

The Effects of Child Poverty Reductions on Child Protective Services Involvement

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ABSTRACT In this study, we use microsimulation methods to estimate the reduction in child protective services (CPS) involvement resulting from implementation of three of the policy packages from a recent National Academy of Sciences proposal

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to reduce child poverty, including the introduction of a child allowance and expansions to the earned income tax credit, the Supplemental Nutrition Assistance Program, and the federal minimum wage. We find that the policy packages have the potential to reduce CPS investigations by 11.3–19.7 percent annually. Moreover, our results are suggestive of a substantial reduction in racial disproportionality in CPS involvement. We estimate an 18.7–28.5 percent reduction in investigations for Black children and 13.3–24.4 percent for Hispanic children, compared with 6.7–13.0 percent for White children. Our results indicate that the nontrivial improvements in child safety accruing from any of the three policy packages should be considered in the calculus of policy implementation.

INTRODUCTION

Child protective services (CPS) involvement is extremely common among children in the United States. In 2019 (the most recent year for which data are available), 4.4 million reports of alleged maltreatment were filed with CPS on behalf of 7.8 million children (HHS 2021). Recent estimates indicate that the cumulative incidence of CPS involvement is much higher than official annual statistics suggest: roughly 37 percent of all US children and more than half of Black children experience one or more CPS investigations by the age of 18 (Kim et al. 2017). Researchers and practitioners have long observed that disadvantaged children are overrepresented in the child welfare system, as are Black, American Indian/Alaskan Native children (Berger and Waldfogel 2011; Doidge et al. 2017; Eckenrode et al. 2014; Esposito et al. 2017; Paxson and Waldfogel 1999, 2002, 2003; Pelton 1987, 2015; Slack et al. 2011; Wolock and Horowitz 1979; Yang 2015). Moreover, recent research suggests that the relation between income and CPS involvement is likely causal, implying that, all else equal, increased household income should directly reduce risk of CPS involvement (Cancian, Yang, and Slack 2013; Wildeman and Fallesen 2017; Berger et al. 2017).

The 2019 National Academy of Sciences (NAS) consensus report *A Roadmap to Reducing Child Poverty* (Duncan and Le Menestrel 2019; henceforth “NAS report”) includes four proposed policy packages that the NAS estimates would reduce child poverty by 19 (package 1) to 52 percent (package 3) within 10 years.¹ The NAS committee employed micro-simulation models to estimate the reductions in child poverty likely to result from the moderate-to-large expansions of existing social welfare

1. See table A1 for a detailed overview of the proposed packages of policies, their costs, and their anticipated reductions in child poverty.

programs and the introduction of new programs, including a universal child allowance. The microsimulations accounted for competing static and behavioral effects that might result from these policy changes, such as changes in labor market participation and income that would, in turn, affect benefit eligibility.

This study builds on the NAS report by simulating the effects of increased income under three of the four proposed policy packages on changes in CPS investigations.² We first simulate the household-level effect on income of three of the four NAS-proposed policy packages in the Current Population Survey (CPS-ASEC), following the NAS methodology. We then use administrative data from the National Child Abuse and Neglect Data System (NCANDS) to compute state-by-year-level probabilities of children's likelihood of experiencing a CPS investigation by child race or ethnicity and by child age, and we assign these probabilities to children observed in the CPS-ASEC. Next, we further weight them by sociodemographic characteristics associated with differential risk for CPS involvement. Finally, we use estimates of the causal effect of income on CPS involvement from the most rigorous studies to date to simulate the reduction in CPS involvement that would occur based on the simulated changes in household income under each policy package.

THE LINK BETWEEN INCOME AND CPS INVOLVEMENT

Child maltreatment is defined as (a pattern of) parenting or caregiver behaviors (actions or omissions) that pose an immediate or imminent risk of harm to a child. The federal statutes set forth in the 1974 Child Abuse Prevention and Treatment Act have been adapted by states and localities differently, resulting in varying definitions of abuse and neglect. States have the autonomy to decide under what circumstances investigation,

2. We focus on packages 2, 3, and 4 because package 1 includes universal implementation of the Work Advance program, which is entirely tied to male workforce participation. As such, the benefits of the program are likely to predominantly be realized for children living in two-parent households. In contrast, research indicates both that children living in single-mother families are disproportionately represented among CPS-involved families and that single-mother families are particularly sensitive to changes in income with respect to CPS involvement (see, e.g., Berger et al. 2017; Cancian et al. 2013). Thus, the antipoverty benefits to CPS involvement from package 1 would inherently be concentrated among those families that are least likely to encounter CPS.

substantiation,³ removal, placement into out-of-home care, or termination of parental rights are merited. Though most state statutes define neglect in terms of parental failure to meet children's material needs "for reasons other than poverty alone," deciding whether neglect arises from poverty alone can be difficult for caseworkers and courts (Children's Bureau 2018). As discussed below, low-income and poor families often lack the financial and material resources to provide state-defined levels of adequate care, structurally placing them at higher risk of CPS involvement, particularly regarding neglect allegations. Low-income and poor families may also face an increased risk of CPS encounters if they interact more frequently than more advantaged families with mandated reporters through social service program participation.

CPS involvement begins with a hotline referral in which a reporter alleges abuse or neglect. However, data on such referrals are not available in any nationally representative data system. Thus, we focus on CPS referrals that are subsequently followed up with investigation. Although state statutes vary in terms of the defined set of omissions and commissions that constitute maltreatment, a CPS investigation indicates that the allegation was credible and substantial enough to render a CPS response regardless of parental intent (Merritt 2020). Following an investigation, caseworkers decide whether to substantiate and, in high-risk cases (and often in collaboration with supervisors and family courts), whether to remove a child from the home. It is important to note that child maltreatment and CPS involvement are clearly separate—though overlapping—phenomena, and that CPS involvement is likely characterized by both over- and underinclusion of potentially abusive or neglecting families. Nonetheless, CPS investigations represent a reasonable and widely analyzed proxy for maltreatment risk. Despite the fact that such investigations can be highly intrusive and potentially traumatic for families, they are also extremely relevant to policy and practice and occur at substantial social and financial cost to the public. For these reasons, and because of limited availability of data that capture detailed income data, parental behaviors, and CPS involvement for families, our analyses focus specifically on CPS investigations.

3. Though not all maltreatment allegations are subject to an investigation, those that receive a disposition wherein the investigator concludes that the child was either not maltreated or that the allegation was substantiated. Here and throughout, the term "substantiation" is used to indicate cases in which the alleged maltreatment was substantiated or the child faced immediate risk of harm.

Low income and poverty are theorized to have both direct and indirect effects on child maltreatment and associated CPS involvement (see, e.g., Berger and Waldfogel 2011). First, limited economic resources may directly affect parental investments in children via food, clothing, medical care, education, and safe and stable housing and child-care arrangements. Low-income and poor families may simply be unable to afford the necessary goods and services to meet children’s fundamental needs in these areas. Failing to meet such needs falls within legal definitions for neglect in most states and may place such families at risk of CPS involvement, particularly for child neglect, even if state law includes a “for reasons other than poverty alone” clause (Rebbe 2018). Indeed, 76.1 percent of substantiated victims were neglected in 2020, with wide variation by state, from 0.8 percent in Vermont to 97.5 percent in Montana (Children’s Bureau 2021).

Limited economic resources are also indirectly linked to both physical abuse and neglect via psychosocial factors such as parental stress, cognitive load, and the quality of the caregiving environment—which, in turn, influence parenting behaviors (warmth, responsiveness, and harshness). Low-income and poor parents experience stress and undiagnosed or untreated mental health problems at higher rates than parents of higher-income families (Pedersen et al. 2019; Smith and Mazure 2021). Research suggests that this increased psychoemotional burden is not solely a function of limited economic resources, income loss, or income volatility, but also of adverse experiences such as systemic racism, intimate partner violence, substance abuse, and criminal justice system involvement, which are correlated with socioeconomic status, mental health issues, and parenting challenges (Berger and Waldfogel 2011; Fong 2017). Together, such factors may simultaneously increase risk of maltreatment and CPS involvement.⁴

In addition, it is possible that—conditional on equivalent parenting behaviors—low-income and poor families may be at disproportionate risk of CPS involvement because they experience greater surveillance and scrutiny by virtue of having more interactions with social services providers and other mandated reporters (Dettlaff et al. 2020; Merritt 2021). Mandated reporting policies require that those who frequently interact with vulnerable

4. See, e.g., Slack and colleagues (2011) for detailed overview of the broader economic effects on CPS involvement and Merritt (2020) for a comprehensive overview on the scope and consequence of systemic racism on the oppression of disenfranchised populations and their residual involvement with CPS.

families—such as teachers, social workers, law enforcement personnel, and clinicians—report suspected abuse and neglect. Although prior empirical studies (Chaffin and Bard 2006; Kim, Drake, and Jonson-Reid 2018; Drake, Lee, and Jonson-Reid 2009; Edwards 2019) have found mixed evidence that such surveillance bias, exposure bias, or class-based visibility bias is a core driver of higher levels of CPS involvement among disadvantaged families (see, e.g., Fong 2020; Merritt 2021), more rigorous investigation is needed, especially projects that consider variation in both maltreatment risk and surveillance by race or ethnicity.

Most low-income families provide safe and adequate care for their children, and only a fraction abuse or neglect their children or are involved with CPS. However, although prior literature has not sufficiently disentangled the range of mechanisms linking income with child maltreatment, scholars consistently identify substantial associations among low income or poverty, maltreatment behaviors, and CPS involvement. This linkage suggests that antipoverty policies might serve as a lever for preventing abuse, neglect, and CPS involvement. As we discuss below, a robust literature documents a vast range of historical and contemporary policies, practices, and institutions in the United States that enable structural racism and promote segregation and large disparities in income and wealth, which are likely core drivers of racial and ethnic disproportionality in CPS involvement.

