

# Mechanistic Causation: Difference-Making is Enough

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## RESUMEN

En este artículo defendemos el punto de vista de que los mecanismos están respaldados por redes de relaciones que establecen diferencias. En primer lugar, distinguimos y criticamos dos tipos diferentes de argumentos a favor de entender los mecanismos a partir de la noción de actividad: un enfoque que prioriza metafísica (Glennan) y otro que prioriza la ciencia (Illari y Williamson). En segundo lugar, presentamos un punto de vista alternativo de los mecanismos entendiéndolos en términos del establecimiento de diferencias y lo ilustramos examinando un caso histórico: la prevención del escorbuto. Usamos este ejemplo para argumentar que la evidencia a favor de un mecanismo no algo distinto a la evidencia a favor de relaciones que establecen diferencias.

PALABRAS CLAVE; *mecanismo, causación, producción, actividades, diferenciación, escorbuto.*

## ABSTRACT

In this paper we defend the view that mechanisms are underpinned by networks of difference-making relations. First, we distinguish and criticise two different kinds of arguments in favour of an activity-based understanding of mechanism: Glennan's meta-physics-first approach and Illari and Williamson's science-first approach. Second, we present an alternative difference-making view of mechanism and illustrate it by looking at the history of the case of scurvy prevention. We use the case of scurvy to argue that evidence for a mechanism just is evidence for difference-making relations.

KEYWORDS: *Mechanism, Causation, Production, Activities, Difference-Making, Scurvy.*

## I. INTRODUCTION

Causal relations are explanatory. If C causes E then C explains the occurrence of E. Mechanisms are widely taken to be both what makes a relation causal and what makes causes explanatory. So, typically, if one explains the occurrence of event E by citing its cause C, i.e., if one asserts that C brings about E or that E occurs because of C, one is ex-

pected to cite the mechanism that links the cause and the effect: it is in virtue of the intervening mechanism that C causes E and hence that C causally explains E. On this account of causation, it is not enough to show that E depends on C — where dependence should be taken to be robust, e.g., a difference-making relation. Unless there is a mechanism, there is no causation. Difference-making is taken to be enough for prediction and control but not enough for explanation [cf. Williamson (2011)].

Now, when it comes to causation there are two competing views available: production and dependence [cf. Psillos (2004)]. On the production account, C causes E iff C produces E. ‘Production’ is a term of art, of course, with heavily causal connotations. The typical way to account for ‘production’ is by means of mechanism. So, C produces E iff there is a mechanism that links C and E. On the dependence account, C causes E iff C makes a difference to E. This difference-making is typically seen as counterfactual dependence, viz., if C hadn’t happened, then E wouldn’t have occurred. As is well-known, both views face problems and counterexamples. For instance, the production account cannot accommodate causation by absences. The lack of water caused the plant to die, but there is no mechanism linking the *absence* of water with death. The difference-making account cannot accommodate cases of overdetermination and pre-emption. For instance, suppose that two causes act independently of each other to produce an effect. There is certainly causation, but no difference-making since the effect would be produced even in the absence of each one of the causes [cf. Williamson (2011)].

The key aim of the present paper is to defend the view that difference-making is more fundamental than production in understanding mechanistic causation. In particular, we shall argue that mechanisms are best understood as networks of difference-making relations. To do this, we shall criticise the popular idea that the productivity of mechanisms requires commitments to activities, qua a sui generis ontic category. There are two routes to this popular view, one top-down and another bottom up. The top-down approach, most ably defended by Stuart Glennan (2017), is the metaphysics-first approach. On this view, in order to account for what mechanisms are *as things in the world*, activities must be posited as a distinctive metaphysical item. Activities are taken to be components of mechanisms, distinct from entities and their properties, and are supposed to account for what makes a mechanism productive. The bottom-up approach, recently defended by Phyllis Illari

and John Williamson (2011), is the science-first approach. On this view in order to account for the pervasive role of mechanisms in science, and in particular, for the fact that mechanisms are (spatio-temporally) localised, we have to think of mechanisms as embodying activities. Sections II and III respectively will argue against both approaches to activities. Section II will show that there is no need to hypostatise activities over and above the properties and relations of things that make up causal pathways; section III will show that the ‘local’ argument for activities does not make a case for an activities-based understanding of mechanisms.

Section IV will revisit activities, this time as part of a productive account of causation. It will be argued that the very idea of production requires difference-making relations. Finally, in section V we will look in some detail at the history of the case of scurvy prevention. This case will drive home the point that it is enough to understand mechanisms as underpinned by relations of difference-making.

## II. AGAINST ACTIVITIES 1

What is a mechanism? Glennan puts forward what he calls Minimal Mechanism: “a mechanism for a phenomenon consists of entities (or parts) whose activities and interactions are organised in such a way that they are responsible for the phenomenon” [Glennan (2017), p. 13]. Though minimal, this account is “an expansive conception of what a mechanism is” [Ibid. p. 106], mostly because it involves commitment to activities as a novel ontological category. “Activities” Glennan claims “(...) cannot naturally be reduced to properties of or relations between entities” [Ibid. p. 50].<sup>1</sup>

Given that activities play a key role in the mechanistic accounts of causation, it’s important to be clear on what they are supposed to be. Here then are some characteristics of activities, according to Glennan.

