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MECHANISMS, COUNTERFACTUALS, AND LAWS[1]

Stavros Ioannidis and Stathis Psillos

1. Introduction

There have been two traditions concerning how the "link" between cause and effect is best understood (Hall 2004; Psillos 2004). According to the first tradition, which goes back to Aristotle, there is a *productive relation* between cause and effect: the cause produces, generates, or brings about the effect. This productive relation between cause and effect has been typically understood in terms of powers, which in some sense ground the bringing about of the effect by the cause. According to the second tradition, which goes back to Hume, the link is some kind of robust relation of dependence between what are taken to be distinct events. On this account, the chief characteristic of causes is that they are difference-makers: the occurrence of the cause makes a difference to the occurrence of the effect (for Hume's theory of causation, see Chapter 10). There are various ways to understand the notion of difference-making (e.g. in terms of laws or probabilities); but arguably the core notion of difference-making is counterfactual, i.e. based on contrary-to-fact hypotheticals. That is, a causal claim of the form "A caused B" would be understood as implying: if A hadn't happened, B wouldn't have happened either. It is in this sense that A *actually* makes a difference for B.[2]

The currently most popular version of the production approach cashes out the link between cause and effect by reference to mechanisms. The central thought behind a mechanistic account of causal production is that two events are causally connected, if and only if there is a mechanism that connects them. Hence, where there is causation, there is mechanism. As we shall see, however, various mechanists tie together a power-based account of production and a mechanistic account. In this chapter, we shall focus our attention on mechanisms and aim to compare mechanistic accounts with counterfactual accounts of causation.

As we will see, some mechanists tend to refrain from using counterfactuals. For others, counterfactuals are needed to ground the laws that characterize the interactions between the components of a mechanism; counterfactuals may in turn be grounded in lower-level mechanisms. Yet other mechanists try to dispense with both counterfactuals and laws, in favor of activities. Hence, understanding the relation between mechanisms and counterfactuals requires clarifying the relation between mechanisms and laws (on the relationship between mechanisms and laws, see also Chapter 12). Laws will thus be central in the argument of this chapter. The key question then, for our purposes, is: can there be a conception of mechanism which does not ineliminably rely on some non-mechanistic account of counterfactual dependence?

To be exact, we want to investigate whether a mechanistic theory of causation ultimately relies on a counterfactual theory (and hence, whether it turns out to be a version of the dependence approach); or whether it constitutes a genuine version of the production approach (either because it altogether dispenses with the need to rely on counterfactuals, or because it grounds counterfactuals in mechanisms). To be clear on these issues presupposes, as we will argue in section 2, a more careful analysis of the notion of mechanism. Here is the central line of argument we want to investigate: since a mechanism is composed of *interacting* components, the notion of a mechanism should include a characterization of these interactions; but if (i) these interactions are understood in terms of difference-making relations and (ii) these difference-making relations are not in turn grounded in mechanisms, then there is a fundamental asymmetry between mechanistic causation and causation as difference-making; for to offer an adequate account of the former presupposes an account of the latter.

As we will see, not all philosophers that stress the importance of mechanisms for thinking about science are after an account of causation. For some of them, mechanisms are important in understanding scientific explanation and theorizing, but it is not the case that causation *itself* is mechanistic (see, for example, Craver 2007, 86). Yet even if these philosophers do not have to provide a full-blown account of the ontology of mechanisms, they have to explain the modal force of mechanisms; hence the issue of the relation between mechanisms and counterfactuals is crucial. So, we can formulate our central question in a more comprehensive way, as follows: given that a mechanism consists of components that interact in some manner, and thus cause changes to one another, does an account of these *interactions* require a commitment to counterfactuals? As we shall see, ultimately, the issue turns not around the need or not to posit relations of counterfactual dependence but around what the suitable truth-makers for counterfactuals are.

2. Mechanisms-for vs mechanisms-of

Two traditions have tried to reclaim the notion of mechanism in the philosophical literature of the twentieth century. In exploring the relation between mechanisms and laws/counterfactuals, it is important to distinguish between these two very different senses of "mechanism."