Within this context, an extensive body of research shows that both family-level indicators of poverty, such as low-income status and economic hardship (Yang 2015; Wolock and Horowitz 1979; Doidge et al. 2017; Slack et al. 2011; Millett, Lanier, and Drake 2011; Font and Maguire-Jack 2020; Hussey, Chang, and Kotch 2006), and aggregate-level indicators of neighborhood, county, and state poverty levels are linked to CPS involvement (Eckenrode et al. 2014; Esposito et al. 2017; Kim and Drake 2018; Klevens et al. 2015; Maguire-Jack and Font 2017; Paxson and Waldfogel 1999; Paxson, Waldfogel, and Hall 2003; Smith, Kay, and Womack 2017). Reports alleging child maltreatment and, particularly, child neglect are disproportionately common among low-income families (Bath and Haapala 1993; Putnam-Hornstein and Needell 2011; Berger and Waldfogel 2011; Pelton 1987; Merritt 2020; Fong 2020; Bullinger, Lindo, and Schaller 2021), and existing research suggests that this link is only partially attributable to parental characteristics (Slack et al. 2004; Trickett et al. 1991; Berger 2004; Slack et al. 2011; Yang 2015), suggesting that substantial resource constraints lead directly to an increased likelihood of CPS involvement. Moreover, although international

studies of links between income and child maltreatment are limited, and the predominance of evidence has come from English-speaking countries (especially the United States), current evidence indicates that low income, poverty, and community disadvantage are “the most consistent and strongest statistical predictors of having an open child protection case” across all wealthy countries (Cameron and Freymond 2006, 11).

An ongoing challenge in empirical analyses, however, is isolating the independent effect of income on child maltreatment or CPS involvement from that of the range of other factors that may codetermine them.⁵ To this end, a modest body of research leverages changes in cash welfare policy facilitated by the Personal Responsibility and Work Opportunity Reconciliation Act of 1996 [U.S. Public Law 104–193] (PRWORA) to examine how plausibly exogenous changes in income are associated with CPS involvement. Though results may not be generalizable to all poor families (and PRWORA reforms likely changed the composition of the welfare-involved population), such studies have largely found decreased welfare generosity to be associated with increased CPS involvement, particularly among single-parent families, at both the family (Fein and Lee 2003; Slack, Lee, and Berger 2007; Slack et al. 2003) and state levels (Paxson et al. 2003; Swann and Sylvester 2006).⁶ In this vein, a rigorous quasiexperimental study of welfare-involved Danish families experiencing a policy change that resulted in a \$400 decrease in monthly income, on average, found the reform to be associated with a 25 percent increase in out-of-home placements (Wildeman and Fallesen 2017).

Several recent studies have examined the effects on CPS involvement of changes in income and earnings resulting from a broad range of exogenous policy and contextual sources of variation. Without a national data set that

5. Several studies compare CPS involvement among families receiving cash assistance, which minimizes selection effects driven by differences between higher- and lower-income families; results generally suggest higher rates of CPS involvement among the more disadvantaged families and communities (Nam, Meezan, and Danziger 2006; Needell et al. 1999). A related group of studies compares CPS involvement among welfare-eligible or economically similar families by public assistance program enrollment status, finding that welfare-involved families, especially single-mother families, face a higher risk of CPS involvement than those who are not enrolled (Courtney et al. 2005; Jones and McCurdy 1992). However, these studies cannot rule out that there may be unobserved differences between enrolled and unenrolled families, such that their estimates do not lend themselves to causal inference.

6. See also Puls and colleagues (2021) for a recent analysis of associations between state-level spending on social welfare programs and state-level CPS involvement rates.

captures both CPS involvement and family income directly, geographically aggregated data (e.g., county-level rates of CPS involvement) enable researchers to adopt rigorous approaches to identification of changes in income. Though aggregated measures of CPS involvement are invariably prone to bias because of ecological fallacy (as the risk of CPS involvement is not the same for all families in a county), the fact that changes in income are exogenously identified minimizes bias from other, endogenous sources of income. For example, comparing CPS involvement risk across parents with and without wage increases might capture both the effect of an increase in income as well as other, unobservable factors—such as punctuality and performance—that might simultaneously drive CPS outcomes. Moreover, identifying exogenous changes in income en masse via expansions to social safety-net policies and programs has the advantage of minimizing bias resulting from the unobserved cross-family differences, conditional on uniform responses by eligible families. For example, several studies harness state- and year-level differences in earned income tax credit (EITC) policies (which directly influence income for low-income working families), finding links between more generous EITC benefits and lower rates of CPS referrals and placement into foster care and fewer instances of abusive head trauma, a severe consequence of physical abuse (Biehl and Hill 2018; Klevens et al. 2017; Rostad et al. 2020; Kovski et al. 2021). Others have taken a related approach, harnessing the exogenous variation in income arising from state and federal policies, including the minimum wage (Raissian and Bullinger 2017), eviction and home foreclosure rates (Frioux et al. 2014; Bullinger and Fong 2021), and access to the Supplemental Nutrition Assistance Program (SNAP; Carr and Packham 2021; Bullinger, Fleckman, and Fong 2021). Others identified changes in disposable income resulting from macroeconomic factors, such as gasoline prices and business cycles (McLaughlin 2017; Brooks-Gunn, Schneider, and Waldfogel 2013; Paxson and Waldfogel 1999), demonstrating the economic vulnerability of families at risk of CPS involvement. All else equal—meaning that all other potential increases and decreases in CPS involvement have been controlled or otherwise accounted for—the resultant estimates represent the average effect of a change in income for families living in the respective geographic unit of aggregation. To the extent that bias arising from endogenous mobility is minimized, the aggregated approach provides a work-around in the absence of nationwide data capturing both CPS involvement and family income. Despite the ecological fallacy limitation, the sum of the findings in the

abovementioned papers can be broadly suggestive of a causal relationship, complementing the individual-level analysis discussed previously.

A related literature adopts a similar aggregated approach to examining associations between rates and conditions of employment with CPS involvement. Results here are largely mixed, with studies finding both positive and negative effects in measurably different contexts (Raissian 2015; Nguyen 2013; Frioux et al. 2014; Slack et al. 2004; Wood et al. 2012; Schenck-Fontaine, Gassman-Pines, and Hill 2017; Brooks-Gunn et al. 2013; Bullinger, Lindo, and Schaller 2021). Moreover, there appears to be considerable heterogeneity by gender. For example, results from several studies indicate that higher rates of male employment are associated with fewer reports alleging maltreatment—physical abuse in particular—and higher rates of female employment are associated with greater maltreatment reports (Paxson et al. 2003; Cherry and Wang 2016; Gillham et al. 1998). Similarly, a recent study leveraging plausibly exogenous changes in unemployment because of gender-specific mass layoffs at the county level finds no overall effects of unemployment rates on CPS involvement. However, mass layoffs in male-dominated industries are associated with increased CPS reports, whereas mass layoffs in female-dominated industries are associated with decreased reports (Lindo, Schaller, and Hansen 2018). Another recent study employing a Bartik shift-share instrument to identify changes in employment finds a large, positive effect of unemployment on substantiated CPS reports for neglect, but also that this effect is partially mitigated by extensions in unemployment insurance benefits (Brown and De Cao 2020). Although these studies point to plausible causal narratives, the possibility of the ecological fallacy precludes causal interpretation of their results (particularly at the individual level), in part because unobserved factors may account for differences in trends across counties or states. Likewise, although country-level comparisons note similar patterns, this approach similarly suffers from the same limitations (Gilbert et al. 2012).

Our analyses simulating the effects of income on CPS involvement require robust causal estimates of the link between them. Two studies have harnessed rigorous quasiexperimental (Berger et al. 2017) and experimental (Cancian et al. 2013) methods to produce plausibly causal estimates of the effect of income on CPS involvement among disadvantaged US families, providing the most credible such estimates to date. Although both studies are limited in terms of generalizability to the full population

of US children, in that each employed a relatively disadvantaged sample of (primarily) low-income single-mother families, these are precisely the family types that are most likely to experience both poverty and to be involved with CPS (see, e.g., Wimer et al. 2021).

Berger and colleagues (2017) harnessed state-level variation in the generosity of the EITC and an instrumental variables strategy to identify the effect of a \$1,000 increase in income on neglectful parenting behaviors and CPS involvement among a disadvantaged sample of urban, unmarried mothers with incomes below \$45,000.⁷ They found a \$1,000 increase in income to be associated with a 3–4 percent decline in neglectful parenting behaviors and an 8–10 percent reduction in CPS involvement. Cancian and colleagues (2013) identified a change in income using an experiment in which families involved in the Temporary Assistance for Needy Families (TANF) program were randomized into a treatment group that received a full child-support pass-through and disregard (allowing the mothers, rather than the government, to retain the payment that is then disregarded in the determination of public benefits) or a control group that received a partial pass-through (who receive smaller payments without benefit calculation disregards). Families in the experimental group realized an increase in child-support income of upward of \$101 annually, on average, and were nearly 2 percentage points (10 percent) less likely to be subject to a CPS referral that is screened-in for an investigation (also known as a CPS report), implying that even a relatively small increase in income may have a significant effect on CPS involvement.

Notably, the magnitude of Cancian and colleagues' (2013) estimate of the effect of increased income on reduced CPS involvement is substantially—nearly 10 times—larger than that of Berger and colleagues (2017). This difference may reflect the fact that Cancian and colleagues' (2013) study was limited to (primarily) single mothers participating in TANF, a particularly disadvantaged group, or that their estimates are also capturing spillover effects of treatment group assignment on other aspects of family well-being. For example, in addition to receiving greater child-support amounts, treatment group families also experienced higher levels of paternity establishment, smaller TANF benefits, and greater personal income (total of earnings,

7. The authors measured neglectful parenting behaviors using 11 indicators, including children bearing witness to intimate partner violence; parental substance abuse; parental engagement with criminal activities; and parental inability to properly provide the child with clothing, food, shelter, and medical care.

child support, and benefits; see, e.g., Meyer et al. 2001). Thus, given both the possibility that Cancian and colleagues' (2013) estimate may reflect the influence of other factors, rather than solely the effect of income, and given the potentially implausibly large magnitude of their estimate, we do not use it to simulate the causal effect of income on CPS involvement in our primary simulations. Rather, we use Berger and colleagues' (2017) more conservative estimate. However, we also present results from supplemental simulations in which we use Cancian and colleagues' (2013) estimate for the least-advantaged families, given both the robust design of the study and the possibility that the larger estimate reflects the particularly large effect of additional income on reduced CPS involvement for low-earning families.