Activities are concrete: “they are fully determinate particulars located somewhere in space and time; they are part of the causal structure of the world [Ibid. p. 20]. Activities are the ontic correlate of verbs. They include anything from walking, to pushing, to bonding (chemically or romantically) to infecting. Given this, activities “are a kind of process — essentially involving change through time” [Ibid. p. 20]. Some activities are non-

relational (unary activities) since they involve just one entity, e.g., a solitary walk. But some activities involve interactions: they are non-unary activities, viz., activities which implicate more than one entity [Ibid. p. 21].

Most activities, Glennan says, “just are mechanistic processes”, i.e., spatio-temporally extended processes which “bring about changes in the entities involved in them” [Ibid. p. 29]. What, then, is a *mechanistic* process? According to Glennan, “To call a process mechanistic is to emphasise how the outcome of that process depends upon the timing and organisation of the activities and interactions of the entities that make up the process” [Ibid. p. 26].

Now, it appears that there is a rather tight circle here. A process is mechanistic when the entities that make it up engage in *activities*. But if activities just are *mechanistic processes*, then a process is mechanistic when the entities that make it up engage in mechanistic processes. Not much illumination is achieved. Perhaps, however, Glennan’s point is that activities and processes are so tightly linked that they cannot be understood independently of each other. Yet, there seems to be a difference—activities (are meant to) imply action. To describe something as an activity is to imply that something acts or that an action takes place. A process need not involve action. It can be seen as a (temporal or causal) sequence of events. In fact, it might be straightforward to just equate the mechanism with the process, viz., the causal pathway that brings about an effect. In the sciences all kinds of processes are characterised as mechanistic irrespective of whether they are ‘active’ or not. Let us illustrate this point by a brief discussion of the case of active vs passive membrane transport, which are the two mechanisms of transporting molecules across the cell membrane. The transportation of the molecules takes place across a semi-permeable phospholipid bilayer and is determined by it. Some molecules (small monosaccharides, lipids, oxygen, carbon dioxide) pass freely the membrane through a concentration gradient whereas other molecules (ions, large proteins) pass the membrane against the concentration gradient and use cellular energy. The main difference between active and passive transport is precisely that in active transport the molecules are pumped using ATP energy whereas in the passive transport the molecules pass through the gradient by diffusion or osmosis. These different mechanisms play different roles. Active transport is required for the entrance of large, insoluble molecules into the cell, whereas passive transport allows the maintenance of a homeostasis between the cytosol and extracellular fluid. But they are both causal processes or pathways, even though only one of them is ‘active’.

Glennan (2017), p. 32, takes it that “the most important feature of activities” is that most or all activities are mechanism-dependent. This, he thinks, suggests that “the productive character of activities comes from the productive relations between intermediates in the process, and that the causal powers of interactors derive from the productive relations between the parts of those interactors”.

But this is not particularly illuminating. Apart from the fact that production is itself an activity, to explain the productive character of activities by reference to the productive activity of intermediaries, or of the constituent parts of the mechanism, just pushes the issue of the productivity of an activity A to the productivity of the constituent activities A<sub>1</sub>, ..., A<sub>n</sub> of the mechanism that realises A. Far from explaining how activities are productive, it merely assumes it. Now, Glennan takes an extra step. He takes it that some producings are explained “in terms of other producings, not in terms of some non-causal features such as regularity, or counterfactual dependence” [Ibid. p. 33]. In the context in which we are supposed to try to understand what distinguishes activities from non-activities, this kind of argument is simply question-begging.

If what makes entities engage in activities are their properties and relations to other entities in what sense are activities things distinct from them? In what sense are activities “a novel ontological category”? Here, we find Glennan’s argument perplexing. His chief point is that thinking of activities as fixed by the properties and relations of things “reduces doing to having; it takes the activity out of activities” [Ibid. p. 50]. The language of relations “is a static language” [Ibid.]. But activities, we are told, are “dynamic” [Ibid. p. 51].

Let us set aside this figurative distinction between doing and having. After all, it is in virtue of having mass that bodies gravitationally attract each other, according to Newton’s theory of gravity. More generally, it is by virtue of having properties that things stand in relations to each other, some of which are ‘static’ e.g., being taller than, while others are ‘dynamic’, e.g., being attracted by. To see why activities do not add something novel to ontology, let us stress that for Glennan activities are fully concrete particulars: “Any particular activity in the world will be fully concrete, though our representations of that activity may be more or less abstract” [Ibid. pp. 95-96]. Now, if activities are always particular, and if they are always specific, like pushings, pullings, bondings, infectings, dissolvings, diffusings, pumpings etc. there is no need to think of them as comprising a novel ontic category. For each fully concrete activi-

ty, there will be some account in terms of entities, their properties and relations. A pushing is an event (or a process) which consists in an object changing its position (over time) due to the impact by another body. Indeed, the very event itself *consists* in a change of the properties of a thing (or of its relations to other things). Similarly, for other concrete activities: there will always be some description of the event or the process involved by reference to the changes of the properties of a thing (that engage in the ‘activity’) or to the relations with other things.

Take the case of a mechanism such as the formation of a chemical bond. Chemical bonding refers to the attraction between atoms. It allows the formation of substances with more than one atomic component and is the result of the electromagnetic force between opposing charges. Atoms are involved in the formation of chemical bonds in virtue of their valence electrons. There are mainly two types of chemical bonds: ionic and covalent. Ionic bonds are formed between two oppositely charged ions by the complete transfer of electrons. The covalent bond is formed by the complete transfer of valence electrons between bonded atoms. Such type of bond is formed by the equal sharing of electrons between two bonded atoms. These atoms have equal contribution to the formation of the covalent bond. On the basis of the polarity of a covalent bond, it can be classified as a polar or non-polar covalent bond. Electronegativity is the property of an atom in virtue of which it can attract the shared electrons in a covalent bond. In nonpolar covalent bonds, the atoms have similar EN. Differences in EN yield bond polarity.

