific theory account for reality; and the availability of an equal argument from scientific development that yields opposite conclusions to the miracle argument. The first of these objections insists on the radical difference between *success* and *truth*: success, the argument goes, may have to do with such features of a scientific theory as simplicity, elegance, or coherence, but this holds as no warrant that the successfully accepted theory is the truest description of the portion of the world it takes into consideration. This has to do with the fact that the miracle argument is an inference of what Charles S. Peirce calls an "abductive" type, in which the truth of the conclusion does not necessarily follow from the truth of the premisses (unlike deductive inferences).

A second objection is associated with the thesis of "the underdetermination of theory by data" associated with the names of Pierre Duhem ([1906] 1991) and W.V.O. Quine ([1953c] 1961).[1] In the many variants of the underdetermination thesis, the aspect that most seriously threatens the tenability of naïve realism is the suggestion that two mutually contradictory scientific theories can account for the same piece of evidence, and therefore one cannot conclude from the explanatory power of a theory to its truth. This is often claimed on the

1. Donald Gillies (1993) has argued that Duhem's and Quine's theses differ so significantly that usual reference to "the Quine-Duhem thesis" is "something of a misnomer" (Gillies, 1993; 98). Here we will simply speak of "the thesis of underdetermination of theory by data" or "the underdetermination thesis".

basis of the Quinean argument that no single scientific claim can be refuted by evidence, since individual statements are part of a logically interconnected web of statements—the theory—over which the refutation must be redistributed. It follows that one can always, at least in principle, readjust a refuted theory through a modification of auxiliary hypotheses so as to make it consistent with the recalcitrant evidence. At a minimum, therefore, for any theory that is supported by evidence one can always produce a rival theory that is equally compatible with the same body of evidence. Yet even if one brackets this extreme and somewhat controversial part of the argument, one can see that the backbone of the thesis of the underdetermination of theory by data lies once again in the fact that the truth of a theory cannot be affirmed with deductive certainty from any positive explanatory feature of that theory, such as e.g. empirical support by evidence. For the underdetermination thesis rests upon the fact that it would be an inductive fallacy—namely, the fallacy of affirming the consequent—to claim that since a theory entails some evidence, then the truth of that piece of evidence entails the truth of the theory.

Finally, the third objection, much like the miracle argument itself, resorts to an abductive argument from the history of scientific theories only to arrive at opposite conclusions. The history of science, the argument goes, offers a large number of examples of theories that could be deemed successful on any reasonable criteria and yet were not true since, as Boyd and Putnam put it, their central terms did not refer. The crystalline spheres of ancient and medieval astronomy, the humoral theory of medicine, the effluvial theory of static electricity, "catastrophist" geology based on the Biblical tale of the Noachian flood, the phlogiston theory of chemistry, the caloric and the vibratory theories of heat, the vital-force theories of physiology, the electromagnetic aether, the optical aether, the theory of circular inertia, theories of spontaneous generation are all theories that were once well-confirmed and yet we now believe them to be non-referential (Laudan, 1981; 33). What is important to notice is that all of these theories have been abandoned *in favour of* supposedly more referential theories. Indeed, one can take this to make for a revised Kuhnian argument holding that scientific development ultimately consists of shifts from an established scientific paradigm to a newer one that is incommensurable with the former one in that the key scientific terms of each have different and mutually incompatible referents. Accordingly, one has all reasons to claim that no matter how mature a scientific theory is at the present state, it will sooner or later be replaced by a future theory which implies the non-referentiality, and hence the falsehood, of the current one. This argument is often referred to as "the pessimistic meta-induction" to tone down the bleak connotation of Putnam's label "disastrous meta-induction" (Putnam, 1978; 37).

The pessimistic meta-induction, too, is based on the discrepancy between the measure of scientific success and the truth value of a theory. There are certainly many possible responses to the way this argument yields a pessimistic meta-induction, such as restricting the conditions for referentiality or for the maturity of a theory. According to Chakravartty, however, the above objections correctly point to the need for a selective scepticism as a corrective to naïve realism. On this selectively sceptical clause, not all aspects of a successful

and well-confirmed theory can be retained as genuinely referential, since there are good reasons to believe that some scientific claims will not survive over time. The point, of course, is to identify precisely *which* is the referring component of scientific theories. Chakravartty lists two classical responses to this problem: entity realism and structural realism. As their names suggest, the two positions stress the mind-independent reality of different components of a theory, namely, the *entities* the theory describes and the abstract *structures* of such entities. The idea that the entities that are of interest to science are mind-independent is a rather straightforward one, and indeed entity realism can be seen a sophisticated position about the referentiality of scientific terms. The whole point of entity realism is to provide refined criteria to tell genuine and spurious reference apart, such as e.g. the manipulability of entities. Ian Hacking is a notable advocate of such idea, which he expressed in a famous formula about the reality of the electrons as emitted by a polarizing gun: "*So far as I'm concerned, if you can spray them then they are real*" (Hacking, 1983; 23, emphasis in the original).

Structural realism is a much more variegated batch of different positions, primarily because of the different meanings one can attach to the notion of "structure". In its modern versions, structural realism moves from John Worrall's (1989) suggestion, inspired by Poincaré ([1902] 1905), that what is retained when scientific theories are discarded is not their content (i.e. the supposedly mind-independent entities they refer to) but rather their *form* or *structure*, which is represented by the central *mathematical equations* of a theory. So, for instance, when Fresnel's wave theory of light was superseded by Maxwell's electromagnetic theory, reference to an elastic solid ether—necessary for the transmission of wave-like vibrations—was abandoned and the electromagnetic field was introduced as the new central scientific entity for the new theory of light. Yet Fresnel's equations for the relative intensities of the reflected and refracted light beams were taken over completely into Maxwell's theory. The *content* of optical phenomena, in other words, did not remain the same through the "paradigm shift", but the relations between such phenomena—the structure of the theory—were preserved. Similarly, Newton's force of gravity was replaced by space-time curvature in Einstein's relativity theory, but Newton's equations continue to function within the new theoretical framework—albeit describing only "limiting-case" relations such as terrestrial gravitation (Worrall, 1989).

The problem that Chakravartty detects with structural realism is that mathematical equations say nothing about the mind-independent *nature* of the world: they are only informative about their *formal* properties. For Chakravartty this makes structural realism too shallow and weak a proposal for realism. Most importantly for our purposes, structural realism is unable to substantiate realism *about causation*, since a purely structural-realist approach to causation would require for causation to be described as a relation of logical dependence—specifically, a mathematical relation—which we have already seen to be conducive to causal eliminativism and therefore to be no causal realism at all. If the structural component is to be a distinguishing feature of causal realism, this has to be made somehow compatible with a *productive* view of causation. In this respect, it is worth following Chakravartty's line of

argument one step further, since his own proposal for realism will provide one solution to this dilemma in a way that also helps elucidate the specific role dispositions play in causal production.

What Chakravartty calls *semirealism* is a middle-ground position between entity realism and structural realism. Selective scepticism, he argues, allows one to affirm the reality of more than structural realism would allow, namely concrete structures as opposed to merely abstract ones. Instead of merely describing the formal properties of relations between objects, concrete structures are also informative about the nature or quality of such relations. Interestingly, to qualify concrete structures Chakravartty takes the cue from Grover Maxwell's (1970) suggestion that causal connections are fully-fledged structural properties. As Maxwell writes: "Causal connection must be counted among these structural properties, for it is by virtue of them that the unobservables interact with one another and with observables" (Maxwell, 1970; 17). On semirealism, formal relations are grounded in concrete structures that allow for such relations to actually refer to mind-independent interactions between unobservable entities and therefore for realism about structures to become a genuine form of scientific realism. This requires that one makes reference to the properties of things in the world, the relations between which are described by the "concrete structures". This is where dispositions enter the equation. When it comes to causal realism, one needs to assume that causal relations count among—indeed, they simply *are*—those concrete structures that produce the formal relations on which structural realists center their focus. Yet this requires, in order for structures to be *concrete* in any meaningful sense, that these are grounded in the properties of entities, namely their *dispositions* to produce events. As Chakravartty puts it:

> Why and how do particulars interact? It is in virtue of the fact that they have certain properties that they behave in the ways they do. Properties such as masses, charges, accelerations, volumes, and temperatures, all confer on the objects that have them certain abilities or capacities. These capacities are dispositions to behave in certain ways when in the presence or absence of other particulars and their properties. The property of mass confers, *inter alia*, the disposition of a body to be accelerated under applied forces. The property of a volume on the part of a gas confers, *inter alia*, the disposition to become more highly pressurized under applied heat, and so on. It is the ways in which these dispositions are linked to one another—that is, the ways in which particulars with various properties are disposed to act in consort with others—that produce causal activity. Causation, ultimately, has to do with relations determined by dispositions, conferred by causal properties. (Chakravartty, 2007; 41–42)

2.2. Dispositions against Humeanism

Chakravartty's case for a dispositional realism about causation proves particularly appropriate here, since the same conclusions can be arrived at via arguments from the inadequacy of causal Humeanism for scientific explanation and even from the explanatory power of mechanisms. In her early work, Nancy Cartwright (1983) has directly criticized the miracle argument for realism which makes "inferences to the best explanation" that conclude from the explanatory power of a theory to its truth. Explanatory power, Cartwright argues, is by no means conducive to truth. This is because an essential component of explanation is the reduction of causal complexity to its more simple components: the law of universal

gravitation ($F = G\frac{mm'}{r^2}$) is one of the most celebrated achievements of modern physical theory, and rightly so, on accounts of its manifest explanatory power. Yet it does not correctly describe how two bodies actually behave when these are, say, charged particles—since in that case Coulomb's law would interact with the law of universal gravitation to determine the final force. Thus, physical laws like the law of universal gravitation do not state the facts. Such failure to meet the requirement of facticity, however, is precisely what secures their explanatory power, since it is vital for the success of physical science that, say, the final force interacting between two charged bodies be reducible to the composition of two factually inaccurate laws even though neither describes how two charged bodies behave. This means that explanatory power and truth trade off, since genuinely explanatory laws, strictly speaking and in Cartwright's own words, *lie* (Cartwright, 1983).

This also means that the covering-law model is literally false. The laws that feature in covering-law explanation are typically empirical generalizations, yet such generalizations are only true *ceteris paribus*, i.e. other things being equal. Interfering causes, in other words, must be absent for law-like generalizations to hold in every relevant context of applicability without a *ceteris paribus* assumption. Yet such condition of causal homogeneity hardly ever obtains in reality, whose causal complexity—along with the possibility of reducing it—is what makes laws explanatory despite their falsehood. To be sure, statistically established correlations must reflect genuine causal relations in order for them to have any empirical import, but such causal relations only hold with respect to the specific test population where the probability that a certain cause will regularly produce an effect has been actually measured. Yet causal claims need to reach beyond the test population if they are to acquire explanatory power and be of any scientific relevance. Consider the example that Cartwright (1989) derives from Germund Hesslow (1976). Birth-control pills may encourage thrombosis, yet they may also inhibit it by preventing pregnancy, which itself tends to produce thrombosis. If we were to establish a causal nexus between birth-control pills and thrombosis by empirically administering those contraceptives to different test populations, the probability measure of birth-control pills causing thrombosis would vary according to whether the women in the test population are pregnant or nor, and whether or not they have a certain chemical in their body that causes the blood to clot and thereby produces thrombosis. The probability measure can only be the same *ceteris paribus*, i.e. if other causing factors such as pregnancy or the presence of certain chemicals remain unaltered across the test populations (Cartwright, 1989; 99–101).

Statistical correlations, therefore, are true only locally, i.e. relative to the specific context where they have been established. Not only would law-like generalizations from empirical correlations make them false (or true *ceteris paribus*); they will also show that causation is not reducible to regularity, as Humeans would have it. For causal processes are neither local nor homogeneous: birth-control pills can either cause or prevent thrombosis depending on the context in which they act, since they can be responsible for blood clots by favouring the production of certain chemicals *where they would not have otherwise been produced*, or by preventing pregnancies *that would have otherwise occurred*. Conversely, they may well

be causally irrelevant with respect to thrombosis if they are used by pregnant women or by women who already have thrombus-inducing chemicals in their bodies. Causes achieve heterogeneous effects because causal process occur through the interaction of different causes, and therefore they are not amenable to regularities. Indeed, the radical difference between causal processes and regularities also implies that correlations fail to capture the nature of causation even locally:

> What makes the causal law true that [the cause] C causes [the effect] E in [the test population] T is not the increase in probability of E with C in T, but rather the fact that in T some Cs do regularly cause Es. The increase in probability is only a sign of that … (Cartwright, 1989; 144–145)

For Cartwright, this invites a tripartite conceptual distinction that allows not only to differentiate law-like correlations from causal processes, but also causal processes from causal properties. In Cartwright's picture outlined above, causal processes occur only locally: so does, for instance, the causal line leading from the intake of contraceptives on the part of non-pregnant women to a reduced risk of thrombosis via the prevention of pregnancy as a risk factor. Yet the same causal property (the contraceptive efficacy of birth-control pills) may yield a different outcome when acting in conjunction with pregnancy, and yet others depending on the patient's individual predisposition to blood clots. Accordingly, causal properties ought be kept distinguished from causal processes. Crucially, Cartwright thinks of those causal properties in terms of *capacities*, a term she chooses in order to indicate the enduring and stable tendency to bring about a certain outcome carried by properties: relations like "Aspirins carry the capacity to relieve headache" indicate that the properties of aspirin carry the capacity to inhibit the enzymes responsible for headache.

Cartwright refuses to discuss the nature of such capacities any further, her goal being to investigate the empirical method of science and to defend the thesis that causal claims are best understood in terms of ascriptions of capacities. "Does this mean", she asks, "that there are not one but two properties, with the capacity sitting on the shoulder of the property which carries it? Surely not. However, I cannot yet give a positive account of what it does mean … My aims … are necessarily restricted, then: I want to show what capacities do and why we need them" (Cartwright, 1989; 9). Nonetheless, it is safe to conclude that capacities clearly have a dispositional character. Although they are meant to be synonymous with what Mill calls "tendencies", Cartwright recognizes the similarity between what she terms "capacities" and what others call "propensities" or "powers" (the difference being that propensities are inherently probabilistic and that powers are ascribed to individuals rather than to properties; Cartwright, 1989; 9). Elsewhere, the notion of "capacity"—itself a possible translation of *dýnamis*, Aristotle's word for "potency"—is associated with Aristotle's notion of "nature", except that the indirect appeal to natures (via capacities) in scientific explanation drops the Aristotelian concern with natures being essences and that, once again, capacities-natures are not assigned to individual substances but to collections or configurations of properties, or *structures* (Cartwright, 1999; chapter 4). The latter distinction is required for Cartwright precisely in order to account for the heterogeneous outcomes produced by capacities as opposed to the homogeneity of causal processes. Consider again the interaction between

two charged bodies. As we have seen, while such interaction will causally produce a certain force that results from electromagnetic and gravitational forces, the outcome will change depending on whether or not the bodies are charged. The capacity to bring about a certain outcome, therefore, must be ascribed to the property of being charged, and not to the body itself.

While the ascription of capacities to properties rather than to individual entities and the conceptual distinction between powers and capacities seem to echo the neo-Aristotelian metaphysics of grounding as reflected in Bird's distinction between fundamental powers and non-fundamental dispositions, Cartwright makes clear that capacities alone are required to make sense of causal explanation in science. Moreover, Cartwright's structural understanding of capacities as assigned to properties converges largely with Chakravartty's conceptualization of scientific realism in terms of "concrete structures" grounded in the dispositional properties of entities. Nonetheless, Cartwright resolutely draws a distinction between capacities and dispositions, in that dispositions "are tied one-to-one to law-like regularities", while capacities "are not restricted to any single kind of manifestation. Objects with a given capacity can behave very differently in different circumstances" (Cartwright, 1999; 59). This is indeed a further step towards the qualification of the underlying metaphysics of causal realism, yet before moving on it is worth pausing to consider a further aspect of Cartwright's philosophy of science.

In order to account for how laws are produced from capacities, Cartwright introduced the notion of a "nomological machine". A nomological machine is "a fixed (enough) arrangement of components, or factors, with stable (enough) capacities that in the right sort of stable (enough) environment will, with repeated operation, give rise to the kind of regular behaviour that we represent in our scientific laws" (Cartwright, 1999; 50). Kepler's laws, for instance, describe the regularities of planetary motion that are produced by such a nomological machine whose components are the sun, characterised as a point-mass of magnitude M, and the planet, a point-mass of magnitude m, orbiting at a distance r and connected to the former by a constant attractive force directed towards it (Cartwright, 1999; 51). For Cartwright, the modelling activity of mathematical sciences like physics and economics can be viewed as directed towards constructing blueprints for nomological machines, such as the aforementioned planetary model for Kepler's laws devised by Newton or the economic model for the persistence of high rates of unemployment, which is based on assumptions about the fixed arrangement of the constitutents of the corresponding nomological machine (loss of skills during unemployment, number of unemployed workers, number of jobs available at a given time, cost of opening a job).

Although Cartwright does not quite put it this way, nomological machines patently fulfill the demand for causal mechanisms as made by advocates of mechanism-based explanation. The analogy with mechanism-based thinking is not limited to the mechanical metaphor used to describe the production of laws from capacities in terms of the inner workings of a "machine". Rather, the use of mechanical language appears in its turn to be justified in the light of a mechanism-based conception of causal production as being generated by a

relatively stable arrangement of real, if unobservable, entities whose properties are thus disposed to regularly generate a certain effect.

2.3. Mechanisms and dispositions

That dispositions are an inherent feature of mechanism-based explanation is an aspect that has been brought to light by much mechanism-based philosophy of science, most notably in the Harré-Madden-Bhaskar tradition. Seeking to underwrite causal claims with other than Humean regularities, Harré and Madden (1975) contend that *necessity* rather than regularity is the truth-maker of the conditional statements that analyse causation in terms of counterfactuals. For them, to oppose the regularity view means to argue that there is something more than regularities and to accept that necessity is a feature of the world. Harré and Madden's (1975) notion of necessity, however, is a *natural* necessity quite distinct from the nomological realism of the Dretske-Tooley-Armstrong view: natural necessity is of a lawless kind that does not require for the world to be law-governed. As mentioned, for Harré and Madden necessity underwrites causal production, yet if it is not laws that govern it, something else is required. Harré and Madden's concept of "generative mechanism" fulfills precisely this role. Generative mechanisms are constituted by powerful particulars—the relevant causing entities—that produce a series of events and therefore bind together into a cause-and-effect relation what would otherwise be Humeanly unrelated events. To be sure, within this framework is the powerful particulars that do the causing, and it is with them that natural necessity lies. Harré and Madden's theory of causal powers serves to explain how properties can be causally necessitating in a non-deterministic (*qua* lawless) way. For causal powers to constitute a theory of *natural* necessity, the ascription of such powers to entities should allow for them to be instantiated in certain conditions while remaining uninstantiated in others. This means that the conditions for causal production cannot be assigned entirely to entities: it is generative mechanisms that determine the conditions for their production. This also means, however, that Harré and Madden's "causal powers" clearly have a dispositional character, in that they do not qualify the intrinsic nature of entities as much as their properties.

Harré and Madden's work undoubtedly acted as a major influence on Roy Bhaskar's ([1975] 2013) seminal treatment of causal powers and mechanisms in scientific explanation. Like Harré and Madden, Bhaskar straightforwardly identified mechanisms with causal powers in that they amount to nothing but "the ways of acting of things" (Bhaskar, [1975] 2013; 3). Yet Bhaksar's theory of mechanisms is the conclusion he draws from what he calls a "transcendental" argument for realism, i.e. his response to the pseudo-Kantian question: what must the world be like for science to be possible? According to Bhaskar, the traditional empiricist answer lies in a Humean ontology entirely exhausted by atomistic facts and their correlations. Yet for Bhaskar this answer is rebutted by the very premisses of his transcendental question, namely, that scientific explanation be possible and, as a corollary, that the experimental activity of science be intelligible. Experiments, he argues, require

almost by definition that the experimenter be able to interfere with the pattern of events forthcoming in the laboratory, yet those who identify causation with empirical regularities fail to take this into account. For reducing causation to regularities rules out the interfering action of the experimenter, and, as a consequence, provides no guarantees that such causal relations will hold outside the context of experimental conditions.

