

CONFUSION AND BAD ARGUMENTS IN THE CONCEPTUAL ANALYSIS OF CAUSATION*

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1. *Introduction*

After a long tradition of monistic approaches, pluralistic approaches are becoming more and more popular in the philosophy of causation. However, pluralism with respect to causation is clearly not yet generally accepted: a considerable group of philosophers keeps searching for a monistic theory of causation. In former articles (De Vreese, 2006; De Vreese, 2007), one of us has argued that one should discern different kinds of causal pluralism, namely conceptual causal pluralism, metaphysical causal pluralism, and epistemological-methodological causal pluralism. Each of these opposes its monistic counterpart. Discerning these three approaches is necessary to avoid confusion on what “causal pluralism” means. We will focus here on conceptual causal pluralism and its opponent, i.e. conceptual causal monism. A conceptual approach to causation is concerned with our everyday causal intuitions, the way we think and reason about causation in commonsense situations, and the causal concepts we use when making everyday causal judgements. We will e.g. not discuss the issue whether there is causation at different levels of reality (that would be a metaphysical question) and we will not talk about the causal concepts that e.g. social scientists or biomedical scientists need in order to do good research (such questions fit into the epistemological-methodological approach).

In general, a conceptual analysis can be descriptive or revisionist. This is also true for a conceptual analysis of causation. The aim of a descriptive conceptual analysis is to develop a “correct definition” of causation that “fits the facts”, i.e. fits the way the concept is used in everyday language. Dowe (2000) gives two quotes of philosophers who conceive their job this way. The first quote is by Curt Ducasse:

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The problem of giving a ‘correct’ definition of the causal relation is that of making analytically explicit the meaning which the term ‘cause’ has in actual concrete phrases that our language intuition acknowledges as proper and typical cases of its use. (from Ducasse, 1926; quoted in Dowe, 2000, p. 2)

The second quote is by Hart and Honoré:

The ordinary man has a quite adequate mastery of various concepts within the field of their day-to-day use, but, along with this practical mastery goes a need for explicit statement and clarification of the principles involved in the use of these concepts. (from Hart & Honoré, 1985; quoted in Dowe, 2000, p. 3)

A revisionist conceptual analysis proposes changes to the way we use the word “cause”. Hume is the prototype of this: after rejecting the way we use the word in ordinary language on the grounds that this is epistemologically suspect (necessary connections cannot be observed) he proposes to use “cause” in a different, acceptable way: cause = contiguity + time order + constant conjunction. Hume is well aware that this definition is not completely consistent with our daily intuitions, but it is (in his view) free of epistemological problems. The fact that his definition is free of epistemological problems, in his view, justifies the revision.

In this paper we discuss three important recent contributions to the conceptual analysis of causation: Hall 2004, Dowe 2000 and Woodward 2003. We will show that, while Ned Hall’s plea for a dualistic conceptual analysis is methodologically sound, there are serious methodological problems with the way Phil Dowe and Jim Woodward defend their monistic theories. This does not entail that Hall is right. And we do not want to decide here who is right. Our aim is meta-philosophical: we want to make sure that the debate continues in a methodologically correct way, by clearing up confusions and exposing bad argumentation strategies.

The structure of this paper is straightforward. In Sections 2–4 we present the core of the theories of Hall, Dowe and Woodward and give a methodological appraisal of them. In Section 5 we describe how the debate on what is the best conceptual analysis of causation can and should continue.

2. Ned Hall’s Plea for Causal Dualism

2.1. Ned Hall is probably the most influential conceptual causal pluralist at the moment. He has recently argued that causation, “understood as a relation between events”, comes in at least two basic and fundamental different varieties:

Events can stand in one kind of causal relation — *dependence* — for the explication of which the counterfactual analysis is perfectly suited [namely, had *c* not occurred, *e* would not have occurred] (...). And they can stand in an entirely different kind of causal relation — *production* — which requires an entirely different kind of causal analysis [namely, *c* produces *e*] (...). (Hall, 2004, p. 226, cf. p. 252–257; emphasis added)

If we put his view into a definition of causation, we get:

C causes E, if and only if, [E counterfactually depends on C] or [there is a causal mechanism by which C produces E].

