Adjusting for Underreporting of Child Protective Services Involvement in the Future of Families and Child Wellbeing Study and Assessing its Empirical Implications through Illustrative Analyses of Young Adult Disconnection

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Abstract

Child protective services (CPS) involvement is common among American families; over a third of all children and more than half of Black children experience at least one investigation by age 18 (Kim et al. 2017). However, studying the causes and consequences of CPS involvement is challenging due to data limitations. National data on children involved with CPS lack a counterfactual group, while state-level administrative data often miss key family and child factors. The Future of Families and Child Wellbeing Study provides detailed data on both CPS involvement and family and child factors but relies on self-reports which may underreport CPS involvement. In this paper, we develop a Bayesian method to correct for this underreporting and estimate the probability of CPS involvement by a given age. We then apply the adjusted measure in empirical analyses of young adult disconnection and compare the estimates to those produced using the unadjusted measure. Results suggest that CPS involvement may be associated with a higher likelihood of disconnection in young adulthood, particularly if that involvement occurred during early childhood. We find modest differences in the estimates produced by each measure, suggesting that empirical estimates based on unadjusted self-reports of CPS involvement may lead to somewhat erroneous substantive conclusions regarding relations of CPS involvement with potential precursors thereto and consequences thereof.

Introduction

Involvement with child protective services (CPS) is a pervasive experience for American children and families. Indeed, more than a third of all children and more than half of all Black children in the United States experience one or more CPS investigations by age 18 (Kim et al. 2017). Yet, identifying the causes and consequences of CPS involvement, and of child maltreatment itself, is complicated by substantial data limitations. National administrative data from CPS systems are limited only to children and families that have already become involved with CPS, thereby excluding a necessary counterfactual condition. Linked state-level administrative data systems, while offering greater opportunities for identifying such relations, are limited to data collected as a result of interactions with public systems and typically exclude detailed information on factors such as family functioning, parenting behaviors (including abusive and neglectful behaviors), and many key aspects of child health and development. While several large national panel surveys collect extensive data on family functioning, parenting behaviors, and child health and development, most lack data on CPS involvement.

The Future of (formerly Fragile) Families and Child Wellbeing Study (FFCWS) is an exception. FFCWS is a large, national birth cohort study that has followed nearly 5,000 children and their families since the children's births in 1998-2000. The study collects, at regular intervals, detailed data on parenting behaviors, child health and development, and self-reported CPS involvement, in addition to extensive social, demographic, contextual, biological, and economic data, most recently when the children, now young adults, were age 22. Parents', primary caregivers', and young adults' self-reports of CPS involvement, collected in multiple waves of FFCWS, have the potential to enable researchers to identify those who have experienced CPS involvement and compare precursors and outcomes for those who did and did

not. However, self-reports of CPS involvement may be biased by systematic reporting and measurement errors, including inconsistency in reporting over time and between parents and their young adult children, as well as systematically missing data at the item level or due to interview nonresponse or sample attrition. As such, relative to national estimates from administrative data, estimates of CPS involvement generated from FFCWS suggest substantial underreporting of CPS involvement. To the extent that such underreporting is systematic in nature, it has the potential to bias estimates of the causes and consequences of CPS involvement in studies thereof.

Our study has two aims. First, we develop and apply a Bayesian statistical method for adjusting parent and young adult self-reports of CPS involvement. We employ this method to estimate the cumulative prevalence of CPS involvement by ages 5, 9, 15, and 18 for each child in FFCWS. Our approach leverages national and state-level CPS-involvement prevalence estimates from administrative data to adjust for underreporting based on a priori expectations. Additionally, we incorporate child-level information to model the probability of CPS involvement. Second, we illustrate the utility of the adjusted CPS involvement measure by conducting empirical analyses and comparing estimates from the unadjusted and adjusted CPS involvement measures. Specifically, we examine associations between CPS involvement and an important young adult outcome—whether a young adult is neither working nor in school at age 22—which we refer to as disconnection. We estimate associations of both initial (unadjusted) self-reports of CPS involvement by age 18 and adjusted measures of CPS involvement by age 18 with young adults' probabilities of disconnection at age 22 and assess consistency and differences of the resulting estimates. The adjusted measures will be available to other scholars using FFCWS data for future analyses, thereby facilitating research both to further test the

adjusted measures and to conduct additional analyses of the causes and consequences of CPS involvement.

The importance and complexity of studying CPS Involvement

In the United States, CPS plays a crucial role in safeguarding children at-risk of or having already experienced child maltreatment by responding to allegations of abuse and neglect and, when deemed warranted, contacting parents or primary caregivers to investigate such allegations. CPS actions include screening reports and making determinations of whether an investigation is warranted, conducting investigations, making abuse and neglect determinations, providing mandated or voluntary support services, and, in severe cases, removing children from their homes or terminating parental rights (see, e.g., Berger and Slack 2020). In this study, we focus specifically on CPS investigations (referred to as assessments in some states; in this paper we adopt the term "CPS involvement"), defined as instances in which a family—including the parent, primary caregiver, or child—has been contacted by CPS, typically through home visitation to investigate alleged abuse or neglect. In our empirical analysis, we further distinguish between children who had CPS involvement and were removed from their homes and those who had CPS involvement but were never removed.

As noted above, research aiming to determine the causes and consequences of CPS involvement, as well as of child maltreatment itself, is severely constrained by limitations of existing data. Because it is not possible to randomize or experimentally evaluate CPS involvement (or most factors that lead thereto), researchers must rely on observational data to assess the precursors to and consequences of such involvement. They must then attempt to leverage such data to employ identification strategies that lend themselves to causal inference. However, even when such strategies are employed, their estimates may be subject to bias due to

systematic measurement error in CPS involvement, including underreporting thereof in self-reported data. Nonetheless, it is crucial to understand not only the causes and consequences of child maltreatment itself, but also of CPS involvement, as research has documented that, distinct from child maltreatment and more intensive out-of-home placement involvement, contact with CPS systems can be invasive and traumatic for families (Fong 2019; Roberts 2006, 2022; Merritt 2021) and may also be associated with adverse child wellbeing outcomes (Evangelist et al. 2023).

Data gaps in understanding CPS involvement

The two national administrative data systems tracking CPS involvement, the National Child Abuse and Neglect Data System (NCANDS) and the Adoption and Foster Care Analysis Reporting System (AFCARS), include state-level administrative data submitted to the federal government but lack data on children and families that are not involved with CPS. Whereas these data have been leveraged to estimate the prevalence of various levels of CPS involvement (Wildeman et al. 2014, 2020; Kim et al. 2017), as well as to estimate the effects of variation over time and between states (or counties) in public policies and population characteristics on CPS involvement rates (Edwards et al. 2021; Yi et al. 2023; Yi and Wildeman 2023), the lack of counterfactual samples of non-CPS involved children and families precludes individual-level analyses of the causes and consequences of child maltreatment or CPS involvement. Likewise, the National Survey of Child and Adolescent Wellbeing (NSCAW), the only national survey-based study to track CPS-involved children and families, includes only children who have been investigated by CPS and also lacks a counterfactual sample (Berger et al. 2009; Jones et al. 2024).

A small number of linked state-level administrative data systems, such as those for

California[1] and Wisconsin,[2] include extensive data on CPS-involved children and families that have interacted with public systems, along with non-CPS-involved counterfactual groups, which facilitates research into the causes and consequences of child maltreatment and CPS involvement. However, such data systems vary considerably in the breadth and scope of programs and data sources included, as well as the span of years covered. Moreover, because they include only data reported by public systems to government agencies, they lack the full range of detailed measures of child and family characteristics and behaviors that is essential to understanding the etiology, consequences, and mechanisms surrounding CPS involvement, including parental behaviors. Also, because these systems are limited to particular states, they preclude both national inquiry and analyses of between-state variation in policies and population characteristics over time.

Several large national surveys, such as the National Longitudinal Survey of Youth 1979 cohort's Children and Young Adult Supplement (NLSY79-CYAS), the Panel Study of Income Dynamics' Child Development Supplement (PSID-CDS), and the Early Childhood Longitudinal Study's (ECLS) birth and kindergarten cohorts, collect extensive data on family functioning, parenting behaviors, and child health and development, but lack data on CPS involvement. Although a few studies have used the parenting measures available in the NLSY79-CYAS to create measures of substandard parenting or child maltreatment risk to approximate child maltreatment-related behaviors (Berger 2004, 2007), such measures do not reflect legal thresholds for child maltreatment. Information about behaviors that potentially reach legal thresholds of abuse and neglect is not typically collected in contemporary surveys because

[1] California's linked administrative data system is housed by the Children's Data Network (https://www.datanetwork.org/).
[2] The Wisconsin Administrative Data Core is housed by the University of Wisconsin Madison Institute for Research on Poverty (https://www.irp.wisc.edu/wadc/).

identifying such behaviors would require mandated reporting to CPS by study personnel and may also be prohibited due to protection of human subjects (Institutional Review Board) concerns.

The National Longitudinal Survey of Adolescent to Adult Health (Add Health) collects self-reported retrospective data on childhood experiences of child maltreatment and CPS involvement. Because Add Health data collection did not begin until adolescence, however, the data preclude precise estimation of relations of the timing of exposure to maltreatment and CPS involvement, precursors thereto, and consequences thereof, as well as analyses of these relations during early and middle childhood. Finally, a few smaller, localized studies, such as the Illinois Families Study[3] and Wisconsin Families Study,[4] have linked detailed survey data to CPS administrative data. Yet, these studies are characterized by relatively small samples drawn from specific geographic locations for which data were collected over a relatively short time period, thus limiting their utility for understanding the overarching causes and consequences of CPS involvement.