This leads Bhaskar to conclude that causes can only be reduced to constant conjunctions in a deterministic world that may very well correspond to the "closed systems" assumed under experimental conditions, but not to the fairly uncontroversial and common-sense picture of a natural world in which natural phenomena can be interfered with (Bhaskar, [1975] 2013; 23–26). Conversely, the requirement that causal explanations remain true in "open systems" involves for Bhaskar making "reference to *causal agents*; that is, to things endowed with causal powers" (Bhaskar, [1975] 2013; 40, emphasis in the original). Thus, the answer to the transcendental question, what must the world be like for science to be possible?, would read something along the following lines:

> The world consists of things, not events. Most things are complex objects, in virtue of which they possess an ensemble of tendencies, liabilities and powers. It is by reference to the exercise of their tendencies, liabilities and powers that the phenomena of the world are explained. Such continuing activity is in turn referred back for explanation to the essential nature of things. (Bhaskar, [1975] 2013; 51)

It is such things that constitute the basic causal structure of the world and that Bhaskar calls "mechanisms" on accounts of their being causally generative.[2]

Crucially, within this framework mechanisms are understood in quite realist terms as the most basic level of a stratified ontology. Like Harré and Madden, Bhaskar argues that the idea that mechanisms correspond to the causal powers of entities also entails that they need not be actualized in order for them to be real. As he puts it:

> It remains true to say of a Boeing 727 that it can (has the power to) fly 600 m.p.h. even if it is safely locked up in its hangar; just as it would remain true to say of a person that he could (would be liable to) get hurt if he happened to be in the way of a herd of stampeding buffaloes, even if as a matter of fact we knew he had no intention of ever going to the Prairies. (Bhaskar, [1975] 2013; 77)

Unlike Harré and Madden, however, and more in tune with Cartwright's tripartite distinction among capacities, causal processes, and *ceteris paribus* laws, Bhaskar concludes that in

2. In the preceding and forthcoming discussion, I am deliberately setting aside the question of "emergent properties". In social ontology, the properties that generate—or materialize into—the mechanisms that produce social phenomena are often ascribed to individuals, or social actors; however, from a metaphysical point of view the question arises as to whether individuals can be said to constitute the "building blocks" of the social world. Those who, like Bhaskar ([1979] 2015), defend the idea of "emergent properties" typically conceive of the properties of individuals as emergent from their micro-structural properties but non-reducible to them—and perhaps of the properties of social structures (e.g. institutions) as emergent from the properties of individuals (see e.g. Archer, 1995; Lawson, 2003; Elder-Vass, 2010; Sayer, 2010). Reductionists, on the other hand, deny emergentism and claim that all allegedly "emergent" properties can be reduced to the properties of the basic constituents of the (social) world. While reductionism of the agentive properties of individuals to their physical (neurobiological) properties is rather unpopular in the social sciences, reductionism with respect to the seemingly autonomous properties of social structures and social "wholes" is the bread and butter of much methodological individualism. The debate on emergentism, however, does not affect the present discussion on the nature of properties *qua* causal properties.

order for the causal powers of which mechanisms consist to be uninstantiated, they must be distinct not only from the experiences that allow for them to be identified under experimental conditions, but also from the very patterns of events that they are capable of generating. What Bhaskar ([1979] 2015; 14) would later call nature's "ontological depth" thus presupposes a threefold ontological layering as represented in the following table:

	Domain of Real	*Domain of Actual*	*Domain of Empirical*
Mechanisms	✓	✓	✓
Events		✓	✓
Experiences			✓

Table 3.2: Bhaskar's overlapping domains of reality. (Bhaskar, [1975] 2013; 46)

Bhaskar's transcendental argument for realism as well as his stratified ontology are among his most original contributions to the philosophy of science debate, yet some commentators noted that these ideas have raised "philosophical suspicions and doubts about their relevance for the social sciences", especially since Bhaskar's followers "have largely tended to repeat or reformulate often quite cryptic original statements by Bhaskar instead of engaging with recent developments in the philosophy of science or using the ideas to explain important social facts" (Hedström and Ylikoski, 2010; 57). But while there is more than a kernel of truth in this contention, the relevance of Bhakar's power-based account of causal mechanisms can be more fully appreciated by situating it within the bigger picture of a theory of dispositions understood as the necessary metaphysical complement to the causally productive account of mechanism-based explanation in the social sciences.

3. A metaphysics for dispositionalism

In distinguishing between the causal powers that constitute a mechanism from the patterns of events that they are capable of generating, Bhaskar also carefully distinguishes between the causal powers and the *natural tendencies* of a thing. Tendencies, Bhaskar argues, are "something more" than powers (Bhaskar, [1975] 2013; 222), namely necessity. In Bhaskar's example, "All men [sic] (living in certain kinds of societies) possess the power to steal; kleptomaniacs possess the tendency to do so" (Bhaskar, [1975] 2013; 222). It is easy to see that tendencies rather than causal powers are what true law-like statements are about, since the realization of tendencies implies the closure of the system in which they are realized, i.e. a necessary connection between events. As Bhaskar puts it, tendencies as occur in tendency statements presuppose power statements, but the converse is not the case. For tendency statements presuppose that a power is *necessarily* realized. The distinction between powers and tendencies pinpoints the distinction between the "level of actual" and the "level of real", since it is the natural necessity of tendencies that underwrites the pattern of events that are revealed in experimental conditions. Not unlike Harré and Madden, Bhaskar takes mechanisms to generate outcomes by natural necessity, i.e. the causal powers of things

manifest themselves in a generative mechanism that is entirely defined by the specific outcome it necessarily produces. This also means that for entities to be endowed with causal powers even when those powers are not realized, there must be a real basis for the possession of powers independent of whether they are exercised or not. Such basis Bhaskar identifies in the *structure* of entities: as he puts it, "To say that X has the power to do ϕ is to say that it will do ϕ in the appropriate circumstances in virtue of its nature (e.g. structure or constitution); that is to say it will do it in virtue of its being the kind of thing that it is" (Bhaskar, [1975] 2013; 229).

Bhaskar's reference to *kinds* of things implies that structure is a *categorical* property of entities, i.e. it can be ascribed to an entity by virtue of it being the kind or category of entity it is. Indeed, things are of the kind they are because of their structure. Hydrogen atoms are anything possessing a particular atomic structure, and it is such structure that gives them the causal powers to bind with oxygen atoms to form water molecules. Bhaskar explains the categorical nature of structures in terms of a transition from a "Lockean" level of scientific knowledge, at which it is entirely contingent whether a thing has the kind of structure it has, to a "Leibnizian" level of knowledge, at which it is no longer contingent—i.e. it is necessary—that X has the structure it has if it is to be the kind of thing it is (Bhaskar, [1975] 2013; 164). It is easy to see that the distinction between a Lockean level and a Leibnizian level corresponds quite neatly to the recurring distinction between the "non-fundamental" level of dispositions and the "fundamental" level of powers. In fact, by allowing for the categorical properties of structures to ground the dispositional properties that entities have in virtue of their causal powers, Bhaskar seems to invoke structures as part of a fundamental metaphysics of grounding.

The appropriate relation between categorical and dispositional properties is a widely discussed topic in the literature of dispositions, and scholars in dispositional metaphysics have endorsed a variety of different positions about the ultimate nature of the properties of entities. Anecdotally, one may note that in the first half of the twentieth century dispositions were discussed by the likes of Popper ([1934] 2002; section 25; appendix *x) and Carnap (1936; 440) in the passages where they focus on the ordinary terminology at use in scientific explanation. It is only in the second half of the twentieth-century, however, that dispositions became a respectable topic of discussion in (analytic) metaphysics thanks to the resurfacing of (neo-)Aristotelian themes. Psillos (2002) attributes this to the resurgence of the idea of that necessity is to be found in the nature of things (that is, in their *essences*). For him, the Aristotelian idea of *natural necessity* banned by Humeans gained new life when the regularity view started to face serious metaphysical challenges:

> It was Kripke's liberating views in the early 1970s that changed the scene radically. By defending the case of necessary statements, which are known a posteriori, Kripke (1972) made it possible to think of the existence of necessity in nature which is weaker than logical necessity, and yet strong enough to warrant the label *necessity*. (Psillos, 2002; 161, emphasis in the original)

Be this as it may, since the 1970s powers and dispositions came to the fore as the central focus of new trends in metaphysics. Stephen Mumford (2009) usefully describes such trends as

originating in different traditions that developed in relative isolation from one another. Harré and Madden's (1975) influence on Roy Bhaskar ([1975] 2013, [1979] 2015) has already been shown, and while they may have also exerted some influence on Nancy Cartwright's work on capacities (see e.g. Cartwright, 1999; 73; 1989; 9, note 15), she developed her theory of capacities and their role in causation and explanation relatively independently, taking a cue from J.S. Mill's notion of "tendencies". D. Hugh Mellor (1974) can also be counted as a member of this mainly British tradition though he essentially provides an independent defence of pandispositionalism against those authors who argued that dispositional predicades must rest on non-dispositional properties.

A different tradition originates in Martin's (1994) and Molnar's (2003) joint effort in establishing a dispositional theory of causation while teaching at the University of Sydney in the 1960s (Mumford, 2009). More recently, Brian Ellis (2001, [2002] 2014) has breathed new life into this Australian tradition, though it is doubtful whether such a lineage can be established beyond a merely geographical continuity (Ellis himself has credited contributions to the position he himself defends to "philosophers from around the world": see Ellis, [2002] 2014; 7). In fact, recent debate on dispositions has involved a cross-fertilization that also brought the Australian tradition to bear on the European one (see e.g. Mumford and Anjum, 2011) as well as on the scholarship that more recently took hold in the U.S. (see e.g. Martin and Heil, 1998; Heil, 2005). Given the large number of authors who have significantly contributed to the debate, therefore, it would be pointless to rubric them all, let alone trace a detailed genealogy. Indeed, more often than not disposition talk resorts to arguments previously advanced by more classic authors, including not only Karl Popper, Rudolf Carnap, and even Carl Hempel, but also Gilbert Ryle, Nelson Goodman, W.V.O. Quine, J.L. Mackie, David Armstrong, Saul Kripke, Sydney Shoemaker, and many others.[3] Moreover, discussion of the nature of dispositions is not always clearly distinct from arguments for their use in causal explanation of the kind considered in the previous section.

It will prove more useful, therefore, to avoid reference to the particular arguments for this or that position on the nature of dispositional properties and attempt instead to provide a taxonomy of different theories that will help investigate the metaphysical presuppositions of the form of causal realism considered so far. George Molnar (2003) suggests that a basic distinction about the reality of properties is that between *monists* and *dualists*. Monists believe that all properties are either of a categorical or of a dispositional type, while dualists are committed to what Bird (2007) calls "the mixed view", according to which dispositional and non-dispositional properties are inherently different, and that instances of both types of properties exist. As Bird points out, the three views that result from this classification exhaust the whole spectrum of possible accounts of the nature of properties as far as dispositions are concerned, in that other internal differences among theories of dispositions can effectively be ascribed to differences in the characterization of the causal relations they

3. Hempel (See e.g. 1965b), Ryle ([1949] 2009; ch. 5), Goodman ([1954] 1983; ch. II), Quine ([1935] 1976, [1953b] 1961; [1960] 2013; ch. 6, sec. 46; 1969; [1970] 1986; ch. 2; [1973] 1990; ch. 1, sec. 4–5; 1975, 1978), Mackie (1962, 1977), Armstrong (1968; ch. 6, sec. 6; 1973; ch. 2, sec. 2), Kripke (1972), and Shoemaker (1980, 1998). Some of these contributions have been separately collected in Tuomela (1978).

result in. Accordingly, one must also recognize that neutral monism, which Molnar (2003) lists as a third monist option alongside categorical and dispositional monism, can in fact be successfully reconduced to one of the other two. Neutral monism as advanced by e.g. Heil ([1998] 2013) and Mumford (1998) sees the distinction between dispositional and categorical properties as a merely conceptual and epistemological one, not one that has to do with the nature of properties. As such, however, neutral monism has nothing to say about the metaphysics of properties, and it must rest upon one of the remaining three alternatives even though it remains silent as to which one is best suited to accommodate it (Mumford has recognized the validity of this objection and has later rejected neutral monism in favour of dispositional monism; see e.g. Mumford, 2004). The alternative metaphysical accounts of dispositions can thus be typified as follows:

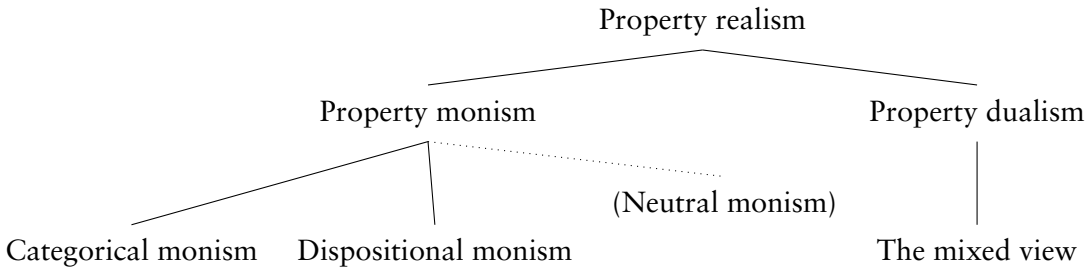

Figure 3.1: Theories of properties (adapted from Molnar, 2003; 149).

Bird notes that categorical monism is a wholly Humean view, in that it denies that there are genuinely dispositional properties and therefore undermines a productive account of causation from a regularity-based point of view. This claim requires further qualification, since the Dretske-Armstrong-Tooley view examined above can also be counted as categorically monist on accounts of its view of laws as relations between the exclusively categorical properties of universals, while at the same time seeing such relations as necessitarian rather than regularity-based. For this reason, Bird calls this view "semi-Humean" but he sees no problem in calling it Humean since the nomic necessity defended by Dretske, Armstrong, and Tooley is by definition one that holds between *kinds* of entities rather than between entities themselves. Whether an entity belongs to a certain kind is, on the Dretske-Armstrong-Tooley view, an entirely contingent fact, and therefore relations between the properties of entities are contingent on those entities being the kind of entities they are: they are, in other words, based on regularities. To be sure, one may require that necessary relations hold between the properties of individual entities rather than between kinds, yet this is precisely what categorical monism denies: to conceive of an individual property a as necessitating the fact that an entity that possesses the property a also possesses the property b is to conceive of a as a dispositional property. Thus, Bird concludes, either categorical monism falls back to a fully Humean view or it is no categorical monism at all and therefore it must be an anti-Humean form of realism about dispositions, namely dispositional monism or the mixed view (Bird, 2007; ch. 4, sec. 5).

At any rate, doubts about the relevance of the Dretske-Armstrong-Tooley view for

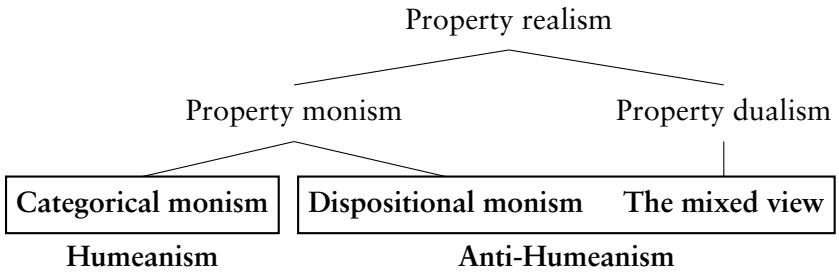

Figure 3.2: Humean vs. anti-Humean theories of properties.

providing a theory of causation capable of accounting for explanation in actual social sciences have already been expressed in the previous chapter. We are thus left with only two viable metaphysical positions that can qualify as anti-Humean by virtue of their realism about dispositions: dispositional monism and the mixed view. As Molnar (2003) notes, mixed-view dualism about properties can come in two flavours, which he calls "dualism of pure types" and "dual-type theory". On the first view, properties are pure types, i.e. they are either wholly dispositional or wholly non-dispositional. This is the view held e.g. by Bhaskar when he claims that entities have both causal powers and natural tendencies, the latter being purely structural and non-dispositional properties.

The second view denies pure types, holding that some or all properties have a dual character, i.e. they are partly dispositional and partly non-dispositional (Molnar, 2003; 149–153). Molnar specifies that the dual-type theory is "a view that may not be held by anyone", since its only recorded proponent, C. B. Martin (1993b; 15; 1993a; 46; 1996; 132–135), has since abandoned it (see Molnar, 2003; 149). However, Cartwright might be seen as gesturing towards such a position—though reluctantly and only obliquely—when she vehemently denies that there are "not one but two properties, with the capacity sitting on the shoulder of the property which carries it" (Cartwright, 1989; 9).

To exemplify the dual character of properties Martin repeatedly evokes the metaphor of the extension of a body, in that it expresses itself both in a purely qualitative size as well as in a shape that affects how the body interacts with other bodies (a round body may be said to have a disposition to roll). This, however, immediately raises the question as to where the difference between the dispositonal and non-dispositional character lies: since the dispositional and the non-dispositional are two faces of the same property, they cannot be themselves two different properties. Addressing this problem requires introducing a third dimension to the metaphysics of properties that cuts across the monism/dualism divide. At last, this is the problem of *grounding*. Typically, what Molnar calls "pure-type" dualists think of dispositional properties as grounded on categorical properties. In this way, pure-type dualists are able to account for an entity being disposed to produce certain effects in terms of that entity possessing certain categorical properties because it is the kind of entity it is. This is the way in which Bhaskar, among others, accounts for the dispositional character of causal powers: entities necessarily acquire certain "causal powers" in virtue of their structure, which is a categorical property that derives from those entities being the kind of entities they

are. Barring countervailing factors, hydrogen atoms will necessarily bind with oxygen atoms to form water molecules because they have a certain atomic structure that endows them with the causal power of binding with oxygen, i.e. because they are atoms of the hydrogen type. To be sure, this solution raises problems of its own, since allowing for entities to derive their dispositional properties from their categorical properties would reduce their whole dispositional power to categorical properties, while making dispositional properties inert and perhaps even otiose (even though there is nothing necessarily problematic with this conclusion: see e.g. Prior, Pargetter, and Jackson, 1982). Most importantly, however, the grounding solution is not a viable one for dual-type theorists, since they allow for properties to be both dispositional *and* categorical, and therefore cannot allow for dispositional properties to be grounded on categorical ones. Therefore, one may object to the dual-type theory that it inevitably faces an inescapable problem of the "missing base". Molnar offers three possible counter-moves to this objections, yet they all amount to purely epistemic solutions and make dual-type theorists remain agnostic as to the metaphysical grounds of the duality of types (Molnar, 2003; 152–153).

Bird notes that the mixed view and dispositional monism "are not so far apart", though he says so primarily because both views argue against categorical monism (Bird, 2007; 3). Dispositional monism and mixed-view dualism do in fact pursue different goals, in that the mixed view needs to ground dispositional properties onto categorical ones to rule out categorical monism. Yet this does not mean that the problem of grounding is alien to dispositional monism. For even in this case, one should provide an account of what dispositional properties derive their dispositional character from. By definition, dispositional monism holds that all properties are dispositions, and therefore the dispositional character of properties cannot derive from their being grounded on categorical properties: it must be other dispositional properties that ground them. Unlike the mixed view, dispositional monism is thus open to infinite regress, since it cannot invoke categorical properties to terminate a potentially endless chain of grounding. Luckily, however, the problem of grounding can be set aside for the present purposes, having been evoked solely to point to the similarities between different varieties of realism about dispositions. As Chakravartty (2008, 2013) notes, once dispositions are accepted as real features of entities one is legitimately entitled to account for causation without making it supervenient on regularities, and therefore one is perfectly in the position to provide a realist theory of productive causation capable of underpinning scientific explanation. As a result, the dispositional realism that is required for scientific realism is indifferent to whether the properties of entities are dual or singular, insofar as one is able to mention real dispositional properties as the causal underpinnings of explanation. To use Bird's classification, one is only required to be anti-Humean, no matter whether dualist or monist, to rule out categorical monism as conducive to causally eliminativist Humeanism. Note further that this does not make causal realism metaphysics-less as far as entities are concerned. As Chakravartty has argued, dispositional realism entails more than merely structural realism would allow. For dispositional properties, *qua* properties *of entities*, carry information about their otherwise unobservable structure. We are thus equipped to outline

the characteristics of entities as they feature in scientific explanation, or in other words, to provide a minimal theory of scientific objects according to the desiderata of scientific explanation.

4. The identity of scientific objects

What, then, must scientific objects look like? We have seen at the end of the previous section that the grounding of dispositional properties is irrelevant to their nature. This is what distinguishes scientific objects from the unobservable entities of metaphysics. For scientific objects are defined by their occurrence in scientific theory, whose foremost goal is explanatory: the only relevant properties for an object to be scientific are its causal properties. Insofar as dispositional properties are the roots of causal production, therefore, the nature of a scientific object is entirely exhausted by the dispositional properties that can be ascribed to it irrespective of their grounding. Within this framework, what Chakravartty calls the "concrete structures" that the causal properties of entities compose are all that is required to counter the weak causal realism encapsulated in the Poincaré-Worrall version of structural realism.

Arguably, Worrall's structural realism is by and large an *epistemic* position concerning the definition of what is knowable rather than a *metaphysical* claim about the structure of reality. Standard (epistemic) structural realism, then, amounts to the weakly realist claim that all we can know is the structure of the world (Ladyman, 1998; Psillos, 2001). Drawing primarily on the philosophy of physics, many have turned structural realism as an epistemic position into a radical metaphysical position, to which French and Ladyman (2003) gave the name "*ontic* structural realism (OSR)" by which it is generally known. According to OSR, structures are all there is to reality. To be sure, these are what Chakravartty refers to as *abstract* structures, since concrete structures are by definition composed of the properties possessed by entities. OSR, on the contrary, is explicitly eliminativist about entities: what epistemic structural realism considers simply unknowable, OSR crosses out as altogether non-existent. Many distinct arguments have been provided for this somewhat counter-intuitive thesis, yet the kernel of OSR is to provide a metaphysics that is consistent with the conceptual parsimony characteristic of structural realism. If restricting our epistemic commitments to the structure of phenomena is required to avoid the anti-realist pitfalls of the pessimistic meta-induction, the argument goes, then either a consistently realist position concludes from this that only structure is real, or it is no realism at all.

As one can see, postulating the existence of "concrete structures" is robust enough a move to avoid eliminativism about objects. The objection to OSR from the point of view of causal realism is that OSR presupposes causal relations without relata, while so far causal realism has been discussed precisely on the basis of concerns about the inherently productive character of causal processes which requires for causation to be thought of as a structural property of entities. Indeed, one may go so far as to say that concrete structures are all is needed to underpin causal realism. This is because, as mentioned in the preceding section, to

go further than this is to address the problem of grounding of causal properties, on which causal realism can provide no guidance. If categorical properties are properties in their own right, i.e. if kindhood is a real feature of the world, then such structures clearly prove to be categorical in nature. Yet this need not be the case, and whether or not concrete structures are dispositional or categorical is irrelevant to the causal productiveness of dispositional properties. Rather, to address this problem is to address the question of the primitive identity of properties.

Nonetheless, some have argued that argue that dispositionalism is not especially problematic for OSR (see e.g. French and Ladyman, 2011). Ontic structural realists can certainly concede that the relata of causal relations are objects, provided that these are not essentialized and continue to be understood as mere "nodes" in a structure. To be sure, this paves the way to an infinite regress of reduction from relations to relata to relations. Insofar as dispositions are required to ground causation and put an end to such regress, however, ontic structural realists would ascribe dispositions to structures rather than to objects. As French and Ladyman put it:

> It may be objected that this threatens to lead to circularity: causal relations hold between causal relata which are resolved into objects which are themselves nothing but "nodes" of (whatever that means) sets of causal relations. The worry dissipates if we think of levels of metaphysical analysis, but ultimately as far as the structuralist is concerned, there is nothing but the structure – that is, the set of relations – and leaving aside the issue of relations without relata, there is the significant concern about how relations can have in whatever sense causal powers. But it is unclear why this should be more of a problem for the structuralist than the non-structuralist who, ultimately, must hold that objects, or more basically, bare substances must have causal powers. (French and Ladyman, 2011; 37)

It should be remarked that OSR is intended as an account of the nature of structural relations at the level of fundamental physics, and therefore its eliminativism about objects need not be extended beyond the domains of non-relativistic quantum mechanics, of relativistic quantum field theory, and of the general theory of relativity. At a very minimum, however, the compatibility between dispositionalism and OSR points to the fact that dispositionalism alone is not sufficient to pinpoint the identity of objects, and that the objection that causation requires relata is not especially problematic for object-eliminativism.

