Response to Invited Commentary

The Authors Respond to “Structural Equation Models and Epidemiologic Analysis”

Anna Arlinghaus*, David A. Lombardi, Joanna L. Willetts, Simon Folkard, and David C. Christiani

* Correspondence to Dr. Anna Arlinghaus, Gesellschaft für Arbeits-, Wirtschafts- und Organisationpsychologische Forschung e.V., Achterdiek 50, 26131 Oldenburg, Germany (e-mail: anna.arlinghaus@gawo-ev.de).

Dr. VanderWeele has provided an insightful commentary (1) on our article (2) on the use of structural equation modeling (SEM) as a tool for epidemiologic analysis. In his commentary, he illustrates the degree to which strong assumptions are made when conducting SEM analyses. For example, as described for regression analysis, linearity assumptions are made across all relations with the outcome variable. Dr. VanderWeele gives alternative methods to SEM for causal and mediation analysis and argues that in light of these wider-ranging assumptions, SEM should primarily be used for exploratory analysis and hypothesis generation when there are a broad number of effects to be investigated (1). While we agree that it is important to weigh the benefits of SEM against the limitations posed by broad assumptions, there is general consensus that an underlying theory about mechanisms is required, and ideally a model should be specified before the data are collected (3–5). VanderWeele states that oftentimes models are constructed on the basis of what researchers feel are important pathways and recommends including more pathways rather than fewer, leading to conservative control for confounding (1). Parsimony requires the simplest model that fits the a priori theory, given that alternative models have an equal fit (3). While we acknowledge the need to control for confounding, commonly several models are in fact tested concurrently using SEM to critically evaluate the alternative models and to avoid the pitfall of simply confirming the researcher’s preferred model. However, as Factor-Litvak and Sher (4) pointed out in their commentary on the use of SEM in epidemiology, although an a priori theory is needed, the results of a confirmatory SEM analysis are unlikely to produce a completely accurate picture of reality.

Although any statistical model makes theoretical assumptions, exploratory analyses conducted prior to the final model specification are useful for testing the influence of potentially confounding variables or conducting sensitivity analyses. However, confounding by unmeasured variables can never be ruled out completely with any modeling method. Variables that are found to be unrelated to the outcome or that do not significantly change the parameter estimates, such as the effect of physical activity on the risk of an occupational injury in our study (2), do not need to be included in the final model. From a practical standpoint, the number of parameters is also limited by the number of observations available for the analysis (i.e., the number of entries in the sample covariance matrix in lower diagonal form) (3). Thus, including too many parameters (e.g., correlations and regressions) can lead to a nonidentified model which is not amenable to empirical data analysis (3). Another practical challenge (not unique to SEM) is the use of existing data or collection of data in the field, in which typically not all variables of interest are measured.

In conclusion, we believe that SEM is a valuable method with which to evaluate complex mechanisms, including relations between covariates, latent variables, and more than one indirect effect. However, we also believe that a statistical model is not always a perfect picture of reality and needs constant refinement. Further research on SEM is clearly needed with regard to mediation and sensitivity analysis (5). Nevertheless, SEM can give a more nuanced and global overview about relations between various exposures, outcomes, and mediating variables than many other methods, can serve as a tool for cross-validating results from different studies using similar constructs but not the same measurements (6), and can inform practical implications, as we noted in our study for injury prevention strategies (2).

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Author affiliations: Gesellschaft für Arbeits-, Wirtschafts- und Organisationpsychologische Forschung e.V., Oldenburg, Germany (Anna Arlinghaus); Department of Environmental Health, Harvard School of Public Health, Boston, Massachusetts (Anna Arlinghaus, David A. Lombardi, David C. Christiani); Center for Injury Epidemiology,
REFERENCES