RACIAL DISPROPORTIONALITY IN CPS INVOLVEMENT

It is well established that families of color—particularly Black and American Indian/Alaskan Native—are overrepresented at all stages of CPS involvement relative to their prevalence in the general population (Pelton 1994; Wildeman and Emanuel 2014; Kim et al. 2017; Billingsley and Giovannoni 1972). There are two predominant hypotheses for why such disproportionality exists (Drake et al. 2011; Merritt 2020; Pelton 2015). First, implicit (or explicit) bias in mandated reporting and decision-making might unnecessarily increase CPS involvement among children of color. Second, Black, non-Hispanic children might face a higher risk of maltreatment and CPS involvement arising from disproportional rates of poverty and material hardship because of systemic and structural inequality and racism. Indeed, a large literature attributes concurrent and intergenerational gaps in income and wealth to geographic redlining, education, lending, and other fundamentally racist policies (see, e.g., Chetty, Hendren, and Katz 2015; McIntosh et al. 2020; Shapiro and Oliver 1995; Shapiro, Meschede, and Osoro 2013).

With respect to the first hypothesis, a handful of studies—typically in the health-care arena—have attempted to account for differential underlying maltreatment risk by comparing the likelihood of a CPS report for children of different racial and ethnic backgrounds with similar injuries. Findings suggest a substantially higher likelihood that Black children are screened and reported for maltreatment by physicians (Jenny et al. 1999; Lane et al. 2002; Hymel et al. 2018). With respect to the second hypothesis, several studies have found that racial disproportionality in CPS reports can be predominantly explained by racial differences in family

socioeconomic status (Font, Berger, and Slack 2012; Putnam-Hornstein et al. 2013; Drake et al. 2011). To the extent that this is the case, poverty-reduction policies have the potential to decrease racial disproportionality in CPS involvement precisely because Black families and other families of color are disproportionately likely to be low income and to be reported to CPS. Thus, even in a context in which expanded income supports do not function to reduce racial bias in reporting or CPS decision-making, they may nonetheless yield disproportionately large benefits for families of color. As such, in addition to showing population-wide effects of poverty reductions on CPS involvement, we also present separate estimates for White, Black, Hispanic, and “other” subpopulations. In addition, given stark differences in both low-income or poverty status and CPS involvement by parental education and marital status—the distributions of which differ by race or ethnicity in ways that are highly relevant to racial or ethnic disparities in income, poverty, and CPS involvement—we also present estimates for subpopulations of families defined by parental education and marital status. As we are unable to identify immigrant families in our data, our estimates are likely conservative for immigrant families and others who are disproportionately involved with CPS (see, e.g., Johnson-Motoyama and Wu 2018).

THE NATIONAL ACADEMY OF SCIENCES REPORT

Despite decades of progress, child poverty in the United States remains alarmingly high (Fox et al. 2015; Bitler and Hoynes 2016; Wimer et al. 2016). Scholars have shown growing attention to how policies can better reduce poverty among families with children (Pac et al. 2017; Fox et al. 2015; Bitler, Hoynes, and Kuka 2017; Chaudry and Wimer 2016; Wimer et al. 2021). To this end, the NAS report, with a target of halving childhood poverty within a decade, proposes four policy packages that are grounded in the most rigorous available research (Duncan and Le Menestrel 2019). The proposed policy packages include both reforms and expansions of existing programs, as well as the establishment of new programs. As noted above, we consider only packages 2–4 in our analyses. Below, we describe the core elements of these packages and associated behavioral adjustments in our simulation models. Details regarding the elements of each package are also summarized in table A1.

Package 2, the “work-based and universal supports” package, comprises two parts: (1) expanding the EITC and the child- and dependent-care tax

credit (CDCTC), two policies found to increase labor force participation for parents, and (2) establishing a universal monthly child allowance. The NAS estimates this package would reduce child poverty by 35.6 percent at an annual cost of \$44.5 billion. The EITC reform included would both increase the maximum credit and alter the phase-in rate so that recipients would be eligible for the maximum credit at a lower point in the earnings distribution. The reform to the CDCTC, which is a nonrefundable tax credit for working families with qualifying child-care expenses, would make the CDCTC fully refundable, increase its maximum credit value, and structure it so that the lowest-income families receive the maximum credit.⁸ Converting the child tax credit (CTC) into a universal monthly child allowance of a \$166 per month (\$2,000 per year) cash payment for each citizen-child under 17 is another core component of package 2.⁹

The NAS committee estimates that package 3—the “means-tested supports and work package”—would reduce child poverty by 50.7 percent at an annual cost of \$90.7 billion. This package includes the EITC and CDCTC expansions discussed above, as well as expansions to the Housing Choice Voucher Program (HCVP) and SNAP. The HCVP is a federal program that provides rental assistance for low-income households, and SNAP is a means-tested federal entitlement program that provides an in-kind benefit to assist families in purchasing groceries. Under current policy, most HCVP program participants pay 30 percent of their adjusted income for rent, with the remaining rent subsidized through a voucher funded by the US Department of Housing and Urban Development (HUD 2020). However, because of funding limitations, only 24 percent of households eligible for a voucher

8. Because the CDCTC is nonrefundable, most low-income families have historically not received the credit; families with annual incomes between \$100,000 and \$200,000 are most likely to have received it (Maag 2013).

9. In the years examined in this study (2013–17), tax filers with children were eligible for a maximum CTC of \$1,000 per child. The CTC phased in with earned income and began to phase out when adjusted gross income reached \$80,000 for single filers with dependents and \$110,000 for joint filers with dependents. The Tax Cuts and Jobs Act of 2017 increased the maximum CTC to \$2,000 per child. However, the credit remained nonrefundable and conditional upon earnings. Specifically, it phased in with earnings at a rate of 15 percent per dollar of earned income and phased out when adjusted gross income reached \$200,000 for single filers and \$400,000 for joint filers. The 2021 temporary expansion under the American Rescue Plan Act made the CTC fully refundable and increased eligibility by eliminating the minimum earnings requirement. However, this expansion expired at the end of 2021.

receive one.¹⁰ Package 3 proposes increasing program funding to allow 70 percent of eligible nonrecipient families to receive a voucher. It also includes an expansion of SNAP, given research indicating that the SNAP benefit calculation is outdated and that more than half of SNAP recipients remain food insecure (Ziliak 2016; Coleman-Jensen, Gregory, and Singh 2014). Moreover, child benefits currently do not vary by age; the SNAP benefit for a 5-year-old is equivalent to that for an adolescent. To address these shortfalls, package 3 proposes to increase SNAP benefits for families with children by 35 percent, increase benefits for teenagers by an additional \$360 per year, and provide additional benefits of \$180 per child during the summer months to compensate for the absence of school meals.

Package 4—the “universal supports and work package”—includes EITC and CDCTC expansions (like packages 2 and 3) and the implementation of a child allowance (like package 2). However, it also includes increasing the minimum wage, providing an assured child-support benefit,¹¹ and eliminating the PRWORA immigration eligibility restrictions (which we do not model in our analyses, given the limitations of our data). The NAS committee estimates that package 4 would reduce child poverty by 52.3 percent at an annual cost of \$108.8 billion.

Note that the EITC expansion and child allowance proposal in package 4 are slightly different from those included in packages 2 and 3. The package 4 EITC reform would increase the credit by 40 percent for all eligible recipients while maintaining the existing phase-in and phase-out structure. The package 4 child allowance would provide a \$225 per month cash payment (\$2,700 per year) for each child and would phase out for families between 300 and 400 percent of the poverty threshold.¹² In package 4, the NAS committee also proposed raising the federal minimum wage from \$7.25 to \$10.25 per hour over 3 years and indexing it to inflation thereafter. In addition, the NAS report notes that more than half of contemporary

10. See Center on Budget and Policy Priorities (2022) for an analysis of low-income, at-risk renters: <https://www.cbpp.org/research/housing/three-out-of-four-low-income-at-risk-renters-do-not-receive-federal-rental-assistance>.

11. A publicly financed “assured child-support benefit” ensures that custodial parents receive a minimal, guaranteed child-support payment each month, regardless of the noncustodial parent’s ability to pay.

12. Consistent with the package 4 strategy of restoring eligibility to federal means-tested programs for currently nonqualified legal immigrants, the child allowance would also be provided to such families under package 4.

US children will spend some time living with a single parent and that, although child support is an important source of income for single-parent families, its receipt tends to be partial and inconsistent, particularly among the least-advantaged families (Pilkauskas and Cross 2018; Bumpass and Raley 1995); fewer than half of custodial parents receive all of the child support due them in a given year (Renwick and Fox 2016). The panel therefore proposed a guaranteed child-support benefit of \$100 per month, per child.

DATA AND METHODS

We employ a two-step microsimulation to predict how the three (of the four) proposed NAS policy packages, if enacted, would affect CPS investigation rates. In the first step, we replicate the simulations outlined in the NAS report using data from the 2013–17 CPS-ASEC, the household income survey data used by the Census Bureau to calculate annual poverty statistics. As in the NAS simulations, we assign transfer amounts according to the terms of each proposed policy package. At the same time, we also account for accompanying changes in benefit receipt, employment and earnings, and tax liabilities or refunds to predict changes in family income (and poverty) measured by the resources considered under the supplemental poverty measure (SPM). Unlike the official poverty measure (OPM), which considers only pretax cash income, the SPM also captures in-kind and cash transfers, as well as tax liabilities and credits; moreover, it adjusts for nondiscretionary out-of-pocket health-care, work, and child-care expenditures. In addition, the SPM thresholds are geographically adjusted, whereas OPM thresholds are not. Finally, the SPM accounts for resource sharing within a broader household unit, which, for example, includes cohabiting partners, who are excluded from the OPM unit. Thus, the SPM is thought to be a more precise measure of economic resources, in that it includes noncash safety-net and tax transfers, reflects geographic cost of living, and includes resources of a greater number of household members. (See Fox and colleagues [2015] and Short [2011] for additional details on the SPM and OPM and the differences between them.)

In the second step of our simulation, we use 2013–17 NCANDS data, the administrative data on CPS involvement reported by the states to the federal government each year, to estimate children’s probabilities of experiencing a CPS investigation in a given state and year based on their race or ethnicity and age. We then assign these probabilities to each child in

the CPS-ASEC and further adjust them by other family characteristics (i.e., parental employment, family income, parental education, social welfare program participation, family structure, and number of children) that are differentially associated with a family's risk of being investigated by CPS. Finally, we employ what we believe to be the best available estimate of the causal effect of income on CPS investigations, using various assumptions about the income groups to which it is relevant, to simulate the expected change in a child's likelihood of experiencing a CPS investigation that would result from the simulated change in their family's income.