Against most ontic structural realists, however, Michael Esfeld (2004; see also Esfeld and Lam, 2008) has defended a more "moderate" version of structural realism than OSR whose main feature is the denial of ontological primacy to relations with respect to objects and the rejection of the eliminativism about objects that derives from the ontological primacy of relations. Moderate ontic structural realists observe that OSR ultimately draws its strength from the fact that current fundamental physics does not make the intrinsic identity of objects available, and therefore the *haecceities* or primitive identities of objects need not be included into the metaphysics of OSR. Nonetheless, the argument goes, this licences no conclusion as to whether relations are metaphysically prior to objects, let alone to objects being reducible to mere "nodes" of relations. Accordingly, moderate OSR defends a metaphysics of objects that does not require appealing to the "essences" of individual entities—i.e. their *haecceities*—to distinguish them from relations at the metaphysical level. More precisely, moderate ontic

structural realists argue that the distinction between objects and relations is a conceptual rather than a metaphysical one, and therefore metaphysical primacy cannot be assigned to either (for an updated version of moderate OSR denying the ontological distinction between objects and relations see Esfeld and Lam, 2011). Metaphysically speaking, objects and relations are on the same footing, and it follows from this that neither term can be suppressed by appealing to the metaphysical primacy of the other, as object-eliminativists contend.

Esfeld and Lam (2011) go so far as to argue that OSR entails a positive metaphysics of objects since, they argue, a theory of individual entities can be provides without evoking *haecceitism*. Flirting with a Spinozian conceptual apparatus, some authors argue that the properties of objects can be conceived as the "modes" in which objects exist, and since objects—like Spinoza's "substance"—do not have any existence in distinction to their modes, one need not draw a distinction between objects and properties (Armstrong, 1989; ch. 5; Strawson, 2008; Heil, 2003; chapter 13; Esfeld and Lam, 2011). (For the sake of basic accuracy, one should note that Spinoza's notion of "substance"—in the singular—makes for an ontologically monist position, while the mode theory of properties applies to a numerical plurality of objects—conceived, in a very much Aristotelian fashion, as "individual substances".) Heil (2003) remarks that viewing properties as modes does not necessarily rule out haecceitism: by way of example, one may cite John Locke as holding such a mode-like view of properties while at the same time positing the existence of an unknowable "substratum", a hypothetical substance that "supports" those properties (Locke, [1689] 1975; particularly bk. II, ch. XXIII). It is also true, however, that Locke conceives of such substratum as a "categorical" or "sortal" substance, i.e. as a substance that underwrites categories or sorts of objects rather than individual ones. Individual substances, on the other hand, are complex modes, or bundles of properties, without anything like an individual substratum: their only "support" is the sortal substratum. Even Locke's categorical essentialism, therefore, does not extend to postulating the existence of haecceities to support his view of objects as modes, or arranged properties. Therefore, whether objects are propertied entities (haecceities plus properties) or simply modes (ways properties are arranged) is, as Heil concludes, an open question, at least as far as the mode theory of properties is concerned (Heil, 2003; 178). This means, at a very minimum, that no further and ineffable essences need to be postulated to make sense of the identity of individual objects insofar as the properties of entities are conceived as modes of their existence.

According to Esfeld and Lam, conceiving properties as modes of existence of objects provides a suitable ground for moderate OSR, i.e. the idea that objects and relations are metaphysically on a par. What moderate OSR needs is reject eliminativism about objects while at the same time avoiding the pitfall of making relations supervenient on objects. For making object metaphysically prior to relations would contradict the very premise of moderate OSR. How, then, can properties alone satisfactorily account for the identity of individual objects? The idea that properties constitute the modes in which objects exist ultimately amounts to a neo-Spinozian variant of the more traditional theories of properties as *tropes* (Esfeld and Lam, 2011; 152; Heil, 2003; 127–128). Conventionally, the birth of

	Supervenient	Fundamental
Haecceitism	Properties	*Haecceities* (e.g. "substrata")
OSR	Objects	Relational properties
Moderate OSR	(Relational) properties *and* objects	"Modes"

Table 3.3: Alternative accounts of the nature of objects.

trope theory is associated with the work of D. C. Williams (1953a, 1953b), though earlier formulations can be found in E. Husserl ([1921] 2001; investigation III), G. F. Stout ([1921] 1930; Moore, Stout, and Dawes Hicks, 1923), and I. Segelberg ([1947] 1999); more recently, trope theory has gained new currency thanks to discussion by K. Campbell (1981, 1990), K. Mulligan (1998), and P. Simons (1994; see also Mulligan, Simons, and Smith, 1984), and critically, by D. M. Armstrong (1978b; ch. 4) and H. Hochberg (1988; 1991; 2002; see Maurin, 2002; 3–4; Heil, 2003; 128).

In the trope-theoretical jargon, "tropes" are "particular properties" as opposed to Armstrong-like "universals". Keith Campbell (1990) illustrates the notion of trope by contrasting examples of *token* properties with the corresponding *type* properties:

> Consider the peas in a pod. They are all very much alike—proverbially so. Suppose in this pod there are half a dozen peas which are all the very same shade of green and all the same near-spherical shape. They differ slightly in size from one another and, of course, have different locations.
>
> Any assay of this situation will have to recognize the presence of colour. But colour occurs here both as type (what all peas have, a certain shade of green) and as token (the particular case of colour which each different pea has, peculiar to itself). There are six tokens of that shade of green present in the pod.
>
> In the same way there are six tokens of the near-spherical shape, but only one case each of the size involved, since in our example each pea is a different size from every other. (Campbell, 1990; 2)

Token properties like particular shades of a certain colour (a type property) or the different sizes in which a shape (again a type property) can manifest itself are examples of tropes. Much like the particular properties of the individual peas in a pod, therefore, tropes need be conceived as real properties—in contrast to Armstrong's theory of universals according to which only categorical properties are real.

Aside from commitment to the existence of tropes, however, what is most distinctive about trope theory is the contention that tropes are metaphysically fundamental and that, as a corollary, tropes are all there is to reality (Maurin, 2002; 5–6). To put it differently, ours is neither "a world of states of affairs" (as per Armstrong's phrase) nor a world of objects; instead, it is a world of (particular) properties. For our purposes, it is relevant to note that this aspect of trope theory typically translates into a theory of objects as consisting

of nothing but bundles of tropes. As Maurin observes, trope theory is in fact compatible with the idea that objects are more than mere compounds of properties or tropes. C. B. Martin (1980), for instance, has advocated the necessity of positing Locke-like substrata to sustain tropes, mainly out of the need to account for the unity of objects: "An object", writes Martin, "is not a collectable out of its properties or qualities as a crowd is collectable out of its members" (Martin, 1980; 8); instead, properties must be properties *of* things, and substrata are required as the bearers of such properties. To put it differently, property-ascription statements like "The passionfruit is round" and "The passionfruit is purple" must be true of one and the same object (Martin, 1980; 9), and the truth-makers of such statements must be the substrata of objects. As Armstrong (1978b) had pointed out, however, to invoke substrata that support properties is to introduce a third, relational term ("support") that must be accounted for metaphysically. This, Armstrong argues, opens the trope-substrate approach to the kind of infinite regress first exposed by Bradley ([1897] 1999; 27–28): if the "support" relation is a real metaphysical entity, then a further relation is required to account for how a substratum relates to it, and so *ad infinitum*.

The trope-bundle approach, by contrast, is capable to overcome Bradley's regress in virtue of its monism about tropes: if all that exists is nothing but bundles of tropes without further concern as to how they coalesce into a coherent whole (if at all), then relations are not part of a trope metaphysics and Bradley's regress is eschewed. Nonetheless, this leaves the question open as to how one can distinguish between an inchoate congeries of enumerated properties and the ordinary, more restricted notion of object to which we attribute a certain *unity*. Many attempts at solving this problem have been proposed (Maurin, 2002, 2010, 2011); typically, solutions go in the direction of invoking some sort of *internal* relation which, unlike *external* relations, is necessitated by rather than added to the presence of its relata. Internal relations, that is, necessary relate what they in fact relate, i.e. "there is no possible world in which the relation exists and relates entities other than those it actually relates or where the relation exists without relating anything at all" (Maurin, 2010; 322). Thus characterized, internal relations are self-sufficient—qua *necessary*: they do not relate in virtue of further relations, and therefore Bradley's infinite regress of relations-all-the-way-down does not ensue.

More importantly for the present purposes, the reduction of objects to bundles of tropes is the reason why some author favour a theory of modes instead of tropes. Heil (2003) believes for instance that objects are distinct entities from their modes, and accordingly rejects the trope-bundle approach on accounts that "[p]roperties are *ways particular objects are*, not parts of objects" (Heil, 2003; 12, emphasis added). Aside from Heil's apparent commitment to underlying essences of objects—the "substrata"—Esfeld and Lam (2011) argue that, insofar as one takes tropes to be relations as well as properties, the trope-bundle theory of objects dangerously approaches OSR's eliminativism about objects. It is for this reason that they characterize moderate OSR as a metaphysics in which objects and relations have equal standing, and although the distinction between the two types of entities is a merely conceptual one, neither type can be fully accounted for in terms of the other. Nonetheless,

the main reason why modes can be presented as a variant of trope theory lies in the fact that both are alternatives to the view of properties as universals. From the point of view of universals, token properties are *instantiations* of type properties: the different shades of green of the peas in a pod are all instances of the same type of properties, "greenness". As Armstrong (1978b) repeatedly points out, one of the main advantages of a the theory that universals are real and that particular properties instantiate them lies in its capacity to provide an elegant solution to the "One over Many" problem already raised by Plato (see e.g. *Resp.;* 595): how is it that distinct particular entities with distinct particular properties bear similarities? The obvious answer from the point of view of realism about universals is that particular properties are all instances of the same kind of property.

The reason why mode and trope theorists are sceptical about the reality of universals, however, lies in the difficulties with the view that particular properties *instantiate* universals. For one thing, instantiation cannot be a relation on pain of a Bradley regress analogous to the one Armstrong himself identified in a trope-substrate approach. If instantiation were a relation, then it would be itself a universal that is instantiated in the particulars; to prevent circularity, one would thus be forced to introduce a further relation of instantiation that allows particulars to instantiate instantiation, and so *ad infinitum* (see e.g. Lewis, 1983; Baxter, 2001; Mumford, 2007). To avoid this problem, Armstrong reformulated his account of instantiation, much like trope theorists, in terms of an internal (i.e. necessary) relation—or, as he also called it, a "fundamental tie" (Armstrong, 1989; 94; see also 2004; 46–48). As he writes:

> I have had a change of heart about the instantiation of universals. In previous work I conceived of the instantiation as a matter of contingent connection of particulars with universals. New work by Donald Baxter, at the University of Connecticut, has made me think that the link is necessary. See his paper "Instantiation as Partial Identity" (2001). I have been convinced by him that what is involved in a particular instantiating a property-universal is a partial identity of the particular and universal involved. It is not a mere mereological overlap, as when two streets intersect, but it is a partial identity. This in turn has led me to hold that instantiation is not contingent but necessary. (Armstrong, 2004; 46–47)

Yet even aside from Bradley's regress, those who are inclined to view properties in terms of tropes or modes tend to more generally regard commitment to the reality of universals as an unnecessary metaphysical complication that is best shaved off with Ockham's razor. After all, both trope and mode theory provide their own solution to the main argument in favour of universals, Plato's One over Many problem. In both variants, properties have metaphysical reality that is not supervenient on the objects that possess them. As Esfeld and Lam (2011) stress, to say that properties and relations are modes, or ways in which objects exist, is to eradicate the metaphysical distinction between objects and properties/relations, which then becomes a merely conceptual distinction. To say that tropes compose objects, on the other hand, is to say that objects with similar features in fact *share the same property*, not just similarities between individually different properties (whether or not a substratum is required to tie them together into a unified whole). Thus, the One over Many problem is solved in a way that utterly entails nominalism instead of realism about universals: as

			Nominal	Real
Realism about universals			• Particular properties (e.g. shades of green) • "Bare particulars" (*haecceities*, substrata) (e.g. "bare peas") • Uninstantiated universals (e.g. "being green")	• Instantiated universals or "states of affairs" (e.g. "The pea is green")
Nominalism about universals	**Mode theory**		• Universals (e.g. "greenness") • Unattributed (particular) properties (e.g. shades of green) • Unpropertied objects (e.g. peas)	• Modes (e.g. peas being particular shades of green)
	Trope theory	**Trope-substratum approach**	• Universals (e.g. "greenness")	• Tropes (e.g. shades of green, sizes) • Substrata
		Trope-bundle approach	• Universals (e.g. "greenness")	• Bundled tropes (e.g. a particular shade of green *together with* a particular size)

Table 3.4: Realism vs. nominalism about universals.

Mumford (2013) put it, "there is no mystery why electrons and other kinds of things behave in a certain way. It is because they behave that way that things are members of that kind" (Mumford, 2013; 17).

To sum up, the mode and trope-bundle theories of objects are two possible complements to the theories of properties that have been considered as providing a metaphysics for dispositionalism: the version of dispositionalism required by the type of realism about causation analyzed above thus entails a theory of particular properties that makes no reference to those properties being tokens of a certain type, or kind. Even when essences are invoked as the "substrata" required to make sense of a theory of objects based on particular properties, they are Aristotle-like "individual essences" that are not conducive to essentialism about kinds. This should come as no surprise, since realism about universals has been deployed by the likes of Dretske, Tooley, and Armstrong to support their nomological realism and the consequent analysis of causation in terms of lawful connections between universals. Conversely, the productive account of causation is a theory about the dispositional nature of particular properties or, as Harré and Madden (1975) put it, a theory of *powerful particulars*, but by no means a theory of kinds.

Particular properties thus appear to be the defining feature of entities that complements the dispositional realism necessary to metaphysically underpin the productive account of causation as demanded by actual social science explanation. Throughout the preceding sections of this chapter, scientific explanation and scientific objects have been referred to without any further qualification as to their applicability to the domain of *social* science. Nonetheless, the very need for a productive account of causation has been advocated on the basis of concerns from mechanism-based explanation in the social sciences, and of criticism to the regularity view of causation as it features in correlational analyses of social phenomena. This should not be read as a form of anti-naturalism, i.e. the thesis that knowledge of the social world is inherently different from knowledge of the natural world, and the equivalent ontological thesis about the qualitative irreducibility of natural and social objects. If anything, the fact that most theories about the nature of causation and of unobservable entities that proved relevant to complement the desiderata of social explanation stem from considerations about the natural sciences should attest to the contrary. However, there is a sense in which some aspects of scientific objects must respond to questions that are peculiar to the social sciences. Recall from chapter 1 that the the various social science disciplines can be thought of as having differentiated from one another by way of the apportioning of the social world into different fields of inquiry. Recall further that such processes prove of great epistemological significance in that such demarcation is consummated through the progressive methodological differentiation, the institutionalization of which comes to define the identity of a particular discipline. This has been labelled, following Mäki (2009), "the issue of scope".

Conclusion

As far as politics is concerned, the issue of scope has been associated with the institutionalization of political science concomitant with the "comparative revolution" of the post-WWII period, and therefore the autonomy of politics understood as a specific domain of investigation has been associated with the comparative method of inquiry. At that stage, the comparative method infelicitously relied on what Sartori denounced as a tendency towards a "conceptual stretching" of classificatory concepts, which he interpreted as built-in into the functionalist definition of the object of politics on accounts of its demand for maximal universality and minimal variability. Yet independently of conceptual streching and its premisses in structural-functionalism, political explanation at the inception of academic political science relied on a covering-law model: comparative analysis was thus carried out as a search for law-like invariances across national contexts. In the light of erosive criticism of the covering-law model of explanation, two distinct but related issues have been raised. On the one hand, the whole idea that methodology alone is both necessary and sufficient ground to demarcate the scope of a science is tightly related to the anti-realist assumptions of covering-law explanation, according to which scientific explanation can be fully explicated by means of purely logical—and hence methodological—criteria. Once these assumptions are rejected, however, one is left wanting for an alternative criterion of demarcation of the scope of a particular science vis-à-vis its neighbouring disciplines. This is the question of the autonomy of politics. On the other hand, and consequently, the demise of a covering-law model in favour of a more realist approach to causal explanation casts serious doubts not only on the possibility of a functional definition of the political system as the object of political science, but also on the meaningfulness of the very concept of "the political system". For the generality of such a concept raises no particular problems insofar as regularities are the focus of scientific investigation. Within this framework, comparative analysis is seen as oriented precisely towards detecting such regularities in order to extract the invariances that constitute the features of the political system. Once the focus is recast on context-specific, really occurring causal processes, however, the object of political explanation needs to be characterized in terms of the causal properties of the relevant entities involved in the production of the outcome at stake. A definition of the object of politics in the abstract, therefore, becomes a much more serious issue to tackle.

With the ground covered so far, we have enough material to outline the conditions for addressing both questions. As discussed in the preceding section, the metaphysics of entities that is required by dispositional realism about causation allows for scientific objects to be defined entirely by their particular properties. Yet, as mentioned, such realism extends to individual entities, not kinds. Scientific objects, therefore, must be seen solely as *individual* objects, since the relevant scientific theory *per se* calls for no metaphysics of kinds. This is no accident, nor should it be seen as an unfortunate limitation of the theory; in fact, there are good reasons for this. The point is that, as mentioned in section 2, whether or not an entity possesses dispositional properties because its kind membership endows it with

certain categorical properties—as property dualists would have it—is irrelevant to their expressing a causally productive behaviour. As Bird stressed, to be a realist about dispositions is to be an anti-Humean, since dispositions are all that is required to reject the regularity view of causation. The demand for kind membership, therefore, cannot descend from the requirements of a theory of causation.

It is easy to show that kinds are just what is required to address the two questions raised above. Let us start with the second question. The brand of causal realism considered so far conceives of regularities as produced by real causal processes that involve the dispositional properties of individual entities. Accordingly, the invariant features of the "political system" as they emerge from comparative analysis can be explained away as supervenient on the causal properties of the individual actors that feature in the political processes taken into consideration. It is those properties, and not the invariances that they produce, that are part of the basic inventory of the world. Yet there is nothing about those properties *per se* that allows for the entities that possess them to be grouped together into a homogeneous class identifiable as "the political system". For there to be such a class, or any other equivalent object of politics, some underlying criterion of kind membership is required. Kinds are therefore needed to provide a unified definition of the object of politics. Coming now to the first question, it is easy to see that the demarcationist pretensions of foundationalist philosophy of science have to be abandoned together with the idea that scientific explanation can be entirely reduced to logical criteria. If scientific explanation is about tracing causal processes as they are produced by the activity of entities, no room is left for a purely methodological foundation of the sciences. Yet if one can grant existence to kinds, there is certainly the possibility of demarcating between different qualities of entities in a way that allows for the scope of a particular science to be defined in terms of kind objects. If the causal theory does not provide a theory of kinds, therefore, one has to look into some other aspect of science. This will be the subject of the next chapter.

Chapter 4

The disordered social

> These ambiguities, redundancies and deficiencies recall those which Dr Franz Kuhn attributes to a certain Chinese encyclopaedia entitled *The Celestial Emporium of Benevolent Knowledge*. In its remote pages it is written that the animals can be divided into: (a) belonging to the Emperor, (b) embalmed, (c) tame, (d) sucking pigs, (e) sirens, (f) fabulous, (g) stray dogs, (h) included in the present classification, (i) that tremble as if they were mad, (j) innumerable, (k) drawn with a very fine camel hair brush, (l) *et cetera*, (m) having just broken the water pitcher, (n) that from a long way off look like flies.
>
> Jorge Luis Borges (1952)

Introduction

This chapter will provide an account of the object of politics that results from the classification of social objects into kinds. Because kinds cannot be grounded onto any universal properties that individual entities share, a non-essentialist theory of kinds is required. In accordance with the current literature on the philosophy of classification, these will be referred to as *cluster-kinds* (or, more simply, *clusters*) as opposed to *natural kinds*. Sections 1 to 4 will explain this difference. Section 1 will introduce the idea of natural kinds with respect to the theory of properties discussed in the previous chapter, while section 2 will clarify their place in classification-building: natural kinds are held to mirror real divisions in reality, and therefore the type of classifications they give origin to can be described as *taxonomies*. By "carving nature at its joints", taxonomies identify well-ordered hierarchies in which the properties of objects that are included in a given class are necessarily and sufficiently shared by members of subordinate categories.

Sections 2 and 3 will relate traditional law-based conceptualizations of the political domain as a scientific object to this logic of classification-building: because the properties

that define higher-order classes of objects are shared by all members of subordinate categories, one can arrive at classifications of political objects by identifying invariant features of political processes across time and space. Because of the anti-realist character of such essentialism about kinds, however, political concepts are best understood in terms of clusters of properties rather than natural kinds. Section 4 will explain how political concepts—including the overarching concept of politics as co-extensive with the scope of political science—do not include the individual properties of the members of the classes that such concepts define; rather, they are defined by "family resemblances" between those properties. The role of family resemblances in classification-building and political concept-formation will be explored in section 5.

Section 6 will draw some conclusions from the preceding discussion. Specifically, the identification of family resemblances will be shown to be dependent on stipulations on the part of the inquirer about the similarities among individual properties. This gives rise to a possibly indefinite number of divergent but equally valid classifications of social objects that result in different conceptualization of politics and therefore in different definitions of the scope of political science. A possible solution to this problem will be discussed in chapter 5.