In the fairly recent literature, mechanisms are always understood as mechanisms *for certain behaviors* (see Chapter 8). In other words, mechanisms are individuated in terms of what they do. For example, there are mechanisms *for* DNA replication, or *for* mitosis. What the mechanism does, what the mechanism is a mechanism *for*, determines the boundaries of the mechanism and the identification of its components and operations (see Chapter 9 for more on components and boundaries of mechanisms). Such mechanisms, which we call *mechanisms-for* (i.e. mechanisms *for* certain behaviors/functions), are the mechanisms that, according to many authors, figure in explanations in biomedical sciences and elsewhere, and are what many scientists aim to discover. Mechanisms-for we find, among others, in Machamer, Darden, and Craver (2000), Bechtel and Abrahamsen (2005), Craver (2007), and Bechtel (2008). This kind of conception of mechanisms is, arguably, the dominant one in various philosophical studies of mechanisms and their role in the various sciences.[3]

The second sense of mechanism is typically found in the context of mechanistic theories of *causation*. These theories aim to characterize the causal link between two events (to fathom Hume's "secret connexion") in terms of a "mechanism." For this second sense, what the mechanism *does* is not important; what is important is that it is actually there underlying or constituting a certain kind of process. More precisely, what makes a process *causal* is the presence of a mechanism which mediates between cause and effect (or whose parts or moments are the "cause" and the "effect"). We call such mechanisms *mechanisms-of*. Mechanisms-of are the mechanisms discussed in, for example, theories that view causation as mark-transmission (Salmon 1984), persistence, transference, or possession of a conserved quantity (Mackie 1974; Salmon 1997; Dowe 2000).

Talk of "mechanisms" in relation to causation goes back to John Mackie (1974), who took it that causation consists in a "causal mechanism"; that is, "some continuous process connecting the antecedent in an observed . . . regularity with the consequent" (1974: 82). His preferred account of a causal mechanism in terms of qualitative or structural continuity, or *persistence*, exhibited by certain processes, faced significant problems which led Wesley Salmon (1984) to argue for an account of causal mechanism that is based on the notion of structure-transference (see Psillos 2002: section 4.1; also Chapter 10 of this book). Salmon kept the basic idea that "[c]ausal processes, causal interactions, and causal laws provide the mechanisms by which the world works; to understand *why* certain things happen, we need to see *how* they are produced by these mechanisms" (1984: 132). But he claimed that the distinguishing characteristic of a causal process (and hence of a mechanism) is that it is capable of transmitting its own structure or modifications of its own structure. Generalizing Hans Reichenbach's (1956) idea that causal processes are those that are capable of transmitting a mark, Salmon noted that any process, be it causal or not, exhibits "a certain structure."

A causal process is then a process capable of *transmitting* its own structure. But, Salmon added, "if a process—a causal process—is transmitting its own structure, then it will be capable of transmitting certain modifications in the structure" (1984: 144). But as many critics noted, the very idea of structure-transference (*aka* mark-transmission) cannot differentiate causal processes from non-causal ones, since *any* process whatever can be such that *some* modification of *some* feature of it gets transmitted after a single local interaction. A typical example was the shadow of a car with a dent—this is a "dented" shadow, and the mark is transmitted with the shadow for as long as the shadow is there. In response to this Salmon strengthened his account of mark-transmission by requiring that for a process P to be causal, it is necessary that "the process P would have continued to manifest the characteristic Q if the specific marking interaction had not occurred" (1984: 148). It should be clear, though, that this kind of modification takes us back to persistence! In effect, the idea is that a process is causal if (i) a mark made on it (a modification of some feature) gets transmitted after the point of interaction and (ii) in the absence of this interaction, the relevant feature would have *persisted*, where the required persistence is counterfactual.

Salmon did modify this view further by adopting Phil Dowe's (2000) conserved quantity theory, according to which "it is the possession of a conserved quantity, rather than the ability to transmit a mark, that makes a process a causal process" (2000: 89). On what has come to be known as the Salmon-Dowe theory, a *causal process* is a world line of an object that possesses a conserved quantity. And a *causal interaction* is an intersection of world lines that involves exchange of a conserved quantity.

Dowe fixes the characteristic that renders a process causal and, consequently, the characteristic that renders something a mechanism. A conserved quantity is "any quantity that is governed by a conservation law" (2000: 91), e.g. mass-energy, linear momentum, charge, and the like. Apart from various issues that have to do with the question of whether this theory can avoid counterfactuals (see Psillos 2002: chapter 4), the main practical concern is that this account of mechanism is too narrow. For even if *physical* causation—and hence physical mechanism—was a matter of the possession of a conserved quantity, it's hard to see how this account of mechanism can even start shedding any light on causal processes in domains outside of physics (biological, geological, medical, social). These will have to be understood either in a reductive way or in non-mechanistic (Dowe-Salmon) terms.