Strengths of FFCWS for CPS research

FFCWS is the only large-scale survey-based population study in the United States to collect data on CPS involvement throughout childhood and adolescence, with parents reporting on whether they have ever been contacted by CPS at interviews when the focal child is age 5, 9, and 15 and with young adults reporting on CPS involvement by age 18 in their age 22 interview. Like the NLSY79-CYAS, PSID-CDS, and ECLS studies, FFCWS also includes a wide range of parenting measures that have been used to construct behavioral-approximations of child abuse and neglect (Berger et al. 2005, 2009, 2017; Slack et al. 2011; Zhai et al. 2013; Font and Berger

[3] https://profiles.uchicago.edu/profiles/display/33816157.
[4] https://uwsc.wisc.edu/the-wisconsin-families-study-wiscfams/.

2015; Ma et al. 2018; Evangelist et al. 2023). Moreover, given that it is an ongoing birth-cohort study that has collected a wide array of social, economic, contextual, biological, and behavioral data on nearly 5,000 children (now young adults) and their parents since the children's births in 1998-2000, it is exceptionally well-suited to interrogating the causes and consequences of CPS involvement. The study strategically oversampled births to unmarried parents in 20 large U.S. cities, resulting in an extremely diverse sample that is nearly half non-Hispanic Black, a quarter Hispanic, five percent other racial groups, and a quarter non-Hispanic White. To date, seven waves of data have been collected from mothers, fathers, primary caregivers, and children. As such, FFCWS has been widely used to study the associations between CPS involvement and a range of potential causes and consequences thereof (Berger et al. 2005, 2009, 2017; Font and Berger 2015; Han et al. 2013; Lee et al. 2014; Slack et al. 2011; Evangelist et al. 2023; Thomas et al. 2023). Indeed, a recent review (Kim et al. 2024) found that FFCWS is "the most frequently used national dataset" to study the links between income, poverty, and child maltreatment, including but not limited to CPS involvement.

Limitations of self-reported CPS involvement measures in FFCWS

FFCWS's design, specifically its oversample of unmarried parents living in large cities, has resulted in a sample that is disproportionately Black, Hispanic, and socioeconomically disadvantaged. Thus, we would expect that CPS involvement would exceed national estimates, particularly in the unweighted data, as it is well established low-income children are more likely to enter the child welfare system than their higher-income peers and that racially and ethnically marginalized children are more likely to enter the system than their White peers. Given the socioeconomic and racial/ethnic make-up of the FFCWS sample, we would also expect higher rates of CPS involvement in the weighted FFCWS data compare to national estimates, as non-

White populations, who are over-represented in CPS, are disproportionately likely to live in urban areas (see, e.g., Parker et al. 2018). Black and Hispanic children are both more likely to live in poverty and more likely to experience CPS contact than their White counterparts, and a recent review of the empirical evidence regarding the drivers of racial disproportionality in CPS involvement indicates that such disproportionality is primarily driven by disproportionate risk of child maltreatment resulting from historical and ongoing structural and institutional racism in U.S. society (Berger and Harden 2025).

In addition to sample limitations, FFCWS CPS-involvement data come from parent (or primary caregiver) and young adult self-reports. This raises concerns about measurement error given that CPS involvement is generally considered to be stigmatizing (Goodman-Brown et al. 2003; Berger et al. 2009; Fong 2019; Le Zhang et al. 2020; Cooley and Jackson 2022). Even in communities where CPS intervention is a common occurrence, families report experiencing shame and trauma as a result of CPS contact (Roberts 2006, 2022). As such, it is likely that some respondents may refuse to answer CPS-involvement-related survey items or neglect to report involvement (Negriff et al. 2017), potentially due to social desirability concerns. Moreover, item-specific refusal (missing data), nonresponse to one or more interview waves (missing data), and misreporting patterns may be systematic in nature (Cooley and Jackson 2022), and may also vary systematically by both actual CPS involvement status and sociodemographic characteristics such as race and ethnicity, education, and income, perhaps as a function of or due to differences in characteristics or perceived stigma among individuals and groups. As discussed below, self-reported prevalence rates of CPS involvement in FFCWS are considerably lower than those produced from administrative data.

We know from prior research that FFCWS self-reported CPS involvement prevalence

rates are, on the whole, downwardly biased due to underreporting (Slack et al. 2011; Thomas and Waldfogel 2022). However, prior research does not shed light on whether and how such under-reporting might vary by race and ethnicity. We therefore hypothesize that there may be relative parity across racial and ethnic groups in the magnitude of underreporting. While there is no explicit empirical evidence to support this hypothesis, given very limited sources of combined administrative and self-reported CPS data, we base this expectation on racial and ethnic groups' documented experiences of CPS involvement and also consider relevant research indicating little to no significant differences by race and ethnicity in self-reporting of other stigmatized experiences, such as criminal legal system involvement (Sohoni et al. 2021). We expect that stigma and fear of further CPS involvement have powerful although perhaps different implications across the Black, Hispanic, and White families most represented in the FFCWS data. Namely, the comparatively uncommon nature of CPS involvement for White families may drive high underreporting, whereas trauma associated with CPS involvement for Black and Hispanic families may also prompt high underreporting (Fong 2019; Merritt 2021). We hypothesize that the net effects of these motivators will produce similar degrees of underreporting across racial and ethnic groups. Underreporting is problematic if it is systematic, rather than random, in nature, within either the full FFCWS sample or socio-demographically-defined subsamples thereof, as it may serve to bias estimates of relations between CPS involvement and its potential causes and consequences.

The current study

To address underreporting of CPS involvement in FFCWS, we use Bayesian methods to assign each focal child an adjusted probability of having had a CPS investigation by a given age, leveraging their self-reported CPS-involvement data (collected from parents, primary caregivers,

and the focal children themselves, as young adults), child and family characteristics, and a priori expected rates of CPS involvement by a given age drawn from prior state and national estimates derived from NCANDS. We selected a Bayesian, or probabilistic, approach because there is uncertainty in the true value of misreported (or non-reported) CPS involvement, such that deterministic imputation would not be appropriate. Moreover, prior information from existing data is available to inform our estimations in this case.

We use these probabilities to construct adjusted FFCWS estimates of CPS involvement by focal child ages 5, 9, and 15, the ages at which FFCWS conducted interviews that included questions about CPS involvement, as well as by age 18, by incorporating self-reports about CPS involvement from birth to age 18 from the age 22 survey wave. It is our expectation that the adjusted FFCWS estimates should more closely approximate or exceed the population cumulative prevalence of CPS involvement, defined as the proportion of children that has experienced CPS involvement by a given age, than those produced by the unadjusted FFCWS estimates. We use the terms cumulative rate of CPS involvement and cumulative prevalence of CPS involvement interchangeably. We also use the terms CPS involvement and CPS investigation interchangeably.

After computing the adjusted estimates, we demonstrate how they perform, relative to the unadjusted self-reported indicators of CPS involvement, by conducting regression analyses estimating associations between CPS involvement by age 18 and young adult disconnection at age 22. We selected young adult disconnection as a relevant outcome for this exercise because prior literature has documented that young adults who were involved with CPS as children, on average, have poorer educational (Piescher et al. 2014; Stone and Zibulsky 2015) and labor market trajectories than young adults who did not experience CPS involvement (Mersky and

Topitzes 2010; Yoon et al. 2021). Our analyses of associations of CPS involvement with disconnection are not intended to yield causal estimates of this relation. Rather, they are presented to exemplify the extent to which estimates from such analyses are likely to differ when the unadjusted and adjusted CPS-involvement measures are used. That is, our main substantive focus is centered on the difference in estimates produced when using the unadjusted and adjusted CPS-involvement measures. Our main models estimate the association between any CPS involvement and disconnection at age 22. Recognizing that this association likely varies by the age of the child, with young children expected to be particularly sensitive, we also estimate a model allowing this association to vary by the age of the child at the time of first involvement. In addition, recognizing the CPS-involvement experiences can range from an investigation by CPS followed by no subsequent system involvement to the child being removed from the home following an investigation, we estimate an additional model examining whether the association between CPS involvement and disconnection varies by whether the child did or did not experience out-of-home placement.

Data and methods

Data

Our data are drawn from FFCWS, a longitudinal birth cohort study of 4,898 children born between 1998 and 2000 in 20 large U.S. cities located in 15 states (Reichman et al. 2001). Of these 20 cities, 16 (Austin, Baltimore, Boston, Chicago, Corpus Christi, Indianapolis, Jacksonville, Nashville, New York City, Norfolk, Philadelphia, Pittsburgh, Richmond, San Antonio, San Jose, Toledo) constitute the national sample which, when weighted, is representative of births occurring in large US cities (the 77 U.S. cities with populations over

200,000 in 1994) between 1998 and 2000.[5] Four additional cities (Detroit, Milwaukee, Newark, Oakland) are not part of the national sample. We include all 20 cities in our adjustment model.

FFCWS children and their families have been followed since their births, with interviews with mothers and fathers (or primary caregivers) taking place shortly after the birth and at focal child ages 1, 3, 5, 9, 15, and, most recently, 22. Focal children were assessed at ages 3, 5, 9, 15, and 22, and interviewed at age 9, 15, and 22. We use the entire FFCWS birth cohort sample of 4,897 to produce adjusted estimates of CPS involvement. To estimate associations between unadjusted and adjusted indicators of CPS involvement by age 18 and disconnection at age 22, we use a subsample of 3,230 young adults who either completed the age 22 interview and provided information on their education and employment status or who did not participate in the age 22 interview, but had a parent or PCG who participated and provided details about the young adult's education and employment status at age 22.

Measures

CPS involvement (CPS involvement had occurred by focal child age 5, 9, 15, and 18)

FFCWS explicitly asked mothers, fathers, and primary caregivers (if someone other than the mother or father) whether they had ever been contacted by CPS due to abuse or neglect concerns about any children in their household in the age 5, 9, and 15 interviews. The young adults (focal children) were asked, in the age 22 interview, whether CPS was ever involved with their family and whether there was ever a period in which they did not live with either of their biological parents as a result of CPS involvement by the time they reached age 18 (see Appendix A1 for verbatim items). We code affirmative responses to any of these items by the mother,

[5] Fragile Families & Child Wellbeing Study: A Brief Guide to Using the Weights for Waves 1-6. (n.d.). https://ffcws.princeton.edu/sites/g/files/toruqf4356/files/documents/using_the_fragile_families_weights_waves_1_6_01052021.pdf

father, primary caregiver, or young adult as indicating that CPS involvement occurred by the relevant age. Our measure of CPS involvement by age 18 indicates whether the child's family was ever investigated by CPS between the time of the child's birth and the time the child was age 18. We also construct measures for involvement by age 5, 9, or 15.