1. Properties and natural kinds

The previous chapter ended with a brief discussion of the role of *kinds* in the light of a metaphysics of individual objects conducive to a nominalist approach to *universals*. Although kinds and universals are tightly related notions, the different positions concerning their respective metaphysical status ought not be confused with one another. By way of clarification, it will prove useful to draw a distinction between *realism about universals* and *essentialism about kinds*. Realism about universal predicates is undeniably a background assumption of those who believe that the reality of *kinds* of entities goes beyond the mere collection of its members. Any kind is defined by the properties its members share, and therefore to argue for the metaphysical reality of a kind is to argue for the reality of its defining properties over and above the entities that possess them—i.e. the reality of universal predicates. This is a fairy anti-nominalist position about universals. For classificatory purposes, however, we typically need to be able to detail precisely which these properties are. Patently, this lies without the scope of realism about universals. For theories of universals are metaphysical theories about the nature of properties, not ontologies that provide criteria for their existence: realism about universals tells us what properties are, but does not thereby tell us which properties there are. Accordingly, from the standpoint of realism about universals we might well be able to say what it is to be a real kind, but not which real kinds actually exist. What is needed to understand the metaphysics of kinds and its taxonomical role, therefore, is a metaphysical criterion that allows us to distinguish between real and non-real kinds.

"Real kinds" in the above sense are typically referred to as "natural kinds". This notion might appear confusing within this context, since it may misleadingly suggest that the relevant distinction here is one between the kinds that classify the "natural world" and those that

classify the "human" or "social world"—whatever this may mean. But while this distinction is not entirely misplaced here—it would seem *prima facie* intuitive to consider the kinds of the natural sciences somewhat "more real" than social or human kinds—the notion of "natural kinds" as it is ordinarily used in the philosophy of classification is simply meant to identify the kinds that are allegedly grounded in real (universal) properties, as opposed to those whose existence is merely nominal. As mentioned, within this framework a theory of universals can only explain what natural kinds are, but not which of the kinds we may be using are factually natural kinds. *Essentialism about kinds* may be seen as providing a criterion for telling natural and non-natural kinds apart by requiring that the properties of natural kinds be metaphysically *essential*. The sense in which the properties possessed by members of natural kinds can qualify as essential—as opposed to the non-essential properties that define non-natural kinds—has been characterized in various ways. Khalidi (2013) lists six different ways of characterizing essentiality that are among the most common in modern advocacy of essentialism about kinds:

Criteria of essentiality	**Description**	**References**
Necessity and sufficiency	Each of the properties associated with a natural kind is possessed by every individual that belongs to that kind, and any individual possessing all of them belongs to the kind in question.	(Ellis, 2001; 22; Soames, 2002; 15)
Modal necessity I	Natural kinds are such that they are associated with the same set of properties in every possible world.	(Wilkerson, 1988; 35)
Modal necessity II	The properties associated with a kind are such that an individual member of the kind would possess them in every possible world in which that individual exists (not just in the actual world).	(Ellis, 2001; 21)
Intrinsicality	The properties associated with a natural kind are possessed by an individual member of that kind independently of that individual's relations to anything else in the universe.	(Ellis, 2001; 20; Wilkerson, 1988; 29)
Microstructure	The properties associated with a natural kind are "underlying" microphysical properties rather than macrolevel properties.	(Wilkerson, 1988; 35)
Discoverability by science	The properties associated with a kind can be ascertained by scientific inquiry and are those properties that will eventually feature in a completed science.	(Wilkerson, 1988; 29)

Table 4.1: Essentiality in essentialism about kinds (adapted from Khalidi, 2013; 12–13).

Note that these are not mutually exclusive features, and that essentialists about kinds typically endorse more than one of these theses about the essential properties that define natural kinds (Khalidi, 2013; 13). More importantly, however, these theses jointly characterize

the *relation* between natural kinds and the essential properties that define them. Broadly speaking, one may say that essentialists believe that the defining properties of natural kinds provide them with *essences* of their own, insofar as the universal predicates that characterize them are deemed essential properties. This thesis lays the metaphysical foundations for the classificatory use of natural kinds: because they have essences, natural kinds correspond to real divisions in the world, not just to abstract divisions conventionally established by the inquirers. Accordingly, to identify the necessary and sufficient properties—or otherwise defined essential properties—that objects must possess in order to be members of a certain kind is to identify real divisions among natural kinds.

2. Political concepts and the philosophy of classification

One of the most appealing features of essentialism about kinds is the great explanatory power it ascribes to natural kinds. As Dupré (1993) has remarked, "what makes a kind explanatorily useful is that its instances share the same properties or dispositions and are susceptible to the same forces" (Dupré, 1993; 80). If there are no such things as natural kinds, then the fact that two or more entities share a certain property is a merely contingent fact that says nothing about their underlying nature. We are thus unable to draw any conclusions about the causal outcomes of the dispositional properties of such entities when they are subject to "the same forces". To see this, consider Dupré's example of a fairly non-natural kind, namely, the kind of all biological organisms of the same sex. Male scorpion flies and male ducks hardly share anything like a "maleness" essence, for even their essential genetic structure and sex-determining genetic features are different. It would be difficult, therefore, to consider maleness as constituting a natural kind in the above sense. Consequently, any behavioural patterns that result dispositionally from the sexual properties of male scorpion flies is scientifically uninformative about the causal patterns generated by the sexual properties of male ducks.

Consider now a prime candidate for a natural kind: chemical substances. The chemical substance "water" and its associated kind of chemical compounds can be seen as endowed with an essence that derives from the essential property shared by all members of that kind, namely that the molecules they are composed of contain one atom of oxygen and two atoms of hydrogen—encapsulated in the formula H_2O. For the sake of scientific explanation, this means that one can always invoke the dispositional properties that derive from chemical composition whenever a member of the kind "water" is involved. When asked a why-question of the kind, why was the fire extinguished?, essentialists may appeal to the fact that water was thrown on the fire coupled with the fact that water necessarily has the disposition to extinguish fire because of its essence—i.e. because it is a member of the natural kind defined by the chemical formula H_2O. Before moving to essentialism in the social and political sciences, let us first consider one further example from the natural sciences, and specifically from biology. Within the natural world, the buzzing and blooming domain of biology is often considered to be the one that bears the closest resemblances to the complexity of the social

world, and the biological sciences have accordingly acted as a favourite methodological model for the conduct of social inquiry (see above, ch. 2, sec. 2). And just because the variability of living organisms makes it very difficult to "carve nature at its joints", the naturality of biological kinds is an especially contested issue in the philosophy of classification.

The taxonomical aspect is a predominant one within the biological sciences, yet the criteria that determine the extension of the taxa of a genealogical nexus are disputed. Higher-order taxa (genera, families, orders, and so on) are often denied correspondence with actual, evolutionarily produced divisions among organisms, while species are more favourably regarded as identifying concrete entities, namely evolutionary units (see e.g. Wiley, 1981; Mayr, 1982; Ghiselin, 1987; Eldredge and Cracraft, 1980). Nonetheless, this distinction depends on a particular evolutionary conception of species, which itself is not unequivocal. Quite on the contrary, at least eight species concepts are available, though with varying degrees of acceptance on the part of empirical and theoretical biologists (Ereshefsky, 1991; Mayr, [1963] 1970; ch. 2; Sokal and Crovello, 1970; Ehrlich and Raven, 1969; Van Valen, 1976; Wiley, 1978; Cracraft, 1983; Paterson, 1985; Templeton, 1989; Mishler and Donoghue, 1982). While the pluralism of species concepts is itself an obstacle to the straightforward application of the metaphysics of natural kinds to biological taxa, for the sake of argument one can still select one taxonomical criterion and accord that the related taxa of a genealogical nexus designate real essences. In traditional Linnaean taxonomy, for instance, the essential properties of a species are defined by the morphological characteristics that its members share; so, for instance, the unusually short and bristly tongue of certain woodpeckers can be seen as both necessary and sufficient—i.e. essential—for those specimens to be members of the sapsucker species. As in the case of water, such an essentialist interpretation of biological taxonomy makes taxa explanatory useful, since by knowing that a woodpecker is a sapsucker we know that it necessarily has a short, bristly tongue that disposes it to feed on sap as well as on insects, and therefore we can provide a fully legitimate causal explanation of why, say, sapsuckers are found on living trees much more prevalently than other species of woodpeckers whose long tongues dispose them to pull insects out of the holes they drill in the trunks of dead trees.

Because of the near-impossibility of identifying exceptionless criteria that are necessary and sufficient for an individual to be a member of any given species, however, essentialism about natural kinds is a remarkably unfashionable position in the philosophy of biology, let alone in the morphological understanding of taxa. Even a prominent advocate of modern scientific essentialism like Brian Ellis admitted quite openly that biological taxa are poor candidates for natural kinds:

> Because of the messiness of biological kinds, and in order to develop a theory of natural kinds adequate for the purposes of ontology, I have broken with the tradition of using biological examples, and taken the various kinds of fundamental particles, fields, atoms, and molecules as paradigms. It does not matter if the resulting theory of natural kinds does not yield a satisfactory biological taxonomy. (Ellis, 2001; 170)

As Ellis notes, this point had effectively been already raised by Locke in his criticism of

Aristotle's theory of natural kinds, at least as Ayers (1981) interprets it. In book III of his *Essay*, for instance, Locke writes:

> That which I think very much disposes men to substitute their names for the real essences of species, is the supposition before-mentioned, that nature works regularly in the production of things, and sets the boundaries to each of those species, by giving exactly the same real internal constitution to each individual, which we rank under one general name. Whereas any one who observes their different qualities, can hardly doubt, that many of the individuals, called by the same name, are, in their internal constitution, as different one from another as several of those which are ranked under different specifick names. (Locke, [1689] 1975; bk. III, ch. X, § 20)

Here, Locke seems to deny that one can identify sharp distinctions between species in nature. A few paragraphs earlier, he explicitly takes issue with the famous Aristotelian definition of man as a rational animal, arguing that if the property "being rational" were to uniquely single out the essence of the kind "man", then we should conclude that those men who engage in blatantly irrational behaviour such as "chance-medley, man-slaughter, murder, parricide", and the like must in fact turn out to be members of a different species (Locke, [1689] 1975; bk. III, ch. X, § 18–19). Of course, the problem for Locke lies not so much with Aristotle's definition of man—whose strongly essentialist interpretation, incidentally, can only rely on piecemeal textual evidence—as with the idea that there are natural kinds that we can unequivocally identify and base our explanatory classifications upon.

What place, if any, the sortal essences of kinds might have in Aristotle's metaphysics is a notoriously controversial issue; nonetheless, Aristotle seems to share the Platonic premise that knowledge of beings is knowledge of their species. This is particularly evident in his theory of definition *per genus et differentiam*: what we can predicate of beings, Aristotle argues, can be established by detailing the genus to which a being belongs together with the specific difference that locates the *species* of that being within the genus—the species being the "secondary substance" of a being along with the "primary substance" that identifies it individually (see e.g. *Cat.;* ch. 5). This is also the method that Aristotle adopts in his biological work, wherein he undertakes a systematic classification of animals through a *per genus et differentiam* mode of analysis (e.g. by distinguishing the class of animals who have red blood from the class of animals that don't, and then proceeding to a further subdivision of red-blooded animals into those that have lungs and those that have gills, and so on: see *Hist. an.;* bk. I–IV). Moreover, one assumption of Aristotelian philosophy of science is that the search for a definition and the search for a causal explanation are converging tasks (see e.g. *An. post.;* 90a), and in his other zoological studies Aristotle seeks to provide a causal investigation of the body and functions of animals taxonomically defined (*Part. an.; De motu an.; IA; Gen. an.*).

As attested by the exceptionally long-lasting influence of Aristotle's biology on modern, pre-Darwinian taxonomy, the possibility of identifying morphologically defined kinds can provide some grounding to biological explanation, and an essentialist interpretation of taxa as natural kinds would lay the metaphysical foundations for causal explanation in biology. But while the role of essentialism in both biological taxonomy and Aristotelian philosophy of the special sciences is doubtful, Aristotle's theory of definition *per genus et*

differentiam is certainly a proximate ancestor of essentialism about political classifications. Let us return to the epistemological underpinnings of traditional political science as detailed in the first part of the present work. Covering-law explanation in political science, it has been argued, methodologically translated into the adoption of comparative methods for empirical analysis: the search for potentially explanatory regularities fostered the development and the expansion of the field of comparative politics, which resulted in the publication of a large body of comparative work on political regimes in the 1960s. Cross-area comparison was seen as the royal road to empirically arrive at explanatory generalities, so much so that in his opening article for the first issue of the *Comparative Politics* journal Harold Lasswell (1968) wrote:

> For anyone with a scientific approach to political phenomena the idea of an independent comparative method seems redundant. Isn't the scientific approach unavoidably comparative, since to "do science" is to formulate and attempt to verify generalizations by comparing all relevant data? (Lasswell, 1968; 3)

Within this framework, the focus on political regimes and the emphasis put on concept formation can only be explained in terms of the logic of construction of explanatory taxonomies. It has already been mentioned how, writing in 1970, Sartori chastised structural-functionalism for relying too heavily on a functional definition of political "structures" and for sloppily avoiding a definition of structures in their own terms, thereby defeating its own premises of structuralism (Sartori, 1970). On closer inspection, however, such hyper-functionalism proves to thoroughly serve the explanatory purposes that motivate the identification of structures. Setting aside the covertly teleological flavour of function-talk, one can see that "structures" can only have a role in political science insofar as they are required for causal explanation, i.e. if their causal—or, in the biology-infused political science jargon of the 1950s and 1960s, functional—properties are deemed to have been relevant in the production of the particular outcome under consideration. To be sure, this leaves unanswered the question of what structures are *qua* structures, i.e. what is the status of structures in the metaphysics of political science. Now it is clear that the structures defined by empirical political science are theoretical terms that result from the cross-area comparisons that constitute its methodology: the causal properties that define them are identified by isolating the features of political processes that remain constant across different national contexts and controlling for the variables that may locally influence them.

Discriminating the defining causal properties of political structures, therefore, depends directly on the extensional breadth of the phenomena taken into consideration. Consequently, the scale of political structures can vary to a considerable degree and designate anything from personality to entire families of national systems of government, depending on whether the focus of analysis is cast on the political psychology of the power-holding elite (for a classic example, see Lasswell, [1936] 1958, [1948] 1962) or on the general orientation to politics embodied in a given "political culture" (as per Gabriel Almond's famous concept, see e.g. Almond, 1956; Almond and Verba, 1963). The overarching "political system", conceived as co-extensive with the overall scope of political science on the whole, could itself

be reconciled with this logic by seeing it as the ultimate political structure. As Eckstein (1963) put it, particular structures can be conceived of as "the elements of political systems, taking 'elements' to mean the parts into which such systems are divided and out of combinations of which they are, for analytical purposes, constituted" (Eckstein, 1963; 4). Moreover, the flexibility with which such elements refer to phenomena of different magnitudes depends on what phenomena are to be explained, since political structures—from political personality to the political system—are entirely defined by the causal properties of the "elements" they purportedly refer to and, functionally, by the role such properties play in causal production. Therefore, political structures are best understood as analytically constituted ideal types or concepts, the formation of which was heartily perceived as a key task of comparative politics—mostly as a way of imposing conceptual order on the wild complexity that results from large-scale comparisons, or small-scale comparisons between a limited number of countries that are however studied over long periods of time.

It should by now be apparent that the dynamics of formation of these concepts—often referred to as "classificatory schemes"—in political analysis corresponds rather straightforwardly to the logic of taxonomy-building examined above. Recognizing this, Sartori diagnosed how for earlier generations of political scientists "the background of comparability was established by the *per genus et differentiam* mode of analysis, i.e., by a taxonomical treatment". In this context, he wrote, "comparable means something which belongs to the same genus, species, or sub-species—in short to the same class" (Sartori, 1970; 1036). Contrary to the Aristotelian *per genus et differentiam* classificatory logic, however, political concept formation climbs rather than descends the ladder of abstraction from lower-order taxa to higher-order ones. Instead of singling out the specific differences between types of phenomena that already classified as belonging to the same "genus", comparative political research arrives at type-concepts by identifying similarities within the diversity of individual phenomena. Political taxonomies are therefore constructed, as it were, bottom-up.

This does not in the least remove the fact that political concept formation leads to fully-fledged taxonomies whose internal logic can be reconstructed using the tools of the philosophy of classification. The "functions" of political structures, for instance, clearly serve as universal predicates that pinpoint the defining type-properties of kinds, while the structures themselves, and political type-concepts in general, can be unequivocally understood as the *kinds* which those properties qualify. Almond and Verba's (1963) "civic culture", for instance, refers to a general orientation to democratic participation based on communication and persuasion that can be characterized by some individual and relational properties—personality tendencies and socialization patterns that enable individuals to "manage the inevitable dissonances among his diffuse primary, his obedient output, and activist input" and therefore function so as (i.e. have the disposition) to support a democratic polity (Almond and Verba, 1963; 33). This resulted from a comparative study on five nations—the U.S., Great Britain, Italy, Germany, and Mexico—which led the authors to conclude that "the civic culture" identified a property shared by socialization patterns that were well-established in such countries as U.S. and Britain, and less so in others. The civic culture thus counts as

a *kind* of political orientation that can be ascribed to participation in the polity of some countries—the U.S. and Great Britain—in contrast to others—Italy, Germany, and Mexico, or even countries with a non-participant, "parochial" culture such as African tribal societies.

At the most abstract level of political taxonomies—or to continue the biological metaphor, at the "domain" level—one can see that the all-encompassing concept of "political system" that embraces any structures whose effects are of interest to political science is also a *kind*, particularly a kind of the highest rank in the ladder of abstraction. It has already been mentioned how the concept of the political system makes no exception in bearing a functional definition, its purpose being first and foremost explanatory. Almond ([1960] 2015) interprets the relevance and influence of this notion in terms of its capacity to overcome erstwhile definitions of politics that were concerned exclusively with the functioning of the state, while the deliberately abstract notion of a "political system" allows to disregard the nature of particular structures and focus on their functions within the broader social order. What functions or properties are intrinsically attached to a political system as a kind is, of course, the result of conceptual analysis and therefore depends on the political taxonomy one adopts. Since the concept of the political system is meant to include all other structures, its defining property must be one that is shared by any structure irrespective of its relative positioning in the ladder of conceptual abstraction. Thus, the kind-defining properties of political systems are established by a reverse *per genus et differentiam* mode of analysis, i.e. by abstracting from the "species-specific" properties of lower-order political structures which are in turn identified by empirical comparison between concrete and individual political objects.

3. Natural kinds and political taxonomies

It is important to stress that the definition of politics as a field of scientific investigation relies heavily on conceptual analysis, specifically in the form of identifying shared properties among a wide variety of phenomena and classifying them into a well-ordered taxonomy. On the whole, politics refers to a domain that constitutes a kind of social objects—the "political system" and its cognate concepts—of the highest possible order. The fact that many different conceptualizations of the political system in terms of its properties are possible and that different demarcations of politics as a field of scientific inquiry have been theorized is an issue that will be examined in the remainder of this section, particularly with regard to how this can be reconciled with the logic of classification. Before moving on, however, a preliminary question to be asked is whether the kinds of political taxonomies can be understood as *natural* kinds.

At first sight, structural-functionalist conceptual analysis seems to imply precisely this, as the formation of concepts is entirely (and behaviourally) grounded in the empirically observable effects of the causal properties of particular structures. The only way in which such concepts can prove explanatory useful, therefore, is by assuming that they map patterns of behaviour that necessarily correspond to the "essences" of social objects, i.e. by assuming

that social objects have certain dispositional properties to generate a certain outcome in virtue of their being the kind of objects they are. This is what makes political classifications explanatory, in that natural kinds allow one to pinpoint the relevant properties of objects that dispose them so as to produce a given effect: one is thus in the position to answer why-questions concerning the causes that brought about the phenomenon under consideration. Recall, however, that the scientific validity of comparative research was originally justified on the basis of a covering-law account of what counts as scientific explanation, which provided guidance in identifying the relevant properties of political structures that ground their classification into a conceptual taxonomy. This would seem to draw the metaphysics of political science closer to the Dretske-Tooley-Armstrong version of nomological realism, in which the logically necessary association between unrelated events that covering laws establishes are grounded in naturally necessary relations between the essential kind-properties of entities. More realistically, however, one can argue that covering-law political explanation does not need to carry such a cumbersome metaphysical baggage. Indeed, one feature of the covering-law model of explanation, and one that has been questioned above, is that it does away with any metaphysical presuppositions about *natural* relations between phenomena, the only scientifically relevant relations being ones of *logical* necessity.

Wanting for a realist account of political concepts, one may thus wish to uphold the existence of natural kinds. The problem, of course, is that hardly any kind of objects that we may draw from our ordinary use of language meets the requirements to qualify as a natural kind. Indeed, as seen in the preceding section, even more formally defined taxonomies like those at use in the biological sciences fail to identify type-properties. While this is reason enough to motivate scepticism about the possibility of discriminating natural kinds in the social world, one can always relax the criteria for typologies of social objects to count as natural kinds. Keith Dowding (2016) has suggested for instance that such a move is required to preserve the explanatory power of conceptual analysis in political science. In order to do so, Dowding adopts the following criteria:

> P1 Objects to which the term is applied are similar in certain respects that guide our ordinary application of the term, fallibly but reasonably accurately, to new cases.
>
> P2 These similarities have a single unifying explanation that involves counterfactual invariant generalizations.
>
> P3 We use the term in generalizations and explanations, and wish to do so without identifying the term with the cluster of observed similarities. (Dowding, 2016; 204–205)

These criteria are admittedly derived from Soames's (2010; 89, see also 2003; ch. 17) reformation of Saul Kripke's prerequisites of natural kinds. Kripke's path-breaking essay *Naming and Necessity* (1972) was crucial, among other things, in breathing new life into scientific essentialism. By combining his theory of meaning with a theory of necessity, Kripke provided an account of linguistic reference in terms of denotation rather than connotation, or definite description; in a famous phrase, names are for Kripke "rigid designators". Like

Putnam (1962) before him, Kripke infers from our "rigid" use not only of proper names, but also of cluster concepts like "gold" or "tiger", that their meaning—i.e. the types of entities they refer to—cannot be anything different than what it actually is. The entities we refer to with common names are, therefore, necessarily of the kind they are: they are, in short, *natural kinds*. Kripke's and Putnam's broadening of the theory of rigid designation to kind-terms as well as their use of biological examples to illustrate it seem to imply a certain degree of essentialism about biological species which, as we have seen, is particularly controversial. More broadly, insofar as the resulting theory of kinds is one of natural kinds, kind-membership becomes a necessary property of entities, and the ascription of those property must be, at least in principle, infallibly of an either-or type (either a certain animal can definitely be called a "tiger" or it definitely can't, either a metal can definitely be labeled "gold" or it can't, and so on).

