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ABSTRACT

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Using Difference-in-Differences models and event-study analysis, we estimate the impact of an exogenous increase in income on the incidence and intensity of intimate partner violence (IPV). Using National Crime Victimization Survey data from 1992 to 2000, we exploit time and family-size variation in the Earned Income Tax Credit (EITC) by comparing IPV victimization of women with one or more children (our "treated" group) to that of women with no children (our comparison group) before and after OBRA-93. The OBRA-93 expansion caused statistically significant decreases in both reports of any physical or sexual assault and counts of physical or sexual assaults per 100 women surveyed with the effects being strongest for those groups more likely to both experience IPV and be eligible for EITC: unmarried women and black women. If increased income (rather than changes in employment) is the only channel by which the EITC decreases domestic violence, an additional $1,000 of after-tax income decreases the incidence of physical and sexual violence of unmarried low-educated women by 9.73% and the intensity of physical and sexual violence by 21%. We explore potential mechanisms behind these findings. After ruling out a decrease in time exposure to a partner (due to more time spent at work than at home) or increases in cash on hand with tax returns, we find suggestive evidence in support of EITC allowing for changes in living conditions during the summer.

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I. Introduction

In the United States, one in four women have experienced physical violence, sexual violence, and/or stalking by an intimate partner or ex-partner at some point in their lives (CDC 2020). In any given year, this represents close to 10 million women who are victims of rape, physical violence, or stalking by an intimate partner.[1] These forms of violence begin early on (before the age of 18) and are most common when women are in their twenties and thirties (Aizer 2011). Black women, low-income women, and unmarried women are also at higher risk of abuse (Rennison and Welchans 2000; Sorenson and Spear 2018). With devastating consequences for women's health (WHO 2013)[2] and employment (Adams et al. 2013; Browne et al. 1999; Lloyd and Taluc 1999), as well as their children's health and development (WHO 2002), intimate partner violence (IPV) has harmful and long-lasting effects on individuals, families, and communities. The CDC estimates that the lifetime economic cost associated with IPV (including medical expenses, lost productivity, and criminal justice costs, among others) amounts to $3.6 trillion—about $103,767 per victimized woman (CDC 2003). Only by improving our understanding of the determinants of this public health crisis can we develop strategies aiming at preventing and reducing IPV.

In this paper, we estimate the causal effect of the Earned Income Tax Credit (EITC), which provides cash transfers to families (Bitler and Hoynes, 2010) and influences women's employment prospects (Whitmore et al., 2021), on the incidence and intensity of IPV against women in the United States. Theoretically, the relationship between the EITC and domestic abuse is ambiguous. On the one hand, the feminist theory argues that IPV is the result of women's economic dependence and weak bargaining power within the household. Hence, as women secure employment outside the household, increasing their earned income, they become more economically independent and increase their bargaining power within the household, making it easier for them to adopt economic or social sanctions against potentially abusive husbands (Choi and Ting 2008), or leave an abusive relationship (Tauchen, Witte and Long 1991; Vyas and Watts 2009). Consistent with this, Aizer (2010) estimates that the decline in the gender wage gap

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[1] Estimate calculated by authors using data from the National Domestic Violence Hotline, and the fact that most intimate partner violence (82%) is committed against women in the US (Truman and Morgan 2014).
[2] According to World Health Organization (2002, 2013), domestic violence is positively associated with many health problems including sexually transmitted infections, induced abortion, premature and low-weight birth, growth restriction in utero, alcohol use, depression and suicidal behavior, injuries, and death from homicide.

witnessed in the state of California between 1990 and 2003 explains about 9 percent of the reduction in female hospitalizations for assault, suggesting that women's higher relative economic power mitigates abuse. In a very different context, Hidrobo, Peterman, and Heise (2016) find that cash and in-kind transfers offered to mothers from poor urban areas in Ecuador decrease the incidence of physical and sexual violence by 38% to 43% from baseline means.

On the other hand, in contrast with the feminist theory, the evolutionary approach argues that women's greater economic independence (and potential exposure to other men) raises IPV because of husbands' paternity uncertainty and jealousy. Violence, in this case, is used by men to re-assert dominance in the relationship.[3] Indeed, several studies have found a direct impact of higher female economic empowerment (via employment, education, or earnings) on IPV in India (Eswaran and Malhotra 2011), Bangladesh (Heath 2014), and Turkey (Erten and Keskin 2018, 2021), suggesting that, in these countries, domestic violence is a vehicle employed by males to enhance their bargaining power.[4]

Aside from the effect on bargaining power, changes to the EITC may also affect IPV through employment effects that reduce women's exposure to violence and through direct effects on household income that impact household stress. Employment, particularly male employment, has been shown to reduce IPV, with the strongest effects in regions with traditional gender norms (Alonso-Borrego and Carrasco, 2017; Tur-Prats, 2021).[5] Increased shared time at home has been shown to increase IPV; Leslie and Wilson (2020) find an 8% increase in domestic violence calls following the imposition of stay-

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[3] For example, Carr and Packham (2021) find that when food stamps scheduled are reformed to more frequently enter the household there is an increase in domestic violence due to internal struggles for resources. Relatedly, there is evidence that as male labor market opportunities decline in the US violence within the household increases. Lindo, Schaller and Hansen (2018) find that a decline in male labor market conditions is associated with increases in child maltreatment.

[4] Instrumenting IPV with height, Eswaran and Malhotra (2011) find that domestic violence drastically reduces women's autonomy in India. Heath (2014) documents a positive association between work and domestic violence in Bangladesh among women with low education or young age at marriage. The author interprets this finding as evidence that women with low bargaining power face increased risk of domestic violence upon entering the labor force as their husbands seek to counteract their increased bargaining power. Using a 1997 compulsory schooling law in Turkey that expanded compulsory education from 5 to 8 years, Erten and Keskin (2018) estimate the causal effect of education on domestic violence using a regression discontinuity model. They find an adverse effect on psychological violence and financial control, but no effect on physical or sexual violence. According to Erten and Keskin (2021), exploiting variation in female employment due to a large influx of refugees in Turkey, a decrease in female employment led to a reduction in IPV against women, suggesting that a negative shock on women's income-generating ability incapacitates men from using violence against women to extract rent.

[5] In contrast, Anderberg et al. (2015) develop a model and find evidence in the U.K of the opposite: that male (female) unemployment decreases (increases) domestic violence.

at-home order in March 2020 as families were forced to shelter in place together. Similarly, Arenas-Arroyo et al. (2021) separately identify the effects of COVID-associated lockdowns and economic stress and find that shared time at home during the lockdown in Spain generated an increase in IPV, primarily through an increase in psychological conflict, and that economic stress also led to a large increase in IPV. Likewise, using longitudinal data from the great recession, Schneider et al. (2017) shows that increased economic stress at both the household and regional level leads to an increase in domestic violence.

Since IPV may be connected to employment, productivity and earnings via non-causal pathways, we exploit an exogenous and sizable variation in after-tax income for low- to moderate-income families with children induced by the 1994 Earned Income Tax Credit (EITC) expansion enacted as part of the Omnibus Budget Reconciliation Act of 1993 (OBRA-93). As shown in Figure 1, following OBRA-93, there was a large differential increase in the maximum credit offered to families with qualifying children relative to those with no qualifying children. For example, the 1994 maximum credit for people with children (our "treatment" group) was seven to eight times that offered to people with no qualifying children, our comparison group. Over time, the maximum credit for the former continued to increase, reaching in 1998 between 18% and 40% of their earned income[6], but it remained practically flat for the latter, at 6% to 8% of their earned income.