A rather liberating conception of causal mechanism was offered by Rom Harré in the early 1970s. Harré connected the traditional idea of power-based causation with the traditional idea that causation involves a mechanism. What he called "generative mechanism" can be put thus:

generative mechanism = powers + mechanisms

As he (1972: 121) put it: "The generative view sees materials and individual things as having causal powers which can be evoked in suitable circumstances." And he added: "The causal powers of a thing or material are related to what causal mechanisms it contains. These determine how it will react to stimuli" (1972: 137). For example, an explosion is caused both by the detonation and the power of the explosive material, which it has in virtue of its chemical nature.

On this view of causation, the ascription of a power to a particular form has the following form:

> X has the power to A = if X is subject to stimuli or conditions of an appropriate kind, then X will do A, *in virtue of its intrinsic nature*.

But this is not a simple conditional analysis of powers, since as Harré stressed, power-ascriptions involve two *analysans*:

> a *specific conditional* (which says what X will or can do under certain circumstances and in the presence of a certain stimulus); and
> an *unspecific categorical* claim about the nature of X.

The claim about the nature of X is unspecific, because the exact specification of the nature or constitution of X in virtue of which it has the power to A is left open. (Discovering this is supposed to be a matter of empirical investigation.)

It is a fair complaint that, as stated above, the ascription of powers is explanatorily incomplete unless something specific is (or can be) said about the *nature* of the particular that has the power. Otherwise, power-ascription merely states what needs to be explained, viz. that causes produce their effects. This is where mechanisms come in. Specifying the generative mechanism is cashing the promissory note. As Harré put it: "Giving a mechanism . . . is . . . partly to describe the nature and constitution of the things involved which makes clear to us what mechanisms have been brought into operation" (1970: 124). So the key idea in this mechanistic view of causation is this: causes produce their effects because they have the power to do so, where this power is grounded in the mechanism that connects the cause and the effect and the mechanism is grounded in the nature of the thing that does the causing.

This, as one of us has noted elsewhere (Psillos 2011), is a broad and liberal conception of causal mechanism. Generative mechanisms are taken to be the bearers of causal connections. It is in virtue of them that the causes are supposed to produce the effects. But there is no specific description of a mechanism (let alone one that is couched in terms of physical quantities). A generative mechanism is virtually *any* relatively stable arrangement of entities such that, by engaging in certain interactions, a function is performed, or an effect is brought about. As Harré explained, he did not "intend anything specifically mechanical by the word 'mechanisms'.

Clockwork is a mechanism, Faraday's strained space is a mechanism, electron quantum jumps [are] . . . a mechanism, and so on" (Harré 1970: 36).

Though this was not quite perceived and acknowledged when Harré was putting forward this conception, this liberal conception of mechanism pointed to a shift from thinking of mechanisms exclusively as the vehicle of causation (mechanisms-of) to thinking of mechanisms as whatever implements a certain behavior or performs a certain function (mechanisms-for). On this broader view, a mechanism is a complex system that consists of some parts (its building blocks) and a certain *organization* of these parts, which determines how the parts interact with each other to produce a certain output. The parts of the mechanism should be stable and robust; that is, their properties must remain stable, in the absence of interventions. The organization should also be stable; that is, the complex system as a whole should have stable dispositions, which produce the behavior of the mechanism. Thanks to the organization of the parts, a mechanism is more than the sum of its parts: each of the parts contributes to the overall behavior of the mechanism more than it would have achieved if it had acted on its own.

One natural question may arise at this point. Can a mechanism be *both* what we called a mechanism-for and what we called a mechanism-of? That is, can it be the case that a mechanism *both* underlies or constitutes a causal process *and* is a mechanism for a specific behavior? Though Harré adopted this view, this position acquired new strength in the early 1990s when Stuart Glennan developed his own mechanistic theory of causation. For him, mechanisms are both what underlie or constitute causal connections between events and thus provide the missing link between cause and effect (mechanisms-of) and at the same time complex systems that are responsible for certain behaviors (mechanisms-for) and are thus individuated in terms of them.