Hall argues that in some cases a reading in terms of production is required, in yet some others a reading in terms of dependence, and, in most cases, a reading in terms of both. Let us start with a quintessential example from Hall, which is intended as an example of a case where only dependence is required:

Suzy and Billy have grown up, just in time to get involved in World War III. Suzy is piloting a bomber on a mission to blow up an enemy target, and Billy is piloting a fighter as her lone escort. Along comes an enemy fighter plane, piloted by Enemy. Sharp-eyed Billy spots Enemy, zooms in, pulls the trigger, and Enemy’s plane goes down in flames. Suzy’s mission is undisturbed, and the bombing takes place as planned. If Billy hadn’t pulled the trigger, Enemy would have eluded him and shot down Suzy, and the bombing would not have happened. (Hall, 2004, p. 241)

Billy’s pulling the trigger *did not produce* the bombing, rather it neutralized a state-of-affairs that would have prevented the effect from occurring. The occurrence of the bombing *was dependent* on Billy’s pulling the trigger, but not produced by it. In this example, the effect counterfactually depends on the cause, but there is no mechanism linking cause and effect. Counterfactual dependence “seems to be the only appropriate causal relation for

such “negative events” to stand in” (ibid., p. 256). Hall counters the obvious counter-response that would read this example in mechanistic terms, as follows:

A remarkably frequent but entirely unsatisfactory response is the following: Billy’s action *is* connected to the bombing via a spatiotemporally continuous causal chain — it’s just that this chain consists, in part, of *omissions* (namely, the various failures of Enemy to do what he would have done, had Billy not fired). (...) For there is no reason to believe that the region of spacetime these omissions occupy intersects the region of spacetime that Suzy and her bomber *actually* occupy; to hold otherwise is just to mistake *this* region with the region she *would have* occupied, had Billy not fired. (ibid., p. 243)

In other cases there is a causal mechanism but no counterfactual dependence (because there is simultaneous over-determination). Suppose that Billy and Suzy are engaged in a competition to see who can shatter a target bottle first. Suppose further that Suzy throws her rock a split second before Billy. Suzy’s throw is spatiotemporally connected to the shattering in the right way, but Billy’s is not:

Suzy’s throw is a cause of the shattering, but Billy’s is not. Indeed, every one of the events that constitute the trajectory of Suzy’s rock on its way to the bottle is a cause of the shattering. But the shattering depends on none of these events, since had they not occurred the bottle would have scattered anyway, thanks to Billy’s expert throw. (ibid., p. 235)

Hall concludes from this that counterfactual dependence is causation in one sense: it is sufficient for causation but not necessary.

An important aspect of Hall’s view is that in typical cases of causation, both relations are present and production and dependence coincide (ibid., p. 254, p. 265). So though the relations are conceptually distinct, in the actual world their extensions overlap in most cases. Atypical cases of causation occur where there is a production relation without counterfactual dependence (e.g. over-determination) or where there is a relation of counterfactual dependence without a production relation (e.g. double prevention).

2.2. It is clear that Hall aims at a descriptive conceptual analysis. In defending his approach, he talks, for example, about “[the] basic principles governing *our application of “cause”*” (ibid., p. 255, emphasis added), and

“the ambiguity in *our causal talk*” (ibid., p. 256, emphasis added)). And he always respects the verdict of our intuitions. As an example, take a look at the way he argues against the following possible objection:

A third, more congenial objection begins by granting the distinction between production and dependence, but denying that dependence deserves to be counted a kind of causation at all. Now, I think there is *something* right about this objection, in that production does seem, in some sense, to be the more “central” causal notion. As evidence, consider that when presented with a paradigm case of production without dependence [...] we unhesitatingly classify the producer as a cause; whereas when presented with a paradigm case of dependence without production [...] our intuitions (well, those of some of us, anyway) about whether a genuine causal relation is manifested are shakier. Fair enough. But I think it goes too far to deny that counterfactual dependence between wholly distinct events is not a kind of *causal* relation. Partly this is because dependence plays the appropriate sort of roles in, for example, explanation and decision. [...] And partly it is because I do not see how to accommodate causation of and by omissions (as we should) as a species of production; counterfactual dependence seems the only appropriate causal relation for such “negative events” to stand in. (ibid., p. 256)

He further presents a counterexample to his own theory, and recognizes that it forms a real problem for his account, necessary to be dealt with in future research:

[T]here are certain kinds of cases that we have some inclination to call cases of causation, but that also elude classification in terms of production or dependence. Here is an example, a slight variation on the story of Billy, Suzy and Enemy: This time, there is a second fighter plane escorting Suzy. Billy shoots down Enemy exactly as before, but if he hadn’t, the second escort would have. (ibid., p. 271)

This example is an instance of preempted double prevention; in this case it is no longer true that the bombing wouldn’t have happened if Billy hadn’t pulled the trigger. Hall admits that one will nonetheless be inclined to grant Billy some causal responsibility for the success of the bombing, just as when there was no second escort. In admitting this kind of counterexample to be “a piece of unfinished business that affects my account of causation” (ibid., p. 272), Hall is very consistent in aiming at his goal, a descriptive conceptual

analysis which respects our causal intuitions. His aim is clear, and the way he argues is consistent with his aim.