Using a Difference-in-Differences approach, we exploit time and family-size variation on the maximum EITC by comparing IPV victimization of women with one or more children (our "treated" group) to that of women with no children (our comparison group) before and after OBRA-93. We focus our analysis on women with less than a four-year college degree and unmarried women with less than a four-year college degree. To assess the validity of the pre-existing parallel trends assumption, we perform an event-study analysis. Analysis by family size, race, age, and education are undertaken to explore whether the effects of EITC differ by socio-economic status. Placebo tests using women with a four-year college degree or higher suggest that our findings are not due to systematic differences between women with and without children.

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[6] In 1998, the maximum EITC represented between 18% and 34% of the earned income for those with one child, and between 31% and 40% for those with two or more children.

Using repeated cross-sectional data from 1992 to 2000 from the National Crime Victimization Survey (NCVS), we find that increased EITC income reduced both the incidence and intensity of IPV among women with less than a four-year college degree, with the estimates on sexual assault being statistically significant at the 1 percent level. The strongest effects are among unmarried women, and among black women—groups that are both more likely to experience IPV (Catalano, 2007) and to be eligible for the EITC (Jones, 2014). Specifically, the intensity of physical and sexual IPV decreased in the post-OBRA-93 period by 1.4 and 0.8 incidents per 100 women, respectively, for unmarried mothers relative to similar women with no qualifying children (relative to the pre-OBRA-93 means for women with children of 3.9 and 0.7 per 100 women). In addition, the incidence of sexual IPV decreased by 0.1 percentage (relative to the pre-OBRA-93 control means of 0.18). These three coefficients are statistically significant at the 5 percent level or lower.

One way of measuring the economic impact of our findings is to make the restrictive assumption that increased income (rather than changes in employment) is the only channel by which the EITC decreases domestic violence. In this case, an additional $1,000 of after-tax income decreases the intensity of physical and sexual violence by 9.73%. The decreases for incidence of physical and sexual violence are larger amounting to 21%. These effects are all statistically significant at the 5% threshold or below.

We investigate the mechanisms though which EITC may affect intimate partner violence. The EITC may generate reductions in IPV through multiple avenues: 1) increased work may reduce women's exposure to violence by reducing time spent at home, 2) the lump-sum payment may lead to more cash on hand that allows for the escape from abusive relationships or short-term reductions in family stress that reduce IPV, or 3) the EITC, by subsidizing wages and encouraging work, allows for higher regular income and spending over time, which may in turn reduce IPV through higher bargaining power, spending that reduces family stress, or social networks associated with working. We investigate each of these mechanisms in turn and find suggestive support for the last set of mechanisms.

We contribute to the existing on several fronts. First, to the best of our knowledge, we are the first to exploit time and family-size variation in the EITC before and after OBRA-93 to identify the causal impact of in-work tax credits on the incidence and intensity of IPV. Second, our work complements Aizer's findings and generalizes them

at the country level, offering a different policy context and giving external validity to her conclusions.[7] Given that almost 20% of all tax filers and 44% of filers with children in the US received the EITC in 2014, our findings are most policy relevant especially given the current political climate of increasing tax credits targeted to families; our results suggest that the expansion of the child tax credit with the enactment of the American Rescue Plan signed into law by President Biden in March 2021 may lead to significant reductions in family violence.

Our work also contributes to the broad literature that analyzes the impact of EITC on maternal employment (Eissa and Liebman 1996; Meyer and Rosenbaum 2001; Eissa and Hoynes 2004;)[8]; fertility (Baughman and Dickert-Conlin 2009) or family formation (Dickert-Conlin 2002; Ellwood 2000; Herbst 2011)[9]; maternal and infant health (Evans and Garthwaite, 2014; Strully, Rehkopf, and Xuan 2010; Hoynes, Miller, and Simon 2015).[10] We contribute to this literature by studying the effects of OBRA-93 on the incidence of domestic violence. Our findings complement earlier studies examining the effects of the same EITC expansion on other health and labor market outcomes. However, our paper differs significantly in its focus on domestic violence outcomes and the channels through which EITC may affect these outcomes.

II. Identification Strategy: the 1993 Expansion of the Earned Income Tax Credit

The earned income tax credit (EITC) provides in-work tax credits that, based on family-size and earned-income eligibility, deduct from the tax liability on the filed tax return with a dollar of tax credit given per dollar of earned income. The credit phases in, plateaus at a maximum credit amount, and phases out based on adjusted-gross-income. Thresholds

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[7] While much of the previous work analyzing the impact of earned income on IPV is descriptive, a handful of studies use different identification strategies to estimate the causal effect. For example, Aizer (2011) exploits county-variation in the sex-composition of the industrial structure in the state of California to construct sex-specific measures of prevailing local wages based on the industrial structure of the county and statewide wage growth in industries dominant in each county. Hidrobo and Fernald (2013) and Hidrobo, Peterman, and Heise (2016) exploit a randomly designed intervention of cash and in-kind transfers in Ecuador. Finally, Carr and Packham (2018) use food stamp distribution as a positive income shock, though in this case the more regular distribution of food stamps increases domestic violence.

[8] These studies find that EITC encourages work among unmarried mothers and decreases it among married mothers. However, they find little evidence that eligible-working women adjust their hours of work because of EITC. In an unpublished working paper, Kleven (2019) argues that the extensive margin employment impacts of the EITC are over-stated. On the other hand, most studies find employment effects of the EITC (Nichols and Rothstein 2015). We consider employment effects as a potential mechanism of our findings but consider other mechanisms as well: such as a reduction in poverty among unmarried mothers.

[9] There is no evidence that EITC affects fertility or family formation.

[10] These studies find that EITC income reduces the incidence of low birth weight and increases mean birth weight. They also find that EITC improves mothers' self-reported health and lowers their counts of the risky biomarkers.

differ due to family size and have changed over time. The EITC is fully refundable: meaning that if it results in a family having a negative tax liability, they receive the remaining credit amount as a payment with their tax refund. Most of these families receive the refund as a lump-sum payment beginning in February.

Our policy experiment leverages the OBRA-93 reform that differentially increased the credit based on family size: no qualifying children, one qualifying child, and two or more qualifying children (where qualifying children are those under age 19, 24 if a full-time student, or permanently disabled, and who reside with the taxpayer for more than half the year). Our analysis focuses on the OBRA-93 expansion because it is the largest expansion of the EITC, and the first to differentially expand the credit between those with two or more children and those with one child, offering additional variation than other large federal EITCs. As shown in Figure 1, beginning in 1994, people with qualifying children were eligible for a credit of up to $2,038 if they had one qualifying child or up to $2,528 if they had two or more qualifying children, an increase of 42% and 67% from the maximum 1993 credit level, respectively.[11] The 1994 maximum credit for people with children (our "treatment" group) was seven to eight times that offered to people with no qualifying children, our comparison group. Over time, the maximum credit for people with children continued to increase, reaching $2,271 for those with one child and $3,756 for those with two or more children in 1998 but remaining practically flat at $341 for those with no qualifying children. By 1998, the maximum EITC represented between 18% and 40% of the earned income for those with qualifying children[12], but only 6% to 8% of those with no children. In comparison, in 1993, the maximum EITC ranged between 12% and 19% for those with children and did not vary by the number of children, and was nonexistent for those without children.

All of the models we estimate use the following basic form. Following linear probability model using a difference-in-differences (DiD) approach, we estimate:

$$y_{iat} = \beta_1 Post_t * (children \geq 1)_{iat} + \gamma_a + \emptyset_t + X'_{iat}\beta_2 + \varepsilon_{iat} \qquad (1)$$

where y_{iat} is an IPV-related outcome for woman i with a number of children in year t. $Post_t$ is an indicator variable for being post-OBRA-93. Since the EITC expansion was implemented in the 1994 tax year and as most filers received the refundable portion of

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[11] In 1993, people with qualifying children were eligible for a credit of up to $1,434 if they had one qualifying child or up to $1,511 if they had two or more qualifying children. Those with no qualifying children were not eligible to receive the EITC.