But, mechanisms-for are *not* necessarily mechanisms-of. Conceptually this is obvious if we think of a mechanism as a causal process with various characteristics (such as those discussed above—e.g., they possess a conserved quantity or some kind of persisting structure). There is no reason to think that this kind of mechanism (e.g., the process by means of which the sum of kinetic and potential energy is conserved in some interaction) is a mechanism *for* any particular behavior. Conversely, if we think of a mechanism as a complex system such that the interactions of its parts bring about a certain behavior, there is no *ipso facto* reason to adopt a mechanistic account of causation. In light of this, we arrive at a tripartite categorization of "mechanistic" accounts present in the literature (or at *three* independent notions of what a mechanism is): mechanisms can be mechanisms-for, or mechanisms-of, or both.[4]

With this map of the conceptual landscape of the philosophical literature on mechanisms in mind, our task now is to examine each case in turn and investigate the relations between each sense of "mechanism" and laws/counterfactuals.

3. Mechanisms-of

Let us first focus on mechanisms-of that are not at the same time mechanisms-for. As noted already, the best known such causal mechanisms are those discussed by Salmon and Dowe. Though these accounts of causation are presented as being compatible with singular causation, it should be quite clear that they rely on counterfactuals. We noted already that in Salmon's account counterfactuals loom large. In fact, counterfactuals play a *double role* in his theory. On the one hand, they secure that a process is causal by making it the case that the process does not just possess an actual uniformity of structure, but also a counterfactual one. On the other hand, they secure the conditions under which an interaction (the marking of a process) is causal: if the marking would have occurred even in the absence of the supposed interaction between two processes, then the interaction is not causal.

On Dowe's account, the very idea of a possession of a conserved quantity for a process to be causal implies that both laws and counterfactuals are in the vicinity. Conserved quantities are individuated by reference to conservation laws and it is hard to think of a process being causal without the conserved quantity that makes it causal being governed by a conservation law. Counterfactuals are also necessary for Dowe's account of causation. Not just because laws imply counterfactuals, but also because an appeal to counterfactuals is necessary for claiming that the process is causal. That is, it seems that without counterfactuals there is no way to ground the difference between objects to which conserved quantities may be applied and objects to which they may not (e.g. a single particle with zero momentum vs. a shadow with zero quantity of charge; the particle, but not the shadow, is a causal process precisely because it could enter into interactions, which could make its momentum non-zero (see Psillos 2002: 126)).

4. Mechanisms-for and mechanisms-of

Let us now turn to an account such as Glennan's, i.e. to an account that takes mechanisms to be both mechanisms-for *and* mechanisms-of. There are two parts in Glennan's definition of mechanisms. First, a mechanism consists of components that interact—in this, it is similar to Salmon's account of a mechanism-of as causal process. However, for Glennan, the mechanism itself is a complex system with a stable arrangement of components (see his 1996, though in more recent work he drops the stability requirement for some kinds of mechanisms—see section 5). So, in contrast to the view of mechanisms-of as processes (which can in principle be singular causal chains of events), such mechanisms are "types of systems that exhibit regular and repeatable behavior" (Glennan 2010: 259).

How should we understand the interactions among the components of such mechanisms? Should they be understood in terms of counterfactuals or not? To answer this question, let us briefly review various possible options.

The first general case we will consider is interactions with laws. Interactions can be governed by laws, where laws are understood in some robust metaphysical sense. For example, according to Dretske (1977), Tooley (1977), and Armstrong (1983), laws are necessitating relations between universals. So, if there is a necessitating relation between universals A and B, there will be a law between them and as a result of this law when A is instantiated, so will be B. Suppose we transfer that to the components of a mechanism: when component X instantiates A at some time t_1, some other component Y will instantiate universal B, perhaps at a later time. Or take the rival view (but metaphysically robust too) that laws are embodied in relations between powers. If this is the preferred account of laws, interactions will be understood in terms of powers. Powers are properties possessed by components of a mechanism, and produce specific manifestations under specific stimuli. Whereas for Dretske, Tooley, and Armstrong the interactions within the mechanism are grounded in the external relation of nomic necessitation, in the powers view, interactions are grounded in the internal relations between the powers of the components of the mechanism. Alternatively, interactions between the components of the mechanism may be viewed as being governed by metaphysically thin laws; e.g., by (Humean) regularities. Here, component A can be said to interact with component B, in virtue of the fact that this interaction is an instance of a regularity.