3. *Phil Dowe's monistic analysis in Physical Causation*

3.1. In the first chapter of *Physical Causation*, Dowe says that he wants an empirical analysis of causation, not a conceptual analysis. This distinction is however very ambiguous, since he admits that his empirical analysis comes down to “a conceptual analysis of a concept inherent in scientific theories” (Dowe, 2000, p. 11), and he further stresses that this differs from a conceptual analysis of scientists' usage of the term. However, if one takes a further look at Dowe's arguments, it turns out that he is doing ordinary conceptual analysis. He criticizes existing theories of causation on the ground that they do not square with his intuitions (which, like every conceptual analyst, he assumes to be shared by the readers of his book) about certain examples. Indeed, most of his examples are scientific: they are drawn from physics. But the intuitions he attaches to them are not scientific: they are not inherent in the theory. They are Dowe's intuitions. Here is an overview of crucial moves of Dowe:

Radioactive decay of an unstable atom (contra Hume's determinism)

Let us start with the following example of Dowe, which should show that Humean deterministic theories of causation have shortcomings:

In order to rattle everyday intuitions, consider the following argument. If I bring a bucket of Pb^{210} into the room, and you get radiation sickness, then doubtless I am responsible for your ailment. But in this type of case, I cannot be morally responsible for an action for which I am not causally responsible. (Dowe, 2000, p. 23)

The argument is that, given the scientifically plausible assumption that the decay of Pb^{210} is an indeterministic process, it follows that indeterministic causation exists and hence that deterministic theories of causation should be rejected because of this counterexample. Contrary to his own intentions, Dowe defends this counterexample to Hume's deterministic approach on the basis of folk intuitions and scientist's causal talk:

It may be objected here that I am making an illicit appeal to everyday intuitions about meaning — illicit since I am using them to criticise Hume's empirical theory. The latter is true. However,

this does not preclude us from utilising such considerations for the purpose of shaking preconceptions, such as causal determinism, if those preconceptions have no scientific basis. I don't think this is an illicit strategy.

But I also have a second argument. This is simply to point out that scientists describe such cases of decay as instances of *production* of Hg^{206} . Now 'production' is a near-synonym for 'causation' (Mellor, 1988:231), so it's not just folk engaged in everyday discourse, but also scientists at work, who pronounce C to be a cause of E. It may be objected here that we should be appealing not to what scientists say at offhand moments, but to the concepts inherent in scientific theory and explanation. I agree that such considerations carry more weight. But scientists' language does illustrate the fact that they see no scientific obstacle to applying our everyday concept of causation to indeterministic cases. (Dowe, 2000, p. 23–24)

However, what Dowe does not seem to be aware of, is that his analysis of the example involves itself a preconception which has no scientific basis. As Steel (2007) points out, it is very clear that Dowe bases his argumentation on a presupposed understanding of the concept of causality according to which moral responsibility entails causal influence. Such a presupposition can nonetheless not be derived from physics. In other words, contrary to what Dowe claimed to be aiming for, his analysis is based here on several causal intuitions which can not at all be derived from a causal concept inherent in the scientific theory under consideration.

Chance-lowering causality and subatomic physics (contra Suppes' probabilistic approach)

