THE AUTHORS REPLY

In their letter to the editor, Goodman and Capitman (1) take issue with our characterization (2) of their research. Whenever we discuss the work of others, we hope to get it right. In this case, we believe that a dispassionate reader will find Goodman and Capitman’s complaints to be unjustified.

Goodman and Capitman (1) begin by taking issue with the word “inappropriate,” claiming that we used this word to describe their carefully conceived analytic strategy. What we wrote was, “if one is interested in ruling out the possibility that depression affects whether a teen takes up smoking, then it is inappropriate to control for potential indicators of depression” (2, p. 468). Obviously, this sentence hit a nerve, and therefore some elaboration is in order. We and Goodman and Capitman label a respondent as exhibiting high depressive symptomatology if he or she scores above a certain cutpoint on the Center for Epidemiologic Studies Depression (CES-D) Scale. A respondent’s CES-D score is based on answers to a series of questions. For example, respondents were asked whether “you felt that you were just as good as other people,” “you were happy,” “you enjoyed life,” and “you felt that people disliked you.” The control variables at issue are also based on self-reports, and they include “self-esteem,” “trouble relaxing,” “bad temper,” and parental perceptions of how “the teen’s life is going.” Clearly, these variables are correlated with the answers to the CES-D questions. Our concern is that this correlation occurs because both reflect the same underlying emotional state or condition. If so, then the Goodman and Capitman covariates are neither confounders nor mediators but are alternative indicators of this underlying condition. When multiple measures of the same condition are simultaneously included as explanatory variables in a regression, it should come as no surprise that the estimated effect of any single variable loses its statistical significance. Goodman and Capitman’s conclusion that “depression does not

Dr. Aldridge’s letter to the editor (5) on our article (2) confuses the magnitude of an estimate with its precision, misrepresents our discussion of association versus causation, and contains factual errors regarding fixed-effects models. She is correct in one respect: we find that most of the association between smoking and depressive symptomatology is reduced by 60–77 percent after controlling for unobservables. The next logistic regression includes fixed effects, which control for the baseline CES-D score as well as all other baseline characteristics, unobservable and otherwise. What we learn from this exercise is that the association between smoking and depressive symptomatology is reduced by 60–77 percent after controlling for time-invariant factors. What would happen to this association if we were to control for time-variant unobservables is a question we will leave for future research.

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REFERENCES


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