In addition, at age 1, 3, 5, 9, and 22, FFCWS asked mothers, fathers, and/or primary caregivers whether there was a period between the prior and current interview in which the focal child did not live with them and, if so, the reason therefor. Response options vary between interview waves but, in each wave, include options that the child was removed by the court or by CPS (see Appendix A2 for verbatim items). We code affirmative responses to any of these items by the mother, father, and/or primary caregiver as also indicating that CPS involvement occurred by the relevant age. We also use these responses to identify whether a child was ever placed out of home.

We use responses to the fore-mentioned items to construct indicators that CPS involvement had occurred *by* (not at) focal child age 5, 9, 15, and 18. It is important to note that the measure for involvement by age 18 differs from the measures for involvement by the earlier ages, in that the measure for age 18 is derived from a retrospective question asked of the young adult and parent/PCG at age 22 about any involvement between birth and age 18, whereas the measures at earlier waves are drawn from interviews with parents or primary caregivers about any involvement since birth (at ages 5 and 9) or any involvement since the last interview (at age 15) (see Appendix A3 for a detailed explanation of how these indicators were constructed at each age).

Our coding strategy explicitly assumes that there are no false positive self-reports of CPS involvement. That is, we assume that, if any interview respondent (mother, father, primary

caregiver, or young adult) reports that the family experienced CPS involvement, as assessed by any of the fore-mentioned items, the family did experience CPS involvement, regardless of what was reported by other interview respondents or by the same interview respondent on other CPS involvement-related items. We argue that this is a sensible assumption given that respondents may have incentives or motivations for nonreporting CPS involvement, due to the stigma associated therewith and associated social desirability, but that they have no incentives or motivation for falsely reporting CPS involvement that did not occur. Furthermore, it is possible that some respondents were not aware or had forgotten that CPS involvement had taken place, but relatively unlikely that they would have sufficient reason to perceive CPS involvement as having taken place when it did not.

As such, we code any child for whom any respondent reported, in any of the relevant survey items, that CPS involvement occurred as having a 100 percent probability that CPS involvement occurred. For all other children, regardless of whether, at an interview wave, responses to the various CPS involvement-related items indicate noninvolvement or are missing (as indicated by responses of don't know, refusal to answer a specific item, or nonparticipation in the survey wave), we predict a probability of CPS involvement as a function of a priori expectations of their likelihood of being CPS-involved by a certain age (drawn from age-, race/ethnicity-, and child's state of birth-specific prevalence rates derived from NCANDS), as well as individual child and family characteristics.

Race/ethnicity

We define race/ethnicity of the young adults using four mutually exclusive categories: Black, non-Hispanic; Hispanic (non-American Indian/Alaskan Native); White, non-Hispanic; and other race/ethnicity (which includes non-Hispanic Asian and Native Hawaiian/Pacific

Islander, Hispanic/non-Hispanic American Indians and Alaska Natives, and those whose race/ethnicity we are unable to determine). We include the "other" category for the sake of completeness, but we note that the sample for that category is small and comprises groups with very different prevalences of CPS involvement. Our categorization for the most part is comparable to that used in NCANDS, as detailed below.

FFCWS collects detailed data on race and ethnicity at multiple timepoints. Our categorization of race and ethnicity prioritizes use of self-reported race/ethnicity data from year 22 and year 15, followed by parental race/ethnicity reported at baseline. Using year 22 data, children were assigned race/ethnicity in the following order: (1) code as Native American if the respondent self-identified as Native American, regardless of whether they self-identified as being of Hispanic origin or of any other race(s), (2) code as Hispanic if the respondent self-identified as being of Hispanic origin and did not self-identify as Native American, regardless of whether they self-identified as being of any other race(s), (3) code as White, Black, or Asian if the respondent self-identified as such and did not self-identify as Native American, Hispanic, or more than one racial group, (4) code as Black if the respondent self-identified as both Black and White or Black and Asian but did not self-identify as Native American or Hispanic, (5) code as Asian if the respondent self-identified as both Asian and White but did not self-identify as Native American or Hispanic. For respondents with missing race/ethnicity data at the age 22 interview, due either to non-interview or item non-response, we followed the same coding scheme but using data from the age 15 interview. Finally, we coded young adult race and ethnicity based on both parents' self-reported races and ethnicities at baseline (see Thomas et al. 2022) for the cases for which we did not have race and ethnicity data at either age 22 or age 15.

For the most part, our coding of race/ethnicity in FFCWS is comparable to how

race/ethnicity is measured in the two NCANDS studies that we use for our a priori estimates. But there are some differences. Kim et al. (2017), from which we obtained national-level estimates of cumulative CPS investigation prevalence, categorized racial/ethnic groups as follows: first Native American (whenever indicated), second Hispanic (when indicated among non-Native Americans), then Black, White, and Asian/Pacific Islander when uniquely identified. Our coding closely aligns with Kim et al. (2017). But we had to group Asian, Native, and "unable to determine" into the "Other" category, which is a deviation from Kim et al.'s (2017) race/ethnicity coding rules. This deviation was necessary due to small cell sizes for those three groups in the FFCWS dataset (119 [2.4 percent] Asian/NHPI, NH; 10 [0.2 percent] AIAN [Hispanic/non-Hispanic], 10 [0.2 percent] unable to determine). For prior state-level estimates, we referenced Yi et al. (2023). Our coding rule also slightly differs from Yi et al. (2023). Yi et al. (2023)has five mutually exclusive categories: non-Hispanic White, Black (Hispanic and non-Hispanic), non-Hispanic Asian or Pacific Islander, non-Hispanic American Indian or Alaska Native, and Hispanic. The discrepancy between our coding rule and Yi et al. (2023) exists in the coding rules for Black and American Indian/Alaska Native individuals, because our study prioritized the racial/ethnic coding rule of Kim et al. (2017). Together, these differences in coding affect a very small proportion of the FFCWS sample – primarily those who are Asian, Pacific Islander, American Indian, or Alaska Native.

Covariates for CPS-involvement adjustment analyses

Because CPS involvement is not randomly distributed, but rather systematically associated with a host of child and family characteristics, and also varies across cities and states, we include in our CPS-involvement adjustment models a wide range of covariates that may be associated with the likelihood that a family becomes involved with CPS (Thomas and Waldfogel

2022; Thomas et al. 2023). Detailed definitions of all variables included in the adjustment model and the timing at which they were assessed vis-à-vis the prediction model for ages 5, 9, 15, and 18, are presented in Appendix B1.

Child characteristics. Child characteristics include the child's race/ethnicity (Non-Hispanic Black, Hispanic, Non-Hispanic White, Other, as described above), the child's self-reported gender (male, female), the child's state of birth, whether the child had low birth weight, the child's age at the Y1, Y5, and Y9 mother interviews (in months), and the child's age at the Y15 PCG interview (in years). See Appendix B1 and B2 for details.

Mother characteristics. Mother characteristics include the mother's age at baseline and at the age 5, 9, and 15 interviews, nativity, whether the mother had more children by the age 5 interview than at baseline, the number of children under 18 in the household at baseline and whether the mother had more children in the household at the age 5 interview than at baseline, and the mother's education level at baseline and at the age 1, 5, and 9 interviews (less than high school, high school or equivalent, some college or technical/trade school, college or graduate/professional school). In addition, we include the mother's marital and cohabitation status at the age 1, 5, 9, and 15 interviews (married and living with the child's father, married and living with a new partner, not married and living with the father, not married and living with a new partner, or not married and not living with any partner), the mother's housing status at baseline and at the age 1, 5, 9, and 15 interviews (owns house, rents without assistance, rents with assistance, public housing, or other), as well as other maternal characteristics, including whether the mother ever worked at the age 5, 9, and 15 interviews; the number of waves in which she experienced severe material hardship at the age 5, 9, and 15 interviews; whether her household was ever in income poverty at the age 5, 9, and 15 interviews; whether she ever

experienced depression at the age 5, 9, and 15 interviews; whether she ever experienced poor health at the age 5, 9, and 15 interviews; whether she ever experienced interpersonal violence (IPV) at the age 1, 5, 9, and 15 interviews; whether she ever used drugs at the age 5, 9, and 15 interviews; and whether she was ever involved in criminal activity at the age 5, 9, and 15 interviews. See Appendix B1 and B2 for details.

Father characteristics. Father characteristics include whether the father had contact with the child more than once per month at the age 1, 5, and 9 interviews, and the father's education level at baseline (less than high school, high school or equivalent, some college or technical/trade school, college or graduate/professional school). See Appendix B1 and B2 for details.

Additional covariates. We also include in our CPS-involvement adjustment models the missing indicator of unadjusted CPS involvement at ages 5, 9, 15, and 18; the predicted probability of CPS involvement at prior waves (e.g. at age 5, 9, and 15 for the model for age 18); and survey participation indicators for the mother's participation in the age 5 and 9 interviews, PCG participation in the age 15 interview, and youth participation in the age 15 interview. See Appendix B1 and B2 for details.

Additional measures for young adult disconnection analyses

Disconnection. We assess disconnection by whether a young adult is neither working nor enrolled in an educational (e.g., college) or training program at age 22. Specifically, a young adult is coded as not being in school or training if they report that they are not attending a program and were not/will not be attending a program, within three months before and after the age 22 interview. They are coded as not working if they report that, during the week prior to the age 22 interview, they did not work for pay, own a business, or serve in the military (those with a job but temporarily absent from work are coded as working).