We model both static changes in income associated with the various policy proposals and anticipated accompanying behavioral or dynamic effects. Static effects are direct and mechanical in nature. For example, the static effect of implementing a child allowance is the direct increase in income experienced by eligible families. Dynamic effects include anticipated behavioral responses resulting from the mechanical increase in family income, which may have implications for work effort, earnings, or benefit receipt and thereby also affect total family income. For example, receipt of a child allowance may induce changes in the number of people in a household who work, the number of hours they work, household composition, and family formation (including marriage) and fertility. Notably, although some of the policy changes proposed in the NAS report, such as expanding the EITC, are designed to increase employment and work hours, others, such as expanding the CDCTC, SNAP, and HCVP, as well as the introduction of a child allowance, have the potential to reduce employment (Hoynes and Rothstein 2017).

Although prior research suggests that behavioral responses to safety-net expansions in these domains tend to be small in the aggregate (Ben-Shalom, Moffitt, and Scholz 2011), accounting for them in our models serves to increase the accuracy of our estimates of the NAS-proposed policy packages on family income. Note that—following the methodology of the NAS report—our simulation models account only for labor market-related behavioral responses and not behavioral responses in other domains. In addition, it is important to acknowledge the possibility that increased public social welfare program participation under the proposed policy packages may be associated with increased surveillance by mandated reporters, which has the potential to increase risk of CPS reporting independent of parental behaviors. We expect that this is a minor concern, given that the proposed policy changes do not necessitate increased interaction with mandated

reporters. Nonetheless, there are no existing estimates that would allow us to account for this plausible effect in our simulations.

We replicate the behavioral adjustments made by the NAS committee in our simulations. For instance, for the EITC expansions, we increase employment and hours worked for single mothers and reduce employment for married mothers in response to greater EITC payments, based on findings from prior work (Eissa and Hoynes 2004; Hoynes and Patel 2018); behavioral effects for single and married men have not been identified. Our simulations also adjust for anticipated increases in maternal employment of 2 percent per 10 percent reduction in the cost of child care resulting from the CDCTC expansions (Blau 2003).

Notably, there is an ongoing debate about the potential labor supply response to a child allowance that results from the “substitution effect” as opposed to the “income effect” (Bastian 2022; Corinth et al. 2021; Goldin, Maag, and Michelmore 2021). Following the NAS approach, we model the income effect resulting from establishing a child allowance, but not the substitution effect. In a recent statement published by the *Washington Post*, two NAS authors, Hilary Hoynes and Robert Moffit, explained their decision to focus on the income effect and not model the substitution effect, stating, “Given the existing law, and the policy we were expanding, it was the assessment of our committee that the ‘income effects’ of the policy change would be more important and the substitution effect would be muted. Why? Because going from \$1,000 to \$3,000 is a large increase in income. And losing the 15 percent phase-in of the existing CTC—which has a small effect on the income gain from working—we regarded to be more modest” (Kessler 2021).

In the years we examine (2013–17), the CTC was much less generous than it is today; the maximum credit per child was \$1,000 (it was raised to \$2,000 in 2018). Several recent working papers examine the possible labor supply effects of converting the CTC to a child allowance in the context of current law (Bastian 2022; Corinth et al. 2021; Goldin et al. 2021). Each of these papers includes responses driven by both the potential income effect and the potential substitution effect. Compared with the years we study, however, the estimated substitution effect would, theoretically, be larger because the maximum baseline credit amount is lower than in the years on which these scholars focus. It is also important to consider the fact that existing studies estimate the substitution effect using simulation methods that rely on evidence studies of work-conditioned benefits written several decades ago, in a vastly different social policy context. Evidence suggests that

such elasticities have decreased over time and are also likely to be upwardly biased, particularly for women (see Bastian and Jones [2021] for a discussion of these issues). Estimates of the antipoverty effects of a child allowance are highly dependent on which elasticities are chosen. The NAS committee argues that small elasticities are most relevant. In contrast, Corinth and colleagues (2021) use much larger elasticities than any of the other existing studies and find substantially smaller antipoverty effects.¹³

As noted above, we use the NAS approach in our primary simulations. Specifically, we simulate a 0.5 percent reduction in employment for men, 1.2 percent reduction for married women, and 0.85 percent reduction for single women per 10 percent increase in income resulting from the benefit. We make parallel adjustments on the intensive margin, reducing work hours by 0.5, 0.9, and 0.7 percent, respectively, for each 10 percent increase in benefit-related income. However, we also test how the substitution effect could affect labor supply in our model using the approach outlined in Goldin and colleagues (2021), which applies midrange elasticities separately by parent sex and marital status. Using this approach, we estimate that an additional 137,000 parents, a very small subset of whose families would be involved with CPS, may stop working as a result of the substitution effect. Finally, we note that the advanced monthly CTC payments paid in 2021 offered the opportunity to study the labor supply response to a near-universal child allowance using empirical, observational data. Evidence suggests that the advance payments did not contribute to significant reductions in labor force participation in the short run (Ananat et al. 2022).

The child-support proposals and the HCVP and SNAP reforms may also potentially (likely modestly) disincentivize work effort. For the child-support assurance program, we follow the same approach as that for the child allowance, but limit such responses to single-parent families with an existing child-support order. Like the NAS committee, we employ prior research on the labor market effects of housing vouchers; we simulate a reduction in the employment of male and female heads of household (regardless of marital status) of 3.3 percentage points if their household gains a

13. It is also likely that the implementation of a child allowance in concert with the other proposed policy changes put forward in the NAS packages (e.g., the EITC and CDCTC expansion) may further mitigate the substitution effect. Given the ongoing debate about the appropriate application of such a response across several working papers, and the fact that the substitution effect is less relevant in the years we examine, we did not deviate from the methodology described in the NAS report in our primary results.

voucher (Jacob and Ludwig 2012).¹⁴ Furthermore, in light of research indicating that SNAP expansions are associated with reductions in work for single mothers at both the extensive and intensive margins (Hoynes and Schanzenbach 2012), we follow the NAS approach and model a 2.4 percentage-point reduction in employment and 64.3 fewer annual work hours per SNAP-recipient single mother. In addition, because the minimum wage increase may result in employers letting some employees go, we assume a 0.33 and 0.11 percent reduction in employment in response to the policy for directly affected adolescents and adults, respectively.

Each of these behavioral responses was determined based on individual policy reforms. The NAS committee needed a method for combining these responses when the various policies were implemented together in the proposed packages. The committee decided that, when a package had two policies that increased the number of parents participating in the labor force, the target number of new jobs for the package would be the midpoint between the number of people with a new job in any of the individual policy simulations and the sum of the numbers of new jobs across the policy simulations.¹⁵ They followed the same approach when a package caused job losses.¹⁶ They then used these new estimates of jobs gained and lost as a result of the combined policy changes to determine the net change in labor force participation for each policy package. We followed the same approach.

To calculate family SPM resources and the changes therein that are expected to accrue under the proposed policy packages, we used information in the CPS-ASEC on earnings, social welfare benefit receipt, other sources of income, household composition, nondiscretionary health-care and work-related expenses, and tax filing status. For example, consider a family of four (two adults, two children) in Texas in 2015 with \$15,000 in earnings, an EITC of \$5,548, a CTC of \$1,800, a \$1,000 CDCTC, and \$2,000 in out-of-pocket nondiscretionary medical and work-related expenses. The family's posttax, posttransfer income would be \$22,348, but would be reduced to \$20,348 after accounting for medical and work-related expenses. Given a

14. Note that these effects were applied only to new voucher recipients.

15. See Duncan and Le Menestrel (2019, app. F, 575) for a thorough description of this application.

16. The one exception to this rule was the minimum wage expansion, which was considered to have an employment effect independent of that associated with other policies; thus, jobs lost because of the minimum wage increase were added after determining the midpoint estimate of job losses resulting from the other policies in the packages.

corresponding 2015 SPM poverty threshold of \$21,851, the family would be considered poor. If NAS package 2 were implemented, however, they would experience increases in their EITC, CTC, and CDCTC of \$1,019, \$200, and \$1,000, respectively, such that the family's net income would increase by \$2,219, from \$20,348 to \$22,567, pushing them above the poverty threshold.

Although we were able to replicate most aspects of the NAS analysis using the 2013–17 CPS-ASEC, there are several critical differences in our approach. First, the NAS analysis used data that had been adjusted using the Urban Institute's Transfer Income Model (TRIM3). TRIM3 corrects for underreporting of income from government benefits and transfers reported in survey data and also adjusts for tax liabilities (Urban Institute 2022). Despite not using the TRIM3-adjusted data, our estimates of the reductions in child poverty associated with each of the NAS packages we model are very consistent with those in the NAS report. Second, NAS package 4 includes restoring social welfare program eligibility in SNAP, TANF, Medicaid, Supplemental Security Income (SSI), and other means-tested federal programs for nonqualified documented immigrants. We did not model this feature because our data did not include information about immigration status. However, as prior research suggests that immigrant families are underrepresented in the US child welfare system (Dettlaff and Earner 2012), we anticipate that the benefits of this reform would do little to change our conclusions. Finally, the NAS analyses use only 1 year of data (2015); we use 5 years of data (2013–17) in our analyses to capture the period after the Great Recession and to ensure that our sample sizes are adequate for disaggregation by family demographic characteristics within states.

As noted above, because CPS-ASEC data include no information on CPS involvement and NCANDS data include no information on family economic resources, for the second step of our simulation, we first assigned each child in the 2013–17 CPS-ASEC a baseline risk of CPS investigation calculated from NCANDS data based on a child's race or ethnicity, age, state, and year of observation. To calculate the risk of CPS investigation, we collapsed report-level NCANDS data into child-level observations.¹⁷ The child-level observations, which count CPS investigations annually, were then aggregated by state, year, child age (in years), and child race or ethnicity to measure

17. It is possible that children appear in multiple states in a single year. We cannot correct for this possibility, as child identifiers are assigned at the state level. However, this likely applies only to a small fraction of children.

the total number of CPS investigations for each demographic subgroup. For example, the number of reports for 1-year-old, White, non-Hispanic Texan children in 2015 differs from that of Hispanic 1-year-olds in Texas in the same year. To construct subgroup-specific CPS report risk, we divide the count of reports for each state-year-demographic subgroup by the population of children in that group. Therefore, we draw age- and race/ethnicity-specific population counts from Surveillance, Epidemiology, and End Results data, which are based on census-adjusted data available through the National Center for Health Statistics. We employed both information sources to compute our subgroup-specific unadjusted (raw) CPS investigation probabilities, which we then link to children observed in the CPS-ASEC in the same period, such that all children in a state, year, age, and race or ethnicity subgroup were assigned the same baseline probability of a CPS investigation.