To reject Suppes' probabilistic approach to causation in terms of a positive statistical relevance (PSR) relation between cause and effect, Dowe bases his argumentation on a single counterexample involving chance-lowering causality. The best-known example of chance-lowering causality is the example of the golf ball that is rolling towards a cup, but is then kicked by a squirrel resulting in an unlikely series of collisions with nearby trees which make the golf ball end up into the cup after all. Although the kick of the squirrel lowers the chance that the golf ball will end up in the cup, it nonetheless causes this outcome in this example. Consequently, we are confronted here with a case in which cause and effect do not stand in a positive statistical relevance relation. This counterexample can nonetheless be solved by some methods proposed by Salmon (1984) and Menzies (1989). However, Dowe claims to have a counterexample of chance-lowering causation from

subatomic physics, which would not be solvable by way of these methods. Suppose an unstable atom may decay by various paths, namely $A \rightarrow B \rightarrow E$, $A \rightarrow C \rightarrow E$ or $A \rightarrow C \rightarrow D$ (in which A stands for the existence of an unstable atom a , and the other capital letters stand for the event of the production of the atoms b , c , d , and e). Suppose further that the probabilities are as follows: $P(C) = 1/4$; $P(E|C) = 3/4$; $P(B) = 3/4$; $P(E|B) = 1$. Take now a particular instance where the decay process moves along the path $A \rightarrow C \rightarrow E$. We would then be inclined to say that the production of the atom c is the cause of the production of the atom e . However, there is no positive statistical relevance relation between C and E , since $P(E|C) = 3/4 < P(E) = 15/16$ ($P(E)$ equals $P(C).P(E|C) + P(B).P(E|B)$). A method proposed by Menzies (1989), namely to specify the effect E more closely, would be able to bring a solution:

Take E' to be the production of atom e together with an α particle and E'' to be the production of atom e together with a β particle. [...] Then $P(E'|C) = 3/4$, but $P(E') = 3/4 \times 1/4 = 3/16$, which is less than $P(E'|C)$. Hence C and E' stand in the right PSR relation. So, according to the probability relations, B causes E'' and C causes E' ; but we cannot say B causes E or C doesn't cause E , because this is description at the wrong level. (Dowe, 2000, p. 36)

Dowe nonetheless does not want to accept this as a solution. His arguments lean on an analogy with ordinary causal talk:

[...] in insisting on fragile events (closely specified events) we are moving away from ordinary causal talk. Normally we would suppose that if C caused E' , then C caused E (where E' is a closer specification of the event E). If smoking caused my lung cancer together with some other side effect, then we happily say that smoking caused my lung cancer, *simpliciter*. [...] Or if my cancer caused me to die in pain, then my cancer caused me to die. So at least sometimes in ordinary causal talk there is an entailment concerning the effect-event: where the fine-grained event is caused, the coarse-grained event is caused. This relation is not mirrored by an account that insists on fine-grained effect-events. Thus it follows from this approach that much of our causal talk is literally false, since it does not describe events to a sufficient extent. Given that we are seeking an empirical analysis, that we move away from ordinary causal talk is not in itself such a bad thing, provided we have sound physical reasons. But there is no clear physical picture here to appeal to. (Dowe, 2000, p. 36)

In other words, Dowe himself admits that the intuitions on which his analysis of the example is based, is not supported by physical theory itself, but rather by his own intuitions regarding the right way to analyse the example.

Spaceship (contra transference account)

The example of the spaceship is used by Dowe as a counterexample against transference theories. The key idea of these theories is that a cause transfers something, such as energy, to its effect. Dowe counters:

There is a type of causation quite different from the type envisaged by the transference theorists, and one that the transference theory rules out of court. This is the case where an object persisting in time is thought of as a causal process. One persuasive example is when an object's inertia is the cause of its continuing motion. For example, consider a spaceship moving through space with constant rectilinear motion, not acted on by any force. We should say that the cause of its continuing motion is its own inertia, and indeed, that earlier states are the causes of the later states. But there is no transfer of energy or momentum from one object to another — in fact, there is no causal interaction. That it fails to allow for such cases of causation is a short-coming of the transference theory. (Dowe, 2000, p. 52)

Dowe argues that this is supported by what science teaches us about how causation works. He substantiates this claim by referring to earlier, similar objections, which were nonetheless refuted. He further refers to the fact that the spaceship moves according to Newton's First Law, which states that a body will continue in motion unless acted on by a force. Dowe argues that "*To say, as one would if one were to deny that the body's inertia is a cause of its own motion, that Newton provided no causal explanation is to take special pleading too far [...]*" (Dowe, 2000:53). Dowe nonetheless seems to confuse two things here: namely, the fact that the spaceship has a property (namely what is called "inertia") which can explain its behaviour, and which is supported by Newton's First Law, and the fact that this property itself would be an instance of a causal process. The former cannot support the latter. Hence here again, instead of scientific theories, it seems to be Dowe's intuition that persistence should be seen as causation which supports his rejection of the transference account.