Covariates. In our analyses of associations of the unadjusted and adjusted indicators of CPS involvement with likelihood of disconnection at age 22, we first control for a parsimonious set of covariates, including the young adult's age at the age 22 interview, gender, race/ethnicity, and low birth weight status at birth, as well as the mother's education and marital and relationship status with the father at the time of the birth. We also control for the unemployment rate in the young adult's state of residence in the month of the interview to capture variation in labor market conditions during the period the young adults were interviewed (between June 2020 and January 2024).

Our second model adds an additional set of controls for family characteristics and parenting behaviors that are likely to be correlated both with CPS involvement and with disconnection during young adulthood. The family characteristics draw on data from the mother from survey waves from birth to age 15 and include variables for whether the mother: ever experienced severe material hardship; was ever in income poverty; was ever was in poor health; ever experienced IPV; ever used drugs; and was ever involved with the criminal justice system. The parenting behaviors include physically aggressive and neglectful parenting, and draw on data reported by the mother in response to questions from the Conflict Tactics Scale[6] at the age 3, 5, and 9 interviews. We model three mutually exclusive indicators for the number of waves the mother reported physically aggressive behavior toward the child: 0 or 1 waves (the reference category); 2 waves; or 3 waves. Likewise, for neglectful parenting, we also model three mutually exclusive categories for the number of waves the mother reported neglectful behavior toward the

[6] Physically aggressive and neglectful behaviors were measured using subscales of the Parent-Child Conflict Tactics Scales, a commonly used self-reported measure of behaviors that approximate child maltreatment. Physically aggressive behaviors include (1) shaking child, (2) hitting child on the bottom with belt or hard object, (3) spanking child on bottom with bare hand, (4) slapping child on the hand, arm, or leg, (5) pinching child; neglectful behaviors include (1) leaving child home alone, (2) wasn't able to show or tell child you loved child, (3) wasn't able to make sure child got food needed, (4) wasn't able to take child got to a doctor/hospital, (5) so drunk/high had problem taking care of child .

child: 0 waves (the reference category); 1 wave; or 2 or 3 waves.

Imputation of missing data

We conduct two sets of imputations of missing data—one for our CPS adjustment analyses and one for our young adult disconnection analyses—using the statistical package Amelia (Honaker et al. 2011) which, based on the assumption that data are missing at random and are drawn from a multivariate normal distribution, fits a model to the observed data and generates imputed values for each missing data point multiple times to account for the inherent uncertainty in imputed values. Although Amelia is often used for multiple imputation, we impute missing values only once. We do not use multiple imputation because our primary aim is to adjust for underreporting of CPS involvement in FFCWS using a priori national estimates and a Bayesian latent variable approach, for which we would expect negligible differences in adjusted CPS involvement values across multiple FFCWS data sets with imputed covariates. As such, we decided against adding computational burden to an already computationally demanding approach.

We impute missing data for our CPS involvement adjustment analyses using the full set of covariates included in those analyses (see above and Appendix B1). We separately impute missing data for our regressions estimating associations between CPS involvement and young adult disconnection using the more parsimonious set of covariates included in those analyses (see above). In both cases we conduct imputations for all 4,897 observations in FFCWS. We include unadjusted CPS involvement in our imputations because it is possible that missing values on the covariates may be correlated with CPS involvement status; as such, not accounting for it in our imputation models may lead to biased results. We do not impute missing data on CPS involvement, however. Rather, we assign a value of zero to missing CPS-involvement indicators

and include in our imputation models an indicator that CPS involvement data were missing. Additionally, while we include the young adult disconnection measures in our imputation model for the disconnection analyses, and impute missing values thereof, because young adult disconnection (and missing values thereof) may be correlated with missing data on the covariates, we exclude cases with initially missing values on these measures in our regressions of associations between CPS involvement and young adult disconnection.

Empirical Strategy

Adjusted CPS involvement estimation

To adjust for likely underreporting of CPS involvement in FFCWS, we develop and apply a Bayesian method that uses rates derived from NCANDS as a priori expectations of CPS involvement in the FFCWS sample. NCANDS compiles national case-level information on investigated reports of child maltreatment (based on data submitted by state CPS agencies). NCANDS includes all investigated reports in participating states, and since 2004, covers most states. This makes NCANDS suitable for estimating state- and racial/ethnic-specific variation in child maltreatment cases (Yi et al. 2023).

We use data on the cumulative prevalence of CPS involvement by a given age in NCANDS from Kim et al. (2017) and Yi et al. (2023). Kim et al. (2017) used 2003-2014 NCANDS data, while Yi et al. (2023) used 2015-2020 NCANDS data. For national-level cumulative prevalence of CPS involvement, we used estimates from Kim et al. (2017), which reports the cumulative national prevalence of CPS investigation from NCANDS by age 18, overall and by racial/ethnic group. Kim and colleagues provided us with an additional dataset of national-level cumulative CPS investigation rates from NCANDS by age (from birth to age 18) and by race/ethnicity. We used these estimates as priors to create adjusted estimates for

cumulative CPS involvement in FFCWS by age 5, 9, 15, and 18 years old, by race/ethnicity.

For the state-level cumulative prevalence of CPS involvement, we used estimates from Yi et al. (2023), which provides the state-specific cumulative prevalence of CPS investigation from NCANDS by age 18, overall and by racial/ethnic group. Yi and colleagues provided us with a supplementary dataset of state-level cumulative CPS investigation rates from NCANDS by age (from birth to age 18) and race/ethnicity. We used these estimates as priors to create adjusted estimates for cumulative CPS involvement in FFCWS by age 5, 9, 15, and 18 by race/ethnicity and by child's state of birth (see the Technical Appendix for more information).

Our (Bayesian) approach allows us to account for differential misclassification error in estimating the probability of the unknown population prevalence of CPS involvement, given that we lack validation data to assess its certainty. In addition, the extent of underreporting in FFCWS (unadjusted CPS involvement prevalence), relative to our a priori expectations (NCANDS prevalence of CPS involvement), varies by age, race and ethnicity, and child's state of birth (as shown in Appendix C). As such, we leverage age-, race/ethnicity-, and child's state of birth-specific NCANDS-derived prevalence rates from Yi et al. (2023), rather than national prevalence rates, in setting a priori expectations. Moreover, an extensive body of research documents that a host of child and family characteristics and behaviors are associated with children's probabilities of CPS involvement (and child maltreatment, itself); see, e.g., Font and Maguire-Jack 2020. Thus, we also account for a range of such factors in producing our adjusted estimates. Rather than solely approximating the overall cumulative prevalence of CPS involvement found in NCANDS, this approach allows us to use the a priori probabilities to better estimate the likelihood of cumulative CPS involvement for each FFCWS focal child as a function of both observed covariates and a discrete latent variable.

Simulation analyses. We first conducted four simulation analyses based on different prior assumptions of the population distribution of CPS involvement and associated misclassification rates in a random subset of the population, using generated population data. We took this approach to assess how incorporating different prior information helps adjust the underestimated prevalence of CPS involvement by demographic subgroups, relative to corresponding population prevalence rates, by estimating probabilities of the unknown values of a binary latent variable. The simulation analyses use prior information on the population prevalence of CPS involvement from NCANDS, including (1) the total rate; (2) race/ethnicity-specific rates; (3) region-specific rates (Southwest, Northeast, Midwest, Southeast, West); and (4) race/ethnicity- and region-specific rates. (We use region, rather than state, in the simulation for ease of computation; but in our subsequent adjustment analysis we take advantage of state data, as detailed below.)

Motivated by underreported rates of CPS involvement in FFCWS relative to national rates of CPS involvement in NCANDS, the simulated data consists of population values of CPS involvement, z, a binary latent variable; misclassified self-reported CPS involvement, y; and observed covariates X. We denote ψ as the probability of z being 1 given our a priori expectations of the prevalence of z for each age-, race/ethnicity-, and region-specific group.

To reflect the features of real-world data, we generate a data set of 100,000 observations to represent our population and randomly draw 3,000 observations to ensure the subpopulation is representative of the original population. We generate the simulated data in four steps. First, we define the coefficients of regression models of the true outcome z and the observed outcome y to be:

$$\alpha = [-0.1,\ 1,\ 0.2,\ -0.2,\ -0.1,\ -1,\ -0.8,\ -0.9,\ -0.3]$$

$$\beta = [1.1,\ 0.25,\ 0.1,\ -0.1,\ -0.15,\ 0.2,\ -1,\ -0.7,\ -0.8,\ -0.2,\ 0.2]$$

where α is a column vector of 8 coefficients for z and β is a column vector of 9 coefficients for y.

Second, we generate the observed covariates X from the multinomial distributions $M_k(n; p_1, \ldots, p_k)$ as follows:

$$X_{\text{race/ethnicity}} \sim \text{multinomial}(0.15, 0.5, 0.25, 0.1)$$

where p_1 is the probability of a child being non-Hispanic White, p_2 is the probability of a child being non-Hispanic Black, p_3 is the probability of a child being Hispanic, and p_4 is the probability of a child being of another race or ethnicity, and:

$$X_{\text{region}} \sim \text{multinomial}(0.12, 0.18, 0.2, 0.27, 0.23)$$

where p_1 is the probability of a child from the Southwest, p_2 is the probability of a child from the Northeast, p_3 is the probability of a child from the Midwest, p_4 is the probability of a child from the Southeast, and p_5 is the probability of a child from the West. We then generate a normally distributed covariate, such that:

$$X_{continuous} \sim \text{normal}(1,2).$$

Third, we generate the Bernoulli outcome z, conditional on X using the logistic regression model:

$$z|X \sim \text{Bernoulli}\big(\text{expit}(X^T\alpha)\big).$$

Finally, we generate the observed misclassified Bernoulli outcome Y, conditional on X using the logistic regression model:

$$y|X \sim \text{Bernoulli}\big(\text{expit}(X^T\beta + \beta_9 z)\big).$$

After generating these data, we set y to zero if its corresponding value of z is zero, ensuring that misclassified cases arise solely from false negatives, where the true value of z is 1

but the observed value of y is 0. This results in 434 misclassified cases in the 3,000 observations, with no false positives, in which the true value z is 0 but the observed value of y is 1. As previously noted, we assume the FFCWS data do not include false positives since there is no reason to suspect that respondents would report CPS involvement, that is $Pr(y_i = 0|z_i = 0) = 1$, that did not occur. However, there is reason to suspect that some respondents would not report CPS involvement that did occur. Our simulation setup results in large discrepancies in CPS involvement prevalence rates between the population and the random samples across all demographic subgroups. Drawing 3,000 random observations from 100,000 population data sets results in slight discrepancies in the mean values of z by demographic subgroups relative to their population values, which serve to reflect sampling bias in real-world data.