[12] In 1998, the maximum EITC represented between 18% and 34% of the earned income for those with one child, and between 31% and 40% for those with two or more children.

the 1994 EITC in a lump sum in February of 1995, $Post_t$ equals 1 if the woman is observed in 1995 or later, and 0 if she is observed in 1994 or before. The variable $(children \geq 1)_{iat}$ is an indicator variable equal to 1 if woman i has one or more qualifying children in the household in year t and 0 if there are no qualifying children in the household. To absorb confounding variation over time and by family structure, we include γ_a: a vector of fixed effect for the number of children in the household correspond to the policy variation in the EITC; and θ_t, a vector of year fixed effects. The former fixed effect accounts for differences in the level of IPV across family size, and the latter fixed effect accounts for differences in the level of IPV across years. The vector X_{iat} is a vector of demographic controls for woman i with a number of children in year t. It includes dummies for: race (White, Black, Asian, Hispanic, Other); being married (and a dummy if the marital status is missing in the data); having less than a high-school degree, being a high-school graduate, and having some college education; and belonging to the following age groups (16-19, 20-29, and 30-40). Robust standard errors are estimated to correct for heteroskedasticity. Following Abadie et al. (2017), we do not cluster the standard errors in main estimates as there is no a priori obvious level to adjust them for clustering in this context. Instead, we show robustness to several different clustering schemes in the sensitivity analysis section below.

The coefficient of interest, $\hat{\beta}_1$, is the effect of the interaction between being in the post-OBRA-93 period and the treated group (having children). It captures the differential change in the IPV outcome before relative to after for women with children relative to no children. We rely on the vector of fixed effect for the number of children in the household to capture fixed differences between the treated and comparison groups that exist even in the absence of the policy change.[13] The remaining difference in the changes in the IPV outcome between the pre- and post-periods can then be ascribed to the expansion of EITC for people with children net of the EITC expansion for people without children. As we do not observe whether individuals received the EITC, our estimates are intention-to-treat estimates. At the end of Section IV, we scale our ITT estimates by first-stage effects on

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[13] A new literature on difference-in-differences critiques the use of two-way fixed effects when there is staggered treatment design—see Roth et al. (2022) for a review. Because we only have treatment occurring at a single point in time this is not a concern to us. An extension of this literature brings up that time varying covariates could also cause additional bias for similar reasons (Goodman-Bacon, 2021). We show our main results with and without including covariates: we find virtually no difference between the two sets of results.

employment and income to get a sense of various magnitudes of treatment on the treated (TOT) estimates under the assumption of different mechanisms.

Because the EITC targets low- to moderate-income working individuals and couples, we focus our analysis on women who have not completed a four-year college degree. It is estimated that 86% of EITC eligible tax filers do not have a college degree (Murray and Kneebone, 2017). At the same time, 82% of EITC eligible tax filers are unmarried (Nichols and Rothstein, 2016) and about three-quarters of the EITC credit payments go to unmarried filers with children (Bitler, Hoynes, and Kuka 2017). Hence, we also present estimates for those who are unmarried.[14]

The critical identifying assumption of the DiD approach is that we have isolated a comparison group that would exhibit parallel trends in IPV in the absence of the intervention. To assess the validity of this assumption, we check for pre-existing diverging trends using an event-study framework:

$$y_{iat} = \sum_{t=1992}^{2000} \delta_t(year = t) * (children \geq 1)_{iat} + \gamma_a + \theta_t + X'_{iat}\beta_3 + \varepsilon_{iat} \quad (2)$$

where in addition to the vector of year fixed effects, θ_t, we include year dummies interacted with the treated group. The 1993 tax year (that is, women observed in 1994) is the omitted year. In the absence of any pre-existing differential trends or policy anticipation between women with and without children, the estimated coefficients $\hat{\delta}_t$ corresponding to the years prior to the 1994 tax year should be non-statistically different from zero.

Since there was a stronger differential increase in the maximum credit for people with two or more qualifying children relative to those with only one qualifying child, we allow for varying family-size policy effects by estimating:

$$y_{iat} = \beta_1 Post_t * (children = 1)_a + \beta_2 Post_t * (children \geq 2)_a + \gamma_a + \theta_t + X'_{iat}\beta_3$$
$$+ \varepsilon_{iat} \quad (3)$$

where now $\hat{\beta}_1$ and $\hat{\beta}_2$ capture the treatment effects of the policy change for women with one child, and two or more relative to those with no children, respectively. This richer specification serves as a check to see if there are greater impacts on women with two or more children who experienced a larger differential increase in the tax credit.

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[14] We define as unmarried women those who are widowed, divorced, separated, or never married.

Finally, one may be concerned that the effects of the EITC may be confounded with other policy changes that differentially affect women with and without children. We also conduct a placebo test using women with a four-year college degree or higher to rule out that our findings are not due to systematic differences between women with and without children before and after OBRA-93.

III. Data and Descriptive Statistics

We use data from the National Crime Victimization Survey (NCVS), an ongoing nationally representative survey administered by the Bureau of Justice Statistics with the objective to measure the frequency, characteristics, and consequences of criminal victimization in the United States. Even though this survey began measuring nationwide criminal victimization in 1973, it was redesigned in 1992 to improve reporting on intimate partner violence and sexual assault (Kindermann et al., 1997).[15] The new design included more detailed screening questions about the associated assault to eliminate subjective interpretations of what constitutes victimization and led to more reporting of IPV and sexual assault (although not more reporting of property crimes). As a result, we only use data after this redesign. Furthermore, there was a smaller differential increase in the EITC for mothers with children in 1991: such that extending the EITC back further would likely pick up these pre-trends.

The survey provides information at the household, person, and incident level. For each household, every household member who is 12 years old or more is interviewed about whether she or he has been the victim of a crime within the past 6 months. If an incident has occurred, the interview asks a battery of questions about the incident and offender. More specifically, the NCVS collects information on nonfatal personal crimes (such as rape or sexual assault, robbery, aggravated and simple assault, and personal larceny) and household property crimes (such as burglary/trespassing, motor-vehicle theft, and other types of theft), regardless of whether they have been reported to the police or not. As the NCVS collects information about the offender, including the victim-offender relationship, for each victimization incident, we can identify whether the offense was conducted by the victim's spouse, boyfriend or ex-partner. Because we have self-reported information on whether the woman worked in the past week, we replicate earlier

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[15] Prior to 1992, the survey was called National Crime Survey.

work that has found impacts of the EITC on the extensive margin of labor force participation.

The survey also includes socio-demographic information on each member of the household who is 12 years of age or older, as well as data on the number of household members under 12 years of age. Socio-demographic information includes age, race, gender, highest educational attainment, and marital status.[16] Crucially, there is information on the number of people in the household under the age of 19. We use such information to identify the presence and number of qualifying children, and hence to construct our "treatment" variable, $(children \geq 1)_{iat}$. While these survey data provide crucial information for our identification strategy, it may also be subject to underreporting or IPV and measurement issues (Aizer, 2010). To the extent that this underreporting is not correlated with our treatment—the number of children at the time of OBRA's passage—one should expect this to lead to less precise but unbiased estimates.

We focus our analysis on the effects of an earned income increase on the incidence and intensity of IPV. Hence, we define the following outcome variables: (1) a binary indicator for whether a woman experienced any physical (or sexual) aggression from a current or previous partner during the previous six months; and (2) the sum of the total number of incidents of physical (or sexual) aggression (of any type) to which the woman was exposed during the six months prior to the survey (by current or previous partner). Table 1 lists the different types of physical and sexual aggression that our outcome variables cover. We both conduct our analysis separately for physical and sexual IPV and look at counts of total incidences (physical and sexual) to have a comprehensive measure of IPV.

