Ceteris paribus conditions and the interventionist account of causality

1. Introduction

In "There Is No Such Thing As a *Ceteris Paribus* Law", James Woodward claims that the scientific legitimacy of causal statements doesn't require that (i) causal statements (claims of direct type-level causation) be laws, or that (ii) causal statements be qualified by a *ceteris paribus* (*cp*) clause. He supports claim (i) by pointing out that causal statements cannot express strict laws (in the sense of exceptionless regularities) because causes aren't nomically sufficient for their effects, and because causal statements may be true for only a few values of the variables that figure in these statements. He supports claim (ii) by arguing that there is "no motivation" to qualify a causal statement by a *cp* clause if this statement is scientifically legitimate (i.e. true, non-vacuous, explanatory and empirically testable) and not a regularity.

The paper aims to defend the thesis that Woodward's interventionist account of direct type-level causation represents an excellent account of causal *cp* laws. This thesis is somewhat surprising but doesn't necessarily contradict Woodward's claims (i) and (ii). It states more precisely that (1) causal statements may be regarded as *cp* statements because Woodward's interventionist account of direct type-level causation implies a statement of truth conditions that makes reference to three conditions that, in the context of the applicability of causal statements, can be referred to as *cp* conditions, and (2) that causal statements may qualify as laws in a sense different from that of exceptionless regularities.

The paper will argue for claim (1) by (*a*) developing a de-relativized (or objectified) variant of Woodward's account of direct type-level causation, by (*b*) suggesting that the relation between *X* and *Y* needs to satisfy three conditions in order to qualify as one of direct type-level causation, that satisfaction of these conditions guarantees the applicability of claims of direct type-level causation, and that the context of applicability motivates referring to these conditions as *cp* conditions, and by (*c*) noting that the de-relativized variant of Woodward's definition of direct type-level causation can be reformulated as a statement of truth conditions for causal *cp* statements. The paper will argue for claim (2) by (*d*) defining the notion of a causal *cp* law in terms of Woodward's notion of explanatory depth. Each of the four steps of this argument – steps (*a*) to (*d*) – will be dealt with separately in sections 3-6. Section 2 will look at Woodward's argument in support of claims (i) and (ii) in a bit greater detail.

2. Woodward on the scientific legitimacy of causal statements

In "There Is No Such Thing As a *Ceteris Paribus* Law", Woodward investigates the question of whether the generalizations of the special sciences may be taken to represent *cp* laws. He argues that an affirmative answer to this question rests on a set of mistaken beliefs: on the beliefs that (1) a genuine science must contain laws, that (2) laws describe exceptionless regularities, i.e. have the form of universally quantified conditionals in which the condition in the antecedent is nomically sufficient for the condition in the consequent, that (3) laws are required for successful explanation, and that (4) a (non-probabilistic) generalization cannot be tested unless it has the form of an exceptionless regularity (cf. Woodward 2002: 303).

Woodward (2002: 303-4) points out that the generalizations of the special sciences typically aren't exceptionless or of the form of a regularity, and that theorists who uphold the set of beliefs (1) – (4) face the following dilemma: they can argue that the generalizations of the special sciences aren't scientifically legitimate or understand these generalizations as *cp* laws – as exceptionless regularities hedged by a *cp* clause. Most theorists opt for the second horn of this dilemma and embark on the project of specifying the conditions under which a *cp* law is scientifically legitimate (i.e. true, non-vacuous, explanatory and testable). According to Woodward (2002: 304), however, "the major proposals in the literature for the analysis of cp laws are, on their own terms, complete failures."

Woodward (2002: 304) himself holds the view that the scientific legitimacy of the generalizations of the special sciences can be defended without relying on the set of beliefs (1) – (4). These beliefs, he argues, are mistaken to the extent that scientific disciplines need not contain laws (in the sense of exceptionless regularities), that successful explanation doesn't require laws, and that generalizations don't need to have the form of exceptionless regularities in order to be testable. When the set of beliefs (1) – (4) is given up, there is "no motivation for the whole cp laws enterprise, understood as the project of construing the generalizations of the special sciences as laws of a special sort and then searching for general conditions for them to be legitimate" (Woodward 2002: 304).

Woodward (2002: 305-6) mentions a whole variety of generalizations that, to his mind, can be regarded as scientifically legitimate without qualifying as laws in the sense of exceptionless regularities. But he puts specific emphasis on causal statements and illustrates his case by using the example of the true statement that

(*E*) administration *A* of drugs *D* according to protocol *P* to human beings with tumors of type *T* causes recovery *R*,

where 'recovery' means that the tumors of type *T* disappear and remain absent for at least 5 years. This statement becomes false, Woodward (2002: 308) argues, when reformulated as exceptionless regularity, i.e. as statement of the form

(*E**) All human beings with tumors of type *T* who undergo *A* will recover.