We aim to estimate the probability that the true value of z is 1 given the observed covariates and prior information, that is $Pr(z_i = 1|y_i, X_i, \psi_i)$, and adjust for underreporting in the full FFCWS population as well as within each demographic subgroup using the a priori information of population mean values of z for each subgroup. Unfortunately, however, estimating $Pr(z_i = 1|y_i, X_i, \psi_i)$ is not straightforward because z is unobserved and cannot be regressed on the predictors. We therefore treat z as a latent variable and marginalize over this discrete latent variable (Li et al. 2018). Treating z as a latent variable and estimating $\psi_i = Pr(z_i = 1|\psi_i)$ based on prior information enables us to fit a model to estimate $p(y_i|z_i = 1, X_i, \psi_i)$; marginalizing over the discrete latent variable z gives us $p(y_i|X_i, \psi_i)$. We use this information to estimate $Pr(z_i = 1|y_i, X_i, \psi_i)$ using Bayes' rule:

$$
\begin{aligned}
p(y|X,\psi) &= \sum_{Z=0}^{1} p\,(y,z|X,\psi) \\
&= p(y|z=1,X,\psi)Pr(z=1|X,\psi) + p(y|z=0,X,\psi)Pr(z=0|X,\psi) \\
&= p(y|z=1,X)Pr(z=1|\psi) + p(y|z=0,X)Pr(z=0|\psi)
\end{aligned}
$$

This result gives:

$$p(y|X,\psi) = \begin{cases} Pr(y=1|z=1,X)Pr(z=1|\psi), & \text{if } y=1 \\ Pr(y=0|z=1,X)Pr(z=1|\psi) + I(y=0)Pr(z=0|\psi), & \text{if } y=0 \end{cases}$$

Let $p = Pr(y=1|z=1,X)$ and $\psi = Pr(z=1|\psi)$, then the results above gives:

$$Pr(z=1|y,X,\psi) = \begin{cases} \dfrac{p*\psi}{p*\psi} = 1, & \text{if } y=1 \\ \dfrac{(1-p)*\psi}{(1-p)*\psi + (1-\psi)}, & \text{if } y=0 \end{cases}$$

Assessment of prior distributions using a Bayesian latent variable model. The simulation analyses consist of four scenarios, each using different prior information for comparing the adjusted prevalence of CPS involvement in the demographic subgroups to their population prevalence of CPS involvement. The first scenario uses the overall population prevalence; the second applies race/ethnicity-specific prevalences; the third applies region-specific prevalences; and the fourth incorporates both race/ethnicity- and region-specific prevalences.

The a priori information is used to estimate the probability ψ from our prior distribution, a beta distribution with parameters A and B. The value of A is determined by the expected (a priori) number of respondents with CPS involvement based on the population data. The value of B is determined by the expected number of respondents without CPS involvement. For example, under the first scenario, which uses only a priori information on the prevalence rate in the full population, the population prevalence of CPS involvement is 0.43. As such, the expected number of cases with CPS involvement is 1,287 out of 3,000, such that A = 1,287 and B = 1,713. We assigned the same prior distribution to each observation, wherein the expected total prevalence of CPS involvement ranges from approximately 0.39 to 0.47. The second scenario incorporates four

prior distributions, one for each of the race/ethnicity-specific groups. The expected prevalence of CPS involvement for non-Hispanic White children ranges from approximately 0.23 to 0.41; non-Hispanic Black children from 0.48 to 0.60; Hispanic children from 0.28 to 0.43; and those of other race and ethnicity from 0.17 to 0.39. The third scenario includes five region-specific groups, and the fourth scenario includes 20 race/ethnicity- and region-specific groups, each with its own corresponding prior distribution.

We perform the simulation analyses using the Bayesian inference program Stan given its flexibility in specifying priors and fitting models, as well as its high-performance statistical computation (Gelman et al. 2013; 2015; Carpenter et al. 2017). We first fit the following joint model:

$$\begin{aligned} p(y_i, z_i, \psi_i | X_i) &= p(y_i | z_i, \psi_i, X_i) p(z_i | \psi_i, X_i) p(\psi_i | X_i) \\ &= p(y_i | z_i, X_i) p(z_i | \psi_i) p(\psi_i | X_i) \end{aligned}$$

such that that y is estimated as a function of z and X (the covariates) and the prior model, $z \sim$ Bernoulli(ψ). As shown in the joint model, there are three conditional distributions in our Stan model:

$$\begin{aligned} y_i | z_i, X_i &\sim \text{Bernoulli}(p_i) \text{ where } p_i = \text{expit}(X^T \beta + \beta_9 z) \\ z_i | \psi_i &\sim \text{Bernoulli}(\psi_i) \\ \psi_i | X_i &\sim \text{beta}(A_i, B_i), \text{ where } A_i \text{ and } B_i \text{ are specified in the prior.} \end{aligned}$$

The joint distribution for y and z is summed to marginalize the latent variable z out of the model:

$$\begin{aligned} p(y_i | X_i, \psi_i) &= \sum_{Z_i=0}^{1} p\,(y_i, z_i | X_i, \psi_i) \\ &= p(y_i | \mathrm{z} = 1, X_i) Pr(z_i = 1 | X_i) + p(y_i | z_i = 0, X_i) Pr(z_i = 0 | X_i). \end{aligned}$$

$Pr(z_i = 1 | y_i, X_i, \psi_i)$ is computed by the estimated likelihood and prior probabilities using

Bayes' rule at the final stage of our Stan model.

The results of our simulation analyses, employing each of the four approaches, are presented in Figures 1 and 2. Figure 1 shows the posterior means and one standard deviation intervals of the adjusted rates for the prevalence of CPS involvement. The approach using both race/ethnicity- and region-specific prior information yields adjusted estimates that are much more closely aligned with the population values than the approaches using lesser prior information. Figure 2 illustrates the distributions of misclassification rates for each approach. Here, we see that the approach using race/ethnicity- and region-specific rates performs best, reflecting that using the most detailed prior information results in the most accurate estimation. In other words, our model performs particularly well at capturing the population prevalence of CPS involvement when we incorporate prior information on all relevant demographic subgroups, as in the fourth scenario.

FFCWS adjustment analyses. To adjust for underreporting of CPS involvement in FFCWS, we apply the same modeling method used in the simulation analyses to generate adjusted estimates for cumulative CPS involvement by each relevant age (5, 9, 15, and 18), using the FFCWS data. Additionally, for each subsequent age, we include the predicted probability of CPS involvement — calculated from the model at the previous age — in our prediction model. As such, we incorporate all information from ages 5, 9, and 15 when predicting the probability of CPS involvement by age 18 (and all information from prior ages when predicting CPS involvement by age 9 and by age 15). Moreover, rather than applying a priori population rates at the race/ethnicity- and region-specific level, as we did in the simulation analyses, we use a priori population rates at the race/ethnicity- and state-specific level when sample sizes permit, on the assumption that state-specific rates are likely to improve the accuracy of our estimates.

Otherwise, the modeling procedure is consistent for each age-, race/ethnicity, and state-specific group, albeit with some variation in the predictors used at each age (see Appendix B1 and B2).

We first estimate the adjustment model using as prior information the total national cumulative prevalence of CPS involvement for four racial and ethnic groups (Non-Hispanic Black, Hispanic, Non-Hispanic White, and other race/ethnicity) by each age (5, 9, 15, and 18), derived from Kim et al. (2017). Kim et al. (2017) computed the cumulative prevalence of CPS involvement, spanning birth to age 18, using a synthetic cohort life table approach (Preston 2000; Yi et al. 2023) and leveraging NCANDS data from 2003 to 2014. Yi et al. (2023)'s computation is based on the same method using NCANDS 2015-2020. FFCWS cohort children were born between 1998 and 2000 and reached age 18 between 2016 and 2018. Thus, while the majority of FFCWS children's childhoods were covered by the period assessed by Kim et al. (2017), FFCWS children were born before and reached age 18 after the observation period used in their analyses. We show in Appendix D that both the national CPS investigation rate and state-specific CPS investigation rates for FFCWS sample states varied relatively minimally between Kim et al. (2017)'s observation period and the period spanning FFCWS children's childhoods, with the sole exception of Indiana, which experienced an exceptional increase in investigations between 2014 and 2019. As such, Kim et al. (2017)'s estimates represent reasonable priors for our adjustments.

The national models incorporate 16 age- and race/ethnicity-specific a priori expectations of population CPS involvement prevalences. We estimate these models, which exclude state-specific a priori expectations, because some age-, race/ethnicity- and state-specific subgroups in FFCWS lack adequate sample size for prediction. Specifically, we adjusted any age-, race/ethnicity-, and state-specific subgroup with fewer than 20 observations in the FFCWS data using national, rather than state, a priori expected prevalence rates. The subgroups for which we

use national rather than state-specific estimates are those without corrected state estimates and intervals in Appendix E1-E5.

