

Comments on:

Gerd Graßhoff, Inferences of Causal Relevance from Experiments

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I have three kinds of comments: praise, constructive doubts, and philosophical doubts. Let me give them in that order:

1. Praise:

As will become clear later on, my attitude towards the regularity theory of causation is ambiguous. Thereby, I am already more sympathetic to it than most of my colleagues. I guess it is fair to say that it is common opinion that the regularity theory of causation is hopeless. The problems it has are overwhelming, if not unsolvable, and other accounts do much better on those problems. There do not seem to be good reasons to continue defending the regularity theory. Agreements are rare in philosophy, but here we seem to have one. Well, unfortunately most agreements in philosophy are premature. Even among us rational philosophers they come about through exhaustion rather than argument. Therefore, I think we really have to be grateful to Gerd Graßhoff and his former collaborators Michael May and Michael Baumgartner that they stir up our complacency and bravely fight for an apparently lost case. It is time to check the reasons speaking against the regularity theory; at the least, Gerd Graßhoff and his collaborators have shown that those reasons are not so telling as they seemed. Indeed, I think Gerd Graßhoff is right in a way: the regularity theory should not be so easily dismissed. However, as I try to sketch later, my reasons for saying so are different.

2. Constructive Doubts

First, I should say that I find the case study sketched by Gerd Graßhoff very impressive; I assume there are many more of a similar kind. They are important because they forcefully connect up philosophy of science with actual science. As to the philosophy, I first have some questions concerning the constructive task of the regularity theory. They are only questions, and may well show that I have not sufficiently studied the constructions.

First, I suspect that the algorithm for inferring causal relevance grows exponentially with the number of variables or factors to be taken into account. Computer scientists don't like such algorithms. So, my first question is: Is my suspicion correct, and if so, is there something to be done about it?

Second, within the regularity theory the statement of causal laws is closely connected with irreducible disjunctive normal forms. The law has the form: "whenever a situation of type D obtains, an effect of type E occurs", or for short: "if D , then E ". There, the description D is in some sense minimal or optimal. Then we look at the irreducible disjunction normal form of D . Each disjunct, then, forms a complex or total cause the conjuncts of which are single or contributory causes. The disjuncts may obtain alternatively, or some of them may obtain jointly, in which case they overdetermine the effect E .

Now, my problem is that the irreducible disjunctive normal form is not unique. Quine gave a relevant example already in 1952. Let

$$D \leftrightarrow (A \wedge \bar{B}) \vee (\bar{A} \wedge B) \vee (B \wedge \bar{C}) \vee (\bar{B} \wedge C).$$

Obviously, D has irreducible disjunctive normal form. There are two others, though:

$$D \leftrightarrow (A \wedge \bar{B}) \vee (B \wedge \bar{C}) \vee (\bar{A} \wedge C), \text{ and}$$

$$D \leftrightarrow (\bar{A} \wedge B) \vee (\bar{B} \wedge C) \vee (A \wedge \bar{C}).$$

Now, when the law "if D , then E " holds, what are the complex or total causes of E ? Is there a way for the regularity theory to distinguish among the equivalent irreducible descriptions?

Third, Gerd Graßhoff spoke, in way, only about one causal step: "if D , then E ", and he implicitly suggested that this is a step of direct causation. However, we have many causal regularities, and they entail many more causal regularities; or rather, we have a huge set of causal regularities, and there is *prima facie* no saying which of them are basic and which of them derived. Since the basic causal regularities presumably are the direct ones, the question hence is: How does the regularity theorist distinguish between direct and indirect causation? Similarly, the many causal regularities entail even more correlational regularities that can at best be understood in terms of common causes. So, again, the question is: How does

the regularity theorist distinguish between causal and entailed correlational regularities? Or, more specifically, how does he distinguish between causal chains and causal forks? I had a dispute with Gerd Graßhoff about the issue; he remained unconvinced, and I remained dissatisfied. I know that Michael Baumgartner has sophisticated constructive answers to these questions. My own efforts indicate that positive constructive answers can indeed succeed. So, these questions are not the death bell for the regularity theory. However, they can succeed only under strong presuppositions, about the relations between time and causation, about the logical form of causal laws, and more. This issue is unresolved, but at least there is issue; that's something positive.

I wonder whether Gerd Graßhoff likes to solve these problems by implicitly alluding to an interventionistic framework. When A , B , and C are arranged as a causal chain, intervening at B makes a difference for C . However, when B and C are common causes of A , thus forming a causal fork, intervening at B does not make any difference for C at all. I assume that an interventionistic picture would be equally compatible with the historical case studies. It seems, however, that the regularity theorist cannot adopt it without giving up the essence of his theory.

3. Philosophical Doubts

The previous remark already leads over to my last point I want to make. The regularity theorist cannot really refer to David Hume as his principal witness. Hume was peculiarly ambiguous. He held both, a regularity and an associationist theory of causation; recall his famous, or infamous, two definitions of causation. In her book *Hume on Causation* Helen Beebe made it intelligible, better than any other book I know, why Hume took his two definitions to be two versions of the same basic idea. Still, for me there is all the difference in the world. The regularity theory is objectivistic, but it simply denies the modal force of causal claims, whereas the associationist theory is subjectivistic, but has an account of that modal force, even if it reduces causal necessity to a mental determination or habit of thought, from which objective modalities can perhaps be reconstructed via some projectivist strategy. It may be open whether this is a convincing account of the modal force of causal claims; counterfactual analyses certainly have a different, more objective picture. My worry is that the pure regularity theorist as such has no picture at all.

So, I wonder where Gerd Graßhoff stands on this issue. If he is a pure regularity theorist, my worry extends to him as well. One may hope to distinguish between laws and mere regularities and say that the notion of a law carries the relevant modal force. Then, however, we sink into the substitute problem of lawlikeness, with little or no hope that it might be more easily solvable. If he should have interventionistic inclinations, then he is in effect changing to a kind of counterfactual analysis. Jim Woodward often emphasizes that he is alluding to counterfactuals only when they are intuitively clearly true or false, but he is strangely silent on how counterfactuals really acquire truth values. One may well doubt whether this is the right way of accounting for the modal force of causal claims.

My way, in any case, is different. I prefer to thoroughly explicate Hume's subjective associationist account of causation in terms of ranking theory, the only theory that specifies a full dynamics of belief, to which Hume's account so crucially refers. The basic idea of the ranking theoretic account is simple and straightforward, its elaboration entails some familiar complexities. In any case, there is no danger to confuse it with a regularity theory. Its great mystery, that was devastatingly objected to Hume's associationist account as well, is its subjectivism. How can the causal relations be relative to our beliefs, to our state of mind? To pose the question is to see its apparent absurdity. I respond with an objectification theory supplementing my subjective ranking theoretic account of causation. The result is, in a way, a regularity of causation. The point, however, is that I do not start with a regularity theory. I justify it, in a way; I have a vantage point from which I can derive various details of the regularity theory (which would otherwise fall from the sky), and from which I can specify the non-trivial presuppositions required for that derivation.

So, to resume, I am full of sympathies for the regularity theory; in a way, I even endorse it. However, I do not take it as the basic theory of causation. Rather, I propose to derive it as an objectified projection of the basic subjectivistic account in terms of ranking theory. In all that, I feel to be a faithful scholar of David Hume.