Official maltreatment statistics from the most recent Fourth National Incidence Study of Child Abuse and Neglect (NIS-4) suggest that the risk of maltreatment varies according to child and family characteristics (Sedlak et al. 2010). We thus inflate children's baseline subgroup-specific CPS investigation probabilities according to the risk factors reported in the NIS-4. We assigned each child a weight as a function of the children's observed risk factors in the CPS-ASEC. Specifically, NIS-4 data indicate that children in households in which no parent is working have three times the risk of maltreatment as those living with a working parent, those in households with less than \$15,000 of annual income have five times the risk of higher-income children, those whose parents have less than a high school education have five times the risk of those with more educated parents, those whose households are participating in one or more social welfare programs have five times the risk of those whose households are not, those living with a single or cohabiting parent have five times the risk of those living with married parents, and those living in a household with four or more children have double the risk of those living in a household with fewer children.

Consider, for example, that a child living in a household without a working parent is three times more likely to be at risk of maltreatment than a child living with a working parent—if this was the child's only risk factor, their baseline probability of a CPS investigation was multiplied by 1.3, effectively inflating their risk by a factor of three. If the child is also in a low-income household, their CPS investigation probability was multiplied by 1.5. If a child has multiple risk factors, we added the inflation factors and divided by the total (15) plus one, so that each risk factor is weighted

appropriately. A child with all of the risk factors has the highest risk (capped at 1.0), and a child without any of the risk factors was assigned their unadjusted baseline probability. We prefer this adjustment method because it closely matches the risk observed in official statistics and is the most conservative among the adjustment strategies we considered. The resultant distributions of CPS investigation risk are shown by children's age and race or ethnicity in figures A1 and A2, respectively.

To simulate the changes in CPS investigation risk expected under each of the NAS policy packages, we first standardized the estimated effects of an increase in income on CPS involvement from Berger and colleagues (2017) to the effect relative to a \$1 increase in income. Specifically, Berger and colleagues estimate a 10 percent reduction in CPS investigation risk to result from an additional \$1,000 per year in income as identified by exogenous variation in EITC generosity, using a sample of unmarried families with incomes below \$45,000 per year. We then multiply the resulting effect-per-dollar multipliers by the simulated increase in SPM resources under each of the three packages. To account for the possibility that the impact of additional income is larger for lower-income families than their higher-income counterparts—in other words, that the relation between income and CPS involvement may not be linear throughout the income distribution—we constructed two specifications in which we limit the families to which we apply the multiplier based on income and marital status. For our restrictive specification, we assign the Berger and colleagues multiplier to single-parent families with less than \$50,000 in annual income, such that we would observe responses in CPS risk only for single-parent families with less than \$50,000 in annual income. All other families are not assigned a multiplier, such that their CPS risk does not change as the result of any of the packages of policies. This approach is our most conservative—it assumes that, for most families, CPS risk remains unchanged as a function of increased income. For our expansive specification, we assign the Berger multiplier to single-parent families with less than \$50,000 in income and to married families with less than \$25,000 in income, thus assuming an effect for very low-income married families, in addition to that for single-parent families.¹⁸

18. In supplemental analyses, we simulated an alternative expansive specification in which we applied the Cancian and colleagues (2013) multiplier to both single-parent and married families with less than \$20,000 in annual income, given that the study was restricted to TANF-receiving families, and the Berger and colleagues (2017) multiplier to single-parent and married families with \$20,000–\$50,000 in annual income.

We weighted all models using CPS-ASEC population weights based on the inverse probability of selection into the sample to account for complex sampling design. We report statistical significance of the point estimates using standard *t*-tests. As prior studies assume a linear relationship between income and CPS involvement, we compute standard errors based on the sampling distribution, incorporating weights to correct for sampling variation (Goedemé et al. 2012; Howes and Lanjouw 1998). Our primary approach assigns effect estimates to a range of eligible incomes to account for variation in the sampling distribution. However, as the estimated increase in income might capture both simulated income and variation in the CPS-ASEC population, we additionally report bootstrapped standard errors with 1,000 iterations for robustness. We present results from our simulations for US families with children as a whole and also for key population subgroups of families defined by race or ethnicity, parent education, and parent marital status.

RESULTS

As shown in table 1, the characteristics of our sample mirror those of the distribution of US children and their families. Approximately 13.8 percent of children in our sample are non-Hispanic Black, 24.9 percent are Hispanic, and the majority, 51.2 percent, are non-Hispanic White. The average estimated risk of CPS investigation is 4.4 percent, though there is considerable variation by child age and race or ethnicity (see figs. A1 and A2). Nearly 30 percent of sample households are headed by single parents, and a similar fraction are headed by an adult with no more than a high school diploma. Total SPM resources fall below the SPM poverty threshold for 16 percent of households, and mean SPM resources are nearly \$67,000 annually.

Figure 1 presents the results of our simulations estimating the percentage-point reductions in the SPM poverty rate associated with NAS packages 2, 3, and 4 compared with those of the NAS report. As the NAS results are specific to 2015, we produced our estimates for the combined 2013–17 sample, as well as for 2015. As the baseline child poverty rate in the TRIM3-adjusted NAS data is lower than the baseline child poverty rate in the unadjusted CPS-ASEC data for the same year, we examine absolute reductions in the poverty rate associated with each package, as opposed to relative reductions. Like the data used to calculate the impact of the policy packages in the NAS report, the partially adjusted data set had a lower

TABLE 1. Summary Statistics

	Mean/Frequency	SD
Children's demographics:		
Race/ethnicity:		
White	51.2%	
Black	13.8%	
Hispanic	24.9%	
Other	10.1%	
Age	8.65	5.20
Male	51.0%	50.0%
CPS investigation risk	4.4%	3.2%
Caregiver's demographics:		
Single	.27	.45
Education:		
<HS	6.0%	
HS	21.1%	
Some college	30.4%	
≥BA	42.5%	
SPM poverty	16.0%	36.7%
SPM resources	\$66,950.20	\$62,874.71

Note.—BA = bachelor of arts; CPS = child protective services; HS = high school; SPM = supplemental poverty measure. $N = 240,225$. Children 0–17 in the 2013–17 CPS-ASEC. Estimates weighted using the CPS-ASEC weight. Caregiver's education is represented as the highest achieved education among both caregivers, if more than one caregiver is present. SPM poverty and resources are calculated for the child's SPM "family" or "resource-sharing unit," which might include more individuals than just the child and caregivers (see, e.g., Renwick and Fox 2016).

baseline poverty rate than the unadjusted CPS-ASEC data. Overall, our estimated reductions in the SPM poverty rate under the NAS-recommended policy packages align relatively well with those of the NAS report, despite being slightly larger in absolute magnitude.

Table 2 presents simulated reductions in SPM poverty and SPM resources expected to result from implementation of NAS packages 2 through 4. We estimate a 5.4 (package 2) to 7.4 (package 4) percentage-point reduction in child poverty relative to a 16 percent SPM poverty rate at baseline, which translates to a child poverty reduction of 34–46 percent. The corresponding simulated increase in SPM resources ranges from \$2,430 to \$3,970 per year, or an increase of 3.6–5.9 percent of the mean SPM resources at baseline.

Our primary results regarding CPS investigations are shown in table 3. Compared with the baseline CPS investigation risk of 4.4 percent, we estimate that a child's risk of a CPS investigation would fall by 0.5 (package 2) to 0.7 (package 4) percentage points, about 11.3–15.6 percent, on

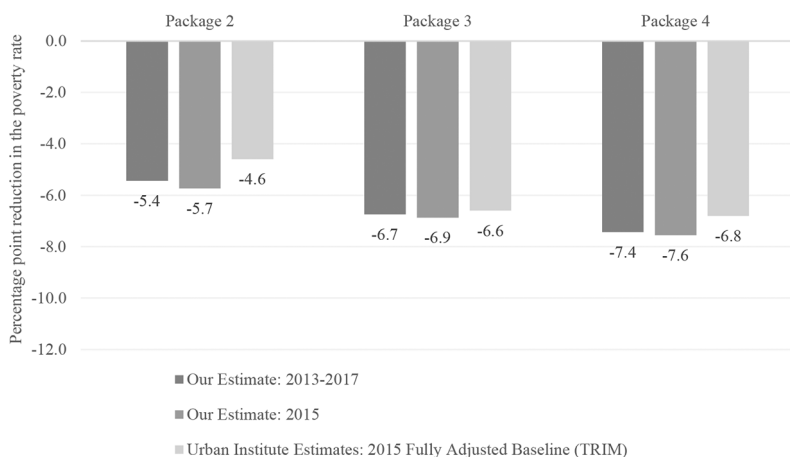


FIGURE 1. Difference between the observed (baseline) and simulated supplemental poverty measure (SPM) poverty rates for children 0–17 in the Current Population Survey (CPS-ASEC) data, accounting for three packages of policies. Package 2 (work-based and universal supports) includes a simulated expansion of the earned income tax credit (EITC), child- and dependent-care tax credit (CDCTC), and introduction of a child allowance. Package 3 (means-tested supports and work) includes a simulated expansion in the EITC, CDCTC, the housing voucher program, and the Supplemental Nutrition Assistance Program. Package 4 (universal supports and work package) includes a simulated expansion of the EITC, the CDCTC, an increase in the minimum wage, and the introduction of a child allowance. Calculations are author’s using parameters from the Urban Institute and Duncan and Le Menestrel (2019).

average, under the restrictive (most conservative) specification. Using the moderate threshold, the reduction in CPS investigation risk ranges from 14.5 percent under package 2 to 19.7 percent under package 4. These estimates translate to 385,738–529,998 fewer children subject to CPS investigations under the restrictive specification and 493,464–669,018 fewer children subject to CPS investigations under the moderate threshold, based on the US population of children 0–17 in the same period.¹⁹ Both

19. Table A2 presents our bounded estimates that result from this simulation when the elasticities from Goldin and colleagues (2021), rather than those from the NAS committee, are applied. Notably, the small decline in labor supply reduces SPM resources by approximately \$10/year per family (results not shown), which, in turn, reduces CPS involvement by about 0.1 percent (around 2,000 children). We show these estimates only for package 4, as this is the only package affected by these assumptions.