Theorists who uphold the set of beliefs (1) – (4) therefore propose to qualify (*E**) by a *cp* clause, i.e. to rewrite (*E**) as

(*CPE*) *Ceteris paribus*, all human beings with tumors of type *T* who undergo *A* will recover.

But when rewriting (*E**) as (*CPE*), theorists have to specify the conditions under which (*CPE*) can be regarded as scientifically legitimate. And Woodward holds that all attempts that have been undertaken so far to specify these conditions fail.

Of these attempts, Woodward only discusses those that cluster around Fodor's notion of a "completer". But thorough criticisms of alternative attempts can be found e.g. in Earman and Roberts (1999). And Woodward is probably right when referring to all of these attempts as "failures". According to the attempts that cluster around Fodor's notion of a completer, a universally quantified statement of the form

(*K*) "All *F*s are *G*s" is a *cp* law if and only if there exists a completer *K* for *F* with respect to *G*,

where a completer is conceived of as a property that together with *F* is nomically sufficient for occurrences of *G* (cf. Fodor 1991: 23). Woodward (2002: 310-2) raises at least three objections to (*K*). His first objection is that (*K*) is too permissive: that (*K*) allows generalizations to qualify as *cp* laws that we usually wouldn't accept as laws. These generalizations include statements that are normally or usually satisfied (i.e. statements like 'All human beings living in East Asia speak Chinese', 'Drivers in England drive on the left') and statements that aren't even normally satisfied (i.e. statements like 'All charged objects accelerate at 10 m/s^2', 'All human beings speak French' etc.).

Woodward's second objection is that (*K*) doesn't rule out that statements of the form '*Ceteris paribus*, all *F*s are *G*s' are vacuous (i.e. vacuously or trivially true). If not all *F*s are *G*s and the system under discussion is deterministic, then there will always be a completer *K* that together with *F* is nomically sufficient for occurrences of *G*. Woodward's third objection is that whether (*K*) is satisfied by some generalization *G* cannot by itself determine whether *G* is scientifically legitimate. The statement 'All *F*s that are *K*, are *G*s', for instance, qualifies as a *cp* law in the sense of (*K*) if *K* is a completer in the sense of (*K*). If this statement is trivially true, however, it isn't scientifically legitimate because it cannot be tested.

Woodward therefore advises against rewriting (*E*) as (*E**) or (*CPE*). He points out that (*E*) cannot express an exceptionless regularity because *A*, *D*, *P* and *T* aren't nomically sufficient for *R* (cf. Woodward 2002: 319-20), and because (*E*) may be true for only a few values of *A*, *D*, *P*, *T* and *R* (cf. Woodward 2002: 308, 317). He also points out that (*E*) is nonetheless scientifically legitimate: non-vacuously true, testable and explanatory. It is non-vacuously true because it is true for only some of the values of *A*, *D*, *P*, *T* and *R*. It is testable because it is possible to carry out randomized controlled experiments: to assign subjects with tumors of type *T* randomly to either a treatment or control group and to test whether the number of subjects who recover is higher in the treatment group than in the control group (cf. Woodward 2002: 313). And it is explanatory to the extent that it can be appealed to in order to explain why the expected rate of recovery of tumor patients in the treatment group is higher than in the control group, or why an individual patient has recovered (cf. Woodward 2002: 320-1).

Woodward concludes that since (*E*) is scientifically legitimate and not an exceptionless regularity, there is "no motivation" to qualify (*E*) as a *cp* law. It will be argued in the remainder, however, that motivations to regard causal statements as *cp* statements do not necessarily stem from the set of beliefs (1) – (4), and that laws do not necessarily have to be understood as strict regularities. The next section will argue in favor of a de-relativized (or objectified) version of Woodward's account of direct type-level causation. The fourth section will try to show that the relation between *X* and *Y* has to satisfy three conditions in order to qualify as one of direct type-level causation, that satisfaction of these conditions guarantees the applicability of claims of direct type-level causation (i.e. guarantees that intervening on *X* would change *Y* in a specific way), and that the context of applicability motivates referring to these conditions as *cp* conditions. The fifth section will state truth conditions for causal *cp* statements and discuss the scientific legitimacy of these statements. The sixth and final section will offer a definition of causal *cp* laws in terms of Woodward's notion of explanatory depth.

