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Commentary

# Causal inferences: Identical twins help and clarity about necessary assumptions is critical

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We welcome Boardman and Fletcher's (BF) comments and agree with their emphasis on the importance of clarifying assumptions. Understanding advances through careful consideration of the assumptions underlying possible causal inferences.

No empirical method is assumption-free. Randomized controlled trails (RCTs) are thought to be gold standards, but have limitations (e.g., Deaton, 2010). RCTs of schooling do not exist and would be challenging and perhaps unethical. RCTs of schooling determinants exist, but they permit obtaining estimates of causal impacts of schooling only under assumptions that such factors work only through schooling even though there may be, e.g., effects through resources or attitudes. Instrumental variable (IV) estimates generally rely on untested exclusion assumptions that identifying instruments only work through variables being instrumented (e.g., schooling) and do not affect dependent variables (e.g., health) through unobserved channels, though often there are plausible unobserved channels through which these variables might work. Monozygotic (MZ, identical) fixed-effects (FE) estimates rely on within-twins pair schooling variation that is assumed uncorrelated with unobserved within-twins pair factors that also directly affect health. Many strategies to obtain causal estimates rely on specific functional form assumptions.

We agree with several of BF's points about MZ-FE estimates: (1) the equal environment assumption, critical for ACE/heritability estimates, is irrelevant for MZ-FE estimates, (2) classical measurement error has minor implications for MZ-FE estimates that can be remedied by reports from others (Ashenfelter and Krueger, 1994; Behrman et al., 1994), and (3) risks of type II errors increase with reductions in sample size.

We also think that some of BF's concerns are less important than they suggest because:

(1) MZ-FE estimates control for first-order sample selection effects that otherwise might raise questions about representativeness and external validity, whether due to concordance on schooling (BF Concern 1, first paragraph), incomplete follow-up (BF Concern 1, last paragraph) or growing up in families with twins being different than growing up in families with only singletons (BF Concern 3, last paragraph). Comparisons of twins data sets with socioeconomic and demographic data on larger populations, moreover, often find no significant nor substantial differences for adults (e.g., Behrman et al., 1980, 1994, 1996; Kohler et al., 2002). BF's Concern 1 about selection of discordant twin pairs, furthermore, is overstated because it is based on incorrect inference.[1]
(2) *Within-twins schooling variations are relevant:* While within-twins pair schooling differences are not due to policies such

[1] BF calculate that the schooling means for discordant MZ MIDUS pairs are significantly higher than for concordant pairs. This would imply selection for the discordant pairs that are central for the within-MZ estimates *if* the schooling distribution were symmetrical. However the schooling distribution for MZ MIDUS twins is right-skewed, which implies that, with no selection, discordant pairs are likely to have higher means than concordant pairs. Therefore it does not follow, with the actual MZ MIDUS right-skewed schooling distribution, that significantly higher means for discordant than for concordant pairs necessarily implies that the discordant pairs are selected.

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as compulsory schooling laws (BF paragraph three), the variation itself is important for policy decisions. Within-twins pair schooling differences are distributed throughout schooling distributions, allowing for examination of health differences between twins with differences in e.g., high school vs. college degrees, which is important for consideration of schooling policies that increase access to higher education. Current IV estimates based on compulsory-schooling-policy-induced variation, such as those cited in BF, in contrast, do not yield insights about health returns to higher levels of schooling.

(3) MZ-FE estimates theoretically and empirically may be greater or smaller than cross-sectional estimates (Behrman et al., 2015; Lundborg, 2013) in contrast to the often reduced absolute values of effects of schooling on health that BF emphasize (second paragraph).

(4) *MZ-FE estimates still may be useful even if* unobserved causes of within-twins pair schooling variation are correlated with unobserved causes of within-twins pair health variations (BF paragraph 3, Bound and Solon, 1999): MZ-FE estimates give upper (lower) bounds on absolute values of true effects if correlations between the disturbances are positive (negative), and such bounds are useful in assessing what are possible true effects (Behrman et al., 2011; Kohler et al., 2011).[2] If the primary concern is that schooling differences are due to factors (e.g., motivation, discount rates), for example, that affect health in the same direction when the true causal effect is believed to be positive, null results from adequately-powered twin samples are strong evidence against positive causal effects (e.g., Amin et al., 2015; Behrman et al., 2011). Sensitivity of MZ-FE estimates can also be explored by including additional variables ("instruments") that affect schooling, and health only through schooling (Kohler et al., 2011).

(5) Epigenetic interactions with environments after conception do not bias MZ-FE estimates if such interactions only directly affect schooling or health (BF Concern 2). If epigenetic–environmental interactions affect schooling and health in the same (opposite) direction, as for point 4, MZ-FE estimates still yield useful bounds of true causal effects.

(6) Twins responding to co-twins' schooling symmetrically in making their schooling decisions does NOT bias estimated schooling impacts (Kohler et al., 2011) in contrast to BF's Concern 3. MZ-FE estimates are biased, however, if one twin responds to the co-twin's health, which causes downward (upward) biases in estimated schooling impacts if responses are positive (negative), again establishing useful bounds on true effects (Kohler et al., 2011).

## Summary

Thus some alleged problems with twins estimates are not problems for MZ-FE estimates (e.g., the equal environment assumption, first-order selection effects, twins responding symmetrically to co-twins' schooling, epigenetic interactions that directly affect only schooling or only health) and others may bias estimates but still establish bounds that are useful and policy-relevant (e.g., individual environments – perhaps with epigenetic interactions – that affect both schooling and health, twins responding in their own health to their co-twin's health). We emphasize, therefore, as BF suggest, MZ-FE estimates are one important tool in our toolkit for estimating important relations including schooling impacts on health, but that it is critical to be clear about under what assumptions these estimates justify inference of causality or perhaps bounds on causal estimates just as it is important for other approaches. We also conclude that several of BF's key concerns about the validity of within-MZ twins pair estimates for causal inference are less important or more qualified than their commentary suggests.

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[2] A related, but separate issue is the comparison between MZ-FE and OLS of the bounds for the true effect that can be obtained for each of the estimates; see Li et al. (2012) for a discussion.