3.2. In all the cases above, the intuitions attached to the examples are in the first place Dowe's intuitions, which we are supposed to share.¹ The fact that Dowe regards conflicts with our intuitive judgments about particular cases as arguments against rival conceptual analyses, is of course no problem: this is what every good conceptual analyst must do if his/her aim is descriptive. The problem is that he does not use this standard for judging his own theory. Dowe argues that it is no shortcoming of his theory that it excludes omissions and preventors as causes, since these are no real causes, according to him. We, ordinary people, are in fact mistaken if we handle omissions and preventors as "real causes" in our everyday causal judgements. Apparent counterexamples to his theory are labelled "causation*" (Dowe, 2000) or "quasi-causation" (Dowe, 2004). If Dowe would use the same standards to judge his own theory as he used for rival theories, he would conclude that the causal intuitions of the majority of ordinary language users falsify his account. But he concludes, on the contrary, that our intuitions are wrong. In the clash between theory and intuitions, theory is suddenly given priority: our intuitions must be revised.

The result of this double standard is that Dowe's analysis is unconvincing from a descriptive point of view: a descriptive conceptual analyst must respect intuitions. From a revisionist point of view, it is also unconvincing. A revisionist conceptual analyst must give us a reason why in the specific cases he indicates, our intuitions must be changed. Dowe does not answer this question. His revisionism is therefore arbitrary.

Here are some examples, referred to by Dowe, of cases that intuitively contradict his theory:

* Clearly, your failure to regularly clean your teeth is the cause of your tooth decay.

* I caused her death by holding her head under water for five minutes.

* The hospital administration caused the death of an elderly patient by refusing to release funds to ship expensive equipment from the USA and thereby allowing her to die by "natural causes".

(Dowe, 2004, p. 190–191)

Though he is aware of these conflicts, he refuses to see this as a problem for his theory. He proposes to rename them as quasi-causation:

¹ Conceptual analysts always have to assume that their intuitions are shared by the majority of the language users. This assumption may be problematic, but we cannot discuss the methodological problems related to it in this paper.

Events that we think of as negative may turn out really to be positive. In those cases, apparent omissions and preventions turn out to be cases of ordinary genuine causation. Alternatively, apparently positive events may turn out to be negative events and, consequently, cases of apparently genuine causation may turn out to be omissions or preventions. The latter is especially convincing. Take the case of "causing" drowning. Actually, this is quasi-causation, since holding her head under water prevents her from getting oxygen. Thus there is an epistemic blur between quasi-causation and causation.

So, preventions and omissions may be very much more commonplace than we commonly recognize. "Smoking causes heart disease," but perhaps the actual effect of smoke is to prevent normal processes from impacting certain cells in a certain way, so that, in the absence of those processes, diseased cells prosper (causation by omission).

These considerations are of merely epistemic concern. We may not know whether a given case is a prevention or genuine causation, but the conceptual distinction between genuine causation and omission/preventions is clear enough. (Dowe, 2004, p. 194)

What Dowe does, is simply to deny a part of our causal intuitions, such that he does not need to take care of what Hall calls *counterfactual dependence*. Such arbitrary proposal for revising our intuitive causal judgments is unacceptable from a methodological point of view. Given the possible aims of conceptual analysis, we should either take intuitive judgments as prior to theory (descriptive aim) or offer reasons for revising a specific part of our intuitions. Only in this way, progress towards better theories can be made.

Summarizing the results of this section, we can say that Dowe is confused about his aims. His idea of empirical analysis is problematic because the difference with conceptual analysis is (at least) unclear. If we treat Dowe's theory as a conceptual analysis, the way he argues makes it problematic both as a descriptive and as a revisionist conceptual analysis.

4. Jim Woodward's monistic analysis in Making Things Happen

4.1. When developing his manipulationist theory in terms of invariance under interventions, Woodward (2003) was in search for a single, univocal set of causal criteria, apt to get a grip on our everyday notion of cause. His theory seems indeed applicable to a wide range of cases. However, he as well rejects certain causal intuitions in favour of his manipulationist approach:

One consequence of these considerations is that a number of properties or conditions that are often thought to be causes are at best problematic candidates for this role; examples include the property of being a member of a certain species, being a member of a particular race, and being a certain age. In each case, the notion of an intervention that changes the values of these “variables” does not appear to be well-defined. It might be thought that it is an arbitrary stipulation to claim that such properties cannot be causes, but in fact, [...], causal claims involving them are genuinely unclear precisely because it is unclear what hypothetical experiments to associate with them. (Woodward, 2003, p. 113–114)