3. A de-relativized (objectified) variant of Woodward's general account of causality

According to the definition that is central to Woodward's general account of direct type-level causation,

(*DC*) *X* is a direct type-level cause of *Y* with respect to a variable set *V* iff there is a possible intervention on *X* that will change *Y* or the probability distribution of *Y* when one holds fixed at some value all other variables *Z* in *V*

(cf. Woodward 2003: 55, 59). Terms that figure in this definition and receive further explication include the terms "intervention" and "possible intervention". Woodward's definition of "intervention" runs as follows:

(*IN*) *I*'s assuming some value z_i is an intervention on *X* with respect to *Y* iff *I* is an intervention variable for *X* with respect to *Y* and $I = z_i$ is an actual (token-level) cause of the value taken by *X*

(cf. Woodward 2003: 98). And his definition of "intervention variable" states that

(*IV*) *I* is an intervention variable for *X* with respect to *Y* iff

- (*I*1) *I* type-level causes *X*,
- (*I*2) certain values of *I* are such that when *I* attains these values, *X* is no longer determined by other variables that type-level cause it but only by *I*,
- (*I*3) any directed path from *I* to *Y* goes through *X*, and
- (*I*4) *I* is statistically independent of any variable *Z* that type-level causes *Y* and is on a directed path that does not go through *X*

(cf. Woodward 2003: 98). Woodward (2003: 99-100) notes that condition (*I*1) requires that there be a directed path from *I* to *X*, that condition (*I*2) implies that for some values of *I*, the value of *X* depends only on the value of *I*, and that conditions (*I*3) and (*I*4) are needed to rule out cases in which we say that *I* type-level causes *X* but *X* doesn't necessarily type-level cause *Y*. Condition (*I*3) rules out cases in which e.g. *I* directly type-level causes *X* and *Y* and it is unclear whether *X* type-level causes *Y*, or in which *I* directly type-level causes *X* and *Z*, *Z* directly type-level causes *Y* and it is unclear whether *X* type-level causes *Y*. Condition (*I*4), by contrast, rules out cases in which e.g. *I* and *Z* are correlated, *I* directly type-level causes *Y* and it is unclear whether *X* directly type-level causes *Y*. Cases like these have to be ruled out because, given the common cause principle, they cannot arise unless *I* type-level causes *Z*, *Z* type-level causes *I*, or a common cause *W* type-level causes both *I* and *Z*, and because in all these cases, it remains unclear whether *X* type-level causes *Y*.

What does Woodward have in mind when saying that an intervention on *X* that changes *Y* or its probability distribution while all other variables remain unchanged needs to be "possible" in order for *X* to be a direct type-level cause of *Y*? This intervention needs to be logically or conceptually possible, of course: "if we cannot think of *X* as a variable that is capable of being changed from one value to a different value – if manipulation of *X* is not a logical possibility or if it is ill-defined for conceptual or metaphysical reasons – then claims about what will happen to *Y* under interventions on *X* will be either false or will lack a clear meaning" (Woodward 2003: 128). But doesn't there need to be any further constraints on the possibility of interventions?

Does an intervention on *X* that changes *Y* or its probability distribution while all other variables *Z* in *V* remain unchanged need to be physically possible in order for the relation between *X* and *Y* to qualify as one of direct type-level causation? Woodward's somewhat astonishing answer is negative. He supports his negative answer by distinguishing a weak and a strong sense of "physically possible" (cf. Woodward 2003: 128). He says that an intervention is physically possible in the strong sense "if and only if it is consistent with the laws of nature and the actually obtaining initial conditions." He further states that when conjoined with determinism, this strong sense of physical possibility implies that "interventions on *X* will not be possible unless they actually occur": that e.g. an intervention in the shape of administration *A* of drugs *D* according to protocol *P* to human beings with tumors of type *T* will be impossible as long as experimenters don't actually intervene to administer drugs *D* according to protocol *P* to subjects with tumors of type *T* in the treatment group. Woodward believes "that it would be much too strong to require that interventions on *X* must always be possible in this sense."

Woodward says that an intervention is physically possible in a weaker sense if and only if there is some set of initial conditions (that are consistent with the laws of nature but not necessarily identical with the initial conditions that actually obtain) such that the occurrence of this intervention is consistent with these conditions and the laws of nature. He further states that (when conjoined with indeterminism or determinism) this weak sense of physical possibility implies that interventions will not be possible unless they are consistent with the laws of nature: that e.g. an intervention that required the acceleration of a particle from a velocity less than that of light to a velocity greater than that of light will be impossible, while e.g. an intervention in the shape of administration *A* of drugs *D* according to protocol *P* to human beings with tumors of type *T* may be possible even if experimenters haven't intervened yet to administer drugs *D* according to protocol *P* to subjects with tumors of type *T* in the treatment group.

Woodward claims that it would be too strong to require that interventions on *X* must be physically possible even in the weak sense. He supports this claim by citing cases in which *X* directly type-level causes *Y* but interventions on *X* aren't physically possible even in the weak sense. One case that Woodward (2003: 129) cites is that of the motion of the tides being type-level caused by changes in the position of the moon with respect to the earth and corresponding changes in the gravitational attraction exerted by the moon on various points on the earth's surface. While the claim of the motion of the tides being type-level caused by the position of the moon is certainly true, an intervention that changes the moon's position is arguably physically impossible. As an example of an intervention that changes the moon's position, Woodward considers the doubling of the moon's orbit. An intervention that doubles the moon's orbit is not physically impossible in the sense that it is physically impossible that the moon's orbit doubles: the improbable but physically possible impact of some celestial body could double the moon's orbit. An intervention that doubles the moon's orbit is physically impossible because physically possible processes that change the position of the moon (e.g. by doubling the moon's orbit) are principally too "ham-fisted" to satisfy (*I*1) – (*I*4).