In *describing* this mechanism, there was no need to think of particular activities as anything other than events (sharing of electrons) or processes (transfer of valence electrons) that are fixed by the properties of atoms (their valence electrons; Electronegativity) and the relations they stand to each other (similar or different EN).

Glennan, however, takes it that “processes are collections of entities acting and interacting through time” [Ibid. p. 57]. Elsewhere [Ibid. p. 83], he notes that a mechanism is a “sequence of events (which will typically be entities acting and interacting)”. If we were to follow Bishop Berkeley’s advice to *‘think with the learned and speak with the vulgar’* we could grant this talk in terms of activities, without hypostatizing activities over and above the properties and relations by virtue of which entities ‘act and interact’. We conclude that ‘activity’ is an abstraction without ontological correlate.

When he talks about entities, Glennan takes it that a general characteristic of entities is this: “The causal powers or capacities of entities are

what allow them to engage in activities and thereby produce change” [Ibid. p. 33]. What produces the change? It seems Glennan’s dualism requires that there are causal powers *and* activities and that the former enable the entities that possess them to engage in activities, thereby producing changes (to other entities). It’s as if the activities exist out there ready to be engaged with by entities having suitable causal powers. Glennan is adamant: “activities are not properties or relations; they are things that an entity or entities do over some period of time” [Ibid. p. 96].

But this cannot be right. The activities cannot exist independently of the entities and their properties (whether we conceive of them as powers or not). What activities an entity can ‘engage with’ depends on the properties of this entity. Water can dissolve salt but not iron, to offer a trivial example. The ‘activities’ an entity can engage in are none others than those that result from the kind of entity it is. If you assume powers, as Glennan does, then the activities of an entity are fixed by the manifestation of its powers (given suitable circumstances). Given a power ontology, the powers are the producers of change; the activities are merely the manifestation of powers.

As Glennan admits: “The central difference between activities and powers is that activities are actual doings, while powers express capacities or dispositions not yet manifested” [Ibid. p. 32]. As just noted, assuming particulars with powers, activities are the manifestation/exercising of these powers. When a cube of salt is put in water, it dissolves. The dissolving is the manifestation (assuming a power-ontology) of the active power of water to dissolve (water-soluble) materials and the passive power of the salt to get dissolved. The dissolving takes time (and hence it is a process); but it is not acting in any sense; it does not produce any changes in the salt; it *consists* in the changes in the salt. The ‘scraping of the skin off the carrot’ (Glennan’s example) *is* the removal of the skin of the carrot (at least on this particular occasion) and hence it does not cause (or produce) the removal. Activities do not produce anything; they *are* the productions (of effects).

### III. AGAINST ACTIVITIES 2

While Glennan’s motivation for activities comes from the metaphysics of mechanisms, other philosophers vouch for activities on the grounds that science requires them. The general motivation appears to

be that science must constrain metaphysics. Not only is it the case that what there is has to be compatible with what science describes, but also the best route to the fundamental structure of the world should be the descriptions that science offers. Thus, proponents of activities have argued that if we take seriously the descriptions offered in such fields as molecular biology or neurobiology, we find that activities are central in these descriptions [Machamer, Darden & Craver (2000); Illari & Williamson (2013)]. Illari & Williamson, in particular, think that “[t]here is a good argument from the successful practice of the biological sciences for the appeal to activities in the characterisation of a mechanism” [Illari & Williamson (2013), p. 71].

Illari & Williamson (2011) offer a bottom-up argument in favour of what they call an ‘active metaphysics’ for the workings of mechanisms, by which they mean a metaphysics in terms of capacities [cf. Cartwright (1989)] or of powers [cf. Gillett (2006)] or of activities [cf. Machamer, Darden & Craver (2000)]. They contrast active metaphysics with ‘passive’ metaphysics, which characterises the working of mechanisms in terms of laws or counterfactuals. In what follows we are going to examine this kind of bottom-up argument, which we are going to call the ‘local argument’.

Although we are here treating the local argument as an argument in favour of activities, Illari & Williamson take the argument to be more general, as it does not differentiate between activity-based and power-based views. In fact, in their (2013) Illari & Williamson offer reasons to prefer an ontology based on entities and activities over an ontology based on entities and capacities, a main reason being that an ontology of activities is more parsimonious. But since these arguments are largely metaphysical, and we are here focusing on bottom-up arguments, we are going to examine the local argument in its general form.

Illari & Williamson argue that biological practice, and in particular the fact that mechanisms are taken to be explanatory, constrains the ontology of mechanisms. More specifically, they think that a metaphysics of mechanisms that views within-mechanism interactions in terms of laws or counterfactuals, is “in tension with the actual practice of mechanistic explanation in the sciences, which examines only local regions of spacetime in constructing mechanistic explanations.” So, passive approaches do not “allow mechanisms to be real and local (...) only active approaches give a local characterisation of a mechanism” [Illari & Williamson (2013), p. 835]. They think then that the local argument establishes that a characterisation of mechanism has to be given in terms of an



active metaphysics and not in terms of “counterfactual notions grounded in laws or other possible worlds” [Ibid. p. 838].

The local argument can be reconstructed as follows:

The practice of mechanistic explanation requires that mechanisms be local (1). This in turn implies that a characterisation of mechanism has to be local (2).