If, for the time being, we bracket laws, can we understand the interactions among the components of the mechanism differently? Perhaps counterfactuals can be of direct help here. So Glennan (2002), following Woodward (2000, 2002, 2003), understands interactions in terms of change-relating generalizations that are invariant under interventions. Such generalizations are change-relating in the sense that they relate changes in component A to changes in component B. They involve counterfactual situations in that they concern what would have happened to component B regarding the value of quantity Y possessed by it, if the value of quality X possessed by component A had changed. These generalizations are invariant under interventions, in that they are about relations between variables that remain invariant under (actual or counterfactual) interventions. These change-relating generalizations, then, are grounded in counterfactuals (called interventionist counterfactuals by Woodward—on Woodward's theory of causation, see also Chapter 10).

But if we are to understand interactions between components in terms of counterfactuals, the next question is: *what grounds these counterfactuals?* In particular, in virtue of what are interventionist counterfactuals true? The answers here are well known (see Psillos 2004, 2007). Counterfactuals can be grounded in laws or not. If they are grounded in laws, following what we said in the previous paragraph, these laws can be either metaphysically robust laws of the sort adopted either by Armstrong or power-based accounts of lawhood, or thin Humean regularities, instances of which are particular token-interactions between components. If the counterfactuals are not grounded in laws, then it's likely that there are counterfactuals "all the way down"; that is, that there are primitive modal facts in the world (see Lange 2009).

In *any* of these accounts of law-governed within-mechanism interactions, counterfactuals have a central place: either by directly accounting for interactions (as in Woodward's theory), or by being part of an account of the nomological dependences that ground the interactions, or as a primitive modal signature of the world.[5] So, if laws regulate the interactions between the components of the mechanism, we cannot do away with counterfactuals in grounding within-mechanism interactions. Before, for completeness, we consider the prospects for a non-law-governed account of interactions, let us discuss an attempt to have *mechanisms themselves* ground counterfactuals. This suggestion is put forward by Glennan (1996). For him, although interactions are understood in terms of interventionist counterfactuals, these counterfactuals are in turn grounded in (lower-level) mechanisms.

Here is Glennan's suggestion in more detail. Interactions among components of a mechanism are governed by laws, which are understood in terms of interventionist counterfactuals; these laws are "mechanically explicable," i.e. there are other mechanisms that ground them; but these (lower-level) mechanisms themselves contain parts, the interactions among which are understood in terms of counterfactuals, and which are in turn grounded in yet other mechanisms, until we finally reach a level where we run out of mechanisms to explain the laws that govern the interactions among components, and thus to ground the relevant counterfactuals. At this fundamental level, interactions among components are *directly grounded in counterfactuals*. But notwithstanding these not mechanically explicable laws, Glennan insists that at all other levels mechanisms can ground interactions. So, even if we need to introduce counterfactuals to account for interactions, mechanisms have priority over counterfactuals, and thus the account is supposed not to be a version of a difference-making theory of causation, but a genuinely mechanical account.

However, given the existence of not mechanically explicable laws, it is not clear how mechanisms can ground counterfactuals *at any level*. That is, given that the mechanisms at the lowest level depend on counterfactuals, the mechanisms at a level exactly above the fundamental must be equally dependent (albeit *derivatively*) on the fundamental counterfactuals, and so on for every higher level. In other words, to ground counterfactuals at any level, we need the whole lower hierarchy of mechanisms *and* counterfactuals, and since we ultimately arrive at a level where there are either only counterfactuals, or only laws (or both), it seems that there is a fundamental asymmetry between mechanisms and laws/counterfactuals. The only way to block the asymmetry would be to argue that the whole hierarchy is not needed to ground the counterfactuals at higher levels. Even if this were to be granted for purposes of explanation—that is, even if explanation in terms of mechanisms at level *n* does not require *citing* lower-level mechanisms—metaphysically the whole hierarchy constitutes the grounds for the mechanism.[6]

In sum, if laws are admitted in our notion of mechanism, a reliance on counterfactuals is inevitable. But can we perhaps avoid counterfactuals if we account for within-mechanisms interactions in some other way?

We move now to a second approach. We have reviewed various options to understand interactions of components of mechanisms, where these interactions are viewed as law-governed. The question now is: can we have interactions without some notion of law in the background (either in terms of regularities, or in some more metaphysically robust sense)? If yes, then this could be a way to have mechanisms-of as complex systems, without the need to put laws and counterfactuals in the picture.