Some passages suggest that by "possible", Woodward not only understands "logically or conceptually possible" but also "non-actual" or "counterfactual". His statement of truth conditions for (*E*), for instance, suggests a counterfactual reading: he says that (*E*) is true if and only if "for some persons with tumors there is a possible idealized experimental manipulation (a possible intervention) which determines whether they receive *A*, and that if such interventions were to be carried out on these persons whether they recover (or the probability of their recovering) would be counterfactually dependent on whether or not they receive *A*" (Woodward 2002: 317). Woodward (2003: 70) further maintains that (*DC*) combines with his definitions of "total cause" and "contributing cause" to embody a counterfactual theory of causation: that these definitions establish "a systematic connection between causal claims and certain counterfactuals." If *X* is a direct type-level cause of *Y*, then *X* will also be a contributing type-level cause of *Y* (cf. Woodward 2003: 56). And Woodward (2003: 70) says that "if *X* is a contributing cause of *Y*, then a certain counterfactual will be true:

there will be some intervention on *X* such that, if appropriate other variables were held fixed, *Y* (or the probability of *Y*) would change."

It is not entirely clear whether Woodward holds that non-actual or counterfactual interventions are always physically impossible in the sense of "ideal". At one point, he notes that only "for *some* causal claims, there are no physical processes that are sufficiently fine-grained or surgical to qualify as interventions" (cf. Woodward 2003: 132, emphasis added). When reverting to more general characterizations of his notion of an intervention, however, Woodward tends to describe interventions as an ideal processes. Consider, for instance, the following passage: "An intervention on a variable *X* is always defined with respect to a second variable *Y* and can be thought of as an ideal experimental manipulation (of a sort that might be realized in a randomized experiment) of *X* for purposes of determining whether *X* bears on one or another kind of causal relationship to *Y*" (Woodward 2008: 201).

Does Woodward hold that interventions on *X* that change *Y* or its probability distribution while all other variables *Z* in *V* remain unchanged are necessarily ideal? This question cannot be answered unless another question is considered first. It is striking that (*DC*) defines direct type-level causation relative to a variable set. Can our notion of direct type-level causation be said to be relative to a variable set? Strevens (2007: 244) is right when answering this question in the negative: "Our notion of causation is surely not relativized to a variable set." But Strevens (2007: 242) supports his negative answer by considering the case of fine-graining: "the causal relationships between variables in a causal network may change as variables are added, with arrows both disappearing and appearing." And the case of fine-graining doesn't create any major difficulties for Woodward's definition of direct type-level causation. In his response to Strevens, Woodward (2008: 208-209) can point out that direct type-level causes either remain direct type-level causes or become contributing type-level causes when new variables are added to *V*.

The reason why Strevens's negative answer is right is a different one. According to Woodward (2003: 56), the selection of a variable set *V* depends on what an epistemic subject is prepared to accept as serious possibility. Since what an epistemic subject is prepared to accept as serious possibility is inter-subjectively (and, perhaps, even intra-subjectively) different, two epistemic subjects can come up with variable sets *V* and *V′* that do not just represent fine- or coarse-grained versions of the other but variable sets that are different in the sense that *X* is a direct type-level cause of *Y* when *X* and *Y* are elements of *V*, and *not* a direct type-level cause of *Y* when *X* and *Y* are elements of *V′*.

Woodward (2003: 89) intends to alleviate worries about subjectivity by noting that often decisions as to whether or not a possibility is serious "are based on facts about how the world operates that seem perfectly objective." But the examples that he discusses in this context relate to boulders that don't materialize out of thin air, balls that do not pass through solid brick walls or injuries that I suffer when I step in front of a speeding bus (cf. Woodward 2003: 89, 119). Woodward (2003: 27) aspires to provide an account of direct type-level causation that is reflected in substantial portions of the practice of the special sciences. And if he aspires to provide such an account, he should look to the special sciences for examples that support his view that relativizing the notion of direct type-level causation to a variable set doesn't turn this notion into a subjective one.

When looking to the special sciences, however, one soon realizes that relativizing this notion to a variable set *does* turn this notion into a subjective notion. Consider e.g. the case that *X* stands for the rate of change of the supply of money, that *Y* stands for aggregate output, that *X* changed shortly after the death of Benjamin Strong (the president of the Federal Reserve Bank of New York who died in 1928), and that the change in *X* was soon followed by a change in *Y*. Friedman and Schwarz (1963: 413-4, 692-3) famously argue that Strong's death brought about a change in the value of *X*, and that the value of *Y* changed as a result of the change in the value of *X*. One may also argue, however, that aggregate measures of the money stock (such as *M*2) aren't set directly by the Federal Reserve but are determined by the interaction of the supply of high-powered money with the behavior of the banking system and the public, and that the changes in the values of both *X* and *Y* result from the decision of firms to shrink production and to decrease their money holdings accordingly – they, after all, need less money for the purchase of intermediate inputs when shrinking the production (a similar case is in fact made by King and Plosser 1984).