Sample Restrictions and Descriptive Statistics

We use individual-level data from survey years 1992 (the tax year 1991) to 2000 (the tax year 1999), covering three years prior to and six years after OBRA-93 was enacted. Because the OBRA-93 benefits were gradually phased in through 1997 for families with two or more children, we cover three years after OBRA-93's implementation was complete. As explained earlier, we focus our analysis on women who have not completed a four-year college degree because the EITC expansion targeted low- to moderate-income

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[16] Because of strict data confidentiality reasons, the NCVS does not disclose information on individuals' state of residence.

working individuals and couples. We further restrict our sample to women between the ages of 16 and 40 because IPV is most common in this age range (Aizer 2011), leaving us with a sample of 239,035 women, of which 170,958 have eligible children. If we further restrict the sample to unmarried women, we have 123,954 women, 77,576 of which have eligible children.

Table 2 presents pre-OBRA-93 descriptive statistics by the presence of qualifying children. Comparing mothers to women with no qualifying children in the household, the former are more likely to be Black and Hispanic (30 percent versus 21 percent) and less likely to be 20 to 29 years old than the latter. Mothers are also less college educated than women with no qualifying children (27 percent versus 48 percent have at least some college) but live in a household with higher annual income than women with no children ($30,705 versus $29,332). Appendix Table A.1 presents similar pre-OBRA-93 descriptive statistics for unmarried women and shows that unmarried mothers tend to be in a more socio-economically vulnerable condition than unmarried women with no qualifying children. For example, they are more likely to be non-White (31 percent) and teenagers (38 percent) than unmarried women with no qualifying children in the household (19 percent are non-White and 21 percent are teenagers).[17] They are also less college educated than unmarried women with no qualifying children (21 percent versus 51 percent), and as many as 60 percent of unmarried mothers live in households with an income below $25,000 per year[18] relative to 58 percent among unmarried women with no children or 38 percent among mothers.

Before OBRA-93, the prevalence of physical assault by a partner or ex-partner among 16- to 40-year-old women in the United States averaged 6.65 incidents per 1,000 with an average number of incidents of 0.0214. The prevalence and intensity of sexual assault by a partner or ex-partner is considerably lower, at 0.95 incidents per 1,000 with an average number of counts of 0.0042.[19]

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[17] Because being Hispanic is not mutually exclusive from other racial categories, the percent of people of color may not add up to the estimates in Appendix Table A.1.

[18] $25,296 was the earned income threshold for people with two or more qualifying children to receive any EITC.

[19] These numbers are similar although somewhat smaller than the IPV statistics reported in Powers and Kaukinen (2012) and Catalano et al. (2009) using NCVS data, which find an incidence of sexual and physical assault of about 9 and 10 victimizations per 1,000, respectively. This difference is likely due to somewhat broader definition of victimization used by these authors that includes both our sexual and physical assault variables as well as threats of violence.

As documented in the literature with hospital and clinical data, IPV increases during pregnancy (Jasinski, 2004) and motherhood (Vatnar & Björkly, 2010). Table 3 shows that, among mothers in our sample, there are 6.6 incidents and 1 incident per 1,000 of physical and sexual assault, respectively, compared to only 4.3 and 0.4 among women with no eligible children in the household. This implies that, in the pre-OBRA-93, mothers were 53.5% more likely to experience physical assault and more than twice as likely to experience sexual assault by an intimate partner than women with no qualifying children. Similar disparities are observed for counts of physical or sexual abuse: mothers suffered, on average, 70% higher counts of physical abuse and almost six times more counts of sexual abuse than women with no qualifying children before OBRA-93.

After OBRA-93, the difference in the prevalence and intensity of physical assault between mothers and women with no qualifying children decreases from 53.5% and 70% to 43.9% and 23.3%, respectively. However, we cannot reject the hypotheses that the gap between mothers and women without qualifying children after the reform differs from that before the reform. In contrast, we can reject that the gaps in the incidence and intensity of sexual assault by a partner before and after the reform are statistically significantly different from each other. In fact, the sexual IPV gap between mothers and women with no qualifying children reverses after OBRA-93, with mothers experiencing both 7% lower incidence and 49.6% lower intensity of sexual IPV than women with no qualifying children. These before- and after-OBRA-93 descriptive statistics suggest that the expansion of the EITC may have reduced IPV. As this is the raw data, the next section presents DiD results and event-study analyses.

IV. Main Findings

Table 4 presents baseline estimates from regressing equation (1) on a set of IPV outcomes (columns 1 to 6) and employment status (column 7). Panel A presents estimates for the whole sample of women 16 to 40 years old with less than a four-year college degree and corroborates results from Table 3. There was a relative decline of incidence and intensity (counts) of sexual IPV in the post-OBRA-93 for mothers relative to women with no children. Specifically, we estimate the OBRA-93 expansion caused a 0.1 percentage point decrease in reports of any sexual assault and 0.5 fewer counts of sexual assaults per 100 women surveyed (relative to means of 0.1 and 0.4 assaults per 100 women). Both estimates are statistically significant at the 1 percent level. The estimate $\hat{\beta}_1$ is also

negative for the incidence and intensity of physical IPV, though neither coefficient is statistically significantly different from zero.

Panel B focuses on an EITC "high impact" sample similar to what is used in the earlier literature: unmarried women with no college degree. Since a higher share of women within this group is eligible for the EITC, we would expect a greater impact of OBRA-93 on the reduction of IPV for unmarried than married women. Indeed, we find that the intensity of physical and sexual IPV decreased in the post-OBRA-93 period by 1.4 and 0.8 incidents per 100 women, respectively, for unmarried mothers relative to similar women with no qualifying children (relative to the pre-OBRA-93 means for women with children of 3.9 and 0.7 per 100 women—shown in Appendix Table A.2). In addition, the incidence of sexual IPV decreased by 0.1 percentage (relative to the pre-OBRA-93 control means of 0.18). These three coefficients are statistically significant at the 5 percent level or lower.

It is well known that the EITC incentivizes employment at the extensive margin for unmarried mothers because it acts as a wage subsidy.[20] Hence, an expansion of the EITC will not deter working taxpayers who already worked and may push those who did not work into employment (Eissa and Hoynes 2011). Consistent with this, column 7 in Panel B shows a 4.3 percentage points differential increase in unmarried-mother's employment in the past week after OBRA-93, which represents an 8.5 percent increase relative to the pre-OBRA-93 mean of 50.7 percent. The size of this effect is twice as large as the one observed for the whole sample, a 3.8 percent increase relative to the pre-OBRA-93 mean of 55.8 percent for this group (or 2.1 percentage points, shown in Panel A). This is approximately similar to what others in the literature have found.[21] To the extent that OBRA-93 increases women's labor force participation, there is both a direct income effect of OBRA-93 expansion on IPV (via the increase in benefits) and an indirect effect (via higher employment). While it is difficult to fully separate these channels, they imply differences on when the timing of treatment should be assigned which we will explore in Section VI. From here onwards, we will focus on unmarried women.

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[20] In the phase-in region, the EITC acts as a pure wage subsidy increasing the net wage by 40% for taxpayers with two or more children and 34% for those with one child in 2000. In the flat region of the EITC, the taxpayer's budget constraint is shifted out an amount equal to the tax credit ($2,353 for taxpayers with one child and $3,888 for taxpayers with two or more children in 2000). In the phase-out period, the credit is reduced at a 21% rate for each dollar earned.

[21] Meyer and Rosenbaum (2001) find a 4.1 percentage point increase in work in the last week from the OBRA-93 expansion for low education unmarried mothers. More recently, Hoynes and Patel (2018) found a 6 percentage-point increase in any work in the past year.