For some mechanists, the interactions of components have to be understood in terms of *activities*. Activities are a new ontological category that, together with entities, are said to be needed for an adequate ontological account of mechanisms (Machamer, Darden, and Craver 2000; Machamer 2004). Activities are meant to embody the causally productive relations between components. Causation in terms of activities is viewed as a type of singular causality, where the causal relation is a local matter, i.e. it concerns what happens between the two events that are causally connected, and not what happens at other places and at other times in the universe (as is the case for the regularity theorist). Activities, thus, have been taken to obviate the need for laws.

A key argument in favor of activities turns on the claim that causation is *singular*. But, does singular causation imply that there are no laws? It would be too quick to infer from singular causal claims that laws are not part of causation. By singular causation we may simply mean that there exist genuine singular causal connections, i.e. causal connections between particular event-tokens. But this is not enough to prove that there are no laws in the background. For it is consistent with the existence of singular causal sequences that there are laws under which these causal sequences fall. To use a quick example, on Armstrong's account of laws, singular causation is *ipso facto* nomological causation since the nomic necessitating relation that relates two universals relates the instances of the two universals too (Armstrong 1997). Interestingly, the same is true if we take singular causation to be grounded in the powers possessed by objects; powers are again *wholly* present in the complex event that constitutes the singular causal sequence. And though there is no nomic relation that relates the two powers, the regular instantiation of the two powers implies the presence of a regularity. So, what both these cases show is that even singular causation can be nomological, i.e. subsumed under laws.[7]

Thus, singular causation does not, on its own, constitute an argument in favor of viewing interactions among components of mechanisms as not being law-governed, or more generally, as not depending on difference-making relations. So, friends of activities need to (i) give more reasons to justify the introduction of this new ontological category, and (ii) explain why activities qua producers of change are themselves counterfactual-free. Although it's conceivable that singular causation just amounts to the local activation of powers which in turn ground activities, powers being universals, it's upon the friends of powers to show that we can understand this co-instantiation without also assuming that there is a law present.[8]

5. Mechanisms-for

So far we have argued that mechanisms-of (mechanisms considered as underlying or constituting causal processes) require laws, and thus difference-making relations must be included in the notion of a mechanism-of. But what about mechanisms-for, mechanisms as complex systems responsible for certain behaviors? What is the relation between mechanisms-for and laws/counterfactuals? Recall that a mechanism-for is a mechanism as a complex system such that the interactions of its parts bring about a certain behavior-function. A mechanism-for need not commit us to a mechanistic (e.g. à la Salmon-Dowe) account of the causal interactions between its parts.

In light of what was said in the previous section, there is an argument as to why mechanisms-for have to incorporate laws and/or counterfactuals: a mechanism-for involves components that interact with one another; but laws and/or counterfactuals are needed to account for these interactions; hence, mechanisms-for need to incorporate laws and/or counterfactuals. However, Jim Bogen (2005) has taken the existence of mechanisms that function *irregularly* as an argument against the view that laws and regular behavior have to characterize the function of mechanisms. In this section we will deal with this argument from irregular mechanisms.

The first point that we want to stress is that irregular and unrepeatable mechanisms are not as ubiquitous as some philosophers want us to believe. So, consider a claim made by Leuridan (2010). He thinks that mechanisms as complex systems ontologically depend on stable regularities, since there can be no such mechanisms (i) without macrolevel regularities (i.e. the behavior produced by the mechanism), and (ii) without microlevel ones (i.e. the behaviors of the mechanism's parts). Kaiser and Craver (2013) have replied to this that Leuridan's first claim is "clearly false" since "[o]ne-off mechanisms are mechanisms *without* a macrolevel regularity," where "one-off mechanisms" are the causal processes discussed by Salmon and others (*mechanisms-of* in our terminology). Moreover, they point to examples where scientists seem to be interested in exactly this kind of mechanism, i.e. when they try to explain how a *particular* event occurred (for example, a particular speciation event).

This kind of reply confuses the two different senses of mechanism we have tried to disentangle. It is not the case that "singular, unrepeated causal chains . . . are a special, limiting case of [complex system] mechanisms, not something altogether different," as Kaiser and Craver insist. For it is not at all clear that such mechanisms-of are at the same time mechanisms-for, i.e. mechanisms *for* a certain behavior. Similarly, we remain unpersuaded by Glennan's (2010) claim that the mechanisms that produced various historical outcomes are mechanisms-for (he calls them "ephemeral mechanisms"). In any case, it would be very implausible to insist that any arbitrary causal chain is *for* a certain behavior (which is identified with the outcome of the causal chain or, alternatively, with the (higher-level) event constituted by the causal chain). For instance, the reflection of a light-ray on a surface is a clear case of a mechanism-of (since it constitutes a causal process), but it is not clear at all that it is a mechanism *for* a certain behavior (unless of course we follow Glennan (forthcoming) and equate "behavior" with "phenomenon"; that is, with causal effect).[9]