This case manifests that researchers can differ with respect to what they are prepared to accept as serious possibility. In the case at hand, both teams of researchers (Friedman and Schwarz, on the one hand, and King and Plosser, on the other) provide empirical evidence to support their analysis. What they are prepared to accept as serious possibility is therefore likely to be determined to a large extent by the economic thought of the respective school to which they adhere. What they are prepared to accept as serious possibility in turn determines two variable sets that are different in the sense that *X* isn't a direct type-level cause of *Y* unless *X* and *Y* are elements of the set that doesn't include any variable relating to the decisions of firms to increase or decrease their money holdings. If the notion of direct type-level causation is defined relative to a variable set, and if *X* and *Y* stand for the rate of change of the supply of money and aggregate output, respectively, then whether or not *X* is a direct type-level cause of *Y* is clearly a subjective matter.

It seems that in order to get rid of this subjectivity, one has to de-relativize and revise (*DC*) in a particular way. The most obvious candidate for a de-relativized variant of (*DC*) runs as follows:

(*dc*) *X* is a direct type-level cause of *Y* iff there is a possible intervention on *X* that will change *Y* or the probability distribution of *Y* when one holds fixed at some value all variables *Z* inside a set *V* of pre-selected variables and all variables *R* outside *V*.

This definition differs from (*DC*) in that it drops the relativity of direct type-level causation to a set *V* of variables. But dropping this relativity requires that one hold fixed at some value not only all variables inside *V* but also outside *V*.

An obvious problem with (*DC*) and (*dc*) is their circularity: the notion of direct type-level causation is defined in terms of the notion of an intervention, the notion of an intervention in terms of the notion of an intervention variable, and the notion of an intervention variable in terms of the notion of direct type-level causation (condition *I*1 refers to direct or contributive type-level causation in equal measure but Woodward defines contributive type-level causation in terms of direct type-level causation, cf. Woodward 2003: 59). Note, however, that Woodward doesn't care a lot about charges of circularity. In the case of (*DC*), he responds to this charge by pointing out that what he aims at is not a reductive analysis of causation but rather a kind of description of the conceptual entanglement of causation and intervention (cf. Woodward 2003: 20-22), and that the circularity in question is not of the vicious kind: that the information needed to decide whether *I* directly type-level causes *X* doesn't coincide with the information needed to decide whether the relation between *X* and *Y* is one of direct type-level causation (Woodward 2003: 104-105). A similar response applies in the case of (*dc*).

4. *Ceteris constantibus*, *ceteris neglectis*, and *ceteris absentibus* conditions

A closer look indicates that the right-hand side of the biconditional expressed by (*dc*) makes reference to two conditions that can be interpreted in two possible ways. The first condition says that

(*CP*1) there is a possible intervention on *X* that will change *Y* or the probability distribution of *Y* when one holds fixed at some value all variables *Z* inside a set *V* of pre-selected variables.

The other variables *Z* in *V* may also include an error variable *U* that summarizes the influence of factors regarded as relevant but negligible. The condition that

(*CP*2) there is a possible intervention on *X* that is followed by a change in *Y* or its probability distribution while $U \in V$ remains unchanged,

is therefore just an instance of condition (*CP*1). When discussing the *cp* character of (*CP*1) and (*CP*2), however, it will become clear that (*CP*1) plays an important role on its own. The second condition, by contrast, states that

(*CP*3) there is a possible intervention on *X* that will change *Y* or the probability distribution of *Y* when one holds fixed at some value all variables *R* outside a set *V* of pre-selected variables.

When the two conditions referred to on the right-hand side of the biconditional expressed by (*dc*) are re-written as (*CP*1) – (*CP*3), then (*dc*) can be re-written as follows:

(dc^{CP}) *X* is a direct type-level cause of *Y* if and only if (*CP*1) – (*CP*3).

This definition differs from (*DC*) only in that it drops the relativity of direct type-level causation to a set *V* of variables. Dropping this relativity necessitates the explicit mention of (*CP*3) on the right-hand side of the biconditional. Conditions (*CP*1) and (*CP*2), by contrast, only replace the phrase "when one holds fixed at some value all other variables *Z* in *V*."