Under our alternative expansive specification in which the Cancian and colleagues’ (2013) multiplier is applied to single-parent and married families with less than \$25,000 in annual income, these estimates range from 27.8 to 36.4 percent, suggesting that approximately 943,716–1,236,528 fewer children would be subjected to CPS investigations.

TABLE 2. Simulated Reductions in Poverty and Resources

	Package 2			Package 3		Package 4	
	Baseline	Simulated Mean	Effect	Simulated Mean	Effect	Simulated Mean	Effect
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
SPM poverty:							
Estimate	.160	.106	-.054**	.093	-.068**	.086	-.074**
SE			(.000)		(.001)		(.001)
Bootstrapped SE			(.000)		(.000)		(.001)
% Baseline			-33.9		-42.2		-46.4
SPM resources:							
Estimate	66,950.20	69,377.29	2,427.09**	69,441.73	2,491.53**	70,918.82	3,968.62**
SE			(13.79)		(12.63)		(14.91)
Bootstrapped SE			(14.38)		(12.38)		(15.42)
% Baseline			3.6		3.7		5.9

Note.—SE = standard error; SPM = supplemental poverty measure. *N* = 240,225. Observed and simulated SPM poverty rates and SPM resources for children 0–17 in the 2013–17 CPS-ASEC. Simulated SPM poverty rates and resources shown for package 2 (cols. 2 and 3), package 3 (cols. 4 and 5), and package 4 (cols. 6 and 7) as defined in Duncan and Le Menestrel (2019). Estimates are weighted using the CPS-ASEC weight.

** *p* < .01.

the standard errors and bootstrapped standard errors are small and consistent, suggesting that the variation in the CPS population is not driving our estimated effects.

Our estimates thus far reflect mean effects for the population of US children as a whole. Although these are helpful for understanding the average effect of each of the policy packages, it is imperative to understand the distributional effects as well. Children of color are disproportionately likely to be poor, and Black and American Indian/Alaskan Native children, in particular, are overrepresented in CPS.²⁰ As such, redistribution of income via the NAS policy packages has the potential to reduce racial disproportionality in CPS involvement. Thus, in tables 4–6, we present results from our simulations disaggregated by child race or ethnicity, parent education, and parent marital status.²¹ Table 4 presents simulated estimated reductions in CPS involvement by child race or ethnicity. Among White non-Hispanic children,

20. Unfortunately, our data lack adequate sample sizes to produce specific estimates for American Indian/Alaskan Native children.

21. Estimated reductions in child poverty for these subgroups are presented in table A3.

TABLE 3. Simulated Reductions in Child Protective Services (CPS) Investigations

	Package 2		Package 3		Package 4		
	Baseline	Simulated Mean	Effect	Simulated Mean	Effect	Simulated Mean	Effect
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Restrictive specification:							
Investigation risk	.044	.039	-.005**	.038	-.005**	.037	-.007**
SE			(.000)		(.000)		(.000)
Bootstrapped SE			(.000)		(.000)		(.000)
% Baseline			-11.3		-12.3		-15.6
Expansive specification:							
Investigation risk	.044	.037	-.006**	.037	-.007**	.035	-.009**
SE			(.000)		(.000)		(.000)
Bootstrapped SE			(.000)		(.000)		(.000)
% Baseline			-14.5		-15.7		-19.7

Note.—SE = standard error. Observed (baseline) and simulated rates of CPS involvement for children 0–17 in the 2013–17 CPS-ASEC. Outcomes shown for package 2 (cols. 2 and 3), package 3 (cols. 4 and 5), and package 4 (cols. 6 and 7) as defined in Duncan and Le Menestrel (2019). Estimates are weighted using the CPS-ASEC weight. Percentage of baseline is the estimated effect size divided by the 4.4% baseline rate of CPS investigations (presimulation).

** $p < .01$.

we estimate a 6.7–13.0 percent reduction in CPS involvement spanning threshold specifications and policy packages. Notably, the estimated reductions in CPS involvement are two to three times larger for Black non-Hispanic children (18.7–28.5 percent) and roughly twice as large for Hispanic children (13.3–24.4 percent) than for White children. Estimates among children in the Other non-Hispanic race or ethnicity group are closer to those in the White non-Hispanic group (8.0–14.5 percent), though slightly larger.

In addition to reducing racial/ethnic disparities in CPS and broader child welfare system involvement, increasing family income should yield distributional effects with respect to other markers of disadvantage. To test for this possibility, we stratify our estimation models by educational attainment (table 5) and caregiver marital status (table 6). Generally, we find that all three packages transmit larger reductions in CPS involvement to relatively disadvantaged children. Our estimates suggest that children living with caregivers with less than a high school degree would experience a 25.5–31.9 percent reduction in CPS involvement under the

TABLE 4. Simulated Reductions in Child Protective Services (CPS) Investigations Stratified by Child Race or Ethnicity

	Package 2			Package 3		Package 4	
	Baseline	Restrictive	Expansive	Restrictive	Expansive	Restrictive	Expansive
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
White:							
Investigation risk	.035	-.002**	-.003**	-.002**	-.003**	-.003**	-.005**
SE		(.000)	(.000)	(.000)	(.000)	(.000)	(.000)
Bootstrapped SE		(.000)	(.000)	(.000)	(.000)	(.000)	(.000)
% Baseline		-6.7	-9.4	-6.3	-8.8	-9.4	-13.0
Black:							
Investigation risk	.077	-.014**	-.016**	-.017**	-.018**	-.020**	-.022**
SE		(.000)	(.000)	(.000)	(.000)	(.000)	(.000)
Bootstrapped SE		(.000)	(.000)	(.000)	(.000)	(.000)	(.000)
% Baseline		-18.7	-20.6	-21.6	-23.7	-26.2	-28.5
Hispanic:							
Investigation risk	.043	-.006**	-.008**	-0.006**	-.009**	-.007**	-.010**
SE		(.000)	(.000)	(.000)	(.000)	(.000)	(.000)
Bootstrapped SE		(.000)	(.000)	(.000)	(.000)	(.000)	(.000)
% Baseline		-13.3	-18.8	-14.9	-21.0	-17.5	-24.4
Other:							
Investigation risk	.045	-.004**	-.005**	-.004**	-.005**	-.005**	-.006**
SE		(.000)	(.000)	(.000)	(.000)	(.000)	(.000)
Bootstrapped SE		(.000)	(.000)	(.000)	(.000)	(.000)	(.000)
% Baseline		-8.0	-10.6	-8.7	-11.5	-11.1	-14.5

Note.—SE = standard error. *N* = 240,225. Observed and simulated rates of CPS involvement for children 0–17 in the 2013–17 CPS-ASEC. Outcomes shown for package 2 (cols. 2 and 3), package 3 (cols. 4 and 5), and package 4 (cols. 6 and 7) as defined in Duncan and Le Menestrel (2019). Estimates are weighted using the CPS-ASEC weight.

** *p* < .01.

restrictive specification and a 34.4–42.6 percent reduction under the expansive specification. In contrast, for those living with caregivers who have a bachelor’s degree or higher, our estimates range from 2.9 to 4.5 percent and from 4.3 to 6.4 percent, respectively (table 5). Turning to marital status, we find the largest benefits for children in single-parent families (table 6). Under our restrictive specification estimates, we find null effects of all three packages for married caregivers and a reduction in CPS involvement risk by 35.0–48.2 percent among single caregivers. Expansive

TABLE 5. Effects Stratified by Education

	Package 2		Package 3		Package 4		
	Baseline	Restrictive	Expansive	Restrictive	Expansive	Restrictive	Expansive
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
<HS:							
Investigation risk	.050	-.013**	-.017**	-.014**	-.019**	-.016**	-.021**
SE		(.000)	(.000)	(.000)	(.000)	(.000)	(.000)
Bootstrapped SE		(.000)	(.000)	(.000)	(.000)	(.000)	(.000)
% Baseline		-25.5	-34.4	-28.6	-38.1	-31.9	-42.6
HS:							
Investigation risk	.050	-.009**	-.011**	-.010**	-.013**	-.012**	-.015**
SE		(.000)	(.000)	(.000)	(.000)	(.000)	(.000)
Bootstrapped SE		(.000)	(.000)	(.000)	(.000)	(.000)	(.000)
% Baseline		-17.9	-22.9	-19.8	-25.5	-23.9	-30.3
Some college:							
Investigation risk	.047	-.006**	-.007**	-.007**	-.008**	-.009**	-.010**
SE		(.000)	(.000)	(.000)	(.000)	(.000)	(.000)
Bootstrapped SE		(.000)	(.000)	(.000)	(.000)	(.000)	(.000)
% Baseline		-12.9	-15.6	-14.1	-16.8	-18.4	-21.9
≥BA:							
Investigation risk	.037	-.001**	-.002**	-.001**	-.001**	-.002**	-.002**
SE		(.000)	(.000)	(.000)	(.000)	(.000)	(.000)
Bootstrapped SE		(.000)	(.000)	(.000)	(.000)	(.000)	(.000)
% Baseline		-2.9	-4.3	-2.8	-3.9	-4.5	-6.4

Note.—BA = bachelor of arts; HS = high school; SE = standard error. *N* = 240,225. Observed and simulated rates of child protective services involvement for children 0–17 in the 2013–17 CPS-ASEC. Outcomes shown for package 2 (cols. 2 and 3), package 3 (cols. 4 and 5), and package 4 (cols. 6 and 7) as defined in Duncan and Le Menestrel (2019). Estimates are weighted using the CPS-ASEC weight.

** *p* < .01.

specification estimates are similarly large for single caregivers and, under package 4, transmit large effects to married caregivers as well.