This means that it would be incorrect to judge that a woman’s sex or race can be the cause of her treatment by others, for example, of being fired. The reason is that there is no intervention which is able to change, for example, the variable “being a woman”. What can be a cause of being fired according to Woodward, are the ideas an employer has about women, since these ideas seem to be able to be changed under interventions. Woodward clearly insists here on making a revision which is not in line with our commonsense notion of “cause”. This revision seems solely based on the characteristics of Woodward’s own theory, and as a result, it is arbitrary:

A manipulability account implies that for something to be a cause we must be able to say what it would be like to change or manipulate it. This in turn suggests (as we have been assuming) that within a manipulability framework it is most natural or perspicuous to think of causes and effects not as properties, but as variables, or more precisely, as changes in the values of variables, where one of the characteristics of a variable is that it is capable of taking two or more values and of being changed from one of these values to another. [...]

Moreover, even with respect to variables that can take more than one value, the notion of an intervention will not be well-defined if there is no well-defined notion of changing the values of that variable. [...]

This restriction on the notion of an intervention to variables for which there is a well-defined notion of change is both implicit in the notion of an intervention itself and also follows from our guiding idea that causal relations are relations that can be used for manipulation and control. If there is no well-defined notion of changing the value of X, we cannot, even in principle, manipulate some other

variable by changing X. (Woodward, 2003, p. 112–113, emphasis added)

Woodward further explicitly argued against the distinction made by Ned Hall (2004) between causation as dependence and causation as production.² Woodward hereby refers to the example of Hall, presented in section 2 of this paper, in which Suzy manages to carry out a bombing because Billy, piloting as her escort, shot down an enemy plane that would otherwise have shot down Suzy’s plane. Woodward argues:

Hall’s own diagnosis of this example is that we operate with two distinct concepts of causation, one of which (“dependence”) involves counterfactual dependence but does not require a spatiotemporally continuous process connecting cause and effect, and the other of which (“production”) at least usually requires such a process but not counterfactual dependence. Billy’s firing is a cause of the bombing in the dependence sense but not in the production sense.

My own view is that we should resist this particular proliferation of “concepts” of causation and that, as the manipulationist account suggests, Billy’s firing is straightforwardly a cause of the bombing. (Woodward, 2003, p. 225)

We think that it does no justice to our causal intuitions to claim that Billy’s firing (and other omission causes in similar examples) will straightforwardly be taken as a cause, just in the same sense as a production cause (in this case, Suzy’s actions) will. One will be inclined to hesitate whether or not to label Billy’s intervention as a cause, and one person may conclude that it is a cause, while another may conclude that it is not. Hall’s approach does justice to these inconsistencies in our causal intuitions, by indicating that Billy’s firing is a cause in one sense, but not in another. Woodward’s approach does not seem to do justice to our causal intuitions, and his arguments for this revision of Hall’s theory are rather superficial. He refers to the following example from biology to strengthen his point of view:

As an illustration, consider the lac operon model for E. coli due to Jacob and Monod, which was widely regarded as a seminal discovery in molecular genetics. When lactose is present in its environment, E. coli produces enzymes that metabolize it, but when lactose is absent, these enzymes are not produced. What determines

² Woodward uses an unpublished draft version of Hall’s paper, which was published one year after Woodward’s book.

whether these enzymes are produced? According to the model proposed by Jacob and Monod, there are three structural genes that code for the enzymes as well as an operator region that controls the access of RNA polymerase to the structural genes. In the absence of lactose, a regulatory gene is active which produces a repressor protein which binds to the operator for the structural genes, thus preventing transcription. In the presence of lactose, allolactose, an isomer formed from lactose, binds to the repressor, inactivating it and thereby preventing it from repressing the operator, so that transcription proceeds. Biologists describe this as a case of “negative control”. Unlike “positive control,” in which “an inducer interacts directly with the genome to switch transcription on” (Griffiths, Miller, Suzuki, Lewontin, and Gelbart 1996, p. 550), the inducer in this case, allolactose, initiates transcription by interfering with the operation of an agent that prevents transcription. [...] A causal relationship is clearly present between the presence of allolactose and the production of the enzymes, and the former figures in the explanation of the latter, but there is no transfer of energy from, or spatiotemporally continuous process linking, the two. (Woodward, 2003, p. 225–226)