Conditions (*CP*1)-(*CP*3) are denoted by '(*CP*1)', '(*CP*2)' and '(*CP*3)', respectively, because there is an important motivation to refer to them as *cp* conditions. Woodward is right when pointing out that causal claims like (*E*) do not represent claims of exceptionless regularities, and that the set of beliefs (1)-(4) cannot motivate referring to (*CP*1), (*CP*2) and (*CP*3) as *cp* conditions. There is thus no metaphysical or logical motivation to refer to them as *cp* conditions. But there may be other motivations, and while one such motivation is of a heuristic kind, the other relates to the context of the applicability of claims of direct type-level causation. When interpreted heuristically or with respect to the applicability of claims of direct type-level causation, (*CP*1) can be referred to as a *ceteris constantibus*, (*CP*2) as a *ceteris neglectis* and (*CP*3) as a *ceteris absentibus* condition.[1] A *ceteris constantibus* condition requires that the values of variables representing potentially interfering causes remain unchanged. A *ceteris neglectis* condition, by contrast, says that the values of variables representing causes that are potentially interfering but too unimportant also need to remain unchanged. And a *ceteris absentibus* condition requires that there be no other potentially interfering causes.

All three conditions represent *cp* conditions in the sense that the term 'paribus' in 'ceteris paribus' is given a specific meaning. But all three conditions play different roles when functioning as heuristic conditions or as applicability conditions, i.e. as conditions that need to be satisfied in order for claims of direct type-level causation to be applicable. When functioning as heuristic conditions, these conditions are used as theoretical assumptions: as a "way of simplifying the logical development of the theory" (Musgrave 1981: 383). Hausman (1992: 133-4), for instance, interprets the *ceteris constantibus* condition as a theoretical assumption when noting that in economic partial equilibrium analysis the *cp* clause is used to pick out things that do not change. Musgrave (1981: 381) similarly interprets the *ceteris absentibus* condition as a heuristic condition when treating it as a domain assumption.

When functioning as applicability conditions, by contrast, *ceteris constantibus*, *ceteris neglectis* and *ceteris absentibus* conditions state what has to be the case if physically possible interventions are supposed to represent direct token-level causes: if physically possible manipulations of the variables that we find on the right-hand side of the structural equations

[1] Boumans and Morgan (2001: 13) introduce the Latin expressions for (*CP*2) and (*CP*3). Following their lead, one may refer to (*CP*1) as a *ceteris constantibus* clause.

used in the special sciences are supposed to be followed by changes in the values of the variables that we find on the left-hand side. Cartwright (1983: 45), for instance, interprets *cp* conditions as applicability conditions when choosing to translate the term *ceteris paribus* by 'other things being right' and not by 'other things being equal'. Boumans and Morgan (2001: 13) elaborate on this interpretation when explicitly contrasting the interpretations of *cp* conditions as heuristic conditions and applicability conditions: "These three readings of the *ceteris paribus* clause, viz. treating other things as respectively constant, absent or negligible, can be considered as a theoretical assumption. But […] [i]f we would like to apply the model for intervening into the world, we might consider these clauses as indicating how one should possibly set the circumstances."

The question is, of course, whether we should interpret (*CP*1)-(*CP*3) as heuristic assumptions or applicability conditions. If (*CP*1)-(*CP*3) were heuristic assumptions, then an intervention on *X* that will change *Y* or the probability distribution of *Y* when one holds fixed at some value all variables in- and outside *V* wouldn't be possible in Woodward's (strong or weak) sense of "physically possible" but only possible in the sense of "ideal". Interpreting (*CP*1)-(*CP*3) as heuristic assumptions would accordingly be in line with Woodward's more general characterizations of his notion of an intervention. But if (*CP*1)-(*CP*3) were heuristic assumptions, then the relation of direct type-level causation would also have to be conceived of as an ideal entity. And it is questionable whether this conception would be compatible with the kind of "metaphysically modest and noncommittal" realism that Woodward (2003: 121) wishes to endorse. It therefore seems appropriate to interpret (*CP*1)-(*CP*3) as applicability conditions. If (*CP*1)-(*CP*3) can be interpreted as applicability conditions, then they state what has to be the case for there to be a physically possible intervention on *X* that is followed by a change in the value of *Y*: potentially interfering causes need to remain constant, negligible or absent.

5. The scientific legitimacy of causal *cp* statements

If a definition is understood as a statement of truth conditions, then (dc^{CP}) may be reformulated as

(dc^{T}) '*X* is a direct type-level cause of *X*' is true if and only if (*CP*1)-(*CP*3).

This statement of truth conditions states truth conditions for '*X* is a direct type-level cause of *X*', not for '*cp X* is a direct type-level cause of *X*'. What is it that might possibly justify regarding causal statements (claims of direct type-level causation) as *cp* statements? The answer recommending itself is that causal statements may be regarded as *cp* statements in the sense that the statement of their truth conditions makes reference to conditions that, in the context of the applicability of the models that encode causal statements, may be referred to as *cp* conditions. This answer recommends itself because it does justice to an important motivation to refer to (*CP*1)-(*CP*3) as *cp* conditions, while at the same time honoring Woodward's verdict according to which causal statements can't be *cp* statements in the sense of claims of exceptionless regularities hedged by a completer.