But only a metaphysics of powers or activities is a local metaphysics (3).

So, a local characterisation of mechanism requires a metaphysics based on powers or activities (4) (2013, 834-838).

In response to this argument for an ‘active’ metaphysics of mechanisms, it seems to us that ‘local’ cannot have the same meaning in premises (1) and (2), on the one hand, and in premise (3), on the other: we can have local mechanisms without a local metaphysics. There are three points to note here.

First, it is certainly true that mechanisms are local to the phenomena they produce. In this context, ‘local’ means that mechanistic explanation involves the localisation of the parts into which the mechanism is decomposed, the operations of which produce the phenomenon for which the mechanism is responsible. Indeed, as Bechtel & Richardson (2010) have argued, localisation is a central strategy in constructing a mechanistic explanation: scientists decompose the phenomenon under study into component operations, and “localise them within the parts of the mechanism” [Ibid Introduction, p. XXX]. But then, localisation of parts can fully capture the sense in which mechanisms are ‘local’, without entailing a ‘local’ metaphysics, which is supposed to underlie a characterisation of the interactions among components, and not only the components themselves. Even if we accept a metaphysics of laws, within-mechanism interactions are interactions between ‘local’ components.

Second, it is not at all easy to account for within-mechanism interactions in terms of a ‘local’ metaphysics. Energy transformations in biological systems obey the laws of thermodynamics. But it is very difficult to reconcile a power ontology with what it seems to be a global principle, like the law of conservation of energy. This is something that friends of powers themselves have recognised [cf. Ellis (2001)]. So, contra Illari & Williamson, a focus on practice seems in fact to imply the opposite conclusion: global principles like the laws of thermodynamics are needed for

accounting for within-mechanism interactions (e.g. as studied by bioenergetics, cf. Nelson et al (2008), p. 489); but only a metaphysics in terms of laws seems to offer an adequate account of such global principles; so, a metaphysics of laws is required for a characterisation of the metaphysics of mechanisms. Again, the point here is that ‘local’ decompositions of mechanistic parts must be kept distinct from ‘global’ or ‘local’ ways to characterise interactions.

Third, there is a historical point to be made against the argument that mechanistic explanation is not compatible with a metaphysics of laws. This combination (‘local’ mechanisms that produce phenomena plus laws of nature) was a dominant view in 17th century mechanical philosophy. Contemporary mechanistic explanations, of course, are very different from their 17th century counterparts, which in many cases just involved parts of matter in motion. But the general pattern of explanation is similar: in giving a mechanistic explanation, one shows how the particular properties of the parts, their organisation and their interactions (which can be captured in terms of the laws that govern them), produce the phenomena.

In view of the previous points, premise (3) above can only be accepted if the meaning of ‘local’ is disambiguated. An option here is to say that mechanisms have to be local, in the sense that within-mechanism interactions have to be grounded in facts in the vicinity of the mechanism. So, one can think of causation as a local matter, i.e. as a relation between the two events that are causally connected, and not as a global matter, i.e. as involving a regularity. But note that so-called singular causation is compatible with a metaphysics of laws. One can view causation as a relation between ‘local’ events, but at the same time adopt an ontology of laws, where laws could be, for example, necessitating relations between universals, or humean regularities, i.e. ‘global’ facts about the universe [cf. Ioannidis & Psillos (2018)].

Note that Illari & Williamson themselves seem to recognise that in understanding scientific practice one need not talk about metaphysics, for they say: “Understanding the metaphysics of mechanisms on this level is now a philosophical problem with no immediate bearing on scientific method, of course” [Illari & Williamson (2011), p. 834]. But they add: “It does, however, bear on our understanding of science” [Ibid. p. 834]. While we agree with the first sentence, we believe (and we shall argue below) that an understanding of mechanism as causal pathways, underpinned by difference-making relations is all one needs in order to understand scientific practice. We conclude, then, that there is no reason coming from

scientific practice for accepting a power-based or an activities-based account of mechanism.

#### IV. CAUSATION AS PRODUCTION

This last point, viz., that difference-making relations are enough to understand mechanisms and hence mechanistic causation and explanations, is contested. Many philosophers take it that causation is production. Glennan, for instance, is one of the defenders of this view. According to him, mechanisms, qua productive, are the truth-makers of causal claims:

(MC) A statement of the form ‘Event *c* causes event *e*’ will be true just in case there exists a mechanism by which *c* contributes to the production of *e* [Glennan (2017), p. 156].

Actually, there are as many causal relations as there are activities. As he puts it: “There is on this view [the new mechanist view] no one thing which is interacting or causing, and when we characterise something as a cause, we are not attributing to it a particular role in a particular relation, but only saying that there is some productive mechanism, consisting of a variety of concrete activities and interactions among entities” [Ibid. p. 148]. This pluralist view leads him to the radical conclusion that “There is (...) no such thing as THE ontology or THE epistemology of THE causal relation, but only more localised accounts connected with the particular kinds of producing” [Ibid. p. 33].

MC tallies with Glennan’s singularism about causation. All causings are singular and in fact fully distinct from each other. Singularism is committed to the view that causation is internal (intrinsic, as Glennan puts it) to its relata. Glennan shares this intuition. He says: “Productive causal relationships are singular and intrinsic. They involve continuity from cause to effect by means of causal processes” [Ibid. p. 154].