Following Hall, allolactose would be a cause in the dependence sense, but not in the production sense. In contrast, Woodward defends the conclusion of his manipulationist account following which allolactose should be straightforwardly taken as a cause, without making any distinctions with regard to those inducers involved in what biologists refer to as “positive control”. Woodward argues thus that biologists also seem to refer to both of these causes as just straightforward causes, and this convinces him that one does not need Hall’s distinction. We think that Woodward is too fast in making this conclusion. In fact, Woodward’s example conflicts with his own conclusion, since biologists do make a distinction. Although they refer to both kinds of influence of inducers as “causes”, they introduce the labels “positive control” and “negative control” to discriminate between both. These labels clearly refer to two discernable ways of causing the transcription. Consequently, also this revision of our causal intuitions seems to be based on the characteristics of Woodward’s own theory, and his belief in it, rather than on convincing arguments.

A counterfactual theory of the sort outlined above is the obvious candidate for capturing this relationship. Allolactose induces or causes production of the enzymes because production would occur when it is introduced via a

properly designed experimental manipulation and would not occur in its absence, again assuming a properly designed experiment in which some other inducer is not present.

4.2. Woodward’s aim is to give a revisionist conceptual analysis:

My project is certainly different in its results from the kind of empirical analysis executed by Dowe. (I leave it to the reader to decide whether it counts as discovering “what causation is.”) But although a significant portion of what I attempt does involve a description of ordinary and scientific usage and judgment, my project goes well beyond this — it is not just “conceptual analysis,” in the sense described above [by Dowe]³. First, my focus is not just on how people use words, but on larger practices of causal inference and explanation in scientific and nonscientific contexts, practices that involve substantial non-verbal components. Second, one of my aims is to make distinctions among different sorts of causal and explanatory claims, distinctions that are often overlooked by those who make such claims. This is not just a matter of describing universally accepted uses. A third and more fundamental difference between my project and conceptual analysis, as conceived above, is that my project focuses on (what I take to be) the purposes or goals behind our practices involving causal and explanatory claims; it is concerned with the underlying point of our practices. Relatedly, my project has a significant *revisionary* or *normative* component: it makes *recommendations* about what one ought to mean by various causal and explanatory claims, rather than just attempting to describe how we use those claims. It recognizes that causal and explanatory claims sometimes are confused, unclear, and ambiguous and suggests how these limitations might be addressed. (Woodward, 2003, p. 7)

[A]lthough fit with (and illumination of) generally accepted judgments and practice, both in ordinary life and in science, is an important constraint, it does not follow, for the reasons described above, that our only goal should be the description of how ordinary folk (or experts) use words like “cause” and “explanation”. (Woodward, 2003, p. 8)

³ Woodward means here: descriptive conceptual analysis.

Woodward's aim is clear. In that respect he scores better than Dowe. But from a revisionist point of view, his argumentation strategy is unconvincing. The revisions which Woodward proposes are arbitrary. As is clear from the examples given in 4.1., he gives no reasons why some intuitions must be respected while others must be changed. Woodward only gives a general justification for a revisionist attitude: ordinary talk is sometimes ambiguous and confused. But that is not enough: a revisionist who wants to revise our causal talk in a completely different direction (like Dowe) can also claim that he solves ambiguity and confusion. Arguments for specific revisions are missing.

5. *The future of conceptual analysis of causation*

A first lesson we can draw is that conceptual analysts should always set themselves a clear aim (descriptive or revisionist analysis) and make that aim explicit in their writings. Then they should adopt an appropriate argumentation strategy, one that fits their aim. In 5.1. and 5.2. we discuss what this implies for respectively the descriptive and revisionist variant of conceptual analysis.

5.1. In order to discuss how descriptive conceptual analysts can and should proceed, we summarise Hall's theory as follows:

Hall dualism

- (1) In each case where we intuitively say that C causes E, there is a counterfactual dependence relation or a production relation between C and E.
- (2) In most cases where we intuitively say that C causes E, there is both a counterfactual dependence relation and a production relation between C and E.
- (3a) There are cases in which we intuitively say that C causes E, though there is only a counterfactual dependence relation (and no production relation).
- (3b) There are cases in which we intuitively say that C causes E, though there is only a production relation (and no counterfactual dependence relation).
- (4) There is no overarching concept available which can adequately account for (1)–(3b).
- (5) If there is a counterfactual dependence relation or a production relation between C and E, we intuitively say that C causes E.
- (Conclusion) The best conceptual analysis of causation is: "C causes E if and only if [E counterfactually depends on C] or [there is a production relation between C and E]."