So, it is not enough to point to singular causal chains to argue that there can be irregular mechanisms, or one-off mechanisms (mechanisms that function only once) (see also DesAutels 2011; Andersen 2012). Still, one may wonder: can there be mechanisms-for *without* a corresponding macrolevel regularity? Thus, the issue that must be clarified is: what are the conditions for being a mechanism *for* a behavior? Is it merely to have a function (which is the mechanism's behavior), or should we, in addition, require that the behavior be regular?

This is not the place to discuss at any length the concept of function (on the relation between functions and mechanisms, see Chapter 8); for the purposes of the current argument, let us interpret this requirement in a wide sense, i.e. as not requiring that for something to have a function it has to be the product of conscious design or the result of natural selection. In other words, we are going to take a function in the sense of Cummins (1975), for whom functions are certain kinds of dispositions (see Craver 2001 for such an approach to the functions of mechanisms). In particular, what it is for a mechanism M to have a function F is to have a disposition to F, which contributes to a disposition of a larger system that contains M. Such functions need not be restricted to living systems or artifacts.[10] Yet, not anything whatsoever can be ascribed a Cummins function. In particular, unrepeated causal chains of events, which might well be Salmon's and Dowe's mechanisms-of, need not have a function. We can follow Cummins and say that talk of functions only makes sense when we can apply what Cummins calls the analytical strategy, i.e. explain the disposition of a containing system in terms of the contributions made by the simpler dispositions of its parts.

There can be systems with Cummins functions that exhibit the corresponding behavior only once; so, there are many biological functions, the realization of which requires that the biological entity that has the function cease to exist. An example is the mechanism for apoptosis, i.e. programmed cell death. Here, the relevant mechanism has a Cummins function, i.e. it causally contributes to the death of the cell; however, this is a function that, when successfully carried out, can occur only once. But even in such cases, the behavior of a particular mechanism of apoptosis is a token of *a type of behavior* that occurs countless times every second.

Can there be genuinely *irregular* mechanisms-for; that is, mechanisms-for without a corresponding macrolevel regularity? Bogen (2005) has offered the case of the mechanism of neurotransmitter release as an example of a mechanism-for that behaves irregularly. As this mechanism more often than not fails to carry out its function, there exists no corresponding macrolevel regularity; but moreover, and more importantly, Bogen thinks that within-mechanism interactions must themselves be irregular, and thus we must abandon the regularity account of causation in favor of activities.

We do not think that this last conclusion follows from Bogen's example. To see why this is the case, it is useful to distinguish between three cases of what we may call "irregular" mechanisms-for. The irregularity of mechanisms-for may be only contingent (irregular$_1$), stochastic (irregular$_2$), or (let us assume) more radical (irregular$_3$).

Irregular$_1$ mechanisms-for are mechanisms that could function regularly, but they in fact do not. A defective machine that only functions once in a while is a case in point. Such a machine (i) is a mechanism for a behavior and (ii) functions irregularly. However, it is certainly not the case that a successful operation of the machine is not subject to laws (e.g. laws of electromagnetism, gravity, or friction). (Nor is it the case that defective machines falsify the regularity account of causation.)

Irregular$_2$ mechanisms are like irregular$_1$ mechanisms in that they operate in accordance with laws, but in this case the laws are probabilistic. So, the existence of such mechanisms does not show that within-mechanism interactions need not be law-governed (or even that the regularity account of causation is false—regularities can be stochastic). What if the operation of a mechanism is completely chancy (e.g. because it involves the radioactive decay of a single atom)? Even if we do not have a law here (perhaps because the relevant law concerns a population of atoms rather than a single one), it is not at all clear to us that such a chancy "mechanism" could be an example of a mechanism-for.[11]

Finally, we can imagine an irregular$_3$ mechanism-for; such a *sui generis* mechanism only operates once, and its unrepeatability is supposed not to be a contingent matter, but this is because the interactions among its components cannot *in principle* be repeated. We are not sure that the notion of an irregular$_3$ mechanism-for actually makes sense. But this is the only kind of example we can imagine, where the irregularity or unrepeatability of a mechanism would be a reason to think that its operation is not law-governed. If that's where the friends of genuinely irregularly operating mechanisms can pin their hopes for a non-law-governed account of mechanism, then so be it!