Event Study

The validity of the DiD approach relies on the assumption that there are no time-varying pre-existing differences between women with and without qualifying children. To assess the validity of the pre-existing parallel trends assumption, Figure 2 presents results from estimating the event study using equation (2) from Section II on all women in our sample with less than a four-year degree (Panel A); unmarried women with less than a four-year degree (Panel B); unmarried White women with less than a four-year degree (Panel C); and unmarried Black women with less than a four-year degree (Panel D). We plot the interaction between year dummies and qualifying children dummy interaction with the coefficient for 1994 normalized to 0. As explained in Section II, the EITC expansion was implemented in the 1994 tax year, which was received during the year 1995. In the graphs, the red vertical line indicates the first year of OBRA-93 EITC receipt. Hence, 1994 is year prior to the first OBRA-93 payment receipt. The event studies show three years of pre-OBRA-93 parallel trends, followed by a decrease in the incidence of physical and sexual IPV corresponding with the increase in EITC benefits. While this is quite evident for Panels A to C, the estimates are less precisely estimated for Black women due to smaller sample sizes (Panel D).

Next, we perform placebo even-study estimates in the sample of women holding at least a four-year university degree (Panel A), and unmarried women with at least a four-year college diploma (Panel B). As women with advanced degrees earn much higher than the EITC qualifying income levels, the OBRA-93 should not impact their employment, income and related outcomes. Consistent with this conjecture, event-study results, presented in Appendix Figure 1, demonstrate that male-to-female IPV perpetration is not a function of the EITC among women with at least a bachelor's degree. Therefore, we infer that our findings are not induced by the systematic differences between women with and without children.

Subgroup Analysis

Table 5 presents subgroup analysis by race, ethnicity, and education levels. These results indicate the strongest effects among some of the most disadvantaged groups and among those most affected by the EITC expansion—namely among women with less education and among Black women. After OBRA-93, the incidence of physical and sexual IPV among unmarried non-White women decreased by 0.4 percentage points relative to their

counterparts with no qualifying children—shown in column 5, Panel B. This is mostly driven by a 0.5 percentage points reduction in physical and sexual IPV among unmarried Black women (which more than doubles the non-statistically significant effect among unmarried White women). At the same time, OBRA-93 reduced the intensity of physical and sexual violence for both White and Black women by 2.4 and 2 percentage points, respectively. Both estimates are statistically significant at the 10 percent level or lower. Among Black, OBRA-93 decreased the incidence of physical IPV by 0.5 percentage points and of sexual IPV by 0.2 percentage points. Among Whites, OBRA-93 reduced the average intensity of physical IPV by 1.5 percentage points and of sexual IPV by 0.9 percentage points. These four coefficients are statistically significant at the 10 percent level or lower. Column 7 in Table 5 shows that the increase in employment in the post-OBRA-93 period was larger among Black women (a 5.9 percentage points increase) than White women (a 3.5 percentage points increase). Panel D shows the effects on IPV and employment for unmarried women with at most a high-school degree. After OBRA-93, the incidence and average intensity of physical and sexual IPV among this group decreased by 0.4 and 3.3 percentage points; and their employment increased by 4.9 percentage points. All three estimates are statistically significant at the 5 percent level or lower.

Placebo Test

Panel C of Table 4 presents a placebo test using 22- to 40-year-old unmarried women with at least a four-year college degree who are less likely to receive the EITC as the treatment group. All estimates of OBRA-93 on IPV are close to zero and not statistically significant suggesting that our findings are not due to systematic differences between women with and without qualifying children. There is a small and positive effect of OBRA-93 on the employment of highly educated mothers relative to childless women, albeit only marginally statistically significant at the 10 percent level.[22] The size of the coefficient is smaller relative to the mean of our main "high impact" sample: 3.1 percent versus 8.5 percent for unmarried mothers with children in the pre-period.[23]

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[22] Since one estimate is significant at the 10% level out of 7 placebo regressions, this is consistent with what we would expect due to type one error.
[23] Before OBRA-93, the employment of highly educated mothers in our sample was 81.88%.

Effects by Number of Children

As explained in Section II, the increase in the maximum credit for taxpayers was larger for those with two or more qualifying children than those with only one qualifying child. Table 6 presents estimates by number of children using equation (3) for the following three samples: unmarried women (Panel A); unmarried White women (Panel B); and unmarried Black women (Panel C). Focusing first on the effects of the OBRA-93 expansion on employment (shown in column 7), we observe a much larger effect after OBRA-93 on the employment of mothers with two or more children than that of mothers with only one child consistent with the earlier literature and the fact that greater EITC benefits lead to higher behavioral impacts. Interestingly, this parity difference is considerably larger among Black women than White women.[24]

Moving to the differential impacts of the reform on IPV, we observe a higher reduction of both the incidence and intensity in sexual and physical IPV among Black women with two children or more after OBRA-93 than among those with only one child relative to their counterparts with no qualifying children. This stronger impact for the higher parity is driven by sexual violence. However, smaller sample sizes for this racial group lead to less precision in our IPV estimates as the relevant coefficients are only marginally statistically significant at the 10 percent level.

Among White women, we also observe a higher reduction in intensity in sexual and physical IPV among those with two children or more after OBRA-93 than those with only one child relative to their counterparts with no qualifying children. This effect is driven by a relatively higher reduction in intensity in physical IPV for those with higher parity. In contrast, OBRA-93 led to a reduction in intensity in sexual IPV among mothers of both one child and two or more children relative to their counterparts with no qualifying children. In general, these estimates are more precisely estimated as the sample sizes are almost four times larger than those of Black women.

Overall, we feel reassured to generally see stronger impacts for mothers with 2+ children relative to 1 (though there is some variation across subgroups and outcomes),

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[24] This larger effect for Black women is also consistent with the literature. For Black women, the employment of mothers with two or more children increased by 7.6 percentage points relative to women with no children (relative to a pre-OBRA-93 control mean of 42.9%). This coefficient is statistically significant at the 1 percent level and is more than twice as large as the effect on mothers with only one child (a non-statistically significant 3.1 percentage points).

which is particularly true for Black women. This implies the groups with the largest treatment in terms of expanded tax credits, generally, saw the largest declines in IPV.

Economic Impact

To first put the economic impacts into perspective, we scale the change in IPV by the amount of after-tax income received from the OBRA-93 expansion. Since there is no detailed information on income or EITC receipt in the NCVS, we used pooled years of the March CPS (1991-2000), along with the NBER taxsim program, to predict the impact of the OBRA-93 expansion on after-tax income (assuming full take-up of the EITC).[25] We inflation adjust all income to be in 2010 dollars. Table 7 shows these results for the economic impact of our main specification comparing women with one or more children to those with no children.[26] More specifically, it translates our treatment effects into treatment on the treated impacts per $1,000 of increased after-tax income. The first row lists the estimated impacts from Tables 4 and 5. The second row lists the estimated average increase in after-tax income that we estimated using the NBER taxsim program in the March CPS. In the third row, we scale the results to be in terms of a $1,000 increase by dividing row 1 by total increase in after-tax income and multiplying by 1,000. We finally divide by the pre-OBRA-93 mean of women with children: so the impacts are as a percent of the mean (the mean itself is given in row 4). This exercise implicitly assumes that the full impacts on IPV are due to changes in income. This assumption is likely unrealistic, because extensive margin employment changes and this could have independent effects on IPV. However, our estimated effects still offer a useful scaling, particularly for comparing these results to the larger literature on income and domestic violence.

For all women, our results imply that an additional $1,000 decreases the intensity of physical violence by 15.86%. We see larger effects for sexual violence, with sexual violence decreasing by 24% and 48%. Overall effects fall between these two estimates,

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[25] March CPS does not ask interview recipients about their EITC receipt. The taxsim program provides an estimate of EITC income (and other tax and transfers) based on household income and other characteristics.

[26] These estimates of the impact of the expansion on EITC dollars received by each of these groups (relative to mothers with no qualifying children) were calculated by estimating equation 1 on predicted after tax income. We follow Hoynes et al. (2015): for women who are heads of households or heads of subfamilies, we impute qualifying amount of EITC using their income and number of children in her family. For those who are not heads of household/family, we impute qualifying EITC using zero children and their own income. TAXSIM then predicts after tax income and EITC using CPS values on marital status, number of dependent, and income, which we use as the dependent variable in our regressions.

respectively, with the incidence and intensity of any type of violence falling by 9.73% and 21%, respectively.