A descriptive conceptual analyst cannot discard the examples supporting (3a) and (3b). Nevertheless, *Hall dualism* can still be opposed in three ways.

First, one can argue that it does not go far enough, because clause (1) is incorrect. A counterexample to Hall's approach that might be used by someone who takes this road is the example referred to in section 2.2. in which Suzy is piloting a bomber, Billy is escorting her in a first fighter plane, and a second escort is ready to help Suzy with a second fighter plane.

Second, one may argue that (5) is not correct. Longworth (2006) gives criticism of this type. One of his examples is this:

Automobile Accident: Billy is driving steadily down a deserted highway when suddenly, without warning, a truck ploughs into the side of his car. It is later revealed that the driver of the truck was heavily intoxicated and had run a red light. (p. 62)

Because we intuitively would say that Billy's driving steadily down the deserted highway is not a cause of the accident, this example — according to Longworth — demonstrates that Hall's production and dependence are *not even jointly sufficient* for causation. We need an extra criterion.

These criticisms do not lead to a radically new theory. The first criticism can be coped with by adding a third relation, resulting in a threefold instead of a dual definition. The second criticism can be coped with by redefining the causal relations distinguished by Hall. One might then, for example, introduce an additional criterion in order to distinguish dependence and production relations that correspond to a causal relation from the other ones. In both cases, the resulting theory fits the following general scheme:

Hall-type pluralism

- (1) In each case where we intuitively say that C causes E, one of the relations A_1, \dots, A_n is present.
- (2) In most cases where we intuitively say that C causes E, all the relations A_1, \dots, A_n are present.
- (3) For every A_i there are cases in which we intuitively say that C causes E, although this relation is not present (while the others are).
- (4) There is no overarching concept available which can adequately account for (1)–(3).
- (5) If one of the relations A_1, \dots, A_n is present between C and E, we intuitively say that C causes E.
- (Conclusion) The best conceptual analysis of causation is: "C causes E if and only if at least one of the relations A_1, \dots, A_n is present."

A_1, \dots, A_n refer to possible relations between C and E as specified by the corresponding causal concepts, each of these concepts being a specific collection of criteria providing a description of the kind of causal relation involved. Hall's conceptual analysis is a dualistic instance of this scheme, with two relations. The two criticisms mentioned above can lead to more complex instantiations of this scheme, but in no way they lead us to a monist conceptual analysis.

A monist has only one option: he has to argue that clause (4) is incorrect. This is difficult, but not impossible. The difficulty is to avoid to become too vague and uninformative. Let us substantiate this claim by considering how a monist could reply to Hall. It is possible that the two relations, production and dependence, have something in common. Hall seems to assume that in both cases, the causal factor should precede the effect factor, which excludes backward causation. However, this criterion does not suffice as a specification of what causation is, since there are much more relations in which one factor precedes another without necessarily causing it (a typical example of which is the succession of the rising of the sun and the moon; more generally: all non-simultaneous effects of a common cause). Hence, one will need to search for additional criteria to define the overarching concept that the monist needs. Simply taking "the greatest common divisor" of production and dependence will not do, because then we incorporate non-causes as well. To unify the two kinds of causation discerned by Hall in a univocal concept, and meanwhile keeping out non-causes, seems not easy at all. However, although it is not straightforward to find such a monist causal concept, the possibility that it is found cannot be excluded.

5.2. Revisionist conceptual analysts face a completely different problem: theory choice. They should try to develop arguments supporting their specific revisions. A possible route is the epistemological one: one could try to argue that there are epistemological differences. That is the only strategy that we could think of. This illustrates that the problem is a tough one: we don't think Dowe and Woodward were lazy revisionists. It is just very difficult to find a good argument for a specific revision. Maybe this means that revisionist conceptual analysis must be given up, because it is bound to be arbitrary.

6. Conclusion

We have argued that here are serious methodological problems with the way Phil Dowe and Jim Woodward defend their monistic theories. The dualist Ned Hall scores better from a methodological point of view. However, this does not entail that Hall is right. But progress in the conceptual analysis

of causation is only possible if the debate continues in a methodologically correct way, as described in Section 5.

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