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Publication Date

2023-12-01

DOI

10.1016/j.chiabu.2023.106442

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Peer reviewed



ELSEVIER

Contents lists available at [ScienceDirect](https://www.sciencedirect.com)

Child Abuse & Neglect

journal homepage: www.elsevier.com/locate/chiabuneg

Letter to the editor

A response to Evangelist et al., 2023

The Fragile Families and Child Wellbeing Study (FFCWS) has made critical scientific contributions. We have serious concerns, however, with the authors' use of its data in a recent article by [Evangelist et al. \(2023\)](#). We believe the analysis did not accurately measure child protection system (CPS) contact due to limitations in the self-reported data and attrition rates between 52 % and 57 %. We are also concerned that the authors drew causal conclusions despite their inability to adequately control for differences in child maltreatment occurrence between groups. In our view, the authors' findings suggest what research already has demonstrated: Child maltreatment has deleterious effects on children. Readers should carefully assess claims that observed differences are due to CPS contact.

1. Measurement

The FFCWS consists of a purposive 3:1 oversample of urban, largely unmarried mothers, yielding a disproportionately Black, Hispanic, and low-income sample ([Princeton University, n.d.](#)). Evangelist et al. cited prior analyses of the cumulative risks of CPS reports among all U.S. children ([Kim et al., 2017](#)). The Kim et al. study estimated CPS investigations by age 9 at 26.5 % for all U.S. children and 37.8 % for Black children. Meanwhile, Evangelist et al.'s caregiver-reported CPS contact rate was 15 % by age 9. Given the sample demographics, the CPS investigation rate for FFCWS is less than half what would be expected. Evangelist et al. also reported that 72 of 3595 children were placed in foster care by age 15, representing only 2.0 % of their sample. Cumulative foster care exposure by age 15 in the United States is 5.4 % for all children and 10.1 % for Black children ([Wildeman & Emanuel, 2014](#)). Again, given the demographics of the FFCWS sample, it seems unlikely that the cumulative placement rate would be less than 8 %–10 %, suggesting an erroneous estimation that could be greater than 75 %.

Large measurement errors of this kind are not without precedent when subject recall is used to estimate encounters with public systems ([Meyer & Mittag, 2019](#)). The Add Health Study ([Harris & Udry, n.d.](#)) found that only 4 % of young adults reported being contacted by CPS as children and only 1.5 % reported being in foster care. But the implications are serious in the context of the Evangelist et al. study. Either the authors' attempt to exclude children with foster care experience failed in most cases or their sample was unrepresentative, a possibility given that only 43–48 % of the original FFCWS sample was included. If more than half of the families with CPS contact were misclassified, as estimates from official maltreatment reports would suggest, then there are more children with CPS histories in the non-CPS group than in the CPS group. This could have systematically diluted the magnitude of the differences found, resulting in underestimates. Conversely, if only more consequential CPS contact was recalled or self-reported, differences between groups would be overestimated, because families with chronic or extensive CPS contact have worse outcomes ([Jonson-Reid et al., 2012](#)).

2. Causal inference

The purpose of the study was to determine if CPS contact has a unique toxic effect separate from any toxic effects caused by child maltreatment. This is no easy task. Identifying the “consequences of CPS contact” (p. 4) requires fully parsing the well-known and powerful negative impacts of child maltreatment, which can substantially increase negative outcomes. Even a very modest failure could account for the generally small effects found. Evangelist et al. used an advanced form of propensity score matching, which controls for observed factors to create groups that have a similar likelihood of a given outcome. The weakness of this approach is that unobserved factors can powerfully bias its estimates. The authors recognized that “observed differences in outcomes between children who had contact and those who did not could be attributable to CPS or another unobserved factor correlated with contact” (p. 5). Of course, the most obvious unobserved factor driving study findings is a child's experience of maltreatment.

As an analogy, it would be questionable if a researcher tried to understand the toxic effect of mental health service provision by controlling for factors predisposing someone to being referred to the mental health system, but not controlling for mental health per se.

<https://doi.org/10.1016/j.chiabu.2023.106442>

Received 7 August 2023; Received in revised form 28 August 2023; Accepted 30 August 2023

Available online 20 September 2023

0145-2134/© 2023 Published by Elsevier Ltd.

The research would likely show that individuals with mental health contacts had worse outcomes, attributable to underlying mental health conditions that were invisible to the researcher.

Evangelist et al. could not adequately control for children's exposure to maltreatment. There are no measures of parent-child interaction, few paternal measures, and no measures of cohabiting paramours over time. Measures used in the matching process were from age 0, yet presumed to fully control for actual maltreatment, which may have happened at any point during the next decade. The authors ran supplementary models, including a measure of child maltreatment based on parental self-report: The Parent Child Conflict Tactics Scale (CTSPC). The authors acknowledged that their use of the CTSPC is problematic, because the measure of maltreatment was captured at age 9, after any CPS contact. Timing aside, we know of no researchers who would argue that this scale can fully capture the presence of actual child maltreatment, which is a necessity of the Evangelist et al. study design. It is also important to note that the FFCWS omitted the seven questions that concern severe physical maltreatment (e.g., grabbed him/her around the neck and choked him/her; threw or knocked him/her down). Finally, given the significant underestimation of self-reported CPS contact, the reliability of any other self-reported maltreatment behaviors must also be questioned.

3. Conclusion

Although many studies suffer from some of the limitations we described, the Evangelist et al. study must reach an unusually high bar, given that the authors position their study as advancing knowledge of “the consequences of child welfare involvement” (p. 3) and argue that their model “mirrors the intuition” of randomized controlled trials (p. 5). It is generally understood that correlational studies have imperfect controls, cannot make causal claims, and are fundamentally distinct from randomized controlled trials. Yet the authors make causal claims that require complete or near-complete control of actual maltreatment occurrence between groups, which is simply not possible with the FFCWS data.

In summary significant underestimation of CPS contact and foster care placement have almost certainly resulted in group misclassification. The attrition of over half the participants is another issue to be considered. These dynamics, combined with a model that cannot rule out exposure to maltreatment per se, suggest that the conclusions drawn in this study are not reliable.

Data availability

No data was used for the research described in the article.

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