DISCUSSION AND CONCLUSIONS

A large and growing body of literature points to poverty during childhood as a factor contributing to myriad disparities in health and human capital throughout the life course. Child poverty affects development both directly (via scarce resources that constrain investment in education and health) and indirectly (through factors that affect parenting). It has long been

TABLE 6. Effects Stratified by Marital Status

	Package 2		Package 3		Package 4		
	Baseline	Restrictive	Expansive	Restrictive	Expansive	Restrictive	Expansive
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Single:							
Investigation risk	.052	-.018**	-.018**	-.020**	-.020**	-.025**	-.025**
SE		(.000)	(.000)	(.000)	(.000)	(.000)	(.000)
Bootstrapped SE		(.000)	(.000)	(.000)	(.000)	(.000)	(.000)
% Baseline		-35.0	-35.0	-38.1	-38.1	-48.2	-48.2
Married:							
Investigation risk	.041		-.002**		-.002**		-.002**
SE			(.000)		(.000)		(.000)
Bootstrapped SE			(.000)		(.000)		(.000)
% Baseline			-4.7		-4.9		-6.0

Note.—SE = standard error. $N = 240,225$. Observed and simulated rates of child protective services involvement for children 0–17 in the 2013–17 CPS-ASEC. Outcomes shown for package 2 (cols. 2 and 3), package 3 (cols. 4 and 5), and package 4 (cols. 6 and 7) as defined in Duncan and Le Menestrel (2019). Estimates are weighted using the CPS-ASEC weight.

** $p < .01$.

observed that child abuse and neglect, and subsequent encounters with CPS, are closely tied to low-income and poverty status during childhood, and recent studies provide plausibly causal evidence that reducing poverty could directly reduce risk of CPS involvement. This study aimed to understand the degree to which large-scale antipoverty policies, if enacted, might affect CPS involvement. Guided by the seminal work of the 2019 NAS report, we replicated three of the four proposed packages of antipoverty policies and estimated their anticipated effects on CPS investigations.

Depending on the policy package and simulation specifications modeled, we find that the child poverty reductions expected under the NAS recommendations have the potential to reduce CPS investigations by 11.3 percent (package 2, restrictive specification) to 19.7 percent (package 4, expansive specification), constituting approximately 386,000–669,000 fewer children investigated per year. Moreover, we find particularly large reductions in CPS involvement for Black and Hispanic children, those living with single parents, and those whose caregivers have low levels of education. Critically, the resulting shifts in the population of CPS-involved families would decrease racial disproportionality substantially, reducing investigations by

18.7–28.5 percent for Black children and 13.3–24.4 percent for Hispanic children, compared to 6.7–13.0 percent for White children. Notably, however, both maltreatment risk, including low-income and poverty status, and racial bias in reporting and case decision-making are thought to contribute to racial disproportionality in the child welfare system (see Merritt 2020). If systemic racism leading to poverty is the dominant pathway through which racial disproportionality in CPS involvement manifests, then decreased poverty should result in reduced disproportionality in CPS systems consistent with the magnitudes we estimate. In contrast, if racial bias is the primary driver, then such effects may be smaller.

Although the policy packages proposed by the NAS may have seemed highly unlikely to be enacted prior to the COVID-19 pandemic, several aspects therein are consistent with the temporary expansions enacted under the 2021 American Rescue Plan Act (ARPA). Moreover, the child poverty-reducing effects of those expansions are expected to be similar to those estimated under the NAS proposals. Thus, our results provide novel insight into the potential benefits of the ARPA expansions. For example, prior to ARPA, the CTC was not fully refundable and excluded families with incomes below the qualifying threshold, resulting in the categorical exclusion of nearly one-third of US children (approximately 23 million; Collyer, Harris, and Wimer 2019; Curran and Collyer 2020). ARPA made the CTC fully refundable and available to all families by eliminating the minimum earnings requirement. Indeed, the CTC was essentially transformed into a universal child allowance, albeit temporarily, providing families with \$3,000 per year (\$250 per month) for each child ages 6–18 and \$3,600 per year (\$300 per month) for each child under 6. These are larger transfers than those of the child allowance schemes proposed in the NAS report and similar in size to the total transfers afforded by the NAS-proposed packages. Enacted during a time of economic uncertainty, this nontrivial source of supplemental income may have enabled families both to smooth consumption and to meet immediate needs pertaining to under- and unemployment that are known to heighten risk for CPS involvement. Moreover, despite the fact that Congress failed to make the expanded CTC permanent, we expect that there will be an ongoing debate over provision of a fully refundable CTC or other universal child allowance scheme for the United States in the coming years. That our study examines expansions prior to the COVID-19 pandemic represents an important baseline for which any subsequent analysis should be compared. Future research assessing the

impact of the expansion CTC (child allowance) on CPS involvement will be crucial to understanding the full costs and benefits of the policy, as well as further expanding knowledge vis-à-vis the causal link between income and child maltreatment.

Our analyses make two key assumptions that require further discussion. First, we assume that income is a primary vehicle for investing in children and families in a way that mitigates risk of CPS involvement. Though based on rigorous, plausibly causal, evidence linking increases in income to reductions in CPS involvement, this assumption may be overly simplistic when considering the range of reasons why families become involved with CPS. For example, evidence points to the salience of holistic interventions that address immediate needs for services (substance abuse treatment, child-care subsidies, housing support) while simultaneously boosting employment or income to allow for safe, supportive, and autonomous parenting (Currie and Rossin-Slater 2015). High-quality home visiting programs have demonstrated positive results in this regard, with rigorous experimental and quasiexperimental studies finding improvements in parenting quality and reductions in the risk of CPS involvement (Avellar and Supplee 2013; Duffee et al. 2017; Eckenrode et al. 2017; Heckman et al. 2017; Howard and Brooks-Gunn 2009; Olds 2006, 2008). Despite these promising results, however, many home visiting programs are limited in reach relative to the policies we examine here, as families are only eligible to receive home visiting services if they demonstrate a prespecified level of maltreatment risk (e.g., prior involvement with CPS) or meet other categorical conditions for program participation. Thus, such programs represent a form of secondary prevention. In contrast, the NAS policy packages constitute primary, or universal, prevention, as benefits are not conditional upon prior CPS involvement or other indications of maltreatment risk. Nonetheless, we note that although antipoverty policies have the potential to reduce CPS involvement, they should not be seen as a panacea for doing so, and many families may also require psychosocial interventions.

The second assumption is that all families in similar positions in the income distribution will respond similarly to additional income. Underlying this assumption is the notion that the relationship between income and CPS involvement risk is linear and independent of other family characteristics (at least within specific income ranges). The empirically rigorous studies from which we drew our multiplier effects assumed linearity in the relationship between income and CPS involvement, subject to

income-related sample selection criteria; both were conducted using lower-income samples relative to the US population as a whole (Berger et al. 2017; Cancian et al. 2013). The paucity of data linking child abuse and neglect, CPS involvement, and other measures of parenting quality to income in samples that include higher-earning families limits our ability to test this assertion. However, we attempt to address this assumption in part by assigning a different multiplier effect to families meeting particular income thresholds (essentially modeling treatment splines) based on the samples for which they were generated.

A related concern is that families' responses to additional income may vary by other aspects of the environment in which they live. For example, states, counties, and communities vary with respect to their definitions of abuse and neglect, as well as in their support for low-income families. It is possible that responses to income transfers may vary by such factors, including access to and generosity of social welfare programs, as well as local support networks. Although the causal effect of income on CPS involvement may vary by such factors, our analyses are silent in this regard.

There are other limitations of our analyses that should be considered in interpreting our results. First, we are unable to account for the full range of subsequent static or behavioral effects that might follow policy implementation. Specifically, although our simulations account for anticipated changes in employment and work hours, we are unable to account for changes in behaviors that might affect CPS involvement in the long run, such as child-bearing, divorce, or marriage. Second, although our simulations harness estimated probabilities of risk of CPS involvement that enable us to make predictions at the population level, we are unable to observe actual CPS involvement experienced by children in the CPS-ASEC. Although our use of representative samples at the national and state levels promotes the generalizability of our findings, research in which CPS involvement risk and family income can be observed in a single data set would provide enhanced evidence; unfortunately, no such national data set currently exists.

Third, our analyses are limited to CPS investigations, an important and costly outcome, but do not directly assess more intensive CPS involvement—such as substantiation and foster care placement—that are potentially even more important and costly for children; families; and local, state, and federal budgets. A back-of-the-envelope calculation in which we assume that reductions in investigations translate proportionately into reductions in substantiations and foster care placement implies 77,000 (package 2, restrictive

specification) to 106,000 (package 4, expansive specification) fewer substantiations and 16,000–23,000 fewer foster care placements per year.²² However, it is unlikely that reduced investigations will translate proportionately into reduced substantiations and out-of-home placements. Rather, it is reasonable to expect that reduced investigations will disproportionately occur among families whose primary risk factors are economic (rather than psychosocial) in nature and who are at lower baseline risk of substantiation and removal. At the same time, however, research from Denmark suggests a strong, plausibly causal relationship between income and foster care placement among welfare-receiving households, such that a \$400 per month decrease in cash benefits resulted in a 25 percent increase in foster care placements (Wildeman and Fallesen 2017).

With these limitations in mind, our analyses bear several important implications. First, reducing the rate of CPS involvement might not only reduce unnecessary intrusion in family life and potentially unwarranted escalating consequences (substantiation, child removal) but also free up limited CPS resources to be invested in higher-risk families. Lowering excess referrals to CPS has implications for services coordination as well. Second, our findings highlight the importance of including CPS involvement (or child maltreatment) in estimating the true short- and long-term effects—and, thereby, costs and benefits—of income-support policies for children, families, and government (such as those enacted under ARPA). Third, as noted above, our analyses were constrained by limitations of available data. Researchers, policy makers, practitioners, and families should support the collection of high-quality administrative data on child maltreatment and CPS involvement that incorporate adequate samples of less observed population groups, including rural, immigrant, and American Indian/Alaskan Native families, alongside high-quality measures of income and hardship.

On the whole, however, findings from this study demonstrate that improving the socioeconomic conditions of families by strengthening economic supports can potentially have significant downstream impacts on CPS involvement. Economic supports have the potential to alleviate financial strain among families and, consequently, may have far-reaching effects on population health and well-being. Still, financial enrichment

22. We estimated the implied reduction in subsequent CPS involvement by multiplying the estimated reduction in children investigated by the national average rate of substantiated CPS reports (20 percent) and removals and placement into out-of-home care (4.2 percent; Children's Bureau 2019).

alone may not be sufficient to mitigate some issues, and it is important that community supports and services are also available to ensure a comprehensive strategy for reducing poverty and its deleterious effects on children and their families.