Recognizing the complications that arise from the essentialist interpretation of biological taxa and, *a fortiori*, political concepts like "government", "minister", or "freedom", Dowding carefully specifies that, strictly speaking, there are no such things as natural kinds in political science. For this reason, he introduces a certain degree of fallibilism in the referential use of concepts while at the same time attempting to preserve their generalizability (see P1 above). Ultimately, he argues, the idea one wishes to defend is that "the structural features of the models [that instantiate those terms] correspond to features in the actual world to the extent that the models provide predictions that (non-miraculously) are corroborated by empirical evidence" (Dowding, 2016; 206). If we replace the term "predictions" with the term "explanations" in the previous sentence, we can see that what is being defended here is the explanatory power that essentialism attributes to scientific taxonomies *qua* classifications of natural kinds:[1] it is because class-concepts reproduce properties that are necessarily shared by all members of a given kind that those concepts are applicable to all new cases where instances of that kind are found, and that reference to (the dispositional characters of) those properties proves explanatory.

It is not entirely clear, however, to what extent the degree of fallibility that Dowding injects into conceptual analysis still allows one to speak of their referents as *natural kinds*: since kind-properties are, by definition, essential, their identification must pursue an either-or mode of analysis and cannot, as Dowding would want it, rest content with "reasonably accurately" applying it to new cases as per P1. This is particularly relevant in that the essentiality requirement is precisely what makes (natural) kinds explanatory: lacking such essentiality warrant, any new explanatory application of a political concept is arbitrary.

1. The extent to which prediction can be presented as a distinctive task of science is the source of much debate. Emphasis on prediction often bears strong positivistic connotations and can be associated with what Hempel (1958) presented as a "symmetry" between the logical character of prediction and the logical character of explanation according to the D-N model. Accordingly, emphasis on the (potentially) explanatory power of predictions is found primarily in the philosophy of science of those authors who influentially defended the D-N model of explanation, including Hempel and Popper ([1934] 2002). Because of the limitations of the D-N model discussed at length above, here we will speak of "explanation" instead of "prediction": this will not affect the thrust of the general argument (for a critical assessment of the role of prediction as described by Dowding, see Hay, 2017; Spikermann, 2017).

Consider Dowding's own example of a vague political concept: the concept of "government minister". Reference to the powers of a government minister may prove of great importance for explaining political processes, in that the resignation of a minister can bring down a government or lead to a cabinet reshuffle, thereby precipitating a political crisis. More generally, ministerial duration "touches on a centrepiece of representative democracy: accountability. Cabinet ministers are not only collectively, but also individually, accountable for government policy" (Fischer, Dowding, and Dumont, 2012). Accordingly, conceptual analysis may focus on durability as a decisive factor in determining the explanatory use of the "government minister" category. Nonetheless, ministerial stability turns out to be only loosely related to the stability of government: in post-war Sweden, high ministerial turnover did in fact secure a strong government stability, while in Italy the average duration of ministerial experience between the post-war period and 1992 was four times the average duration of the relative governments (Bäck et al., 2009; Verzichelli, 2009). As Fischer, Dowding, and Dumont (2012) argue, this variability can be related to the amorphousness of the assortment of phenomena that "durability" describes. The duration of a ministerial office, as well as its consequences for government stability, are likely to be affected by "environmental" factors like the type of governmental system, constitutional rules, cabinet composition, ministerial rank, etc., along with the personality characteristics of the particular minister and PM and exogenous shocks like scandals. Empirical studies, in particular, show that the resignation of ministers in democratic systems has important political consequences due to considerations about public opinion and accountability. The problem, of course, is that government ministers play a decisive role not only in democracies, but also in authoritarian regimes. In this sense, the concept of "government minister" encompasses a wide variety of different roles with different characteristics, and a general theory of the political importance of ministerial players in terms of their durability in office would be of no explanatory use. Without the possibility of anchoring ministerial durability to any structural—i.e. essential—features of the ministerial office under any regime, "durability" itself becomes so abstract a term to be referentially vacuous: we are thus unable to causally explain the political consequences of a ministerial resignation by simply referring to the capacity to produce government crises that the durability in office of a government minister carries with itself.

As one can see, the explanatory power of political concepts like "government minister" is irremediably weakened by the fact that they do not refer to natural kinds. It is because there are no dispositional properties that are *essentially* shared by all ministerial offices that we are unable to pinpoint the causal powers of ministerial "durability", and therefore reference to government ministers as such yields no conclusions that can count as causal explanations of political processes such as cabinet crises. Importantly, the impossibility of identifying natural kinds that could underwrite the explanatory use of political concepts squares nicely with the metaphysics of dispositions defended above. As the Dretske-Tooley-Armstrong view of laws shows most clearly, the essential properties that define natural kinds are what law-like statements are about, since the entities that carry them necessarily have the capacity to bring about a certain class of events; the ascription of those dispositional properties

to a certain class of entities thus implies a necessary connection between the activation of those dispositions and the realization of their correlate events. As Bhaskar has argued, such necessary connection between events implies the nomological closure of the system in which they occur, as presupposed by the covering-law model of explanation. Contrary to the Humean view according to which events are tied together by a relation of *logical* necessity, but not a truly *causal* connection, the "productive" view of causation demands that causal relations be understood as genuine processes. As argued above, such view is required by the realist assumption that underlies non-nomological political explanation, and its metaphysical complement can be identified in a theory of *individual* properties. Mechanism-based models of explanation, for instance, distinctively refer to the organized way in which particular entities interact as the decisive explanatory factor, thus recognizing implicitly that causation is produced by the contextual activation of some causal properties of scientific objects. Generally speaking, scientific realism bids that causal explanation correspond to the way in which phenomena are brought about in a lawless world, and therefore identify the particular causes of potentially recurring causal processes. Metaphysically, this translates into the presupposition that it is the individual properties of particular entities that ultimately ground the phenomenal *explananda* of science, and therefore underwrite both the regularities that we may experience and the causal processes that generate them. The type-properties that universal predicates express, therefore, can be thought of as supervenient on token-properties, and the kinds of entities they define ought be treated nominalistically.

Within this framework, the explanatory value of political concepts remains an open question to be answered, as is their overall status and value within political science as a scientific endeavour. Ultimately, addressing this issue will also provide an answer to the general question about the nature of politics as a separate domain of scientific investigation since, as we have seen, this can be understood as a superordinate kind-concept belonging in the highest taxonomical rank of social objects according to a ladder of classificatory abstraction.

4. Political concepts as clusters

Given that many kinds of scientific objects, like biological species or political concepts, do not present unequivocally essential (e.g. necessary and sufficient) characteristics that could serve as guidelines for attributing kind-membership to individual objects, an influential tradition in the philosophy of classification has altogether discarded essentialism about kinds. On this view, the scientific class-concepts that are ordinarily employed for explanatory purposes do not refer to anything like natural kinds. Indeed, the very idea of a natural kind is often considered to be non-referential, since there are no such things as essential type-properties and therefore sharp demarcations between different types of objects do not usually obtain. For this reason, Nelson Goodman (1975) spoke of our classificatory practices as consisting of "relevant kinds" rather than "natural kinds", in that "'natural' suggests some absolute categorical or psychological priority, while the kinds in question are rather habitual or

traditional or devised for a new purpose" (Goodman, 1975; 63). Revealingly, Goodman developed this idea in connection with the "new riddle of induction" he famously posed with respect to the problem of confirming universal hypotheses. Generalizations, Goodman argues, derive their law-like character from their confirmation, yet confirmation in turn depends on the predicates they refer to: in a famous example, by observing that all emeralds observed thus far have been green we may arrive at the universal conclusion that all emeralds are green just as we can formulate a hypothesis that all emeralds are "grue" (i.e. that they will be green until a certain date, and blue afterwards). Since those predicates are mutually exclusive, the particular law-like hypotheses we assume depend on the predicates we choose: emeralds cannot be at the same time green and grue, and therefore to hold that all emeralds are green implies that we take the class of green emeralds to be real at the expense of the class of grue emeralds. The reality of kinds, therefore, is only a matter of their relevance to our explanatory theory; in Goodman's words, "The uniformity of nature we marvel at or the unreliability we protest belongs to a world of our own making" (Goodman, 1975; 63)

Drawing on Goodman's notion that kinds correspond to "ways of worldmaking" rather than real essences, Ian Hacking has developed a now standard nominalist account of what he called "human kinds". Unlike natural kinds, Hacking argued, human kinds are "peculiar to people in a social setting" (Hacking, 1995; 25). For Hacking, the properties of entities that require a social setting to be applicable are by definition non-natural and not anchored to essences, and therefore allow us to select and organize social objects into kinds which are *constructed* according to Goodmanian criteria of relevance rather than *discovered* (Hacking, 1991a, 1992; see also Hacking, 1999; ch. 5). In Hacking's famous example, "child abuse" is a socially constructed idea (though Hacking is reluctant to talk about social construction) because the kind of phenomena it refers to is "made and molded", as is the kind of social actors that are responsible for those phenomena—the "child-abusers". The term itself even has a specific place and date of birth: Denver, 1961. In that year, a group of Denver-based pedraticians led by C. H. Kempe identified and announced a "battered child syndrome" caused by parents beating up their babies and based on X-ray proofs of unreported fractures or bruises. This is not to say, of course, that before 1961 no physically abusive behaviour could be identified in parental care. But the Hempe group constructed—or, in Hacking's jargon, "molded"—it as a kind by selecting radiological evidence of bruises and fractures at different stages of healing as its defining property.

As Hacking (1991b) notes, human kinds are typically malleable categories. In everyday language, for instance, parents are often identified as child-abusers without requiring for the extent of their abusive behaviour to involve breaking their small children's bones. Even Kempe's choice of label for the "battered children syndrome" does not match the larger meaning that common law attaches to battery, which also includes burning children with cigarettes, scalding them, and the like (Hacking, 1991b; 270). Hacking's account of the reasons why the Hempe group focused on bruises and bone fractures in molding child abuse will prove particularly instructive for our purposes, and we shall return to them in section **??** Before asking why "human kinds" are constructed the way they are, however, it is important

to ask *how* they are constructed. In the absence of essentially shared properties that ground kinds onto natural divisions between real-world types of entities, assessing the scientific use of non-natural kinds from a realist standpoint requires investigating the criteria that actually guide those kind-making processes. Such criteria lie outside the scope of Hacking's work, and Hacking seems to allow for all sorts of criteria to be adopted in kind-making (Hacking, 1991b). This is because human kinds involve a selection of "social properties" which are, by definition, identified as such in a "social setting"; they are selected not by nature, as it were, but by the (socially situated) inquirers.

For this reason, human kind-concepts are often treated not as Kripkean rigid designators, but as words whose meaning is determined by their particular use. This echoes some fundamental aspects of the theory of reference developed by the later Wittgenstein, who coined the term "language-games" to stress the fact that the referential use of language is regulated by contextual and agreed-upon rules which resemble the rules of a game. Like the meaning of common names, criteria of kind-membership are not determined by metaphysical criteria of necessity and sufficiency. Indeed, rules of kind-making have no grounding whatsoever outside the mind of the beholders; they are instead determined by their specific classificatory use. For this reason, Wittgensteinian metaphors are often invoked to emphasize this contingency and artificiality: the defining properties of human kinds are often characterized as "family resemblances" that the observer identifies between objects rather than properties that kind members possess essentially. The practice of kind-making thus appears, in Wittgenstein's terms, like spinning a thread by twisting fibre on fibre: "the strength of the thread resides not in the fact that some one fibre runs through its whole length, but in the overlapping of many fibres" (Wittgenstein, [1953] 2009; §66). Kind-concepts, that is, do not refer to a hard core of necessary attributes that its members possess; rather, they are defined by a whole spectrum of different attributes which stand in a relation of affinity to one another. Following Boyd (1989, 1999a), Chakravartty (2007) characterized the kinds that result from the application of such non-essentialist criteria of membership as *clusters*: unlike natural kinds, which are characterized by necessitarian criteria of kind-membership, cluster kinds are polythetic, in that "the possession of a clustered subset of some set of properties, no one of which is necessary but which together are sufficiently many, entails kind membership" (Chakravartty, 2007; 158).[2]

2. In the attempt to deal with anti-essentialist criticism while at the same time trying to rescue the notion that natural kind-concepts may serve explanatory purposes, Richard Boyd (1988, 1989, 1991, 1993, 1999a, 1999b, 1999c) has introduced the idea of "homeostatic property clusters" (HPC). On the HPC conception, some causal mechanisms may be active that tie particular properties together into clusters and keep them in homeostasis. As a result, natural kinds may be real despite essentialism being false. So, for instance, biological species are the product of a variety of homeostatic mechanisms—gene exchange between certain populations and reproductive isolation from others, effects of common selective factors, co-adapted gene complexes and other limitations on heritable variation, developmental constraints, the effects of the organism-caused features of evolutionary niches, and so on—that establish stable evolutionary patterns (Boyd, 1999a). Accordingly, the species that manifest those patterns may be recognized as real kinds.

However, the presence or absence of such homeostatic mechanisms in the social world is an entirely contingent fact that can, at most, be established empirically. As a consequence, whether or not political concepts designate HPCs does not affect the contention that political classifications do not map onto natural kinds in any *metaphysical* sense.

Let us return briefly to the child abuse example. Arguably, each case of child abuse is different from the others in many respects, including crucial aspects concerning the nature and extent of the abuse. And even if one confines their own attention to battered children with unrecorded fractures, each fracture may be the result of being thrown at the wall or down the stairs as well as beaten up. Individual cases of child abuse, in short, bear more differences than similarities, at least as far as essential properties go. Focusing on roentgenographic examination, however, allowed for the Kempe group to play down the specificities of individual abuses and identify "family resemblances" between different cases, and therefore arrive at a common definition of the "battered child syndrome" and its associated kinds, "battered children" and "child-batterer". Let us now move to political concept-formation. As Goertz (2006) points out, the concepts at use in social research are often characterized in a way that makes them based on variants either of the necessity-and-sufficiency approach to kinds or the family-resemblance one. Indeed, while a latent essentialism about social science concepts has always been the standard view, a family resemblance concept structures has been implicitly but manifestly adopted in many strands of quantitative analysis (Goertz, 2006; ch. 2). Consider again the example of cabinet ministers. While emphasizing the relevance of ministerial stability in explaining political processes, Fischer, Dowding, and Dumont (2012) note that stability is determined by the theoretical concept of "durability". Just like the concept of "minister" is an abstract term that identifies a class of heterogeneous political offices, the theoretical properties that define it and determine its explanatory role—stability and durability—are abstractly constructed from the factors that empirically and even theoretically influence ministerial duration. No one single property affecting the length of time ministers are in office is necessary and sufficient to unequivocally pinpoint the concept of minister. Instead, durability identifies a *cluster* of different properties that are not necessarily shared by all members of the class of ministers. Ministers in authoritarian regimes, for instance, are less likely to be removed from office depending on the length of time "their" leaders are in office, while cabinet reshuffles are a recurring feature of democratic regimes. For these reasons, Fischer, Dowding, and Dumont (2012) argue, "a 'general theory' of ministerial durability is likely to be somewhat amorphous" (Fischer, Dowding, and Dumont, 2012; 516): the properties that affect ministerial duration bear at best *family resemblances* between one another that allow for all of them to count as "durability" factors.

The idea of "family resemblances" was introduced in political conceptual analysis by Collier and Mahon Jr. (1993), in open contrast to the "Aristotelian" mode of concept formation critically discussed by Sartori (1970). Collier and Mahon Jr. note that family-resemblance categories are rather common in comparative politics (e.g. in the literature on corporatism and political participation). In all these cases, they argue, concept formation does not necessarily involve climbing the ladder of abstraction in order to increase the extensional coverage of concepts. This is because family-resemblance concepts only require a certain affinity between members of the kinds they define, not necessary and sufficient properties. Therefore, the intensional definition of those concepts can be defined contextually,

and the inclusion of new members does not require a higher level of generality. In these respect, Collier and Mahon Jr. conclude, the typologies that are constructed according to a family-resemblance logic are more similar to *configurational* typologies than to hierarchically ordered taxonomies. Unlike necessity and sufficiency typologies, in configurational typologies the attributes of a category are not added to those of superordinate categories, thereby increasing the specificity; rather, the subordinate categories that "radiate" outward from a superordinate category are fully contained within the latter. Lower-order categories are, in short, diminished rather than augmented subtypes (Gerring, 2012; 728). In this respect, it is often argued that democracy constitutes such a "radial" category, in that the differentiating attributes associated with "participatory," "liberal," and "popular" democracy are contained within the primary category (Collier and Mahon Jr., 1993; 824).

Whether the political kind-concepts that are constructed according to a family resemblance mode of analysis are best viewed as forming a taxonomy or a configurational typology, family resemblances still provide a basis for constructing *ordered* typologies. Consequently, one can still see the underlying general conception of politics as the primary kind-concept from which all secondary categories radiate or differentiate. Consider again the example of democracy. Where taxonomists see a concept with decreased intension (but increased extension) with respect to its sub-types ("participatory democracy", "liberal democracy", "popular democracy"), the configurational typologist sees an ideal type which is instantiated in its particular forms. On a non-essentialist view of kinds such as the one that family-resemblance criteria of kind-membership imply, however, the concept of democracy cannot be but a theoretical abstraction that is arrived at—or constructed or molded—by selecting some relevant affinities manifested by its real-world members. Similarly, "authoritarianism" constitutes a nominal kind that is constructed by identifying some family resemblance between, say, the authoritarian features of populist regimes and those of bureaucratic regimes, irrespective of how one conceives the relation between authoritarianism and its populist or bureaucratic subtypes. It is possible, along these lines, to identify further family resemblances between democracy and authoritarianism, thus tying them together into a cluster of "regimes", and so on and so forth, all the way up to "the political system" or related kind-concepts.

5. Scientific realism and family-resemblance typologies

The non-essentialist view of kinds as clusters rather than essences seems to pose a *prima facie* problem for realism. Because kinds are nominal, they belong in our language: it is through our classificatory discourse that kinds are constructed. In this sense, kinds are mind-dependent entities rather than real type-essences. From a realist point of view, this makes kinds non-referential and of little explanatory use, since they do not allow one to refer to actual causal process triggered by the particular dispositions of social objects. Reference to the family resemblances between those dispositions is not sufficient to make inferences to the causal processes they are capable of producing. Participatory democracy, for instance, is characterized by involvement in the decision-making processes that directly affect people's

lives, while representative democracy involves the election of representatives to whom is delegated the authority to adopt and make public decisions, and therefore to rule. As a result, the policy-making processes that result from these different democratic forms will also be very different from one another. The Rousseauian ethos that animates social movements demanding participatory inclusion can be seen as emphasizing precisely this aspect, which cannot be captured simply by referring to democracy as an abstract concept characterized by "rule by the people"—a family resemblance between very different decision-making processes. This is even more evident if democracy as a cluster of different systems of collective decision-making is extended to include other procedures whose affinity to traditional democratic types is more remote, such as e.g. economic democracy.

There is, however, a further dimension to many constructivist approaches to human kinds that turns them into partially mind-independent entities. Hacking (1995), for instance, famously spoke of "the looping effect of human kinds" with reference to the performative action that classificatory practices can exert on the objects they classify. Human kinds, the argument goes, refer to kinds of self-conscious people who can therefore take those kinds into account as part of their self-knowledge. Unlike the inanimate entities of the natural world (e.g quarks), people who are typified as, say, "woman refugees" can become aware that they are identified as such and *act accordingly*. As Hacking put it, "Women refugees who do not speak one word of English may still, as part of a group, acquire the characteristics of women refugees precisely because they are so classified" (Hacking, 1999; 32). By acting in accordance with the way they are classified, that is, people can acquire the defining characteristics of the kind they are ascribed to. In this way, kinds which were merely nominal entities at the time of their construction can turn into real kinds (although, perhaps, they lack the requirement of essentiality that would make them natural kinds).

The fact that kinds are real not in virtue of some mind-independent essence implies that their construction is not itself regulated by some principles of realism. It is, to some extent, contingent. Nelson Goodman captured this feature by referring to non-natural kinds as *relevant* kinds: as he put it, "'natural' suggests some absolute categorical or psychological priority, while the kinds in question are rather habitual or traditional or devised for a new purpose" (Goodman, 1975; 63). Relevance is in fact a crucial factor in selecting the family resemblances that define cluster kinds. As Hacking noted with respect to child abuse, battery is but one possible form of abusive behaviour, and parents battering their children to the effect of leaving radiographically detectable consequences are a yet smaller subset of abusive carers. For Hacking, the reason why the Kempe group narrowly focused on X-ray evidence is a matter of *relevance*. Specifically, what allowed them to identify a specific kind of parents as "child-batterers" was its relevance in accounting for the consequences of their seriously abusive behaviour: battered children may be repeating the type of childcare practiced on them once they become parents, and it is necessary to counter the physicians' generalized reluctance to relating certain aspects of fracture healing to parental abuse. Although Kempe et al. (1962) extended the "battered child syndrome" to include *any type of injury*, they concluded their article with a warning to physicians about the importance of diagnosing the

signs and symptoms of child abuse to avoid its future repetition.

That classifications are contingent because they are a matter of what is contextually recognized as relevant by those who construct them also implies that multiple classifications of the same phenomena are possible. Hacking points out for instance that before the Denver group of paediatricians presented their findings in 1961 child abuse had been a widely discussed social issue since the Victorian age. Yet what the Victorians identified as "cruelty to children" was also shaped—or molded—by what they perceived as relevant. To cite but one of the most striking examples, sexual assault did not count as cruelty to children in Victorian courts—though it was surely dealt with as a serious offence. This is because, Hacking argues, "cruelty to children was one of the last of the great Victorian crusades, and came after anti-slavery, factory legislation about child employment, temperance, the extension of the suffrage, anti-vivisection, and cruelty to animals" (Hacking, 1999; 134). Cruelty to children was, in other words, an issue that liberals saw as demanding social reform, and therefore it was framed in class-based terms as a matter of poor people hurting their children, not in medical terms as a multi-faceted pattern of harmful parental care. In this case, different family resemblances (primarily of a social nature) were being selected among different cases of cruelty to children that led to a different extension of the concept of child abuse than Kempe et al.'s definition of the "battered child syndrome".