But is causation a relation, after all? And if yes, what are the *relata*? Events, is the answer that springs to mind. Glennan agrees but takes events to involve activities: “Events are particulars — happenings with definite locations and durations in space and time. They involve specific individuals engaging in particular activities and interactions” [Ibid. p. 149]. Or as he put it elsewhere: “an event is just one or more entities engaging in an activity or interaction” [Ibid. p. 177].

We have already argued in section 2 that activity is far from being a *sui generis* ontic category. Besides, there is the received account of events as property-exemplifications: events are exemplifications of properties (or relations) by an object (or set of objects) at a time (or a period of time). As Glennan admits: “If exemplifying a property were the same as engaging in an activity, then the two views would coincide”. However, he takes it that “there are important differences between exemplifying properties and engaging in activities” [Ibid. p. 177].

The chief difference between property-exemplification and engaging in activities is, Glennan says, that “properties are paradigmatically synchronic states of an entity that belong to that entity for some time.” Unlike activities, properties “do not involve change”. Events, Glennan argues, “involve changes”. It is indeed true that events involve change. The collision of the Titanic with the iceberg took time and during it, both the Titanic and the iceberg suffered changes in their properties, which resulted in another event, viz. the sinking of the Titanic. It is true that to account for this we have to introduce relations: the collision is between the Titanic and the iceberg. But relations, we are told, are not “activity-like”. Glennan insists: “only events (which involve activities) can be causally productive”. Properties, he says, “cannot produce anything” [Ibid. p. 178].

When all is said and done, the key question is: is causation production? Or is it difference-making? Glennan is clear: “While I grant that production and relevance are two different concepts of cause, I will argue that production is fundamental” [Ibid. p. 156].

Descriptively, Glennan distinguishes between three kinds of productive relations:

- Constitutive production: An event produces changes in the entities that are engaging in the activities and interactions that constitute the event.
- Precipitating production: An event contributes to the production of a different event by bringing about changes to its entities that precipitate a new event.
- Chained production: An event contributes to the production of another event via a chain of precipitatively productive events [Ibid. p. 179].

All this is fine but what is the chief argument for causation being *production*?

It seems to be this: “Mechanisms provide the ontological grounding that allows causes to make a difference” [Glennan (2017), p. 165].

Glennan's problem with the claim that mechanism is itself a network of relations of difference-making between events is that on the difference-making account "the causal claim depends upon the truth of a counterfactual, whereas on the mechanist account the truth depends upon the existence of an actual mechanism" [Ibid. p. 167]. Furthermore, it is claimed that the truth of the counterfactual requires contrasting an actual situation — where the cause occurs — and a non-actual but possible situation in which the cause does not occur.

Does the production account avoid counterfactuals? Glennan acknowledges that causation as production relies on some notion of relevance but takes this to require actual difference-makers. He takes it that actual difference-makers are "features of the actual entities and their activities upon which outcome depends" [Ibid. p. 203].

What is an *actual* difference-maker? A factor such that had it not happened, the effect would not have followed. But a) in an actual concrete sequence of events which brought about an effect *x*, all events were necessary in the circumstances; all were difference-makers. If any of them were absent, the effect, in its full concrete individuality, would not follow. A different effect would have followed. But b) what makes true the counterfactual that 'had *x* not actually happened, *y* would not have followed'? To 'delete' *x* from the actual sequence is to envisage a counterfactual sequence (that is, a distinct sequence of events) without *x*. It is then to compare two sequences: the actual and the counterfactual. This requires thinking in terms of counterfactual difference-making. What makes the counterfactual true is not the actual sequence of events but the fact, if it is a fact, that *x*s are followed by *y*s, which is a causal law.

Take the example of a ball striking a window while a canary nearby sings. The actual causal situation — the mechanism in all its particularity — includes the process of the acoustic waves of the canary's singing striking the window (say, for convenience, at the moment when the ball strikes the window) as well as the kinetic energy of the ball (which was a red cricket ball) etc. Despite the fact that the acoustic waves are part of the actual concrete mechanism and clearly contributed to the actual breaking (no matter how little), we would not say that it was the singing that caused the window-breaking. It clearly didn't make a substantial contribution to the breaking. Had it not been there, the window would still have shattered. How can *this* counterfactual be made true by the actual situation? In the actual situation, the singing was a difference-maker since it was part of the mechanism that made the difference. To show

that it did not make a difference (better put, that it made a difference without a difference) we have to compare the actual situation in which the singing took place and a non-actual but possible situation in which the singing did not happen. Whatever makes this counterfactual true, it is not the actual situation, in and of itself.

Not only production does not avoid counterfactuals (if actual difference-makers are to be shown that did not make a difference) but it seems that the very idea of production requires difference-making relations if the producer of change is nothing more specific than everything that happened before the effect took place.

#### V. CAUSATION AS DIFFERENCE-MAKING: THE CASE OF SCURVY

Given the difficulties with activities and the mechanistic production outlined above, it seems more promising to start with difference-making and give an account of mechanisms in terms of it. Such difference-making accounts of mechanism have been offered by various authors. For James Woodward (2002), difference-making is required to account for within-mechanism interactions. As he puts it, “components of mechanisms should behave in accord with regularities that are invariant under interventions and support counterfactuals about what would happen in hypothetical experiments” [Woodward (2002), p. 374]. Peter Menzies (2012) uses the interventionist approach to causation to give an account of the causal structure of mechanisms.