NOTE

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APPENDIX

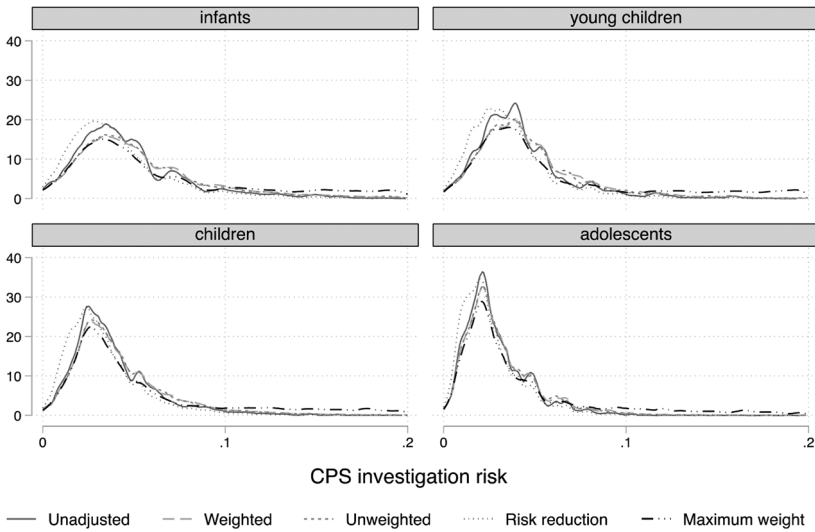


FIGURE A1. Measurement of CPS investigation probability by child age

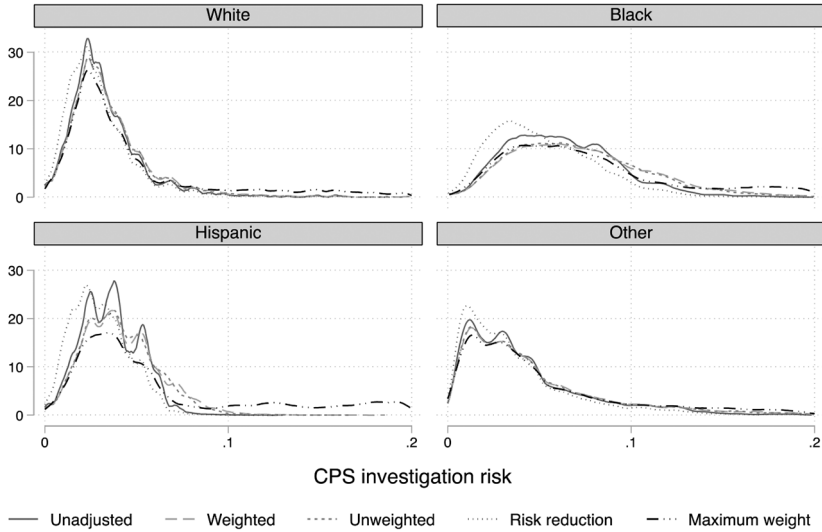


FIGURE A2. Measurement of CPS investigation probability by child race/ethnicity

TABLE A1. Elements of the National Academy of Sciences (NAS)-Recommended Policy Packages

Policy/ Program	Description	Policies Included in Each of the Four NAS Packages			
		1. Work- Based	2. Work- Based and Universal Supports	3. Means- Tested and Work Package	4. Universal Supports and Work Package
Child allow- ance policy 1	Pay a monthly benefit of \$166 per month (\$2,000 per year) per child to the families of all children under 17 who were born in the United States or are naturalized citizens.		√		
Child allow- ance policy 2	Pay a monthly benefit of \$225 per month (\$2,700 per year) per child to the families of all children under 17.				√
Child-care policy	Convert the child- and dependent-care tax credit to a fully refundable tax credit and concentrate its benefits on families with the lowest incomes and with children under the age of 5.	√	√	√	√
Earned in- come tax credit 1	Increase payments along the phase-in and flat portions of the earned income tax credit schedule.	√	√	√	

TABLE A1 (continued)

Policy/ Program	Description	Policies Included in Each of the Four NAS Packages			
		1. Work- Based	2. Work- Based and Universal Supports	3. Means- Tested Supports and Work Package	4. Universal Supports and Work Package
Earned in- come tax credit 2	Increase payments by 40% across the entire schedule, keeping the current range of the phase-out region.				√
Housing voucher policy	Increase the number of vouchers di- rected to families with children, so that 70% of eligible families that are not currently receiving subsidized housing would use them.			√	
Immigration policy	Restore program eligibility for nonqualified legal immigrants. This option would eliminate eligibility re- strictions for nonqualified parents and children in the SNAP, TANF, Medicaid, SSI, and other means- tested federal programs.				√
Minimum wage	Raise the current \$7.25-per-hour federal minimum wage to \$10.25 and index it to inflation after it is implemented.	√			√
SNAP policy	Increase SNAP benefits by 35%; in- crease benefits for older children.			√	
Work advance	All male heads of families with chil- dren and income below 200% of the poverty line would be eligible for Work Advance programming. Train- ing slots would be created for 30% of eligible men.	√			
Estimated reduction in poverty	% Reduction in the number of poor children	-18.80	-35.6	-50.7	-52.3
	% Reduction in the number of chil- dren in deep poverty	-19.3	-41.3	-51.7	-55.1
Estimated change in workforce	Change in number of low-income workers	1,003,000	568,000	404,000	611,000
Cost of package	Annual cost (in billions)	\$8.70	\$44.50	\$90.70	\$108.80

Note.—SNAP = Supplemental Nutrition Assistance Program; SSI = Supplemental Security In-
come; TANF = Temporary Assistance for Needy Families.

TABLE A2. Alternative Labor Supply Effects

	Package 4		
	Observed	Effect 1	Effect 2
Restrictive specification:			
Investigation risk	.044	-.007	-.007
SE		(.000)	(.000)
Bootstrapped SE		(.000)	(.000)
% Baseline		-15.6	-15.5
Investigations	3,399,281	-529,998	-527,868
Substantiations	679,856	-106,000	-105,574
Removals	144,583	-22,543	-22,452
Expansive specification:			
Investigation risk	.044	-.009	-.009
SE		(.000)	(.000)
Bootstrapped SE		(.000)	(.000)
% Baseline		-19.7	-19.6
Investigations	3,399,281	-669,018	-664,824
Substantiations	679,856	-133,804	-132,965
Removals	144,583	-28,456	-28,277

Note.—SE = standard error. *N* = 240,225.

TABLE A3. Simulated Effects on Poverty by Subgroup

	Package 2		Package 3		Package 4		
	Baseline	Simulated Mean	Effect	Simulated Mean	Effect	Simulated Mean	Effect
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Single parents:							
Poverty	.303	.185	-.118	.161	-.142	.154	-.149
SE			(.001)		(.001)		(.001)
% Baseline			-38.9		-46.8		-49.2
Resources	40,378.51	45,005.59	4,627.08	45,568.80	5,190.30	47,123.21	6,744.70
SE			(35.12)		(34.84)		(37.92)
% Baseline			11.5		12.9		16.7
Married parents:							
Poverty	.106	.076	-.030	.067	-.039	.060	-.046
SE			(.000)		(.000)		(.001)
% Baseline			-28.6		-37.1		-43.4
Resources	76,999.45	78,594.52	1,595.07	78,470.33	1,470.88	79,918.18	2,918.73
SE			(13.24)		(10.67)		(14.14)
% Baseline			2.1		1.9		3.8
<HS:							
Poverty	.432	.270	-.162	.211	-.221	.205	-.227
SE			(.003)		(.004)		(.004)
% Baseline			-37.6		-51.1		-52.6
Resources	31,962.10	37,563.82	5,601.73	39,073.00	7,110.90	40,235.55	8,273.45
SE			(75.24)		(68.10)		(81.36)
% Baseline			17.5		22.2		25.9
HS:							
Poverty	.276	.176	-.100	.148	-.128	.141	-.135
SE			(.001)		(.001)		(.002)
% Baseline			-36.3		-46.3		-48.8
Resources	41,752.15	45,805.19	4,053.05	46,371.74	4,619.59	48,019.16	6,267.02

TABLE A3 (continued)

	Package 2			Package 3		Package 4	
	Baseline	Simulated Mean	Effect	Simulated Mean	Effect	Simulated Mean	Effect
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
SE			(27.94)		(32.88)		(33.06)
% Baseline			9.7		11.1		15.0
Some college:							
Poverty	.164	.105	-.058	.095	-.069	.087	-.077
SE			(.001)		(.001)		(.001)
% Baseline			-35.5		-42.0		-47.1
Resources	52,956.39	55,876.29	2,919.90	55,786.09	2,829.70	57,710.18	4,753.79
SE			(23.83)		(23.58)		(26.32)
% Baseline			5.5		5.3		9.0
≥BA:							
Poverty	.062	.049	-.014	.047	-.015	.041	-.021
SE			(.000)		(.000)		(.000)
% Baseline			-22.1		-24.8		-34.0
Resources	94,378.63	95,201.29	822.67	94,925.04	546.41	96,041.43	1,662.80
SE			(20.11)		(12.31)		(19.22)
% Baseline			.9		.6		1.8

Note.—BA = bachelor of arts; HS = high school; SE = standard error. N = 240,225.

TABLE A4. Implied Effects on Substantiated Reports and Removals

	Baseline	Package 2	Package 3	Package 4	Average
	(1)	(2)	(3)	(4)	(5)
Restrictive specification:					
Simulated % change		-11.3	-12.3	-15.6	
Investigations	3,399,281	-385,738	-419,786	-529,998	-445,174
Substantiations	679,856	-77,148	-83,957	-106,000	-89,035
Removals	144,583	-16,407	-17,855	-22,543	-18,935
Expansive specification:					
Simulated % change		-14.5	-15.7	-19.7	
Investigations	3,399,281	-493,464	-532,265	-669,018	-564,916
Substantiations	679,856	-98,693	-106,453	-133,804	-112,983
Removals	144,583	-20,989	-22,639	-28,456	-24,028

Note.—N = 240,225.

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