6. Conclusions

In this chapter, we have examined the relation between mechanisms and laws/counterfactuals by revisiting the main notions of mechanism found in the literature. We distinguished between two different conceptions of "mechanism." What we have called *mechanisms-of* tally with the general conceptions of mechanisms offered in discussions of causation. A "mechanism" in these views is what underlies or constitutes a causal process or connects the cause with the effect. What we have called *mechanisms-for*, on the other hand, are complex systems that function so as to produce a certain behavior. According to some mechanists, a mechanism fulfils both of these roles simultaneously.

We have argued that for both mechanisms-of and mechanisms-for, counterfactuals and laws are central for understanding within-mechanism interactions. We have examined two main arguments in more detail. Concerning mechanisms-of, we have seen that singular causation is compatible with several quite different ways of understanding within-mechanism interactions, in all of which laws and counterfactuals are central. Concerning mechanisms-for, we have argued that the existence of irregular mechanisms is compatible with the view that mechanisms operate according to laws. Both of these arguments point to an asymmetrical dependence between mechanisms and laws/counterfactuals: while some laws and counterfactuals must be taken as primitive (non-mechanistic) facts of the world, all mechanisms depend on laws/counterfactuals.

Notes

1 We wish to thank Phyllis Illari and Stuart Glennan for valuable comments on an earlier draft of this chapter.
2 On a nomological account of causal dependence (i.e. B depends on A if there is a law that connects the two), counterfactuals are required to account for the modal strength of laws (for more on this see Psillos 2002). So, even if it were to be admitted that the alternative notions of dependence are distinct, counterfactuals play a key role in all versions of the dependence approach to causation.
3 We take it that to be a mechanism-for is tantamount to being a system performing a function; this is not always made explicit in general accounts of mechanisms-for (but see Bechtel and Abrahamsen (2005), Garson (2013), and Chapter 8). The minimal mechanism of Glennan and Illari (see Chapter 1), which is defined as a mechanism for a phenomenon, can be understood as either mechanism-of or mechanism-for in our terminology, according to how we understand "phenomenon," as a function or as causal process.
4 See Levy (2013) and Andersen (2014a, 2014b) for similar distinctions.
5 In Lange's (2009) theory of laws it is a counterfactual notion of stability that determines which facts are lawful and which are accidental. In other theories of lawhood, counterfactuals come "for free," so to speak, as they must be part of any metaphysically robust theory of laws (such as that of Dretske, Tooley, and Armstrong): any such theory must show why laws support counterfactuals. It is not obvious how exactly counterfactuals are part of a regularity view of laws. But note that this is a problem (if at all) for the regularity theorist, and not for the view that interactions have to be understood in terms of laws/counterfactuals. For an attempt to reconcile regularity theory with counterfactuals see Psillos (2014).
6 See Glennan (2011) for an attempt to respond to this argument, and Casini (2015) for a detailed criticism; see also Campaner (2006).
7 There is debate among friends of powers whether such a powers-ontology yields an account of laws in terms of powers (Bird 2007), or a lawless ontology (Mumford 2004). But this need not concern us here.
8 For more on activities see Chapters 9 and 10; see also Waskan (2011) for a mechanist account of the contents of causal claims that is not based on counterfactuals and Woodward's (2011) answer that causation as difference-making is fundamental in understanding mechanisms; Menzies (2012) provides an illuminating account of mechanisms in terms of the interventionist approach to causation within a structural equations framework; lastly, Glennan (forthcoming: chapters 5 and 6) offers a detailed treatment of mechanistic causation as a productive account of causation not reducible to difference-making relations.

9 In many examples where scientists refer to "mechanisms," it may not be clear whether it is the notion of mechanism-of or the notion of mechanism-for (or both, or neither) that they have in mind. For example, such uses as "mechanism of chemical reaction," "mechanism of speciation," "mechanism of action (of a drug)" can be construed in various ways; to insist on a widening of the concept of mechanism-for based on scientific practice (without further argument) seems too quick.

10 However, note that for some (e.g. Kitcher 1993) we cannot ascribe even Cummins functions to entities that are not products of (either conscious or nonconscious) design, i.e. that are neither artefacts nor living systems.

11 For a notion of "stochastic" mechanism see DesAutels (2011), as well as Andersen (2012).

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