These estimates are in line with those in the literature. For instance, Aizer (2010) estimates that the decline in the gender wage gap witnessed in the state of California between 1990 and 2003 explains about 9% of the reduction in female hospitalizations for assault. More recently, González and Rodríguez-Planas (2020) find that one standard deviation increase in gender equality in the country-of-ancestry is associated with a 28% decrease in the incidence of IPV (with respect to the mean), and a 43% decrease in the intensity of IPV among first- and second-generation immigrants in 28 European countries in 2012. In Ecuador in 2003, Hidrobo and Fernand (2013) find that a cash transfer of 100,000 Ecuadorian sucres (approximately USD 15 or the equivalent to a 6% to 10% of an average household's pre-transfer expenditure for households in the bottom two poverty quintiles) per month for women with greater than primary school education decreased by 14% the probability that a husband or partner engages in emotional violence and by 25% the probability that a husband or partner engages in controlling behaviors by 25%. In Northern Ecuador in 2011, Hidrobo, Peterman, and Heise (2016) find that 6 monthly cash transfers of $40 dollars per month (the equivalent of 11% of a household's pre-transfer monthly consumption) offered to refugees and poor mothers from poor urban areas in Ecuador decreased controlling behavior by 27% and moderate physical violence of 23% from baseline control means.

V. Robustness Checks

One concern is how to cluster our standard errors. Abadie et al. (2017) argues that clustering should happen at the level at which treatment is assigned. In our case there is only variation across three groups in treatment: no children, one child, or two or more children. It is impossible to estimate standard errors with so few clusters, even with the available small cluster corrections (Cameron and Miller, 2015). We therefore take the following strategy: for our main results we present robust standard errors. Then, we show the robustness of our baseline findings to clustering on a range of different reasonable categories that plausibly could have autocorrelation between them to attempt to assuage concerns that our estimated standard errors are too small.

Appendix Table A.3 shows these results. The first row presents our baseline estimates using heteroskedasticity robust standard errors. We then cluster in subsequent

rows by: number of Children (0, 1, 2+) by year, number of children by year-quarter, number of children by race-quarter, number of children by racial group,[27] and number of children by race group-year. There is little change in the standard errors regardless of how we cluster. This is true even when moving from more aggregated clusters by time (# of children by year) to less aggregated ones (# of children by year-month). We take this as evidence that our results are largely robust to clustering.

One indication of a valid quasi-experiment is that adding exogenous demographic controls should have no little to no effect on the estimated treatment effect. We show this in Appendix Table A.4 by demonstrating robustness to model specification. The first row shows the coefficients when dropping all of our control variables[28], the second row shows our baseline specification for comparison, the third row additionally controls for our number of children groups (0, 1, 2+) interacted with the various demographic controls (age, education, race groups). This later specification allows for differential effects of our main exogenous variables by treatment. Across these different models there is little to no change in the coefficients.

VI. Mechanisms

So far, we have documented a decline in IPV for those mothers likely to qualify for the earned income tax credit. We find larger effects for Black mothers, unmarried mothers, and those who qualify for a larger credit due to having two or more children. While we have scaled these impacts in terms of predicted increases in income, there could, in fact, be several mechanisms by which the OBRA-93 expansion of the EITC decreased domestic violence. Below, we in turn investigate: (1) increased work at the extensive margin, (2) increased household after-tax income, (3) changes in time use associated with working more, (4) more "cash on hand" with cash returns, and (5) changes in living conditions.

Two first-order impacts of the EITC are increasing employment at the extensive margin and increasing after-tax income. Income effects come from both more work and the credit itself. Work could independently decrease IPV by increasing self-sufficiency and the social networks of the mother. Alternatively, higher monthly income could

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[27] The idea behind clustering on racial group is that there might be race specific issues in the trends over time in domestic violence that generates autocorrelation in the error term that could make the standard errors too small but would be corrected for by clustering.

[28] We have also checked to make sure the event study did not change when dropping the time-varying control variables. The event study is nearly identical without controls: with results available open request.

directly provide the resources a mother needs to escape an abusive relationship. Figure 3 takes an initial descriptive approach to answer this question by estimating equation 1 on domestic violence, employment, and after-tax income across different subgroups.

The top panel of Figure 3 plots coefficients from the impact of the OBRA-93 expansion on employment (the X-axis coordinate) and IPV (the Y-axis coordinate): where each point in the graph represents the effects for a different subgroup. The bottom panel of Figure 3 does the same thing but instead with predicted after-tax income (estimated from the 1992-2000 March CPS using taxsim) on the X-axis. In essence, we are using variation by subgroup to test the relative degree with which employment/income seems to correlate with IPV impacts. We see a relatively strong correlation with employment: subgroups experiencing the largest increases in employment also experiencing the largest declines in domestic violence (ex: Blacks, unmarried mothers, and those with a high school education or less). The bottom panel of Figure 3 shows a mostly flat but slightly positive relationship between IPV and predicted after-tax income. While it is important not to over-interpret correlations, these results suggests that the impacts of increased work at the extensive margin plays a greater role than increased income in decreasing IPV. This could be because having some regular money through a paycheck matters more than a gradient in the amount, the social network effects associated with working, or simply that those subgroups most likely to increase work in response to the policy are also more likely to see declines in IPV.

The above analysis in Figure 3 is largely descriptive. To further explore direct effects of employment through exposure or time use, we examine whether there are stronger effects during different hours of the day. Since one might expect increased work to change in the time at which domestic violence occurs due to reducing time spent at home.[29] The data allows us to identify whether the incident occurred between the hours of 6AM and 6PM, a time period that covers the standard workday.[30] Table 8 shows the impact of the EITC by the timing of the incident. We see an equal decline in IPV both during the day and night, indicating that reduced exposure to a partner from changes in time use is unlikely an important mechanism.[31]

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[29] Aizer (2014) uses a similar strategy to examine exposure effects on IPV, by using weekend versus weekday incidents.

[30] The data allows us to identify only if the incident occurred within four time ranges: 6AM to noon, noon to 6PM, 6PM to midnight and midnight to 6AM.

[31] Since low-wage workers often have non-standard hours, we view this test as suggestive rather than conclusive.

To explore the role of an increase in a large lump sum payment with the tax returns (or "cash on hand"), we turn to Table 9 which estimates impacts of the EITC on IPV by quarters. In columns (1) to (4), the dependent variable indicates the presence of IPV for the associated quarter. The reason for estimating effects across quarters of the year is that the vast majority of EITC recipients receive their payment as a lump sum check in February (Lalumia 2013). If the benefit of the EITC for reducing IPV comes primarily from having more "cash on hand", then we would expect strongest impacts in the first quarter of the year. Table 9 instead shows that, while the impacts are negative across quarters, the largest and only significant effects are in quarter 3—which ranges from July to September. This suggests that having more "cash on hand" is not the key mechanism.

Why would the EITC largely decrease IPV in the summer? The literature has documented a causal effect of high temperatures on violence, with high temperatures leading to an increase of 20% in prison violence (Mukherjee and Sanders, 2021) and a large increase in intimate partner violence and femicides (Sanz-Barbero et al, 2018; Henke and Hsu, 2020). Goodman-Bacon and McGranahan (2008) and Fisher and Rehkopf (2022) find that large EITC influxes generate particularly strong spending on durable goods, including household goods, home appliances and cars. As such, one possible mechanism for the reduction in IPV may be the purchase of durable goods, such as air conditioning units and fans that may allow recipients to avoid some of the negative effects of high temperatures, or through the purchase of cars, which allows mobility outside of the household.[32]

Another related possibility is that leases tend to end in the summer allowing families to move. More money could allow an unmarried woman to move away from an abusive domestic partner. Table 10 estimates the impact of the EITC on a variety of measures of moving: months since last move, and an indicator for moving in the past six months or a year. We find no overall relationship between the EITC and an increased likelihood of moving. However, it is still possible that the EITC promoted housing stability by decreasing the likelihood of eviction (which could lead to moving in with an abusive partner).