More recently, Gillies (2017) and Ioannidis & Psillos (2017; 2018) have offered difference-making accounts of mechanism by discussing particular case-studies. Common to both of these more recent accounts is the thought that a mechanism in life sciences should be viewed as a causal pathway connecting a cause with a particular effect. Gillies sums up his account as follows: “Basic mechanisms in medicine are defined as finite linear sequences of causes ( $C_1 \rightarrow C_2 \rightarrow C_3 \rightarrow \dots \rightarrow C_n$ ), which describe biochemical/ physiological processes in the body. This definition corresponds closely to the term ‘pathway’ often used by medical researchers. Such basic mechanisms can be fitted together to produce more complicated mechanisms which are represented by networks” [Gilles (2017), p. 633].

In our (2017; 2018) we have argued that when scientists talk about a ‘mechanism’, what they try to capture is the way (i.e. the causal pathway) a certain result is produced. Suppose, for instance, that pathologists want to find out how a certain disease is brought about. They look for a

specific mechanism, i.e. a causal pathway that involves various causal links between, for example, a virus and changes in properties of the organism that ultimately lead to the disease. In pathology, such causal pathways constitute the pathogenesis of a disease, and when pathologists talk about the mechanisms of a disease, it is such pathways that they have in mind [cf. Lakhani et al (2009)]. This leads to the following view: “[t]o identify a mechanism ... is to identify a specific causal pathway that connects an initial ‘cause’ (the causal agent) with a specific result” [Psillos and Ioannidis (2017), p. 604]. So, mechanisms in biomedicine are “stable causal pathways, described in the language of theory” [Ibid. (2018), p. 1181], where to identify a *causal* pathway is to identify difference-making relations among its components.

Moreover, we have argued that in giving a characterisation of mechanism as a concept of scientific practice, one need not be committed to a specific view on the metaphysics of mechanisms: mechanism in our sense is a concept used in scientific practice and as such it is primarily a methodological concept. An important point here is that if we take this truly minimal account of mechanisms, then the burden is on the defender of a particular metaphysical characterisation of mechanism to say why such a methodological account is not enough and why it should be inflated with metaphysical categories (such as entities and activities).

To motivate further this difference-making account of mechanism, as well as the view that difference-making is prior to production, let us look briefly at the case of scurvy. This, we now know, is a disease resulting from a lack of vitamin C (ascorbic acid). If you think of it, the *absence* of vitamin C in an organism causes scurvy, which starts with relatively mild symptoms (weakness, feeling tired, and sore arms and legs) and if it remains untreated it may lead to death. If we take seriously the thought that absences, qua causes, are counterexamples to mechanistic causation, we should conclude that there is no mechanistic explanation of scurvy. But this would be clearly wrong. What is correct to say is that the lack of vitamin C disrupts various biosynthetic causal pathways, that is, mechanisms, e.g., the synthesis of collagen. In the latter process, ascorbic acid is required as a cofactor for two enzymes (prolyl hydroxylase and lysyl hydroxylase) which are responsible for the hydroxylation of collagen. Some tissues such as skin, gums, and bones contain a greater concentration of collagen and thus are more susceptible to deficiencies. But ascorbic acid is also required in the enzymatic synthesis of dopamine, epinephrine, and carnitine. Now, humans are unable to synthesise ascorbic acid, the reason

being that humans possess only 3 of the 4 enzymes needed to synthesise it; (the fourth enzyme seems to be defective). Hence humans have to take vitamin C through their diet [for a useful survey cf. Magiorkinis et al. (2011)].

The disrupted causal pathways that prevent scurvy can be easily accommodated within the difference-making account of causation. Had vitamin C been present in the organism  $x$ ,  $x$  wouldn't have developed scurvy. In fact, the very causal pathway can be seen as a network of relations of dependence (or difference-making). Abstractly put, had vitamin C been present in human organism  $x$ ,  $x$ 's lack of working GULO enzyme would not have mattered; enzymes prolyl hydroxylase and lysyl hydroxylase would have been produced etc. and scurvy would have been prevented. [For a description of the causal pathways of the synthesis of vitamin C in the mammals that can synthesise it, see Linster & Van Schaftingen (2007)].

The history of scurvy is really interesting. During the Age of Exploration (between 1500 and 1800), it has been estimated that scurvy killed at least two million seamen. Although there were hints that scurvy is due to dietary deficiencies, it was not until 1747 that it was shown that scurvy could be treated by supplementing the diet with citrus fruits. In what is taken as the first controlled clinical trial reported in the history of medicine, James Lind, naval surgeon on HMS *Salisbury*, took 12 patients with scurvy “on board the Salisbury at sea” [Lind (1753), p. 149]. As he reported, “Their cases were as similar as I could have them”. The patients were kept together “in one place, being a proper apartment for the sick” and had “one diet in common to all”. He then divided them to 6 groups of 2 patients and each of which was allocated to 6 different daily treatments for a period of 14 days. One group was administered 2 oranges and 1 lemon per day for 6 days only, “having consumed the quantity that could be spared” [Ibid. p.150]. The other groups were administered cyder, elixir vitriol, vinegar, sea-water, and a concoction of various herbs, all of which were supposed to be anti-scurvy remedies. As Lind put it: “The consequence was that the most sudden and visible good effects were perceived from the use of the oranges and lemons; one of those who had taken them being at the end of six days fit for duty, (...) (t)he other was the best recovered of any in his condition” [Ibid.]. Lind's experiments provided evidence that citrus fruits could cure scurvy. He said that oranges and lemons are “the most effectual and experienced remedies to remove and prevent this fatal calamity” [Ibid. p. 157].