The fact that multiple, overlapping, and mutually incompatible classifications of the same phenomena are possible does not *per se* speak in favour of the validity of one to the detriment of another. In fact, all such classifications can be constructed (or re-constructed) in a way that makes their relative kinds equally real. John Dupré (1993) recognized this consequence of the non-essentialist view of kinds in his defence of "promiscuous realism" about biological taxonomies:

> My thesis is that there are countless legitimate, objectively grounded ways of classifying objects in the world. And these may often cross-classify one another in indefinitely complex ways. Thus while I do not deny that there are, in a sense, natural kinds, I wish to fit them into a metaphysics of radical ontological pluralism, what I have referred to as "promiscuous realism". (Dupré, 1993; 18)

Similar considerations can be found in Abraham Kaplan's (1964) influential textbook on the philosophy of the social sciences (Kaplan, 1964; see especially ch. 2, §6). Every concept, Kaplan argues, is but "a prescription for organizing the materials of experience so as to be able to go on about our business. Everything depends, of course, on what our business is". So, for instance, the concept of an "individual" can be many different things—"different for the economist and the anthropologist, different even for the physical and the cultural anthropologist, and perhaps different according to whether a prehistoric or a contemporary culture is in question, and so endlessly" (Kaplan, 1964; 46). Consequently, the purpose of scientific classifications based on those concepts—including social science classifications, such as e.g. Lazarsfeld's (1954) "latent structure analysis"—is "to facilitate the fulfillment of any purpose whatever, to disclose the relationships that must be taken into account no matter what" (Kaplan, 1964; 51)

As far as politics is concerned, different aspects of social phenomena can be and effectively have been emphasized as characteristic of political reality. Insofar as these are seen as uniquely characterizing the nature of politics, however, these typologies are conducive to different classifications of what counts as "political". So, for instance, Easton's (1957) presents the political system as a unitary system for making binding decisions (the "authoritative allocations of values") that is functionally separate from the setting it is immersed in (4.1): in this case, family resemblances are identified with respect to the institutional locus in which authority is exerted, while the similarities with other methods of making authoritative decisions are downplayed. Robert Dahl (1976), by contrast, defined the political system as

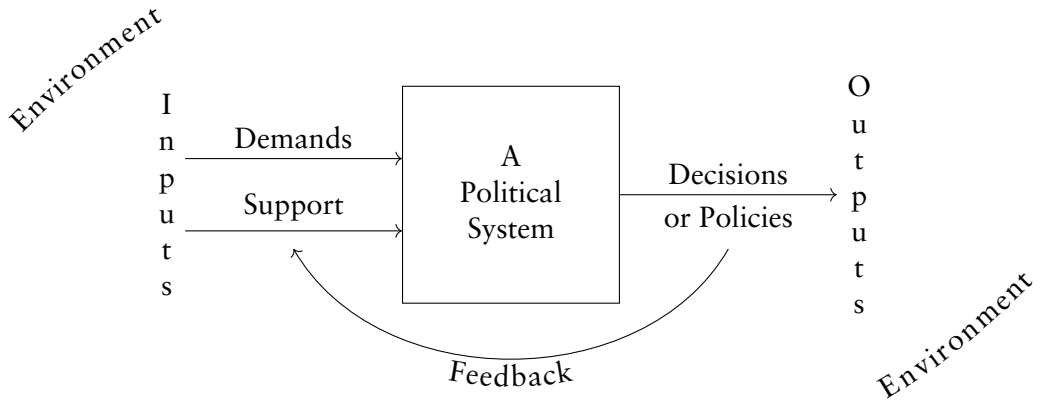

Figure 4.1: Easton's model of the functioning of political systems (Easton, 1957; 384).

"any persistent pattern of human relationship that involves, to a significant extent, control, influence, power or authority" (Dahl, 1976; 10), thus suggesting that the use of coercive means for making decisions is more relevant than the site in which those decisions are made. Yet other theories, typically Schmittian or neo-Schimittian, emphasize sheer violence or antagonism over the coercive achievement of decisions (Schmitt, [1932] 2007; Mouffe, 1993, 2000, 2005); and still other classifications are possible.[3]

One can certainly provide more nuanced accounts of the general character of politics that are less easy to classify in terms the particular features of social life they emphasize. One way to do so is to shift the focus away from the site in which binding decisions are made as well as from the means by which those decision are implemented (authoritative or otherwise). In defining politics as "the authoritative allocation of values for a society", for instance, Easton (1965) suggests that the outputs of the political system—the allocated values—are crucial to the characterization of its functioning, thus reinforcing Lasswell's ([1936] 1958) dictum that politics is at once about "*who* gets *what*, *when*, *how*". According to Bartolini (2016), Easton defines politics *simultaneously as institutional locus and as allocation*. Nonetheless, the authoritativeness of such allocation is what ultimately gives it a political character as distinct, e.g., from the economic allocation of goods. What distinguishes political goods (i.e.

3. Bartolini (2016) has provided an extensive list of different conceptions of politics, which he grouped into six families depending on the "family resemblances" they emphasize: politics as activity, politics as institutional locus, politics as conflict, politics as the use of coercive means, politics as allocation.

values) from goods of other sorts is the fact that their allocation is enforced authoritatively by the political system. The institutional locus of political authority, therefore, is where the distinction between the allocation of values and the allocation of other goods lies: the site of politics thus becomes the ultimate factor that determines the boundaries of the political sphere.

In fact, one may claim that the reason why the allocation of values is different from the allocation of other goods is not that the political system makes binding decisions about them, but that the political system guarantees that people are *entitled* to the achievement and distribution of any goods in virtue of guaranteed rights (Stoppino, 2001; Bartolini, 2016). In this conception, the authoritative means by which values are allocated as well as the nature of the outputs of such allocation are less decisive factors in the definition of politics than is the fact that the entitlement to those goods is guaranteed in the form of rights. This leaves open the question as to what distinguishes guaranteed vs. non-guaranteed claims over final values. As with allocation, further distinctions about the nature of social interactions need be drawn in order to identify the specifically political character of certain social processes. Typically, to talk about *guaranteed rights* is to talk about the *legitimacy* of the authority that confers them, and therefore in the collective acceptance of the power of such authority. Compliance to the system of rights, in other words, is necessary in order for the political arena to be distinguished from other ways of obtaining access to goods, such as e.g. market allocation or coercive allocation by means of brute force.

This does not mean, however, that conceptualizations of politics on the basis of collective acceptance are less prone to the relativity that derives from the fact that concept formation requires a stipulation about the relevant properties that define such concepts. Each of the general theories of politics emphasizes a specific aspect of certain social phenomena according to which they count as "political phenomena", thereby making the objects that dispositionally generate them belong in an analytically separate sphere of society. The selection of family resemblances among those phenomena, as Goodman put it, a matter of relevance: typologies necessarily discard some family resemblances in favour of others while climbing the ladder of generality. Political phenomena can be seen as both taking place in a more or less institutional locus and as decision-making processes that are carried out through coercive means: whether one focuses on the former or on the latter aspect is a matter of selection. This results in partially overlapping conceptions of the political sphere that are at times mutually incompatible with one another. On the one hand, therefore, general theories of politics can be made compatible with the requirements of scientific realism, and causal realism in particular; on the other hand, promiscuity in Dupré's sense seems to be the inevitable outcome of our classificatory practices. For kind-concepts are both real and dependent on how we go about defining it in a way that makes different, mutually exclusive typologies equally valid on the basis of realist criteria.

Conclusion

The apparent price that scientific realists have to pay because of their nominalist metaphysics about universal properties seems to be the arbitrariness of scientific classifications, even when these are constructed for explanatory purposes. The causal realist view that sees *explanandum*-phenomena as generated by context-specific causal processes leads to formulating causal explanations that trace causal processes to the dispositional properties that endow individual entities with the causal power to generate certain phenomena. The scientific metaphysics that results from this causal realist picture of the social world has been shown to be one of "powerful particulars", or particular objects whose causal powers derive from their individual properties. The universal properties that natural kinds presuppose seem to have no place in the causal-realist metaphysics.

This is because, as argued in the previous chapter, a realist view that sees universal properties as much real as particular properties would result in the well-known problem of instantiation that has led philosophers either to abandon realism about individual properties in favour of the view that all properties are categorical, or to adopt a downright Humean view about the causal irrelevance of individual properties. These are, respectively, the Dretske-Tooley-Armstrong version of nomonological realism and the anti-realist view of causation. These have been shown to be utterly incompatible with causal realism, and therefore the realist solution to the problem of instantiation—which concerns the dubious relation between individual and universal properties—has proven to be nominalism about universals.

In this way, however, classifications can only be grounded on family resemblances between particular properties and not on the universal properties that would metaphysically sustain the existence of natural kinds. Therefore, the formation of scientific typologies, of which political concept formation is an instance, must be based on metaphysically unwarranted stipulations about the relevance of family resemblances, which produce divergent but undecidable definitions of political concepts. Moreover, the definition of politics as a scientific domain has been shown the be the product of such classifications of social objects by way of political concepts, and therefore the scope of political science remains necessarily—i.e. metaphysically—indeterminate on the criteria of scientific realism.

The next chapter will outline a possible solution to this problem by comparing the formation of classificatory concepts to the construction of scientific models. Like scientific typologies, models can be seen as representations of reality whose validity cannot be established by confronting them with any features of the objects they represent. However, the explanatory purpose of experimental models, like that of scientific classifications, makes them testable on the way the phenomena they purportedly account for are modelled. Explanatory classifications, in particular, are constructed on the basis causal models of phenomena—the models of explanation discussed in the first part of the present work. While classifications *per se* cannot be directly judged through the lenses of scientific realism, their explanatory adequacy can be judged in terms of the degree of causal realism of the model of explanation they presuppose.

Chapter 5

Modelling politics

> "What a useful thing a pocket-map is!" I remarked.
> "That's another thing we've learned from *your* Nation," said Mein Herr, "map-making. But we've carried it much further than *you*. What do you consider the *largest* map that would be really useful?"
> "About six inches to the mile."
> "Only *six inches*!" exclaimed Mein Herr. "We very soon got to six *yards* to the mile. Then we tried a *hundred* yards to the mile. And then came the grandest idea of all! We actually made a map of the country, on the scale of *a mile to the mile*!"
> "Have you used it much?" I enquired.
> "It has never been spread out, yet," said Mein Herr: "the farmers objected: they said it would cover the whole country, and shut out the sunlight! So we now use the country itself, as its own map, and I assure you it does nearly as well."
>
> Lewis Carroll (1894)

Introduction

This chapter seeks to provide evaluation criteria for conceptualizations of politics. At the beginning of the present work, it has been argued that the concept of politics emerged first and foremost as a scientific concept that could accommodate the definition of the scope of political science. Because this goal was achieved on the basis of the demarcationist philosophy of science on which the covering-law orientation of early philosophy of science, the realist criticism to the covering-law model of explanation inevitably extended to the possibility of drawing just such demarcation of scope. Specifically, causal realism sees *explanandum*-phenomena as the outcomes of observable causal processes rather than as primitive components of reality, and therefore it demands that the goal of scientific explanation is recast from one that allows

for causal relations to be defined in purely inferential terms to one that sees causal relations as corresponding to the metaphysical structure of reality. It should not strike one as especially surprising, thus, that abandoning the demarcationist pretensions of anti-realism leads to the impossibility of providing a demarcation of the scope of political science that is not based on actual divisions in social reality. Because such sharp divisions are not available, however, a metaphysical definition of classificatory concepts is, in realist terms, wishful thinking.

On the other hand, because of the scientific nature of domain-concepts, it seems quite natural that they still be accounted for in scientific terms. More specifically, the classificatory concepts that serve as definitions of the boundaries of particular fields of scientific investigation have been shown to be the products of classifications that are built for explanatory purposes, and therefore one may demand that they be assessed on the basis of their explanatory value. In this chapter, I will show how this can be achieved by considering those concepts as nothing else than scientific *models*. While it may seem counterintuitive to compare theoretical concepts to the representational models of the empirical sciences, the next two sections will argue that some views of models, particularly some variants of the so-called "semantic view", can capture the elusive nature of classifications in terms of the ineliminably representational aspect of science. In particular, I shall refer to what Ronald Giere (1999b) has termed "the representational view of models" to show how scientific models require a *definition* that involves a *stipulation* about what they can represent. The resulting view of models is one that sees them as objects that are able to describe some aspects of reality not because they represent some features of the world more accurately than others, but because they can "stand in for" real-world objects by being similar to them (section 1).[1]

Because the representational power of models derives entirely from the way they are defined as well as from stipulations about what they can or cannot represent, their scientific value rests upon the purpose on which they are defined (section 2). This also means that the modelling activity of science may result in the production of scientific objects that are equally valid on the basis of their representational character. The analogy with concept formation is rather straightforward: like models, classifications are the product of definitions of classificatory concepts that involve a stipulation about the similarities between particular properties on the basis of which the objects that possess them are included into class-concepts. Like models, classifications are built for explanatory purposes, and the multiplicity of classifications that can be produced correspond to the different ways of defining the conduct of empirical social science or, in other words, to different models of explanation.

The experimental models of science, however, can be compared to reality by way of models of data that determine the fit of the model with the world, thus potentially reducing the pluralism of models. In the final section of this chapter I shall argue that what determines the fit of classificatory concepts with the world are just the models of explanation on which

1. Of course, the idea that models are (abstract) objects that are devoid of truth value has not gone uncriticized. Nonetheless, even critics of Giere's account do not necessarily reject the notion that models have an inherently representational aspect (see e.g. Mäki, 2011). Moreover, the representational view of models is invoked here mainly because of the analogy between the function of models and that of political concepts.

they are constructed and which provide their ultimate anchor to reality. This view thus sees models of explanation as the testing ground for classificatory concepts and, by extension, for the ways in which the boundaries of scientific domains are defined. Just like experimental models determine possible realizations of data and can be discarded by failures of such realizations, political concepts can be discarded because the models of explanation they presuppose fail to match the way in which reality ought be metaphysically conceptualized.

1. The representational view of models

The differences in the conceptualizations of politics that result from this process of selection lie not only in the extension of the concept of politics, but also in the overall image of politics they yield. Focus on the locus of politics, for instance, allowed Easton to speak of the political system as a spatially situated arena of decision-making, which for Searle is co-extensive with government. Dahl's concern with coercion, on the other hand, implies a more diffuse—or "pluralist"—conception of politics that includes non-institutional "interest groups" capable of exerting influence on decision making: what Dahl refers to as "political influence" is dispersed because coercive power is distributed unequally across all strata of society rather than concentrated (Dahl, 1961). As Figure 4.1 shows most clearly, these conceptions can be represented as visual *models* that reproduce the salient features that have been selected in the definition of the concepts that such models represent.

Clarke and Primo (2012) argue that models are ubiquitous in political science, though the models they refer to are issue-specific ones that are use to account for very specific phemomena. Zupan (1991) resorted for instance to game-theoretic models—specifically, the Prisoner's Dilemma—to explain ticket splitting in American elections, i.e. the practice of casting a ballot for one party when selecting who will represent them in Congress and a different party when selecting the president. Baron and Ferejohn (1989) also drew on game theory and utilized the divide-the-dollar game by modelling legislative policy-making as a bargaining process among legislators interested in maximizing their share. As these game-theoretic examples show, many social-science models (including political models) draw on economics, a vastly mathematized science where theoretical and empirical models are common tools that are used primarily to deal with a highly complex subject matter (Sugden, 2000). In this respect, one should also mention that many fields of physics also employ models for ease of treatment (Redhead, 1980). As a result, the status of models is a widely discussed matter in the philosophy of those special sciences, and many conflicting views have been proposed regarding the status and purpose of models in particular disciplines.

In contrast, it is rather uncommon to characterize whole domains of investigations in terms of models. What follows is an attempt to do so by selectively identifying some features that are commonly attributed to models, particularly in Ronald N. Giere's version of "the semantic view of theories". Broadly speaking, this view takes models to be theoretical objects that are primitively defined and constructed in such a way as to be capable to represent, or stand for, other objects that occupy a portion of the world. In the remainder, the features

of models thus understood will be linked to scientific domains understood as higher-order concepts that include large classes of objects on the basis of similarities between their causal properties. A tentative answer to the problem of promiscuity will be sketched on the basis of those very features of models.

Definition

Not unlike Van Fraassen (1980), Giere (1988) argues for instance that models are primarily characterized by a *definition*. What Giere has in mind here are physical models like the harmonic oscillator, which are *defined* by equations like the force law $F = -kl$, but the similarity between the definition of models and the definition of classificatory concepts is almost trivial: what kind-concepts include is literally defined by the selection family resemblances between properties. The concept of "government minister", for instance, is *defined* in the abstract by identifying a theoretical cluster of features that affect, among other things, the duration of the office as well as the political consequences of office termination.

By the same token, insofar as political conceptions can be seen as higher-order concepts within an ordered typology, their comparison to models simply requires climbing the ladder of abstraction. Emphasis on the site where binding decisions are made concerning "the allocation of values for a society", for instance, allows one to identify a higher-order status function about the deontic power of a government which in turn rests upon the collective acceptance of lower-order status functions—the constitutive rules of the institutions that governments regulate (family, education, money, the economy, private property, the church) as well as those over which they have monopoly (the police and the military).

Stipulation

Furthermore, to say that models are characterized by a definition implies a *stipulation* about the validity of models: as Giere remarked about the harmonic oscillator, "the equations truly describe the model because the model is defined as something that exactly satisfies the equations" (Giere, 1988; 79). We have already seen how stipulation is crucial in concept formation: like the force law, a selection of family resemblances truly describe a kind of entities because the kind is defined by those family resemblances. Family resemblances, that is, are identified on the basis of their relevance to constructing explanatory kinds; what counts as family resemblance, therefore, is determined by a stipulation about what counts as a member of the kind to be constructed. Early definitions of the political system in terms of certain functions of social structures, for instance, were meant to displace earlier equations between politics and the functioning of the state apparatus because of the latter's principled exclusion of certain structures that nevertheless could be comparatively found to perform analogous functions: as Almond ([1960] 2015) wrote, "the articulative, aggregative, and communicative functions may be performed diffusely within the society or intermittently though the kinship or lineage structure". Law-like identification of *similar* functions across national contexts thus served the purposes of constructing a concept of the political system

that "characterize[s] all of these functions and not simply those performed by the specialized political structures" (Almond, [1960] 2015; 12). Similarly, Easton's (1957) three-function model of the political system was premissed on the dismissal of the distinction between state and non-state as a relevant one for political concept formation: the reason for this, Easton (1953) writes, resides in the fact that "we are interested in concepts that pick out the major properties of the concrete political system" (Easton, 1953; 108). The concept of the state, Easton argues, is inadequate to construct the class of objects that "the political system" refers to and which must include pre-state societies because of a *stipulation* about the family resemblances their functioning bears with that of state societies.

Objects and representation

An interesting consequence of this conception of models is that it makes little sense to speak of models as true or false. As Suppes (1967) and Van Fraassen (1980) had already noted, a model is, strictly speaking, neither true nor false: its relationship to the portion of world it models is, as Giere (1999b) put it, one of *similarity* rather than *true* correspondence. This means that models are *objects*, and the fact that they are *theoretical* entities or *scientific* objects does not affect the way they relate to other objects, namely, by being similar to them. This is what Giere (1999b) calls *the representational view of models*: all models are representations, and representations are objects that stand in for other objects. As Van Fraassen put it, "There is no representation except in the sense that some things are used, made, or taken, to represent some things as thus or so" (Van Fraassen, 2008; 23).

While certain physical models, like the Rutherford-Bohr planetary model of the atom or electrical circuits, can be clearly considered to be objects in a very lay sense, the notion that models are objects seems to apply less straightforwardly to non-physical—mathematical or geometrical—models like the groups of group theory and vector spaces. This is, however, exactly the core contention of the representational view: any physical or abstract entities (like numbers or geometrical points and lines) are objects insofar as their *definition* allows for them to be interpreted as representations of existing objects. To see how this concept of an objects can be applied to whole domains of investigation as much as to abstract models, let us briefly consider Daston's (2000) concept of a "scientific objects". Unlike quotidian objects, Daston argues, scientific objects are not simply "there to be discovered": they are real objects because they are the product of historical constructions. Social science objects like "race" or natural science objects like "quarks", for instance, can be considered as real objects because they are constructed—and therefore brought into being—by the decades-long or even centuries-long history of the sciences themselves.

For Daston, the shaping and molding of whole domains of investigation present the characteristics of scientific objects in their coming into being (and passing away). As we have seen in chapter 1, this is the trajectory followed by the division of labour among the social science disciplines. According to Peter Wagner (2000), for instance, the coming into being of "society" as a scientific object from the mid-eighteenth century onward was determined by

"an observable change in the structure of social relations, i.e., in the ways the lives of human beings are connected to each other" (Wagner, 2000; 134). The idea of society as "neither state nor household", in other words, was brought into being through the scientific investigation of the functioning of the broader social order: "*Statistical* and demographic research was said to reveal some solid and lawlike features in the characteristics and movements of a population and thus to underpin the idea of the existence of 'society'" (Wagner, 2000; 142, emphasis in the original). Similarly, the mathematical principles of marginal utility served to create the new scientific object "economy" as detached from politics, which would at first remain confined to the ambit of the state. The subsequent broadening of the scope of politics to the inner logic of an abstract "political system" also amounted to the creation of a new scientific *object* on the basis of the lawlike functions that social structures fulfill in societies, as revealed by a covering law-oriented model of conducting comparative research.

2. The promiscuity of models

That classificatory concepts can be real in the sense that they are social objects has already been discussed in section 5. Let us now focus on the *representational* aspect of models. Where others tend to present the model-world relation in terms of direct representation or *isomorphism* (Suppes, 1960; Van Fraassen, 1980), Giere speaks instead of a looser relation of *similarity*: while isomorphism entails a perfect correspondence between elements of the model and the elements of the world, similarity only entails a partial analogy between them. This is particularly evident in cartographic models, which can serve as the quintessential epitome of the representational view. Consider Giere's example of a standard tourist map of the city of Pavia (Fig. 5.1). Like all cartographic models, this map is *partial* in that it represents only some features of the central area of the city (only very few buildings are included, their difference in height is ignored, etc.), and even the features of Pavia that are actually included in the map are represented with *limited accuracy*: relative distances on the map, for instance, do not correspond exactly to relative distances on the ground.

