Author's response to reviews

Title: Causal inference based on counterfactuals

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Author's response to reviews: see over
Dear Drs Newmark and Temper,

Thanks a lot for sending me these two excellent reviews; I am most grateful for the comments of both referees. The revised version addresses all of their suggestions. Please find my responses to all their commentaries below.

Sincerely,
Michael Höfler

Reviewer I

The reviews of Hernan MA in J Epidemiol Community and Winship C & Morgan S in Annual Review of Sociology should cited.
The paper of Hernan is cited on page 6 [ref. 14]. I have generalized the definition of a causal effect as done in Hernan’s paper – according to another suggestion by this reviewer (page 6, see below). The paper of Winship C & Morgan S is now been cited [ref. 35] in the paragraph on instrumental variables (p. 19) and in the first point of the summary where I argue that the counterfactual model demonstrates the limitations of observational data (p. 31).

The paper requires language editing
The whole paper has been proofread by a certified translator and several formulations have been corrected.

In the abstract “more and more” should be replaced with “increasingly”.
“More and more” was be replaced with “increasingly” (abstract).

Heckman argued that non-maniputable variables like age and gender should not be excluded from causal inference.
I have cited the forthcoming paper of Heckman [ref. 10] as well as one of Susser and Schwartz [ref. 11] for conflicting opinions on this issue on page 5.

The difference was privileged in the definition of a causal effect.
Right, I decided to use the simple notation as done e.g. in Hernan (2004): a counterfactual effect is present if $Y_{i,t} \neq Y_{i,c}$, that is, if the outcome in an individual $i$ is different across the two conditions. Throughout the paper the difference e.g. between observing $X = c$ and Pearl’s SET($X = c$) is expressed verbally as done in most other reviews. Now, after the more general definition of a causal effect, I mention that the magnitude of a causal effect can be defined in various ways (p. 6).

*The discussion of causal order is awkward by the avoidance of causal diagrams* (p. 8).
I have extended the wording on page 8 to clarify my argument and I’ve added a causal graph for a very simple example of an indirect effect (Figure 1).

*The example of ballistic evidence is uncertain* (p. 9):
I have mentioned the possibility that the person could have died of sudden coronary failure at the moment the bullet was fired in brackets (p. 9).

*The discussion of target population is vague, no reference is given, and only selection bias is mentioned here* (p. 9).
I have extended the definition of the target population now by differentiating between target and source population. I have cited Rothman and Greenland (1998) in this respect and mentioned that the definition of different biases (e.g. confounding) depends on the choice of the target population (p. 10).

*Cite Halloran & Struchiner (1995) instead of old references 8 and 13 for violation of the stable unit treatment assumption* (p. 9).


*It is not necessary that an effect modifier is a causal antecedent, genes that act together when causing an outcome are causally contemporaneous* (p. 11).
Indeed antecedence is not required in most definitions of causal synergy (e.g. in the textbook of Rothman & Greenland). Therefore I have dropped this requirement (pp. 9 and 13).
The condition for causal interpretation of the OR is not quite correct (p. 10).
I don’t understand this point exactly. In the paper Greenland (AJE, 1987) the most general case is examined where there might be different risks and different ORs for each individual. Here, the interpretation of the summary OR as the mean over the individual ORs becomes increasingly false with an increasing number of individuals with a high risk for the outcome (under one or both conditions). A central point of that paper is the independent of which strata are used to aggregate the individuals in a particular model or analysis. However, I have changed the wording to make this point clearer (p. 10).

Reference 20 is not appropriate for causal synergy.
It is, in the appendix of the paper of Greenland (1993) the claims made in this paper are shown. However, I have now added the citations Rothman & Greenland (ch. 18) and Greenland & Poole (1988, ref [26]; pp. 14-15).

Mention Holland (2001) for a conflicting opinion on interactions with intrinsic variables.
I have added the sentence “On the other hand nonmanipulable properties are hardly subject to counterfactual considerations” and cited Holland (2001) (ref. [9], p. 16). I’ve also cited this paper on page 5 where causal effects of immutable properties are discussed.

A confounder need to to precede exposure (p. 19).
I have corrected that the confounder must not be affected by either X or Y (pp. 19-20).

Pearl’s backdoor criterion was incompletely and falsely described (p. 20).
I have now mentioned the frontdoor criterion as well. Explaining these criteria appropriately, however, would have required much more space. Therefore, I have decided just to mention these criteria and that their application requires assumptions about the causal system that causes X and Y (pp. 21-22). The false claim that all confounders had to be known was deleted.

A high association does not indicate that causality is more likely (p. 21).
This statement was indeed misleading. I changed the sentence to: “In conventional analyses, the farther the left boundary is from the null, the more room there is for bias and extra-variation (p. 22)”. Rubin is no longer cited in that respect.
Pearl did not extend g-estimation but rather g-computation and developed the methods independently from Robins (p. 23).
I have deleted that sentence (which is not essential, p. 23).

Pages 24-26 are only a synopsis of ref. [41], the work of Greenland and Pearl from the late 1980s should be integrated here.
I have shortened that paragraph and integrated some other papers of Greenland, Robins and Pearl. (pp. 25-27).

On page 28 citation [23] appeared twice.
This was a mistake. I had forgotten to reference Greenland, Pearl & Robins (1999, old ref. 30) and this reference appeared twice.

The synopsis of the replies to Dawid’s paper lack a synthesis, critique or development (pp. 29-20).
I have shortened this section now and I’ve added a new synopsis (pp. 29-31).

General comment: The paper is too vague and should use a narrower scope.
As mentioned in my answers above I have omitted or shortened some issues while other topics were explained in more detail. However, the aim of this paper is still to present a broad overview on counterfactual causality and I hope that the residual impreciseness is acceptable.

This reviewer pointed out that extensive language editing wouuld be necessary. This has been done now.

Reviewer II

p. 17, sentence wiss a missing word.
That was just a typo: “it is” was mistyped as “is it”. This was corrected now (p. 17).
p. 17: The paragraph on randomisation detracts from the overall theme and should be deleted.
I followed this suggestion and deleted this paragraph because it is not essential and more explanation would be necessary to make it really useful (p. 17).

p. 18, delete or better integrate the paragraph on instrumental variables.
I decided not to delete that paragraph because it provides some basic information on an important method to analyse broken experiments. However, I improved the wording so that it now better fits to the paragraph before (pp. 18-19).