Though Lind had identified a difference-maker, he was sidetracked by looking for the cause of scurvy, which he found in the moisture in the air, though he did admit that that diet may be a secondary cause of scur-



vy [cf. Bartholomew (2002); Carpenter (2012)]. But in 1793 his follower, Sir Gilbert Blane, who was the personal physician to the admiral of the British fleet, persuaded the captain of HMS *Suffolk* to administer a mixture of two-thirds of an ounce of lemon juice with two ounces of sugar poured to each sailor on board. As Blane reported the warship “was twenty-three weeks and one day on the passage, without having any communication with the land (...) without losing a man” [quoted by Brown (2003), p. 222]. To be sure, scurvy did appear, but it was quickly relieved by an increase in the lemon juice ration. When in 1795 Blane was appointed a commissioner to the *Sick and Hurt Board*, he persuaded the Admiralty to issue lemon juice as a daily ration aboard all Royal Navy ships. He wrote: “The power [lemon juice] possesses over this disease is peculiar and exclusive, when compared to all the other alleged remedies” [cf. op.cit.]. But even when it was more generally accepted that citrus fruits prevent scurvy, it was the acid that was believed to cure scurvy.

The first breakthrough took place in 1907 when two Norwegian physicians, Axel Holst and Theodor Frølich, looked for an animal model of beriberi disease. They fed guinea pigs with a diet of grains and flour and found out, to their surprise, that they developed scurvy. They found a way to cure scurvy by feeding the guinea pigs with a diet of fresh foods. This was a serendipitous event. Most animals are able to synthesise vitamin C; but not guinea pigs. In 1912, in a study of the etiology of deficiency diseases, Casimir Funk suggested that deficiency diseases (such as beriberi and scurvy) “can be prevented and cured by the addition of certain preventive substances”. He added that “the deficient substances, which are of the nature of organic bases, we will call ‘vitamines’; and we will speak of a beriberi or scurvy vitamine, which means, a substance preventing the special disease” [Funk (1912), p. 342]. By the 1920s, the ‘anti-scurvy vitamine’ was known as ‘C factor’ or ‘anti-scorbutic substance’ [cf. Hughes (1983)]. In 1927, Hungarian biochemist Szent-Györgyi isolated a sugar-like molecule from adrenals and citrus fruits, which he called ‘hexuronic acid’. Later on, Szent-Györgyi showed that the hexuronic acid was the sought-after anti-scorbutic agent. The substance was renamed ‘ascorbic acid’. In parallel with Szent-Györgyi’s work, Charles King and W. A. Waugh identified, in 1932, vitamin C. The suggestion that hexuronic acid is identical with vitamin C was made in 1932, in papers by King and Waugh and by J. Tillmans and P. Hirsch [cf. Hughes (1983)].

The breakthrough in scurvy prevention occurred when scientists started to look for what has been called ‘the mediator’, which is a code-

word for the ‘mechanism’, which “transmits the effect of the treatment to the outcome” [Pearl & Mackenzie (2018), p. 270]. As Baron and Kenny put it, mediation “represents the generative mechanism through which the focal independent variable is able to influence the dependent variable of interest” [Baron and Kenny (1986), p. 1173]. This mechanism, however, is nothing over and above a network of difference-makers: Citrus Fruits → Vitamin C → Scurvy. One such difference-maker, citrus fruits, was identified by Lind and later on by Blane. This explains the success in preventing scurvy after citrus fruits were administered as part of the diet of sailors. It is noteworthy, however, that Lind and the early physicians did not look for the mediating factor in the case of scurvy. As Bartholomew (2002), p. 696, notes, Lind did not try to isolate a single common constituent in citrus fruits in particular and in fruit in general which makes a difference to the incidence of scurvy. Instead he was trying to find out the contribution of different sorts of vegetable to the relief from scurvy. Still, even without knowing the mediating variable (vitamin C), the intake of citrus in a diet did make a difference to scurvy relief.

In order to find the difference-maker in the case of vitamin C deficiency it was necessary to find a model (animals) that does not synthesise its own vitamin C. In the late 1920a, Szent-Györgyi and his collaborator J. L. Svirebely used the recently isolated by Szent-Györgyi hexuronic acid to treat the animals in controlled experiments with guinea pigs. They divided the animals into two groups. In one the animals were fed with food enriched with hexuronic acid, while in the other the animals received boiled food. The first group flourished while the other developed scurvy. Svirebely and Szent-Györgyi decided that hexuronic acid was the cause of scurvy relief and they renamed it ascorbic acid. Ascorbic acid was the sought-after mediating variable: the difference-maker [cf. Schultz (2002)].

It is useful to discuss the case of scurvy in relation to what has become known in the recent philosophical literature on mechanisms as the Russo-Williamson thesis (RWT) ([Russo & Williamson (2007)], i.e. that in the health sciences, in order to establish a causal connection between A and B, one needs evidence both for the existence of a difference-making relation between A and B *and* of a mechanism linking A to B. Williamson (2011) relies on this thesis to raise a problem for mechanistic and difference-making theories of causation. The problem is supposed to be that these theories, taken on their own, are not compatible with the causal epistemology adopted in biomedicine and other scientific fields, which conforms to RWT.