When causal statements can be regarded as *cp* statements, however, the question of whether they are scientifically legitimate resurfaces. Causal statements are statements of relations of direct type-level causation between variables. When true, these statements are non-vacuously true because their truth requires that there be interventions that are possible in Woodward's sense of (ideally or physically) "possible".[2] Causal statements may also be said to be explanatory. Following Woodward, one may say that the statement that *X* is a direct type-level cause of *Y* is explanatory if it is true. This statement is true if the structural equation expressing it is invariant under interventions, i.e. if this equation continues to hold in at least some situations in which a hypothetical or actual intervention changes the value of *X* (cf. Woodward and Hitchcock 2003: 13,15).

Causal statements might turn out to be non-testable, however. The *cp* statement that *X* is a direct type-level cause of *Y* cannot be confirmed unless there is at least one test intervention, i.e. a physically possible intervention on *X* that is followed by a change in the value of *Y*, while (*CP*1)-(*CP*3) are satisfied. And how are we to decide whether (*CP*1)-(*CP*3) are satisfied? We might carry out or observe an intervention that leads to a change in *X*, and we might observe that *Y* also changes. But unless we can resort to a procedure that randomly assigns subjects to a treatment and control group, we cannot rule out that *Y* changes as a result to a change in the value of some other variable in *V*. And there is never a guarantee that it isn't a change in a variable outside *V* that leads to the change in the value of *Y*. Logically speaking, it is therefore impossible to test any causal *cp* statements.

Practically speaking, researchers have developed effective means of controlling at least the variables that lie inside the set *V* or pre-selected variables. They carry out experiments whenever they have the opportunity of randomly assigning the set *V* of variables of their interest to a treatment as well as to a control group. The purpose of this randomizing procedure is to insure that the values of all variables in *V* that might possibly influence *Y* are (roughly) evenly distributed between the two groups so that any change in *Y* that occurs only in the treatment group but not in the control group will be attributable to a change in *X*. It was pointed out in section 2 that Woodward holds that such randomized controlled trials can be carried out to test (*E*).

[2] There is, of course, substantial disunity among philosophers of science over the question whether causal statements can be accepted as true if there can't be any physically possible interventions for these statements. Woodward, however, is prepared to accept a causal statement as true as long as there is at least one (ideally or physically) possible intervention for that statement.

Woodward (2002: 319) also holds that "randomized experiments are just one of many possible ways of testing and providing evidence for causal generalizations […] like (*E*)." He says that what these ways of testing and providing evidence have in common is that "they often involve creating new situations or discovering naturally occurring ones in which some or all other possible causes of some effect *E* besides some putative cause *C* are known to be distributed on average in the same way across different levels of *C*" (Woodward 2002: 320). And he adds that he finds it "baffling that so little attention is paid to the logic of such arguments in the *cp* laws literature" (Woodward 2002: 320).

It has to be pointed out, however, that all other possible causes of some effect *E* besides some putative cause *C* are never "known" to be distributed on average in the same way across different levels of *C*. In a footnote, Woodward (2002: 326n) cites the "familiar assumption in the context of multivariate regression that the error term is uncorrelated with any of the independent variables" as an example of this knowledge. But while this assumption can be made plausible when randomized controlled trials can be carried out, it certainly cannot be inferred from non-experimental data. Social scientists and econometricians sometimes pretend to be able to infer this assumption from non-experimental data. But the statistical tests that they employ (e.g. the *Hausman* and the *Sargan* tests) cannot be accepted as adequate tests of this assumption.[3]

It also has to be emphasized that "creating new situations or discovering naturally occurring ones" (conducting or observing natural experiments) amounts to intervening on *X* in a particular system *A*. If there isn't any system *B* that is sufficiently similar to *A* except that in *A* there is an intervention on *X*, then a possible change in *Y* that occurs in *A* isn't necessarily attributable to a change in *X*. The change in *Y* may also be attributable to a change in a variable lying outside the system. It therefore isn't very "baffling" that the alternative ways of testing causal statements that Woodward mentions don't receive the attention in the *cp* literature that he thinks they deserve.

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[3] The problem with the *Hausman* test is that it relies on the notion of an unbiased instrumental variable (IV) estimator: on the notion of an unbiased estimator for a variable that is itself assumed to be uncorrelated with an error. Before conducting a *Hausman* test, one would in fact have to test the hypothesis of no correlation between an instrument and the error term. And the frustrating thing about such a test is that it is not available. Something that comes close to an econometric test of the correlation between an instrument and an error is only available in the case in which the equation into which the instrument is introduced is "over-identified": in which this equation includes at least two instruments for the regressor that is correlated with the error. This test (usually referred to as the *Sargan test*) assumes that among the instruments is at least one valid instrument per regressor correlated with the error. It involves the estimation of the parameters and residuals of the equation in question as well as the regression of the residuals on the instruments. And it is supposed to be successful if the regression of the residuals on the instruments yields coefficient estimates that are insignificantly different from zero. But the problem with this test is that even a successful test doesn't tell us which of the instrumental variables are valid instruments. It doesn't tell us, that is, which of the instruments might be used to calculate an IV estimator for the *Hausman* test.