Overall, we can rule out a decline in IPV just from having more "cash on hand" in the month of tax returns or from an increased likelihood of moving, and we find some evidence that reduced exposure to violence due to time spent at work is not a key

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[32] Unfortunately, we do not have data on household purchases to test this theory.

mechanism. In contrast, we find suggestive evidence that increased work, with related increases in income, a regular paycheck, and improved social networks: may ameliorate the worst effects of IPV during the summer, either through increased bargaining power or by allowing purchases that prevent violence.

VII. Conclusion

In this paper, we estimate the causal effect of a major expansion of the Earned Income Tax Credit on IPV. An additional $1,000 in after tax income is associated with reduced counts of sexual and physical violence by about 21% for unmarried women, with larger relative effects on sexual violence and for unmarried Black women. We test mechanisms related to work, higher after-tax income, higher "cash on hand" from tax returns, changes in living conditions during the summer, and exposure effects from time spent at work. Our results are robust to a large range of placebo and specification tests, and we find no pre-trends through an event study.

These findings affirm a feminist theory of IPV, which argues that increased resources, empowerment, and economic self-sufficiency enable women to avoid being abused. Likewise, our findings contradict the "evolutionary" view which posits that when women gain more resources and self-sufficiency men compensate with violence in order to re-assert control in the relationship or extract resources from their female partners. While we cannot rule out the fact that such retribution occurs, our findings imply that the net effect of increased earned income from the EITC expansion is to decrease IPV.

A quick back of the envelope analysis provides a sense of the monetary benefits to society from decreased IPV. According to the report "Costs of Intimate Partner Violence Against Women in the United States" (CDC 2003) total costs come to $103,767 per victimized woman (including criminal justice and lost productivity costs). We estimate that pre-1993 there were on average 444,133 unmarried women between the ages of 16-40 without a college degree who reported IPV (physical or sexual).[33] Taking our treatment on the treated estimates from table 7: $1000 spending per recipient of the EITC, decreases physical and sexual abuse by 9.37% implying about 41,615 fewer women experiencing IPV after the expansion.[34] This in turn generates a gross benefit of roughly

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[33] Weighted the NCVS survey says there are about 41,500,000 unmarried women ages 16-40 without a college degree pre-1993; and 0.0107 percent of these report an incidence. 41,500,000*.0107 = 444,133 women. This is potentially a lower bound estimate if IPV is under reported in the NCVS.
[34] 444,133*0.0937 = 41,615.

4.3 billion in 2003 dollars.[35] Alternatively, looking at the direct effect of medical and mental health costs, the CDC estimates a cost of $92 per incident with our estimates from Table 7 suggesting a 48% decline in the total counts of incidences.

A caveat to the above back of the envelope calculation is that we do not have causal estimates of reduction in costs, and these reflect average costs rather than marginal costs. Regardless, the above analysis descriptively implies large monetary benefits to society by investing in improving women's economic self-sufficiency to reduce domestic violence.

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[35] 41,615 women multiplied by the CDC cost of $103,767 is approximately 4.3 billion dollars.

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Figure 1. Maximum Credit for Federal EITC by Tax Year and Number of Qualifying Children
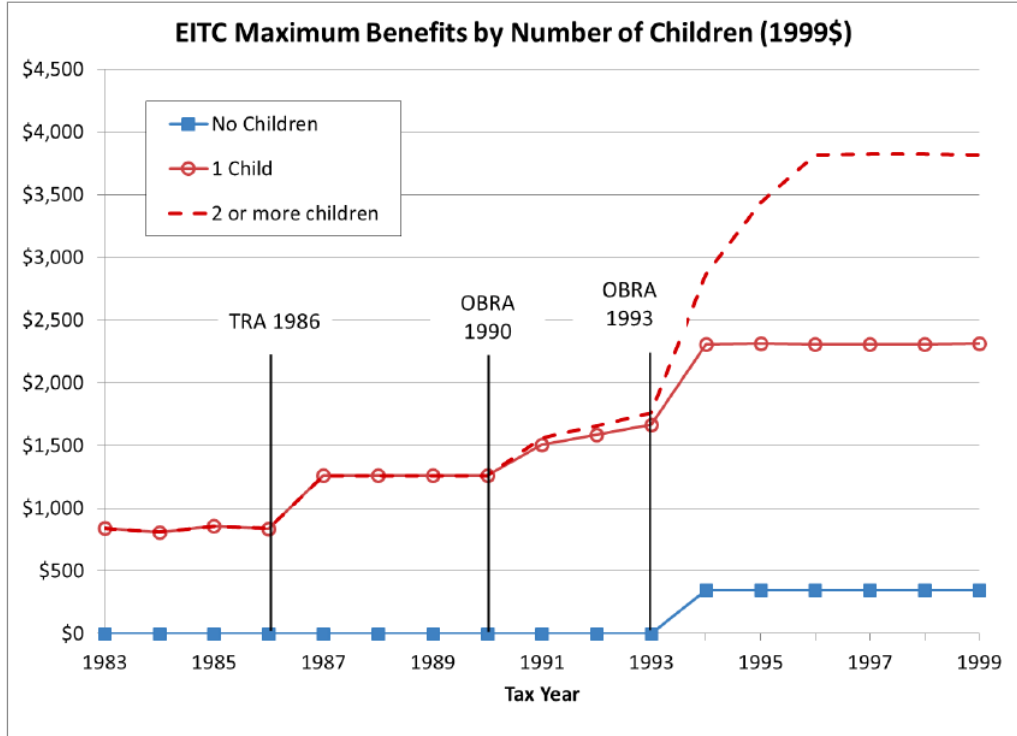
Source: Reprinted from Hoynes, Miller, and Simon (2015).

Figure 2. Event Study Analysis of Physical and Sexual Intimate Partner Violence Counts among Women with Less than a Four-Year College Degree

Notes: Standard errors robust to heteroskedasticity are in parenthesis. *** p<0.01, ** p<0.05, * p<0.1. All samples include age groups 16- to 40-year-old women with less than a four-year college degree. In our sample, the NCVS, year 0 corresponds to survey round 1995, when citizens started to receive the EITC payments for 1994, in which the OBRA-93 went into effect. Year 3 corresponds to survey round 1998, representing the tax year 1997, when the OBRA-93 was fully implemented. Event study coefficients were obtained from the estimates of equation (2). Each model controls for race indicators, age, educational attainment, year and month fixed effects, and the number of children.