The 20 FFCWS cities are clustered in 15 states, and our analyses focus on estimates for four racial and ethnic groups at each age, resulting in a set of 240 subgroups specified by age (5, 9, 15, 18), race/ethnicity (Black, Hispanic, White, Other), and child's state of birth (15 states). We assign each child in FFCWS a prior distribution based on their race/ethnicity and state of birth at each age, in which the value of the latent probability of having experienced a CPS investigation, ψ, is randomly sampled. We fit models for each age, race/ethnicity, and state subgroup, continuously updating the estimate for the national prevalence of CPS involvement with each iteration. We also incorporate predicted probabilities of CPS involvement produced by the model at the previous age as predictors for the subsequent age model. In all, we fit 64 models using 256 prior distributions across subgroups.

State-specific unadjusted and adjusted rates of cumulative CPS involvement for FFCWS, along with the a priori expectations, are presented in Appendix E1-E5. As discussed in the Results section below, the adjusted rates of cumulative CPS involvement are often higher than the associated a priori expectations when compared to national rates; this is not surprising given that FFCWS is a more socioeconomically disadvantaged sample.

Associations between unadjusted and adjusted CPS involvement and young adult disconnection

We estimate associations between unadjusted and adjusted indicators of CPS involvement and disconnection, using standard ordinary least squares regressions in which we regress an indicator of disconnection on either the estimated probability (from our adjustment model) that a child's family was ever involved with CPS, or an unadjusted or adjusted indicator

that a child's family was ever involved with CPS, and covariates as described above. We estimate linear probability models rather than logit or probit models for ease of interpretation. Our adjustment procedure assigns each child an estimated probability of having ever had CPS involvement but does not assign them a dichotomous indicator (1 or 0 value) of such. We use two separate approaches to assign each child an indicator of having ever experienced CPS involvement, which we refer to as fixed and random methods.

In the fixed method, we assign a child a value of 1 (having experienced CPS involvement) or 0 (not having experienced CPS involvement) using a threshold for assignment derived from the posterior mean probabilities of the age-, race/ethnicity-, and state-specific subgroup to which the child belongs, such that the overall cumulative CPS involvement rate in FFCWS approximates the a priori cumulative CPS involvement rate for that subgroup as closely as possible. For example, if the cumulative CPS involvement rate by age 18 for non-Hispanic Black youth in California has a posterior mean probability of 0.60, we assume that the true prevalence rate for this subgroup in the population represented by the FFCWS data is 60 percent. We therefore dichotomize the continuous variable for non-Hispanic Black youth in California so that approximately 60 percent are classified as having experienced CPS involvement and 40 percent as not having experienced CPS involvement. Specifically, individuals who self-report a value of 1 already have posterior mean probabilities of 1, under the assumption of no false positives. Individuals who self-report a value of 0 (indicating no CPS involvement) but whose posterior mean probabilities are the highest are then reclassified as 1 (indicating CPS involvement) until a total of 60 percent non-Hispanic Black youth in California have a value of 1.

In the random method, we randomly assign each child a value of 1 or 0 by sampling these binary values from a Bernoulli distribution, where the parameter is the estimated posterior probability of CPS involvement for that child. This random assignment simulates the uncertainty inherent in the process of converting continuous posterior probabilities into binary outcomes. Through this method, we generate thousands of different combinations of newly estimated CPS predictors, each representing a possible realization of the CPS involvement for each child. These combinations serve as alternative representations of the data, incorporating the variability in the CPS predictor estimates. Once the binary combinations are generated, we fit a regression model for each combination to estimate the relations between the predictors and the outcome. After fitting these models, we select the combination that produces regression results most closely aligned with those obtained when fitting a model with the continuous posterior probabilities, which represent our best estimate because continuous posterior probabilities offer greater flexibility in estimating the exposure effect compared to binary classifications. Converting probabilities into binary outcomes introduces additional sources of error, which can distort the accuracy of the estimated effect. Therefore, we prioritize finding the binary combination that best approximates the results from our preferred continuous model.

Although converting probabilities into binary using either the fixed or random method results in some loss of information, we can efficiently achieve the same outcome as regression with continuous probabilities. By first regressing the binary variable on the continuous probabilities and extracting the residuals, we can include these residuals as an additional predictor when modeling the binary variable's effect on an outcome of interest. This approach ensures that the residuals capture the variation in the binary variable that remains unexplained by the continuous probabilities, thereby preserving essential information from the continuous

probabilities. While this method enhances interpretability, it comes at the cost of an increased standard error, which is comparable to that when using probabilities as a predictor.

We estimate four separate regressions to examine similarity and differences in the estimates of associations of CPS involvement with young adult disconnection between estimates produced using as the key predictor: (1) the self-reported CPS-involvement indicator, (2) the predicted probability of CPS involvement from our adjustment model, (3) the adjusted CPS-involvement indicator produced by the fixed method, and (4) the adjusted CPS-involvement indicator produced by the random method. Our preferred adjusted measure is the continuous measure of the predicted probability of CPS involvement because it both leverages more information than and accounts for uncertainty in actual CPS involvement better than the bivariate indicators. We estimate each model for the full sample as well as for subgroups defined by young adult race/ethnicity.

Results

Adjusted CPS involvement estimates: Full sample

Figure 3 presents overall and race/ethnicity-specific unadjusted and adjusted (using the fixed method) cumulative prevalence rates of self-reported CPS involvement in FFCWS, and cumulative national prevalence rates of CPS involvement derived from NCANDS, by ages 5, 9, 15, and 18. We present both weighted and unweighted FFCWS estimates because, whereas the weighted estimates are more appropriate for comparison to the national estimates, FFCWS assigned weights to respondents only in the 16 of the 20 cities that constitute the national sample. Thus, we present weighted estimates for the national sample of 3,442 focal children and unweighted estimates for the full sample of 4,897 focal children (see Appendix C for detailed estimates). In addition, because FFCWS is an urban sample, it is disproportionately comprised of

non-White families, even when weighted. As such, for the full sample (Panel A in Appendix C) we compare the FFCWS estimates to NCANDS prevalence and additionally compare the FFCWS estimates to reweighted NCANDS prevalence, in which FFCWS distributions of children by race and ethnicity has been applied to NCANDS to mirror the same composition of children by race and ethnicity. The reweighted NCANDS prevalence rates are our preferred benchmarks because they account for differences in the racial and ethnic compositions between the NCANDS population and the FFCWS sample, such that differences in prevalence rates between FFCWS and the reweighted NCANDS should reflect only differences in the accuracy of self-reports of CPS-involvement in FFCWS and not also differences in the racial and ethnic compositions of the two data sources.[7]

We find that self-reported CPS involvement in FFCWS is substantially underreported relative to national rates derived from NCANDS. The unadjusted weighted cumulative prevalence of CPS involvement in FFCWS ranges from 45 percent (at age 18) to 57 percent (at age 15) smaller than the reweighted national prevalence rates derived from NCANDS, and the unadjusted unweighted FFCWS prevalence rates range from 34 percent (at ages 5 and 18) to 46 percent (at age 15) smaller (see Appendix C Panel A). It is not surprising that these differences tend to be larger when weighted than when unweighted given that the FFCWS sample is drawn from large cities and that the study oversampled, by a three-to-one ratio, nonmarital births, which disproportionately occur to low-income and racial- and ethnic-minority families, relative to marital births. As such, the unweighted data overrepresent low-income and racial and ethnic minority families. It is well established that such families are at disproportionately high risk of CPS involvement. As a result, the most socioeconomically advantaged families in the FFCWS

[7] Reweighting of the NCANDS data is irrelevant to the subgroup comparisons given that these estimates are derived from within each group and are not influenced by the racial and ethnic distribution of the samples.

sample—those that are at lowest risk of CPS involvement—receive the largest weights and those at highest risk receive the smallest weights. Our adjustment strategy serves to bring FFCWS estimates of CPS involvement substantially more in line with national estimates, ranging, in the weighted data, from 6 percent smaller (at age 5) to 18 percent larger (at age 18) and, in the unweighted data, from 10 percent (at age 15) to 20 percent (at age 18) larger, relative to the reweighted NCANDS-derived estimates (see Appendix C Panel A). As noted in the methods section, it is important to recognize that FFCWS items at ages 5, 9, and 15 specifically assessed whether CPS involvement had occurred by those ages. In contrast, the age 22 item assessed whether CPS involvement had occurred at any point between birth and age 18. As such, the relatively large increase in the prevalence rates between age 15 and age 18 may, at least in part, reflect CPS involvement that occurred prior to age 15 but was not reported in an earlier interview, rather than solely reflecting first-time CPS involvement between ages 15 and 18. As such, we omit from Figure 3 the solid line showing the trend in prevalence rates across ages 5-15 from the age 15-18 period.

Overall, our adjusted estimates indicate that, over the course of childhood (by age 18), 41 percent of all FFCWS children will have ever been involved with CPS. By comparison, NCANDS prevalence reweighted to reflect FFCWS sample composition indicate that about 35 percent of U.S. children will have been involved with CPS. It makes sense that we find higher cumulative prevalence of CPS involvement in the FFCWS sample of families (births) in large cities than among the full U.S. population given that, even when weighted to be representative of births in all large cities, FFCWS families are disproportionately socioeconomically disadvantaged relative to the national population.