In what sense, then, does this map represent Pavia? For Giere, the answer is that this model is spatially *similar* to what it represents. The lines on the map have similar spatial orientations to the corresponding streets, the two-dimensional surface of the map is similar to the two-dimensional surface of the city, and so on (Giere, 1999b; 45). The difference between representation as isomorphism and representation as similarity can be compared to the difference between natural kinds and clusters, or more precisely between the essentiality requirement and the family-resemblance requirement for kind-membership. Goertz (2006) has operationalized the difference between necessity/sufficiency typologies and family-resemblance typologies in terms of the different logical operators they adopt. By applying a two-valued Aristotelian logic with dichotomous variables, the typologies that are based on necessary and sufficient conditions can be seen as modelled by a conjunction (the logical operator AND) between the variables that interpret the properties that are necessary and sufficient for kind-membership. Family resemblances, on the other hand, are modelled

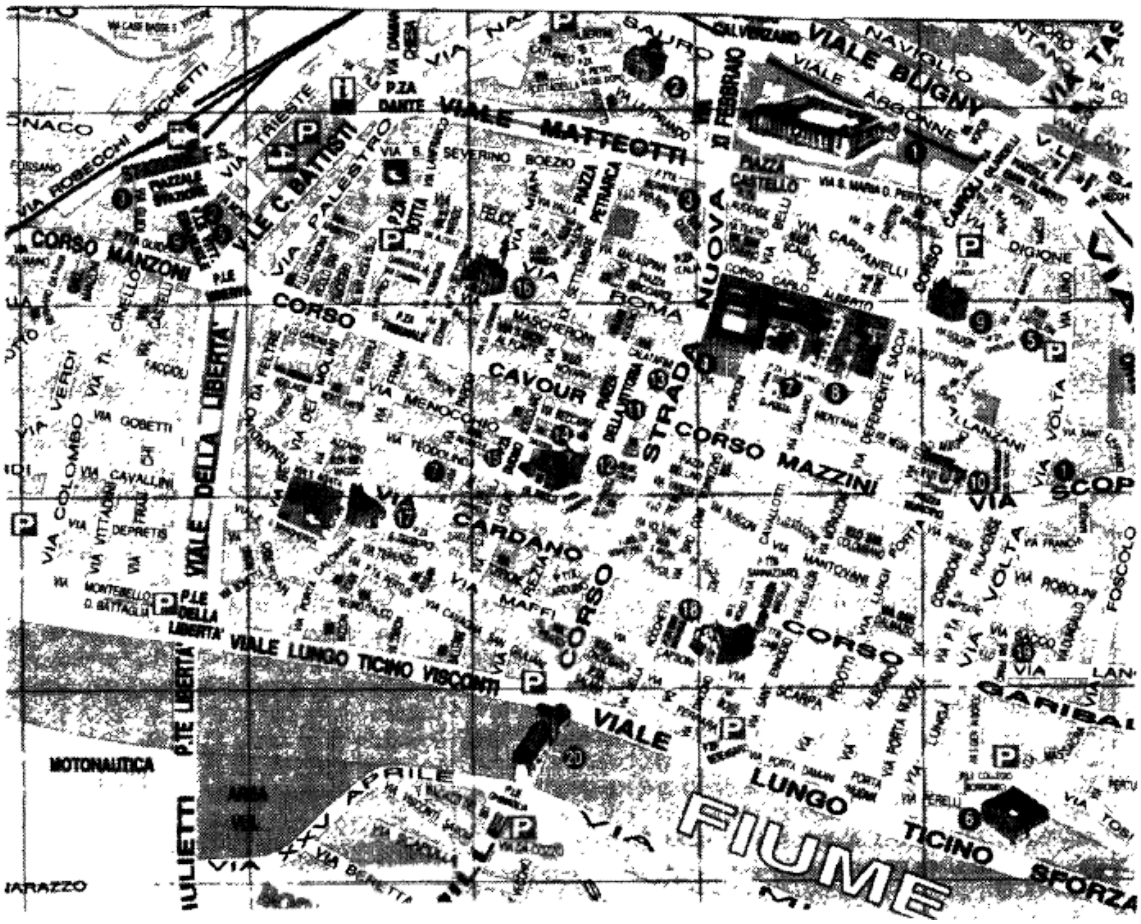

Figure 5.1: Standard tourist map of the central area of Pavia, circa 1998 (Giere, 1999b; 45).

by a disjunction (the logical operator OR) in that none of them are necessary and sufficient. This can also be expressed in set-theoretic terms as an alternative between "intersection" and "union", or in fuzzy-logic terms as "minimum" and "maximum", respectively (Goertz, 2006; 39–44). As one can see, the fact that the essential kinds of the former type of classifications require a conjunction of properties means that the properties that define a kind-concept must also be possessed by each and every of its members, thereby resulting in a one-to-one correspondence (*isomorphism*) between the type-properties of a kind and the token-properties of its members. Cluster-kinds, on the other hand, are modelled by a disjunction of properties, and therefore such one-to-one correspondence does not ensue. In this case, the relation between the properties of a kind and the properties of its members can be characterized as one of *similarity* or, as Wittgenstein characterized family resemblance, "affinity".

2.1. Purpose-relative models

To be sure, similarity thus defined is a vague notion that makes the representational aspect of models trivial. Lacking some specification, anything can be said to be similar to anything else, and even if similarity between two objects involves a selection of what features of those objects are relevant for relevant, one needs to specify what counts as relevant. In the absence

of such a criterion, countless legitimate but possibly contradictory models can be defined. This is the problem of pluralism discussed in section ?? As far as models are concerned, it is customary to rebut this charge by mentioning the fact that models are *purpose-specific*, and therefore similarity need not necessarily be vacuous nor vague: usually, the purpose for which a model has been designed also determines the respects in which the model needs to accurately represent reality as well as the accepted degree of similarity. In this sense, the tourist map in Fig. 5.1 can be said to be accurate for the purpose of directing tourists in that it accurately represents the orientation of the streets as well as their junctions. Indeed, the fact that not all buildings are indicated on the map is what makes it useful in identifying notable monuments for tourism purposes. Subway maps, for instance, are usually less spatially correct than tourist maps, and therefore they are useless for the purpose of walking or driving around the city whose subway system they represent even though they spatially represent subway stations in a way that corresponds to their relative position in the real world. Even the subway system is usually represented inaccurately on subway maps: this is because they are designed in order for commuters and tourists to navigate the subway, and therefore they need to represent intersections more clearly than they need to accurately represent the distance between subway stations.

Moving now from the cartographic to the political domain, we can see that even the models of the special sciences that cannot be represented as maps are usually devised for a purpose. Clarke and Primo (2012), for instance, provide a categorization of the theoretical models used in political science in terms of their purpose, arguing that they can serve four different roles: foundational, organizational, exploratory, and predictive. Empirical models, by contrast, can be used for one or more of the following four purposes: theory testing, prediction, measurement, and characterization (Clarke and Primo, 2012; ch. 4, sec. 2; ch. 5, sec. 3; the four roles of empirical models are derived from the four roles of econometrics as characterized by Hoover, 2006). According to this categorization, Baron and Ferejohn's (1989) legislative bargaining model ought be considered a *foundational* model, insofar as it constructed a framework that is flexible enough to be adapted to answer different questions related to weighted voting, government formation in parliamentary democracies, and the provision of pork-barrel projects versus public goods in legislatures (Clarke and Primo, 2012; 85–86). Identifying a purpose for which whole scientific domains are constructed kinds and which can serve to assess them as models, however, is far less intuitive, even though what Clarke and Primo call the *organizational* and *exploratory* role of models can indicate an answer. The organizational role defines a model in terms of a "framework wherein information may be defined, collected and ordered", while the exploratory role identifies the function of a model that is used to "investigate the putative (causal) mechanisms or motivations underlying phenomena of interest" (Clarke and Primo, 2012; 87, 90; these definitions are derived from Haggett and Chorley, 1967 and; Little, 1991, respectively).

Interestingly, both the "organizational" and the "exploratory" aspect of models can be found in Dudley Shapere's ([1977] 1984) pioneering account of how scattered information about phenomena come to be unified into bodies of information which he calls "domains".

By examining the development of natural-science fields, Shapere concludes that, given an association based on some relation—one may say, family resemblance—between the items considered, the unity of a scientific domain results from bodies of information which have the following characteristics:

(1) The association is based on some relationship between the items.
(2) There is something problematic about the body so related.
(3) That problem is an important one.
(4) Science is "ready" to deal with the problem.

Accordingly, a domain can be defined as the total body of information for which, ideally, an answer to a problem that has arisen is expected to account (Shapere, [1977] 1984; 279–281). Lindley Darden (1974, 1976) also emphasized the role that problems posed by facts play in hypothesis-construction and theory-building in general—although she is less concerned with how this shapes the unity of entire scientific domains (see e.g. Darden and Maull, 1977). More intriguingly, Darden stresses the link between problem solving and arguments from analogy: by generalizing the form of a problem posed by facts, and by identifying similarities between facts, the solutions of problems in a general form can constitute plausible solutions for further problems (see e.g. Darden, 1976; 142). If one combines this with Shapere's account, one can see how domains can also play a *foundational* role in Clarke and Primo's sense.

One obvious problem with the above accounts is that the vagueness of the way models can be said to be similar to reality is only shifted one step back. Whether something can be deemed problematic, whether the problem is important, and whether science is "ready" to deal with it is an arbitrary matter that can give origin to a virtually indefinite conceptualizations of the domain. Shapere is aware of this problem when he consider what he calls the different "lines of research" about the problem regarding the domain:

> … there are often alternative lines of reasoning available which lead to different conclusions; and the issue of which line has the strongest arguments in its favor is not always clear-cut. Thus, whether a certain body of information constitutes a domain or not; whether a certain item is or is not a part of a domain (that is, whether or not a theory for that domain is responsible for accounting for that item); the extent to which a certain problem is important; the extent to which the state of science is "ready" to investigate a certain problem; the degree of promise of a certain proposed line of research; whether a certain specific sort of answer to a problem is reasonably to be expected; whether a certain proposed answer to a problem is adequate—all these can be, and in any given situation in the history of science are apt to be, debated. (Shapere, [1977] 1984; 293–294)

However, Shapere does not see this as particularly threatening in that the future development of a domain can be expected to automatically resolve the disagreement (Shapere, [1977] 1984; 295). But while this Whiggish picture may have some currency in those fields of the natural sciences where new discoveries regularly emerge, its applicability to the social sciences, and politics in particular, is dubious. More generally, this picture is not compatible with the view that conceptualizations of domains are models, i.e. representations which the facts cannot discard as false.

3. Judging models

Models thus face the same problem of pluralism that results from promiscuous typologies: which features one selects as relevant is a matter of stipulation about the purpose of models. A great advantage of models, however, is that this problem is considerably reduced by the fact that models are not confronted directly with reality, but with other models of reality that are lower down in the hierarchy of models. Suppes (1962) has introduced this theory with respect to the formal theories of the natural sciences which are tested experimentally, thus stressing the fact that the experiments that validate models in turn involve an interpretation of the data in terms of the parameters of the models, i.e. they involve a model of the data (Figure 5.2). Physical models, Suppes argues, derive their validity from the fact that their

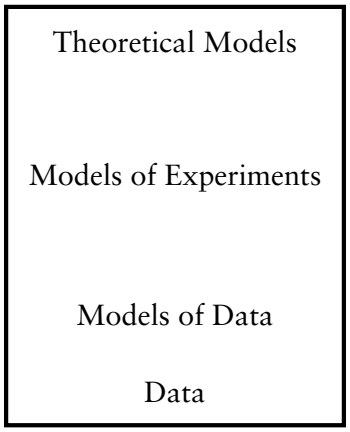

Figure 5.2: Patrick Suppes's (1962) hierarchy of models. (Adapted from Giere, 1999b; 654)

definition also specifies the parameters relative to which they represent they represent reality. The fit of experimental models with the world, for instance, is inevitably tested on the basis of assumptions about the sort of data which the model incorporates. For models are defined in such a way that they specify what aspects of the data they represent, which provides a model of the data. Experimental models can thus be tested on a possible realization of the data—i.e. a model of the data: data are thus processed so as to fit into a model of the data.

This can be extended also to the empirical models that are ordinarily used in political science. The events that political science studies, Clarke and Primo argue, are no less complex than the data with which physics is concerned. This is the reason why political models cannot be but purpose-relative: the purposes of experimental models—theory testing, prediction, measurement, and characterization—specify possible realizations of data, i.e. models of data which provide the conditions for testing the fit of models with the world. But what about *theoretical* models? Clarke and Primo acknowledge that

> our refusal to provide "cookbook" answers to theoretical model evaluation runs the risk of sounding like an "anything goes" approach. If there are no firm metrics for evaluating a model, how can we assert that one model is superior to another, or that one family of models is superior to another? We argue that researchers already make such determinations. (Clarke and Primo, 2012; 101)

By devising a theoretical model, the argument goes, researchers also determine its purpose,

which can be foundational, organizational, exploratory, or predictive. In this respect, the only difference between theoretical and empirical models is that the latter are usually of a statistical nature, which means that they should describe accurately the dependencies within a given data set. More generally, the assumptions of empirical models are subject to a rigorous testing that establishes the adequacy of the model in a way that remains, at least in part, beyond the control of the modeller, while the testing of theoretical models relies more heavily on the internal logic of the model as well as on the scientific interests of the modeller (Clarke and Primo, 2012; 105–106). Yet both theoretical and empirical models, *qua* models, are objects that are neither true nor false, and therefore their adequacy depends on their representational character. Just like empirical models, theoretical models are partial and purpose-relative representations whose adequacy relies on the extent to which they fulfil a foundational, organizational, exploratory, or predictive role.

Theoretical models can thus be judged depending on their usefulness for further model-building (foundational role), for defining, collecting, and organizing information (organizational role), for investigating un(der)known causal processes (exploratory role), and for generating hypotheses that can be used in statistical tests on existing data (predictive role). Just like empirical models are tested, given their purpose, on the basis of their (statistical) adequacy to data, theoretical models can be tested on the basis of their purpose in terms of their *fecundity*—i.e. does the model produce a series of important insights or just one?—and *importance*—i.e. does the model yield vapid or insightful results? (Clarke and Primo, 2012; 100-101)

Finally, not unlike the experimental models of physics, the purpose of the theoretical and empirical models of politics makes their testing based on models of data, and not on the data themselves. This is because Suppes's contention that models are never tested on data but only on models of data derives from a representational view of models: since models are objects that are neither true nor false, they can only be defined in such a way as to represent some aspects of the world, and therefore they specify the parameters for which the data are realized and the fit of the model to the world. Analogously, the purpose of political models specifies what portion of the world in respect of which their empirical adequacy or their fecundity and importance can be judged. Depending on the purpose of the model one can thus present questions to be asked of it in order to judge its fecundity and importance:

> ***Foundational:*** How flexible is the model? Can it be adapted to many different situations? Does the model cause scholars to overhaul an existing line of inquiry or move in entirely new directions?
>
> ***Organizational:*** Does the model subsume a large set of models, or a perhaps smaller set of seminal or important models? Alternatively, does the model subsume a large set of empirical generalizations or facts, or perhaps unify a small number of previously puzzling empirical findings? If a classification system, does the classification system help us better understand how cases are connected?
>
> ***Exploratory:*** Does the model generate a significant number of interesting statements or probe counterfactuals in new ways? Does the model lead the

researcher to examine data in new ways?[2] (Clarke and Primo, 2012; 101)

With respect to political models, Suppes's hierarchy can thus be redesigned as follows:

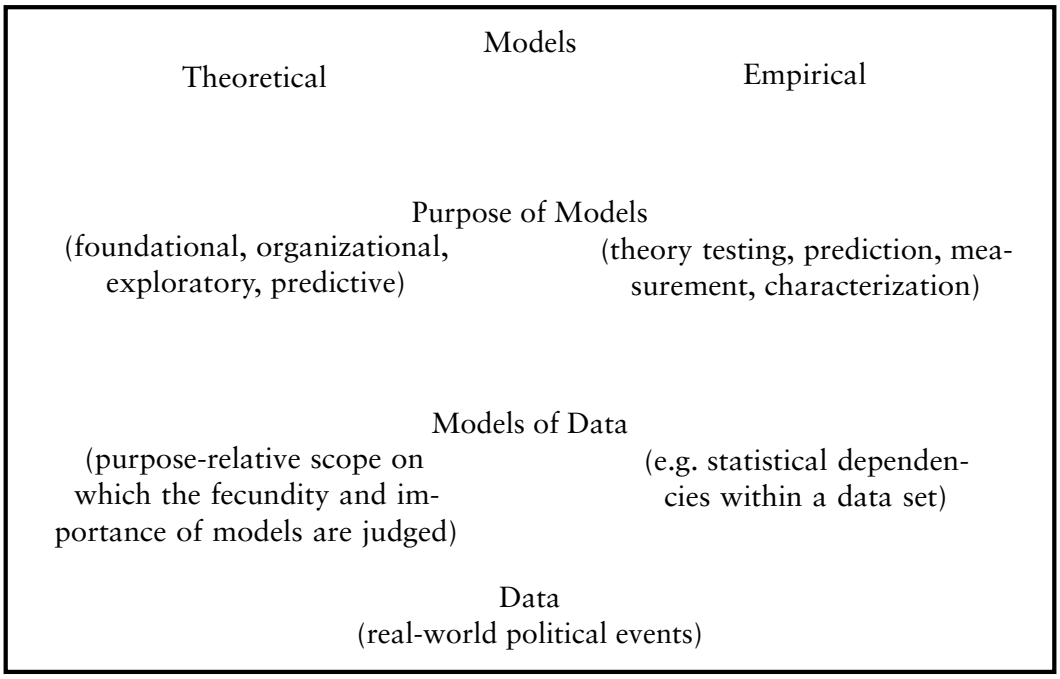

Figure 5.3: Fit of political models with the world.

One way of transposing the assessment of theoretical models at the level of whole domains of scientific investigation is to consider the fact that they are the outcomes of classifications that are constructed for explanatory purposes. The lower-level models which they are confronted with are, therefore, the models of explanation. Classified objects are real-world entities endowed with dispositional properties that underwrite the causal powers that entities have to generate *explanandum*-events; classifications describe classes of entities that are defined on the basis of family resemblances between those properties. Their explanatory value, therefore, depends on the explanatory power of the model of explanation on which they rely. As repeatedly mentioned, the case for moving away from a state-centred conception of politics was originally based on epistemological concerns about the possibility of discovering "political functions" in non-state structures. This contention was in turn based on a covering-law view of comparative research which prescribed the search for functional regularities across time and space. It is on this basis that the emphasis on cross-national taxonomies in concept formation led to the characterization of an overarching "political system" which was defined in terms of general characteristics about its (institutionally situated) functioning as well as the outcomes it produced. Traditional conceptions of politics and the model of reality they resulted in were thus based on a covering-law model of explanation.

2. Although models are often presented with a predictive goal, Clarke and Primo argue that theoretical models with a mainly predictive role are quite rare to be found in practice. Most purportedly predictive models generally focus instead on generating new (testable) hypotheses, and therefore fall under one or another of the other three categories of models. This matches the doubts about the actual role and value of prediction in science that have been expressed in footnote 1 above.

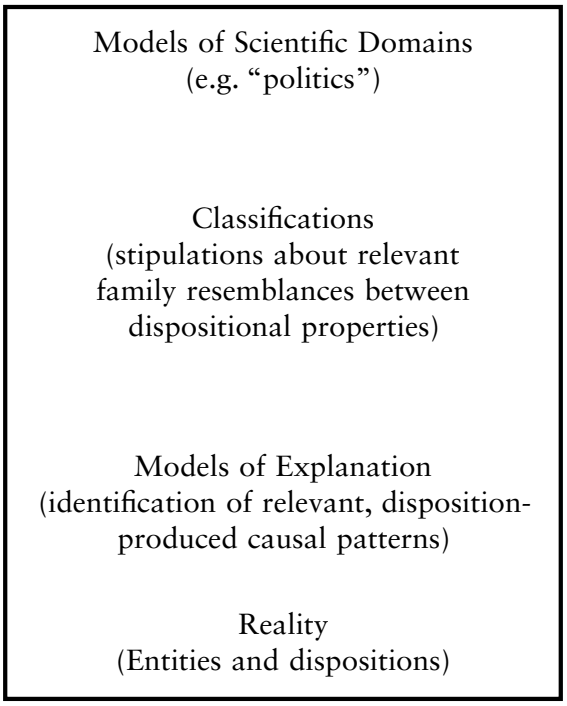

Figure 5.4: Domain-world relationship.

Clarke and Primo argue that because models are purpose-relative there is no firm metrics for testing them. Indeed, for Clarke and Primo to argue on the contrary is tantamount to subscribe to a D-N conception of science according to which the purpose of theories is to generate testable hypotheses that can be validated empirically. This is because, as they argue, the desire to test theoretical models is concomitant with the desire to choose between explanations (Clarke and Primo, 2012; 164). Yet models of explanation are not representational objects that are neither true nor false; even though it may be misplaced, strictly speaking, to say that a model of explanation is "true" or "false", their adequacy to reality can nonetheless be assessed in some other way. The D-N model, for instance, was fiercely criticized for some of the puzzling features it implied about the confirmation of scientific hypotheses, which eventually led to its discredit among philosophers of science and practicing scientists alike.

More generally, it has been argued that recent methodological debates about adequate explanation in the social sciences in fact belie irreconcilably divergent views about the nature of causal explanation. The alternative between covering-law and mechanism-based models of explanations, for instance, can be interpreted in terms of an alternative between different goals ascribed to causal explanation. In chapter 2, this has been phrased in terms of a choice between anti-realist eliminativism (which underpins the formulation of covering laws, and the D-N view of science that was based on it) and realism about causation (which justifies the search for generative mechanisms). The contrast between causal realism and anti-realism is particularly relevant in that it implies stipulations about the makeup of the world in which the *explanandum*-phenomena take place. The flaws of anti-realist models of

explanation, it has been argued, cannot but derive from underlying inadequacies about the way in which they conceptualize the nature of causation. Conversely, a realist perspective about causal explanation and a productive conception of the metaphysics of causation have been advocated in the light of correspondence to the way in which phenomena are actually produced in reality, namely, as materializations of the dispositional properties of entities.