This argument seems to raise a problem for the difference-making account of mechanism presented in the beginning of this section. If *A* causes *B* in virtue of a mechanism linking *A* to *B*, where a mechanism involves a chain of events linked by difference-making relations, it seems that evidence of difference-making is enough to establish a causal claim, contrary to what RWT asserts. In other words, ‘mechanistic’ evidence need not be different in kind from difference-making evidence. However, Williamson & Wilde (2016) assume that there is a distinction between these two kinds of evidence. They think that “in order to establish that *A* is a cause of *B* there would normally have to be evidence both that (i) there is an appropriate sort of difference-making relationship (or chain of difference-making relationships) between *A* and *B* — for example, that *A* and *B* are probabilistically dependent, conditional on *B*’s other causes —, and that (ii) there is an appropriate mechanistic connection (or chain of mechanisms) between *A* and *B* — so that instances of *B* can be explained by a mechanism which involves *A*” [Ibid. p. 38].

In contrast to this, the case of scurvy shows that looking for mechanistic evidence is just looking for a special kind of ‘difference-making’ evidence and not for a different kind of evidence. This special difference-making evidence involves looking for the ‘mediator’. As we have seen, Lind’s experiments provided evidence for a difference-making relationship between Citrus Fruits and Scurvy, but no evidence about how exactly Citrus Fruits acted so as to prevent scurvy. When it was realised by Funk that scurvy is a ‘deficiency disease’, i.e. it was produced because of the lack of a particular substance, it became obvious that Citrus Fruits acted to prevent Scurvy by providing that preventive substance. So, scientists started looking for this preventive substance that was the mediating factor between Citrus Fruits and Scurvy. As we have already seen, however, what was required for finding the mediator and establishing the pathway Citrus Fruits → Vitamin C → Scurvy, was the isolation of a substance (hexuronic acid) from citrus fruits that was such as to prevent scurvy in controlled experiments with guinea pigs by Svirbely and Szent-Györgyi. So, the evidence for identifying the mediator was not evidence about particular entities engaging in activities, or some *sui generis* type of mechanistic evidence, as one would have believed if the activities-based account of mechanism were true; it was evidence about more difference-making relations, this time between the two initial variables (Citrus Fruits and Scurvy) and the mediating variable Vitamin C.

The case of scurvy thus shows that RWT can be accepted, without being committed to the existence of a special type of ‘mechanistic’ evidence over and above difference-making relations. Moreover, acceptance of RWT does not automatically lead to a rejection of a difference-making account of causation. Given a difference-making account of mechanisms, RWT can be understood as follows: typically, to establish a causal connection between A and B, we have to have both evidence for a difference-making relation between A and B, and evidence for one or more mediators; but all this evidence is, ultimately, evidence for difference-making relations. In his (2011), Gillies offers a similar formulation for RWT. He suggests: “In order to establish that A causes B, observational statistical evidence does not suffice. Such evidence needs to be supplemented by interventional evidence, which can take the form of showing that there is a plausible mechanism linking A to B” [Gillies (2011), p. 116].<sup>2</sup>

## VI. CONCLUSIONS

In this paper we have defended the view that mechanisms are underpinned by networks of difference-making relations and have shown that difference-making is more fundamental than production in understanding mechanistic causation. Our argument was two-fold. First, we have argued against the view that the productivity of mechanisms requires thinking of them as involving activities, qua a different ontic category. We have criticised two different routes to activities: Glennan’s top-down metaphysics-first approach and Illari and Williamson’s bottom-up science-first approach. Second, we have looked in some detail at the history of the case of scurvy prevention, in order to illustrate the difference-making account of mechanisms and to argue that mechanistic evidence in science is evidence about difference-making relations. The search for mechanisms is clearly a pervasive feature of science; but it is nothing else than the search for stable causal pathways.

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NOTES

<sup>1</sup> This section is an expanded and reworked version of Psillos (2018).

<sup>2</sup> Hill's influential (1965) has been viewed as offering a version of RWT [cf. Russo & Williamson (2007); Clarke et al. (2014)]. Note, however, that he does not talk explicitly about mechanisms in his paper. He offers 'plausibility' as a criterion for establishing causal claims, which can be understood as the existence of a biologically plausible mechanism; but he does not regard it as particularly important, since "[w]hat is biologically plausible depends upon the biological knowledge of the day" [Hill (1965), p. 298]. As 'strongest support' for causation he takes experimental evidence, e.g. whether some preventive action does in fact prevent the appearance of a disease. Lastly, his 'Coherence' criterion involves, among others, establishing a mediator; his example is "histopathological evidence from the bronchial epithelium of smokers and the isolation from cigarette smoke of factors carcinogenic for the skin of laboratory animals" [Ibid.], which was important in establishing a causal connection between smoking and lung cancer.

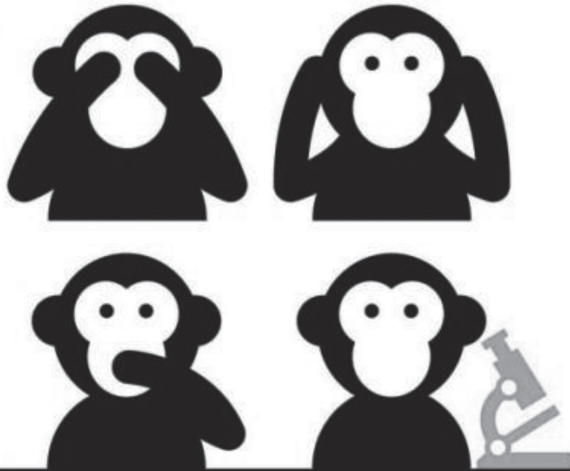
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# The Scientific Attitude

Defending Science from Denial,  
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