Adjusted CPS involvement estimates: Heterogeneity by race and ethnicity

Figure 3 reveals considerable incongruence between the FFCWS and national estimates for each of the racial and ethnic groups we consider, though patterns thereof differ substantially when considering weighted and unweighted FFCWS data (see Appendix C for detailed estimates). The unweighted data suggest that non-Hispanic Black and Hispanic children and families are considerably more likely to underreport CPS involvement than non-Hispanic White children and families, and that children and families in the other race/ethnicity group overreport CPS involvement. Specifically, relative to national estimates, unweighted FFCWS cumulative prevalence estimates for CPS involvement by age 18 are 10 percent lower for non-Hispanic White, 43 percent lower for non-Hispanic Black, and 25 percent lower for Hispanic children; they are 58 percent higher for children of another race or ethnicity. However, this pattern reflects the composition of the unweighted sample. Once the data are weighted, we find relatively similar patterns of underreporting for non-Hispanic White (46 percent lower than national estimates), non-Hispanic Black (43 percent lower), and Hispanic (48 percent lower) children, as well as for children of another race or ethnicity (29 percent lower). As in the full sample, for non-Hispanic White, non-Hispanic Black, and Hispanic children, our adjusted FFCWS estimates for the cumulative prevalence of CPS involvement are considerably more consistent with, though modestly higher than, national estimates (again, reflecting that FFCWS is an urban and low-income sample), with adjusted weighted FFCWS cumulative prevalence rates of CPS involvement by age 18 for non-Hispanic White, non-Hispanic Black, and Hispanic children being 19 percent, 13 percent, and 16 percent higher than national prevalence rates, respectively.

Our cumulative estimates of CPS involvement for children of the other race/ethnicity category are substantially higher than the national estimates, with FFCWS adjusted cumulative CPS involvement rates by age 18 being 86 percent higher when weighted and 110 percent higher

when unweighted. This reflects that the other race/ethnicity group in the FFCWS sample is both small and heterogeneous, consisting of Asian populations (which are typically underrepresented in CPS caseloads), American Indian/Native American populations (which are substantially overrepresented in CPS caseloads), and other populations. This other race/ethnicity group comprises just 4.5 percent of the unweighted (full) FFCWS sample and 8.6 percent of the weighted (national) sample, reflecting just 220 and 295 observations in each sample, respectively. Given the limited number of observations for this group, combined with its heterogenous composition, we cannot be confident in the precision or accuracy of our adjusted estimates therefor. As such, these estimates should be viewed with extreme caution. In addition, as discussed earlier, while our approach to coding race/ethnicity is largely consistent with the framework used by Kim et al. (2017), we combined Asian, Native American, and "unable to determine" into a single "Other" category. In contrast, Kim et al. (2017) used distinct categories for Native American and Asian/Pacific Islander.

In all, our adjusted estimates indicate that 33 percent of White, 60 percent of Black, 37 percent of Hispanic, and 29 percent of other race/ethnicity FFCWS children will have been involved with CPS by age 18, whereas NCANDS-derived estimates indicate rates of 28, 53, 32, and 16 percent for these groups of children, respectively. Again, the finding that CPS-involvement rates are higher among FFCWS families than the U.S. population as a whole is consistent with our expectations.

Characteristics of re-classified families

Table 1 provides weighted descriptive statistics by unadjusted and adjusted CPS-involvement status (using the binary indicator produced using the fixed method). The table includes a range of factors associated with CPS involvement for the full FFCWS sample and for

families that are characterized as non-CPS involved under both the unadjusted and adjusted measures, those that are CPS involved under both the unadjusted and adjusted measures, and those that are characterized as CPS involved under the adjusted but not the unadjusted measure (by design, any family classified as CPS involved under the unadjusted measure is also considered CPS involved under the adjusted measure). These estimates provide key insights into which families likely underreported CPS involvement and were thus, based on our priors, recategorized as having been CPS involved.

Overall, the pattern of results in Table 1 indicates that families that were reclassified as being CPS involved have, for the most part, demographic characteristics that are less strongly associated with CPS involvement than those of families that self-reported being CPS involved. Reclassified families are more likely to have a mother who was non-U.S. born and was older and married to the focal child's father at the focal child's birth, as well as a mother who is more highly educated and more likely to have a work history. They are less likely than families that self-reported being CPS involved to report that the child is Black and more likely to report that the child is of another race/ethnicity (non-Black, White, or Hispanic). Reclassified families reported lower rates of severe material hardship, income poverty, maternal depression, poor health, intimate partner violence, drug use, and criminal justice system involvement than families that self-reported being CPS involved. In addition, while there is no clear pattern of differences between families that self-reported being, and those that were reclassified as being, CPS-involved with respect to self-reported physically aggressive and neglectful behaviors toward the focal child at ages 3, 5, and 9, families that were reclassified as CPS involved—like families that self-reported being CPS involved—reported higher rates of physically aggressive and neglectful behaviors than families that were classified as non-CPS involved under both the adjusted and

unadjusted measures.

These descriptive analyses indicate that reclassified families—those that are likely to under-report being CPS involved—are more socially and economically advantaged than those that self-report being CPS involved, with the exception that the former disproportionately have non-U.S. born mothers. This pattern suggests that there is systematic selection into self-reporting of CPS involvement such that more socioeconomically advantaged families and those with non-U.S. born mothers are less likely to report having been CPS involved. Such systematic underreporting has the potential to bias estimates of empirical analyses of association between CPS involvement and its causes and consequences. Furthermore, that recategorized families self-report similar rates of physically aggressive and neglectful behaviors (which we do not use as predictors in our adjustment models) as families that self-report CPS involvement increases our confidence in the accuracy of our approach to predicting likely CPS involvement among families that did not report it.

Empirical application: Estimating associations between unadjusted and adjusted CPS involvement and young adult disconnection

To examine whether underreporting of CPS involvement in FFCWS may result in biased estimates of associations between CPS involvement and subsequent young adult wellbeing, we estimate regressions of the unadjusted self-reported indicator, the predicted probability from our adjustment model, the fixed method adjusted indicator, and the random method adjusted indicator of CPS involvement with young adult disconnection at approximately age 22, and assess consistency and differences among the resulting estimates.

Our first model includes a parsimonious set of covariates: the young adult's age at the age 22 interview, gender, race/ethnicity, and low birth weight status at birth; the mother's

education and marital and relationship status with the father at the time of the birth; and the unemployment rate in the young adult's state of residence in the month of the interview. These results are shown in the top panel of Table 2, in which each cell presents the coefficient and standard error for the CPS involvement variable from a single regression for the unadjusted, predicted probability, and fixed adjusted measures, as well as the mean of the coefficients and mean of the standard errors for the CPS investigation indicators for the random adjusted method. We include results for the other race/ethnic group in the table for completeness but do not discuss them due to the small sample size (N=157) and heterogeneous composition of this group.

Overall, these results indicate that the adjusted estimates with our preferred measure (the continuous predicted probability of CPS involvement) tend to be either modestly larger (in most cases) or similar in magnitude (in a few cases) to those with the unadjusted measure, and that this pattern holds for both the full sample and the subgroups. This suggests that, at least with respect to young adult disconnection, estimates using self-reported (unadjusted) CPS involvement may be biased toward zero and that using adjusted measures of CPS involvement may produce more accurate estimates. Notably, the pattern of differences in magnitude between estimates using the adjusted binary measures and the self-report measure is less consistent than that found when comparing estimates using the predicted probability (adjusted continuous) measure and the self-report measure.

Our second model includes a more extensive set of controls for family factors and parenting behaviors that are likely to be correlated both with both CPS involvement and disconnection in young adulthood: whether the mother reported ever experiencing severe material hardship; ever being income poverty; ever being in poor health; experiencing IPV; ever using drugs; ever being involved with the criminal justice system; engaging in physically

aggressive parenting behaviors (number of waves); and engaging in neglectful parenting behaviors (number of waves). The results, in the bottom panel of Table 2, suggest that controlling for these additional variables attenuates the associations between CPS involvement and disconnection, but does not eliminate them. Moreover, the addition of these controls has no differential impact on the relative estimates produced when the unadjusted and adjusted CPS involvement measures are used.

Our models thus far do not consider depth of CPS involvement. Most children involved with CPS are never removed from home, but a small share (about 15%) experience removal. To provide evidence as to whether associations of CPS involvement with disconnection vary by depth of involvement, we estimated models using our unadjusted and (binary) adjusted CPS-involvement measures, in which we differentiate children who were CPS involved but never placed out of home and children who were CPS involved and ever placed out of home. These models include the full set of basic and additional controls. The results, shown in Table 3, indicate that, in general, for the full sample and for Black young adults, the association between CPS involvement and young adult disconnection is smaller for those who were involved with CPS but never placed than for those who were placed, regardless of whether the unadjusted or adjusted CPS-involvement measures are used, but in the vast majority of cases the coefficients for the two groups do not statistically differ at $p<.10$. For Hispanic families we find that the association between CPS involvement and disconnection to be smaller for those who were not placed out of home than for those who were, whereas we find the opposite pattern when using the adjusted CPS measures; however, in no cases do the coefficients for the two groups statistically differ. Finally, for white families we find the association between CPS involvement and disconnection to be smaller for those who were not placed out of home than for those who

were regardless of whether the unadjusted or adjusted measures are used; again, however, the coefficient for the two groups are never statistically different. In short, we find no consistent evidence that the relative pattern of the estimates produced when using the unadjusted and adjusted CPS involvement measures changes substantially when taking placement into account for White or Black young adults, but some suggestive evidence that it may for Hispanic young adults.

Finally, it is possible that the association between CPS involvement and young adult disconnection may vary by the age at which CPS involvement first occurs given considerable theory and empirical evidence suggesting that young children are particularly sensitive to adverse experiences (Council et al. 2000; Shonkoff et al. 2012; Lanier et al. 2018). For this reason, we also estimated models allowing the association between CPS involvement and later disconnection to vary by the child age by which involvement first occurred. Specifically, we define a set of mutually exclusive indicators for: first investigation by age 5; first investigation between ages 5 and 9; first investigation between ages 9 and 15; and no report of investigation by age 15 but the young adult or PCG reported having an investigation between the young adult's birth and age 18 in the age 22 interview. The results, shown in Table 4, reveal little evidence of statistically significant differences in associations between CPS involvement and disconnection by age at first CPS investigation, regardless of whether the unadjusted or adjusted measures are used. Nor do they suggest consistent differences in patterns of association by age at first CPS investigation when the unadjusted or adjusted measures are used for the full sample or for any of the racial and ethnic subgroups.