As an illustration, consider the statement that private aggregate demand is a direct type-level cause of the level of inflation. In the case of this statement, it is impossible to observe the level of inflation and private aggregate demand in a treatment and in a control group: no two economies are sufficiently alike so that all factors that might possibly influence the level of inflation would be roughly evenly distributed. Natural experiments involving a change in private aggregate demand also rarely unfold: exogenous shocks to private aggregate demand are rarely observed, and government policies to the effect of stimulating private aggregate demand are carried out irregularly and under varying circumstances. Even if a natural experiment involving a change in private aggregate demand could be observed or carried out, and even if it is followed by a change in the level of inflation, this natural experiment wouldn't provide any conclusive evidence for the statement that private aggregate demand is a direct type-level cause of the level of inflation. A natural experiment involving a change in private aggregate demand would amount to an intervention on private aggregate demand in a specific economy *A*. If there isn't any economy *B* that is sufficiently similar to *A* except that in *A* there is an intervention on private aggregate demand, then a possible change in the level of inflation isn't necessarily attributable to a change in private aggregate demand. The change in the level of inflation may also be attributable to a change in inflation expectations or to a change in some unknown factor that causes the change in private aggregate demand or is caused by this change or by a change in any other unknown cause that also causes the change in private aggregate demand.

One may accordingly say that researchers have developed effective means of testing causal claims but that it is, logically speaking, impossible to test them: that empirical evidence provided by test interventions necessarily remains inconclusive, especially when test interventions cannot be carried out in the context of randomized controlled trials. Note that the inconclusiveness of the evidence provided by test interventions cannot come as a surprise when (*CP*1)-(*CP*3) are interpreted as *cp* conditions in the sense of applicability conditions. Applicability conditions tell us what would have to be the case in order for an intervention to qualify as a test intervention. But we can never know whether these conditions are in fact satisfied.

7. Explanatory depth and lawhood

If causal statements are explanatory if true, then an account of causal *cp* laws is easily obtained from Woodward's account of the explanatory character of causal statements. Remember that Woodward holds that the statement that *X* is a direct type-level cause of *Y* is explanatory if it is true, and that it is true if the structural equation expressing it is invariant under interventions, i.e. if this equation continues to hold in at least some situations in which a hypothetical or actual intervention changes the value of *X*. Now, note that Woodward also holds that claims of direct type-level causation have explanatory depth, and that the explanatory depth of a causal statement is a positive function of the width of the range of the (ideally or physically) possible interventions under which a causal statement (or the structural equation expressing it) remains invariant. In this sense, “the van der Waals equation will be invariant under a wider ranger of interventions than the ideal gas law […], General Relativity will be more explanatory than Newtonian gravitational theory, and so on” (Hitchcock and Woodward 2003: 184).

Woodward is prepared to refer to claims of direct type-level causation as generalizations and to say that one such “generalization can provide a deeper explanation than another if […] it is invariant under a wider range of interventions” (Hitchcock and Woodward 2003: 198). But he also takes great pains to show claims of direct type-level causation can be explanatory without representing laws in the sense of exceptionless regularities (cf. section 2). Now, if an account of causal *cp* laws is to be obtained from Woodward’s account of the explanatory character of claims of direct type-level causation, then a definition of “law” is needed that doesn’t make reference to exceptionless regularities. The obvious candidate for such a definition is a definition of “law” in terms of Woodward’s notion of explanatory depth:

(CL^{CP}) A causal *cp* statement is a causal *cp* law if and only if this statement is explanatorily deep, i.e. if and only if the structural equation expressing it remains invariant under a sufficiently wide range of (ideally or physically) possible interventions.

There is of course the difficulty that it isn’t necessarily clear when a causal statement’s explanatory depth is great, or when the range of interventions is sufficiently wide. This difficulty, on the other hand, reflects the fact that scientists often disagree over the question as to whether or not a generalization is to be accepted as a law.

A causal *cp* law in the sense of (CL^{CP}) is obviously different from a *cp* law in the sense of an exceptionless regularity hedged by a completer. Understanding causal statements as *cp* laws in the sense of (CL^{CP}) therefore doesn’t conflict with Woodward’s general thesis that there is no such thing as a *cp* law. It is, of course, a bit ironic to develop an account of causal *cp* laws from Woodward’s account of the explanatory character of causal statements. But Woodward and Hitchcock (2003: 3) explicitly state that their account of explanatory generalizations may also be read “as a new account of laws, rather than as an argument that generalizations that are not laws may figure in explanations.” The account of causal *cp* laws defended in this paper is therefore not inconsistent with Woodward’s claim that the explanatory character of causal statements doesn’t require that causal statements be causal *cp* laws.

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