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In Reply

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David Reardon has questioned our methods and conclusions. He chastises us for “belatedly noting” that some abortions may be misreported, calls our decision to adjust for history of miscarriage in some of our models “unwise” and “unexplained,” suggests that the risks would be statistically significant if abortions were not concealed as miscarriages, and calls for us to publish a new analysis excluding women reporting miscarriages. Below, we address his concerns.

Reardon’s first reason for questioning our conclusions is that women in the childbirth group may have had an abortion but did not report it. We note this possibility in the Discussion.¹ He suggests that “hundreds of women” in the sample concealed abortions. Some undoubtedly did so, but the number does not appear to be substantial. We compared the proportion of women reporting an abortion in this sample with independent data on lifetime prevalence and found only a small difference.² More importantly, possible misclassification would not change the implications of our results. Our key finding is that abortion is related to mental health problems in unadjusted analyses but not when adjusted for prior mental health problems. Analyses with and without adjustment were done on the same women, so the degree of inaccuracy of classification is the same in all of the models. Reardon gives no explanation as to how he thinks inaccuracy could have led to the different results in the contrasting models.

Reardon’s second point involves our “unwise and unexplained decision to include miscarriage as a control variable in Models 3 and 4.” Doing so is hardly unwise and did not seem to require much explanation. It is standard practice to control for relevant variables whose prevalence differs between comparison groups and could lead to a spurious relationship. A careful reading of our findings shows that, although the prevalence is higher in the abortion than in the childbirth group, miscarriage does not appear to play much of a role in explaining mental health outcomes. In Model 2, in which only mental health history is controlled for, the hazard ratios for all except substance use disorders are reduced and become nonsignificant. Results from Model 2 are essentially the same as results from Model

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4, which also includes history of miscarriage, sociodemographics, and prior adversities in addition to mental health history. The finding that the hazard ratios shown in Model 3 (where we control for other covariates but not for mental health history) show little to no reduction from Model 1 supports the conclusion that prior mental health is what is accounting for the postpregnancy mental health differences between the abortion and childbirth groups. Reardon requests a new analysis excluding the women reporting a miscarriage. We conducted such an analysis, and, not surprisingly, the results are the same.

Reardon's third point is that Model 4 results might have been significant had we had a much larger sample. This is, of course, possible, but such observations have been criticized as noninformative because they can be made about any study reporting nonstatistically significant results.³ Moreover, with 250 women in the abortion group and 677 in the childbearing group, we had adequate power to find a meaningful effect. If one needs huge numbers of women to detect a difference, it suggests that the difference is not likely to be clinically meaningful.

Reardon also criticizes our limiting the analysis to first incidence of a mental health problem after abortion or childbirth. Doing so maximizes specificity in relation to abortion or childbirth as a contributing event. The longer the time after the reproductive event, the more other experiences will come into play and complicate the relationship.

Finally, Reardon declares that our analysis is incomplete and that it is necessary to measure the frequency, duration, and intensity of mental health problems. If this is the bar, no studies, including those cited in writings by Reardon himself, would be considered adequate. The data we used, which assess mental health disorders based on Diagnostic and Statistical Manual of Mental Disorders, 4th Edition, criteria, which incorporate duration and intensity criteria for clinical-level mental disorders, are the "gold standard" for measurement of mental health.

Before closing, we would like to put Reardon's critique of our article in context. Reardon founded and heads an organization devoted to documenting and publicizing the dangers of abortion, including "educating others about abortion's injustice, trauma, and risk to all involved..."⁴ The findings from our study pose a challenge to this mission and the organization's support of policies that require women be told that abortion increases their risk of mental health problems. Thus, it is understandable that he has questioned our methods and conclusions, and we welcome the opportunity to address his unsubstantiated criticisms.

Our findings support the preponderance of evidence on this topic,^{5,6} and we stand by our conclusion that, "After accounting for confounding factors, abortion was not a statistically significant predictor of subsequent anxiety, mood, impulse-control, and eating disorders or suicidal ideation."¹

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