Scientific realism thus demands that such models provide causal explanations in terms of the causal processes *as they have actually occurred in the real world*. In this way, scientific realism provides the missing link between models of explanation and reality that can serve to judge "higher-order" models such as general conceptions about the boundaries of a specific domain of reality, which derive from a classification of the actual properties of real-world entities. Even though "higher-order" models cannot be directly judged by confronting them with reality, the classificatory concepts they assume can be assessed on the basis of the model of explanation they correspond to.

Conclusion

Throughout this work, I have argued that the different conceptualizations of the nature of politics that characterize different political theories can be related to the purpose of guiding scientific explanation. This is because, as an analytical construct, the very idea of politics has been shaped by the progressive compartmentalization of social knowledge into separate disciplines. Politics thus denotes the scope of political science, and therefore I have referred to the sphere of phenomena it refers to as a "scientific object". It has been further argued that this object is constructed by means of a classificatory practice. Insofar as a scientific object denotes a class of empirical phenomena whose causes can be identified by means of the explanatory apparatus of a given science—i.e. its model of explanation—, those phenomena make for a unified scientific object because they are included into a class that is amenable to a homogeneous causal treatment through the chosen model of explanation. The causes of political phenomena, it has been argued, ought to be identified with the particular dispositional properties of individual entities that interact to produce certain outcomes, and therefore each phenomenon is produced, strictly speaking, by a particular cause—or set thereof. The only way in which the particular causes of phenomena can be homogenized into a unified class is through the recognition of some *similarity* between those causes that holds over and above their specificities. However, such similarity only exists, as it were, in the mind of the beholder: it is the purpose of the classification that determines the classificatory criteria, and therefore the way in which different phenomena are included into a single class depends on what the inquirer considers to be relevant similarities for classificatory purposes.

These aspects are in fact implicit in the idea of "ideal-type" that has been influentially developed by Max Weber. As Weber defines it,

> An ideal type is formed by the one-sided *accentuation* of one or more points of view and by the synthesis of a great many diffuse, discrete, more or less present and occasionally absent *concrete individual* phenomena, which are arranged according to those one-sidedly emphasized viewpoints into a unified *analytical* construct (*Gedankenbild*). In its conceptual purity, this mental construct (*Gedankenbild*) cannot be found empirically anywhere in reality. (Weber, [1904] 1949; 90, emphasis in the original)

For Weber, the elaboration of ideal types into unified analytical constructs is an important part of theory-building, in that it allows for the inquirer to go beyond the mere reconstruction of particular phenomena and establish their interconnectedness and relations. Mental constructs like the idea of "the church" or the idea of "the state"—and, one may add, the idea of "politics"—have no immediate empirical counterpart in reality, where only concrete

and particular phenomena exist. Yet they are of great heuristic value in that they offer guidance to the construction of explanatory hypotheses concerning individual concrete patterns that are significant in their uniqueness, e.g. by revealing familiar but unapparent characteristics in the Christian church or in the German and the American states. This, as we have seen, is precisely the function of *scientific models*. As Clarke and Primo (2012) argue, all representational models, particularly the models of political science, are purpose-relative; the purposes of theoretical models, in particular, can be variously characterized as *foundational*, *organizational*, or *exploratory* depending on whether they are capable of being adapted to a variety of contexts, can organize empirical items of knowledge in an orderly fashion, or allow to uncover previously undiscovered causal processes. Despite these internal differences, however, it is clear that all purposes of theoretical models as identified by Clarke and Primo can be characterized as "heuristic" in Weber's sense, in that they all purport, so to speak, to reduce the unfamiliar to the familiar. As a model that represents the objects that fall within the scope of political science, the political sphere is a prime example of an ideal type: its aim is to show that the great diversity of phenomena it encompasses can be explained in their uniqueness by means of a similar methodological treatment.

This also makes it important to stress that these mental constructs are radically different from real-world objects, and that they ought not be confused with them. In this respect, Weber revealingly emphasizes that the relationship between ideal types and reality can be generally considered to be a relationship between a class-concept and the members of that class. However, it is clear from Weber's conception of ideal types that such class-concepts cannot be taken to describe what I have referred to as "natural kinds": insofar as the construction of an ideal type is determined by the arrangement of concrete phenomena into a purely theoretical construct, ideal types are mind-dependent rather than "natural". As Weber put it, "A 'definition' of such synthetic historical terms according to the scheme of *genus proximum* and *differentia specifica* is naturally nonsense" (Weber, [1904] 1949; 93): the meaning of those terms is instead the outcome of a *stipulation*, which for Weber depends on the one-sided accentuation of the viewpoint of the particular inquirer. Ideal types are thus *cluster*-concepts of the kind that, I have argued, lies at the basis of any conceptualization of the political domain.

The similarities between ideal types and the dynamics of political concept-formation have not gone unacknowledged; Burger ([1976] 1987), for instance, emphasizes that Weber's theory of ideal types accounts for the formation of the general concepts of social science in a way that utterly conflicts with the classical understanding of the value and nature of typology-building in the social sciences, while Collier and Mahon Jr. (1993) explicitly relate this particular understanding of political science concepts to the procedures for making generalizations with "family resemblances" and "radial categories". There are, however, important differences between the account of political concepts that I have proposed and the theory of ideal types.

Weberian ideal types are, ultimately, *value-concepts*. Weber argues that the one-sided accentuation of a certain viewpoint, which lies at the basis of the ideal-typical reliance on

analytical constructs, arises from the cultural significance that the inquirer attributes to the particular events under consideration. Such significance is not to be found in any concrete features of the particular phenomena, not even in the way these are theoretically arranged in the abstract form of ideal types. The significance that is attributed to the phenomena on which ideal types are constructed is instead *presupposed* on the idea that they can be related to social and economic *problems*, and that the theorization of ideal types through the application of the analytical methods of social science can determine more precisely those *problematical* questions. With reference to the work featured in his own *Archiv für Sozialwissenschaft und Sozialpolitik*, the journal in which his discussion of ideal types appeared, Weber writes for instance that the emergence of the question of labour conditions in Germany and its inclusion in public and legislative discussion in the 1880s led social researchers to shift the accent of their work to the analysis of more universal dimensions of the problem. The economic focus of the journal thus expanded from the study of individual motives behind economic exchange to the overall economic conditioning of the conditions of all social classes in all modern states. As with the case of "politics" as a scientific object, the construction of an ideal type is thus capable of determining the boundaries of a field which a particular empirical science investigates.

The meaningfulness of ideal types, however, is rooted in a dimension of scientific relevance which is exogenous to their scientific investigation as such. Ideal types, writes Weber, are valuable concepts "*only* because and *only* insofar as their existence and the form which they historically assume touch directly or indirectly on our cultural *interests* and arouse our striving for knowledge concerning problems brought into focus by the evaluative ideas which give *significance* to the fragment of reality analyzed by those concepts" (Weber, [1904] 1949; 81, emphasis in the original). For this reason, "the significance of cultural events presupposes a *value-orientation* towards these events":

> The concept of culture is a *value-concept*. Empirical reality becomes "culture" to us because and insofar as we relate it to value ideas. It includes those segments and only those segments of reality which have become significant to us because of this value-relevance. Only a small portion of existing concrete reality is colored by our value-conditioned interest and it alone is significant to us. It is significant because it reveals relationships which are important to us due to their connection with our values. Only because and to the extent that this is the case is it worthwhile for us to know it in its individual features. We cannot discover, however, what is meaningful to us by means of a "presuppositionless" investigation of empirical data. Rather perception of its meaningfulness to us is the presupposition of its becoming an *object* of investigation. (Weber, [1904] 1949; 76, emphasis in the original)

I have argued instead that the one-sided representation of whole domains of phenomena through abstract class-concepts is *internal* rather than *external* to the logic of science. To be sure, the conceptual typologies of science must follow a family-resemblance logic that makes them based on a more or less arbitrary *stipulation* about the purpose and validity of those classifications. Political concepts, I have argued, can only be based on family resemblances between the particular entities they stand in for, which in turn depend on their relevance for the purpose of the classification. For this reason, the logic of classification-building is similar to the one-sided accentuation of the viewpoints that leads to the formation of ideal

types: the purposes for which classifications can be constructed are as many as the points of view on what particular phenomena are problematically significant and worthy of typization. The result of this variety of purposes is what I have referred to, following Dupré (1993), as the "promiscuity" of classifications: like ideal types, no classification can be said to be unequivocally valid on the basis of scientific criteria; in fact, all conceptualizations have an equal claim to validity insofar as they all stress some equally valid similarity between the real-world causes of phenomena. Nonetheless, I have characterized the purposes of political concept-formation as *explanatory* rather than based on its extra-scientific *cultural interest*. Insofar as political concepts are meant to denote causal similarities between the processes that produce the phenomena of interest, their purpose is to define the applicability of a given model of explanation. Conceptualizations of the political sphere thus acquire significance from the *model of explanation* that they assume, and not from the *value-orientation* of the theorist. Unlike ideal types, the meaningfulness of scientific objects is best characterized as *explanatory*, and not as *evaluative*. It is for this reason that the concept of a representational *model* has been introduced: all models, it can be argued, are purpose-relative; yet their use is first and foremost scientific, and therefore their purpose can be subsumed under what a scientific theory can explanatorily account for.

Not all scientific models, of course, have an explanatory purpose that can be directly related to the model of explanation they implicitly adopt. Even though the theoretical models of political science may all have a heuristic purpose that orients the explanatory theories of political science, this cannot always be measured in purely explanatory terms. Foundational models, for instance, may achieve their purpose because they prove fruitful in further model-building, or because they reveal the previously unacknowledged importance of the aspects they model. Clarke and Primo argue for instance that Baron and Ferejohn's (1989) model of legislative bargaining has provided fruitful insights on the bargaining dynamics of international negotiations, which has led international relations scholars to study and model the conditions for international bargaining (Clarke and Primo, 2012; 85–87). Similarly, models can meet the purpose of organizing knowledge in a non-explanatory way, i.e. by generalizing an existing set of models. Exploratory models, too, can be positively judged in non-explanatory terms, even though their primary use is in investigating explanatory mechanisms. This is because models can shed light on the relevance of certain causal factors without implying anything as to how these are to be scientifically explained. Romer and Rosenthal (1978), for instance, reveal a puzzling feature of political voting on tax expenditure that arises from the way in which voters' preferences can be modelled in relation to the tax-expenditure decision. The agenda setter proposing a budget, in particular, can be modelled as a budget-maximizing bureaucrat who requires voter approval to impose some tax, while the voter's behaviour is modelled according to choice theory. Romer and Rosenthal show that agenda setters can exert a monopoly power that depends on facing voters with a "take-it-or-leave-it choice", and that is greater the worse the *status quo* is. However this monopoly power is causally explained, this model reveals an existing feature of decision-making on tax expenditure that depends on the relative position of voters vis-à-vis agenda setters and which

the models uncovers thanks to its simplification of the behaviour of the relevant actors.

Unlike the theoretical models of political science, however, the conceptualizations of "politics" as a scientific object have a quintessentially *explanatory* purpose that depends on the model of explanation they presuppose. The scientific object they refer to, it has been shown, is constructed on the basis of a pre-existing assumption of what political science can explain. As a class-concept, what "politics" includes is determined by the boundaries of the domain of phenomena that constitutes the field of investigation of political science. That such boundaries do not mirror any real-world divisions between different social phenomena has been shown to be an implicit assumption of much empirical research, and I have tried to account for it in metaphysical terms by referring to the dispositional properties that constitute the building blocks of the empirical world. From a realist point of view, the causal structure of phenomenal reality is determined by the "activation" of the particular causal properties of entities, and therefore to explain a phenomenon is to identify the way entities causally interact in virtue of their properties. This also reveals a further, more fundamental difference between the theory of ideal types and the model theory of scientific objects. The scientific relevance of ideal types lies in the fact that the arrangement of concrete phenomena into analytical constructs allows for causal relations to be established. Without the help of mental constructs, Weber argues, the causal explanation of an individual event is impossible: since the number of causes that influence any given event are always infinite, their construct-free investigation can only yield "a chaos of 'existential judgments' about countless individual events" (Weber, [1904] 1949; 78). In contrast, ideal types set apart only the particular aspects of this complexity to which we collectively attribute cultural significance and therefore allow us to put into less chaotic causal relations those objects that we deem worthwhile knowing. This is not, however, a practical solution to a merely epistemic limitation of the inquirers with respect to the causal complexity of the world. As Weber carefully specifies, "an *exhaustive* causal investigation of any concrete phenomena in its full reality is not only practically impossible—it is simply nonsense" (Weber, [1904] 1949; 78, emphasis in the original). The relata of causal relations can only be the "essential" features of events that we extract from concrete reality in virtue of judgements that are related to our cultural values; causal connections as such only exist in the interconnected arrangement of the empirical material into theoretical constructs. Even as far as individual phenomena are concerned, causal explanation is, as Weber put it, a matter of theory-laden "*imputation*" as a consequence of their ideal-typical arrangement.

A realist account of causation, on the other hand, allows us not only to explicate the way in which "politics" as a class-concept can play a distinctively *scientific* role. It also helps us to account for the plurality of conceptualizations of the political sphere as representational *models*. As detailed in the first part of this work, the anti-realist view of scientific explanation implicit in the covering-law model was presupposed on a *foundationalist* approach to science. By defining causation in terms of inferential subsumption under covering laws, traditional political science adopted an anti-realist approach to causal explanation: what counts as a valid explanation is defined on the basis of logical criteria of inferential validity regardless

of whether the causal relations thus established correspond to the actual structure of the factual world. As I have argued, this anti-realist approach depended on the foundationalist approach that political science methodology shared with the philosophy of science of logical empiricism. Logical empiricists insisted that the distinctive character of scientific claims lies in their truth or falsity—as opposed to the downright "meaninglessness" of metaphysical statements. From an anti-realist standpoint, the truth or falsity of empirical statements, particularly statements about the causal relation between empirical phenomena, can only be established by appealing to criteria of inferential validity. The claim that causal links are established by subsumption under covering laws rather than found in the actual structure of phenomena implies that causal statements organize unrelated empirical phenomena into a causal connection whose validity is secured by the confirmation of laws. Insofar as laws are well-confirmed, the truth-preserving nature of inferential logic implies the validity of empirical claims about causal relations that can be inferred from those laws. As discussed at length in chapters 1 and 2, traditional approaches to political explanation relied heavily on a covering-law understanding of causal explanation—typically in the form of comparative analysis as a means to empirically arrive at general laws, or by relying on correlational analysis for their confirmation.

The foundationalist presuppositions of the anti-realist approach to causal explanation, however, conflict with a pluralistic view of the way in which the political domain can be conceptualized. The logical foundation of science implies a fundamental demarcation between the valid claims of science and invalid claims or, more generally, between whatever empirical phenomena can be subsumed under covering laws and what lies outside their scope. As I have aimed at showing, the traditional demarcation of the political domain by means of the abstract concept of "the political system" rests upon a covering-law definition of the relationship between the empirical phenomena that political science investigates and the functions that they perform. As a scientific object, the political system is constructed through the investigation of covering laws that describe the general form of the phenomena that different social objects can produce in different contexts. Insofar as the explanatory validity of covering laws is rooted in their capacity to inferentially yield valid empirical claims, however, the political system has no representational value beyond what covering laws are said to describe: the object of politics is entirely exhausted by the logical criteria on which the validity of covering laws is claimed to rest. This also means that, on this view, the scope of political science—and therefore the definition of what politics is—must be unequivocally defined on the basis of logicist criteria rather than in accordance with realist principles. This is the sense in which, I have claimed, realism can be said to provide the tools for judging the validity of models on the basis of the logic of explanation they presuppose. Realism *per se* does not favour any specific model of the political domain—in fact, I have claimed that realism can account for the scientific pluralism of models by ascribing a *representational* instead of a *foundational* value to the demarcation of a domain of phenomena. The reason for this is the same reason why anti-realism does favour a distinct, law-based definition of the political sphere. For anti-realists, scientific validity lies in the logical structure of scientific

theory, and therefore whatever can be subsumed under the covering laws that constitute it—the scope of the scientific theory—is also determined by that structure. Realism, on the other hand, is a theory about the causal structure of the world. Explanation is, in this view, piecemeal and corresponds to the particular causal processes that generate the phenomena to explained. The way these phenomena are bound together is a matter of *representation*, which can only be partial and open to pluralism.

For this very reason, however, realism demands that we do away with the idea that any particular conceptualization of politics can make an exclusive claim to validity. More importantly, realism demands for conceptualizations of politics to not be based on a covering-law understanding of the political sphere. This conclusion is particularly relevant here, since definitions of politics are uncritically passed on from one generation to the other political scholars. Indeed, the covering-law model of explanation itself is still considered by many political scientists as the royal road to political explanation despite the general discredit in which it has fallen in the philosophy of science. Clarke and Primo (2007) report for instance the results of a survey of the three leading generalist journals in political science: the *American Political Science Review*, the *American Journal of Political Science*, and the *Journal of Politics*. 10 percent of the 738 articles published in these journals between 2001 and 2005 were randomly sampled, and using very conservative coding rules, nearly half of them were identified as using or promoting a variant of the covering-law model of explanation. Far from being rare, Clarke and Primo argue, this model is "practiced in all subfields, with the exception of political philosophy, and … is often seen as the gold standard of political science research" (Clarke and Primo, 2007; 746). Because of the "promiscuity" that arises from the realist view of models as representational objects, no unequivocal prescriptions can be derived from realism as to what an adequate model of political objects would look like. In fact, from a realist point of view all models are, strictly speaking, redundant for the purpose of causal explanation—safe for heuristic or communicative purposes. For this reason, however, realism does yield prescriptions as to how explanation can be produced. As I have argued, the debate on mechanism-based explanation vis-à-vis covering laws can be read through the lenses of a realist opposition to an anti-realist understanding of explanation in political science.

Let us conclude with a note on the place of generalizations within the causal-realist framework that I have defended throughout this work. That realist explanation must reconstruct the causes of specific phenomena in terms of the manifestation of the dispositional properties of "powerful particulars" seems to imply that causal explanation ultimately comes down to a "historical" or etiological or even "phenomenal" reconstruction of the causes of particular events, albeit with reference to the dispositional properties of the entities involved in the causal process in question. This conclusion would appear to be problematic, in that it conflicts with the common-sense understanding of what science—and therefore explanation—is: as mentioned in the introduction, a basic feature of scientific explanation is that it is capable of "explaining much by little", or at least by isolating the relevant factors, instead of providing detailed descriptions of just what happened. Thus understood, the

tension between dispositional explanation and generalization would appear to escalate to an unmanageable conflict between different scientific goals that trade off—as expressed e.g. in the traditional distinction between an "idiographic" and a "nomothetic" explanation. On the one hand, causal processes involve the activation of the particular generative properties of individual entities that are capable of bringing about specific events—the *explananda* of science. On the other hand, we wish to generalize those causal processes beyond the specific context in which they occurred in order for explanation to be of any relevance to scientific explanation; if only intuitively, scientific relevance has to do with the regularity or generality with which a certain causal pattern occurs.

The problem, however, lies with the understanding of what generalities are, and therefore what type of "generalization" can be set as a desirable goal for scientific explanation. Realist criticism to the covering-law model of explanation, it has been argued, involves a fundamental distinction between a *causal* and an *inferential* understanding of explanation. While the former implies a commitment to the assumption that causal processes are "out there" to be discovered, the latter is implicitly based on the idea that causal relations are ultimately logical relations, and therefore they only exist, strictly speaking, in the mind of the inquirer. In this sense, the search for logically established generalizations of the links between causal variables cannot obliterate, from a realist point of view, the way in which those very causal factors actually combine—in a mechanistic fashion—to produce specific outcomes. But while it is certainly true that the way in which entities combine into mechanisms is highly contextual and therefore its reconstruction can only be generalized across different contexts to the detriment of causal realism, this is no *a priori* case against regularities. As Bhaskar put it, regularities may well be given in experience and mechanisms required to *explain* them. Thus, regularities may actually occur, and mechanism-based variation in how entities causally interact can provide a causal account of how similar outcomes obtain under different conditions.

Moreover, dispositional explanation is not necessarily conducive to etiological explanation. One way of appreciating this is to contrast two different ways of providing causal explanations that are in line with a dispositional realism. In his seminal defence of causal realism, for instance, Salmon (1984), clearly equates explanation with the *etiological* redescription of the process at stake: in this way, the antecedent causes that produce the *explanandum*-phenomenon can be fully revealed and therefore an adequate explanation is provided. On the other hand, causal explanation may aim at describing the dispositional properties in virtue of which a mechanism will generate the *explanandum*-phenomenon (instead of a different phenomenon). For want of a better term, such explanation can be called, following Craver and Kaplan (forthcoming), *constitutive* rather than etiological. As opposed to etiological explanation, constitutive explanation aims at revealing the features of the mechanism for a phenomenon, i.e. the properties of the entities that constitute the mechanisms and that interact so as to produce the phenomenon to be explained. In fact, mechanism-based explanation as described in this work is synonymous with constitutive explanation thus conceived: qua causal properties, dispositions alone can be taken to be the

genuine components of the generative mechanism which explanation mirrors.

Notice that this defeats the notion that causal explanation is automatically improved by providing more detail and that dispositional explanation accordingly comes down to the idiographic description of particular causal events. On the contrary, only a few properties of the entities involved in the process at stake can typically be considered relevant to the causal production of the *explanandum*-phenomenon. It follows that explanatory power does not lie with detailed description, or merely "phenomenal" accuracy. Mechanism-based explanations do map onto the ontic—i.e. worldly—causal processes that generate the effects that are of interest to scientific explanation, as per causal realism. Yet they do so precisely because they are capable of *abstracting* from detail and single out the genuine factors involved in causal production—namely, the dispositional properties of the relevant entities in which the mechanism consists. *Abstraction* thus plays a role within the framework of a realist approach to causal explanation, and indeed a crucial one insofar as abstraction in the above sense is the core of mechanism-based explanation—it is the bearer of explanatory power, as it were.

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