Discussion

In this study, we develop and test a Bayesian method for adjusting for underreporting of CPS involvement by FFCWS families. We then demonstrate the utility of the adjusted measures by estimating associations between CPS involvement and young adult disconnection using the unadjusted and adjusted measures and assessing differences in the estimates produced. We interpret such differences as indicative of the unadjusted (self-reported) measures potentially resulting in biased estimates of relations of CPS involvement with precursors thereto and consequences thereof.

This endeavor is particularly important given that limited data are available for comparing children and families involved with CPS to their non-CPS-involved peers. FFCWS provides unique opportunities to examine the causes and consequences of both child maltreatment and CPS involvement by offering detailed longitudinal data, measured at regular intervals, and spanning social, demographic, contextual, biological, and economic factors, including parenting behaviors, CPS involvement, and child health and development, for a population-based sample. Indeed, it is the only large-scale population-based study in the United States to enable such analyses. However, CPS involvement in FFCWS is self-reported by youth, their parents, and primary caregivers, and reflects substantial underreporting relative to national CPS involvement prevalence estimates. This underreporting does not appear to be random. As such, it has the potential to bias estimates of relations of CPS involvement with potential causes and consequences thereof.

Our Bayesian approach, which incorporates age-, race/ethnicity-, and child's state of birth-specific priors on the prevalence of CPS involvement derived from NCANDS national administrative data, as well as observed child and family characteristics that are associated with CPS involvement risk, enables us to adjust for underreporting for the full FFCWS sample and

demographically defined subgroups thereof. Our adjustments result in CPS involvement rates in FFCWS that are considerably more aligned with, though modestly higher than, national estimates. Indeed, our weighted adjusted cumulative CPS involvement rate by age 18 is 18 percent higher than the national estimate for the full sample when adjusting for differences in the racial and ethnic compositions of FFCWS and NCANDS, and 19 percent, 13 percent, 16 percent higher than national estimates for the non-Hispanic White, non-Hispanic Black, and Hispanic samples, respectively.

That our adjusted estimates are larger in magnitude than national estimates attests to the efficacy of our approach: FFCWS is comprised of a relatively economically precarious urban sample of disproportionately low-income families that are more likely than higher-income families to experience CPS involvement; it is also disproportionately comprised of racially and ethnically marginalized families that are more likely than White families to experience CPS involvement. Moreover, a comparison of the characteristics of families who did not self-report being CPS involved but whose characteristics led to a high predicted likelihood of CPS involvement such that they were re-classified as being CPS involved in our adjusted binary measure thereof, suggests that these families (those who likely under-reported being CPS involved) were less socially and economically disadvantaged than, but engaged in similar rates of physically aggressive and neglectful behaviors as, families that self-reported being CPS involved. Mothers in families that were reclassified were also less likely to have been U.S.-born than those who self-reported CPS involvement. Thus, underreporting of CPS involvement in self-reported data, at least in FFCWS, appears to be systematically concentrated among more advantaged families and those with non-U.S. born mothers who are engaged in behaviors that are highly related to child abuse and neglect and, thereby, risk of CPS involvement.

To demonstrate the utility of our adjusted indicators relative to the unadjusted self-reported indicators of CPS involvement, we estimated associations between CPS involvement by age 18 and young adult disconnection at age 22, separately, using each measure. Our findings indicate that estimates using unadjusted self-reported measures of CPS involvement are likely to be biased—in this case toward zero, such that they likely understate the magnitude of association between CPS involvement and disconnection among young adults—although the magnitude and direction of such bias may differ for estimates between CPS involvement and other potential consequences or causes thereof. Notably, the magnitude of such bias in our empirical application to young adult disconnection tended to be largest when comparing estimates produced using unadjusted measures to those produced using the predicted probability of CPS involvement derived from our adjustment model (relative to those produced using bivariate adjusted measures). The general pattern of our results is robust to accounting for depth of CPS involvement (whether the child was never or ever placed out of home) and the child's age at the time of the first CPS investigation. For future research, we recommend using the predicted probability (continuous measure) as the preferred adjusted measure given that it both leverages more information than and accounts for uncertainty in actual CPS involvement better than the bivariate indicators.

Substantively, our disconnection analyses indicate that, after accounting for a wide range of family characteristics and behaviors, there appears to be a statistically significant association between childhood CPS involvement and young adult disconnection only among Hispanic young adults, which results in a statistically significant association in the overall sample, but not among White or Black young adults. This finding persists when using both the unadjusted and adjusted CPS involvement measures, although the magnitude of association varies. It is also robust to

accounting for depth of CPS involvement and the child's age at the time of the family's first CPS investigation.

That we find an association of CPS involvement with disconnection among Hispanic young adults is consistent with prior scholarship that has documented an inverse relation between CPS involvement and wellbeing among Hispanic children and families (Garcia et al. 2012; John 2024). Scholars have noted disparate outcomes for Hispanic children, relative to non-Hispanic White children, in the U.S. child welfare system (John 2024). Hispanic children are more also likely than non-Hispanic White children to experience out-of-home placement and to spend long periods in foster care (Ayón 2011; Ayón et al. 2011; Davidson et al. 2019) (although we find no evidence of significant differences in the association between CPS involvement and disconnection by out-of-home placement status in our analyses). These patterns for Hispanic children have been attributed to limited access to resources, cultural incongruencies with caseworkers, and a high likelihood of low-income status—especially for immigrant families (Garcia et al. 2012; Chenot et al. 2019; John 2024). They may also reflect, at least in part, that CPS efforts may be attenuated by constrained resources and culturally incongruent services. Considering that Hispanic children represent the fastest growing population of children in the child welfare system (Ayón 2011; Ayón et al. 2011), it is important for future research to understand both risk and protective factors that may influence their successful transition into adulthood following contact with CPS.

Self-reported (unadjusted) CPS-involvement measures are currently available in FFCWS public release data. In future FFCWS data releases, we will make publicly available both the predicted probability that a child's family had been CPS involved by a given assessment age (5,

9, 15, and 18; our preferred measure of CPS involvement) and the adjusted (fixed) indicator of CPS involvement by each assessment age, to facilitate their use by other researchers.

We see two important directions for future research. First, future research should further test the accuracy of the adjusted measures vis-à-vis the self-reported measures. One direct test, which we hope to conduct in future work, is to match children in FFCWS with children in state administrative data systems and examine the extent to which the CPS data for a given child in FFCWS does or does not line up with the administrative data for that child. Such research will only be possible in states where administrative data is available to researchers for the period covering FFCWS focal children's entire childhoods, and where permission can be obtained to link such data to FFCWS. But certainly, other tests are possible. For example, researchers could conduct a falsification test, estimating the effect of both the adjusted and self-reported measures on outcomes that should not be plausibly related to CPS involvement to confirm that both (and, in particular, the adjusted measures) are unrelated to those outcomes.

Second, future research should make use of FFCWS data to further examine predictors and outcomes associated with CPS involvement and, when possible, leverage quasi-experimental identification strategies to examine the causes and consequences of CPS involvement. We recommend that, in such research, investigators test the sensitivity of their results to using the adjusted measures of CPS involvement versus the self-reported measures. Our recommendation, in the event results differ, would be to use the adjusted measures and, in particular, the continuous measure of each child's predicted probability of CPS involvement, which takes into account uncertainty in the estimation, because the self-reported measures are certainly underreported. Applying both and reporting the sensitivity of results will advance the knowledge

base and inform future research, including providing additional information on the likelihood and direction of potential bias when self-reported measures of CPS are employed.

Several limitations should be considered in interpreting and contextualizing our findings. First, the NCANDS cumulative prevalence of CPS involvement (both national and state-level), which we use as priors to adjust CPS involvement reported by the FFCWS study participants, is not computed for a perfectly comparable cohort to the FFCWS sample. This poses limitations to the precision of our adjustment. Second, although we employ the best available empirical approach for adjusting for underreporting of CPS involvement, including estimating a predicted probability of involvement and adjusted indicators of involvement for each FFCWS child by each assessment age, we cannot be certain that we have correctly identified which children did and did not actually experience CPS involvement. For this reason, our preferred measure is the continuous measure of the predicted probability of involvement (rather than a binary indicator of involvement, which is more prone to error). Third, as noted earlier, we assume that there are no false positives in the self-reported data; this assumption should be tested in future research (e.g., by comparing self-reported CPS measures to child-specific administrative data on CPS involvement). As discussed above, we also call for future research testing the accuracy of the adjusted measures vis-à-vis the self-reported measures. Fourth, our analyses of associations of CPS involvement with young adult disconnection are purely descriptive in nature and do not lend themselves to causal interpretation. We recognize that there are many factors associated with both CPS involvement and young adult outcomes that we are not able to capture, even in an extremely rich dataset such as FFCWS. Fifth, we examine associations of CPS involvement with only one young adult outcome, disconnection. This is, arguably, a core component of a successful transition to adulthood but is, of course, not the only outcome of import. We include it

here for illustrative purposes but, as noted above, future research should examine additional outcomes, such as health, mental health, criminal justice involvement, and family formation, and also conduct falsification tests by examining the associations of unadjusted and adjusted measures of CPS involvement with outcomes that should not be related thereto. Relatedly, future research should leverage the longitudinal data in FFCWS to examine potential mechanisms linking CPS involvement with young adult wellbeing. Moreover, CPS is not the only consequential system with which young people may be involved. Many of the young people in our sample have had contact with the police or other parts of the criminal legal system during childhood, adolescence, and young adulthood, and some young people have been involved with both CPS and the criminal legal system. Future research should also examine the associations between multi-system involvement and outcomes for young adults.

Despite these limitations, this study illustrates the utility of employing Bayesian methods to correct for underreporting of CPS involvement by survey respondents and provides evidence that doing so may illuminate potential bias in empirical analyses using underreports thereof. Indeed, we tend to find differences in the magnitudes of associations of CPS involvement with young adult disconnection using the adjusted measures and unadjusted measures. These differences are particularly important as systematic measurement error in observational data may lead to biased estimates of associations or causal effects.

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