Figure 3: Treatment Effects on Employment, Income, and IPV across Subgroups

The Scatterplot of the Effect Sizes on
Domestive Violence Versus Employment by Sub-sample

(Y-axis: The Impact of OBRA-93 on # DV Count)
(X-axis: The Impact of OBRA-93 on Employment)

Data points labeled: Unmarried Non-College, All, Non-White, Black, Unmarried, White, HS or Less

The Scatterplot of the Effect Sizes on Domestive Violence Versus Income by Sub-sample
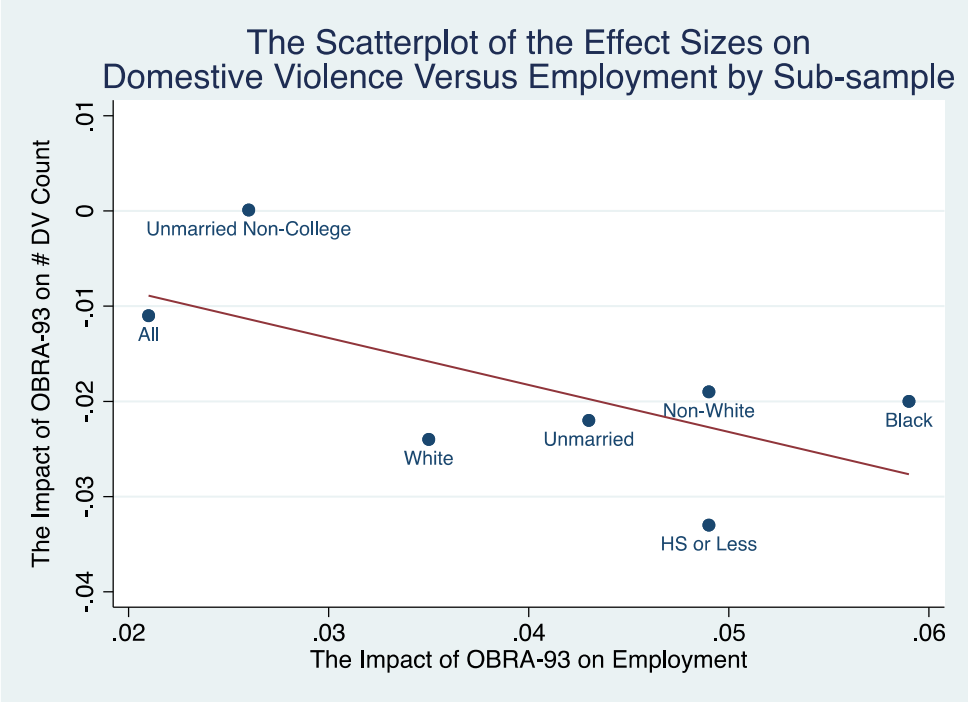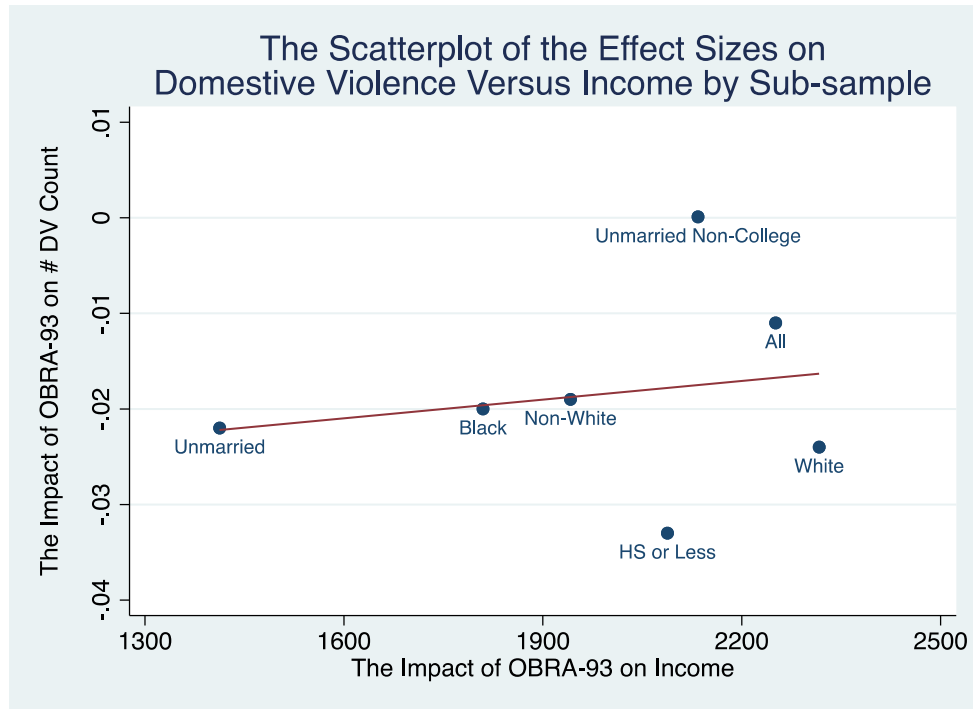
Notes: The X-axis on the top panel of figure 4 shows the impact of the Obra93 expansion on employment using the NCVS. The X-axis on the bottom panel shows predicted after tax income (with EITC receipt predicted using taxsim) using data from the 1992 to 2000 March CPS. The Y-axis on both figures is the impact on domestic violence (from the NCVS). Each point represents a different subgroup whose effects (on work, income, and domestic violence) were estimated using equation 1. See the text for more details.

Table 1: Coding of Physical and Sexual Assault by Partner in Past 6 Months

Physical Assault	Completed aggravated assault with injury
	Attempted aggravated assault with weapon
	Threatened assault with weapon
	Simple assault completed with injury
	Assault without a weapon and without injury
	Any physical assault that included an attempted or completed robbery
Sexual Assault	Completed rape
	Attempted rape
	Sexual attack with serious assault
	Sexual attack with minor assault
	Sexual assault without injury
	Unwanted sexual contact without force

Source: National Crime Victimization Survey (NCVS). Variable for the type of crime code V4528. Variable to identify relationship to offender V4245.

Table 2. Summary Statistics Before OBRA-93, Women 16 to 40 years old with less than a four-year college degree

	(1)	(2)	(3)
Variable	Children >=1	Children ==0	(2)-(1)
Other Race	0.0085	0.0079	0.0004
	(0.0919)	(0.0886)	(0.0007)
Black	0.1736	0.1264	0.0407***
	(0.3788)	(0.3324)	(0.0028)
Asian	0.0258	0.0287	-0.0020
	(0.1585)	(0.1669)	(0.0013)
Hispanic	0.1297	0.0852	0.0425***
	(0.3360)	(0.2791)	(0.0026)
Ages 16 to 19	0.1807	0.1551	0.0193***
	(0.3848)	(0.3620)	(0.0029)
Ages 20 to 29	0.3240	0.5116	-0.1851***
	(0.4680)	(0.4999)	(0.0038)
Ages 30 to 39	0.4585	0.2949	0.1660***
	(0.4983)	(0.4560)	(0.0039)
1 if completed high school, 0 otherwise	0.4753	0.4321	0.0453***
	(0.4994)	(0.4954)	(0.0040)
1 if some college, 0 otherwise	0.2717	0.4780	-0.1990***
	(0.4448)	(0.4995)	(0.0037)
Married	0.5397	0.3092	0.2350***
	(0.4984)	(0.4622)	(0.0039)

HH Income	30,705.8	29,332.4	1,373.4663***
	(22056.2)	(22743.9)	(187.0852)
HH Income < 25,000	0.4742	0.5134	-0.0399***
	(0.4993)	(0.4998)	(0.0042)
Observations	53,767	21,948	

Notes: Standard deviations in parentheses. Source: National Crime Victimization Survey (NCVS). *** p<0.01, ** p<0.05, * p<0.1.

Table 3. Outcome Variables, Women 16 to 40 years old with less than a four-year college degree

Variable	Pre-OBRA-93			Post-OBRA-93			Post-Pre
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Children >= 1	Children == 0	(1)-(2)	Children >= 1	Children == 0	(4)-(5)	DiD (6)-(3)
Physical Assault Dummy	0.0066	0.0043	0.0023***	0.0059	0.0041	0.0018***	-0.0005
	(0.0813)	(0.0655)	(0.0006)	(0.0766)	(0.0639)	(0.0004)	(0.0007)
Physical Assault Count	0.0214	0.0126	0.0095**	0.0148	0.0120	0.0028	-0.0066
	(0.5071)	(0.3651)	(0.0039)	(0.4100)	(0.4137)	(0.0023)	(0.0043)
Sexual Assault Dummy	0.0010	0.0004	0.0004*	0.0005	0.0007	-0.0001	-0.0006**
	(0.0309)	(0.0210)	(0.0002)	(0.0230)	(0.0268)	(0.0001)	(0.0002)
Sexual Assault Count	0.0041	0.0006	0.0035**	0.0012	0.0028	-0.0011*	-0.0047***
	(0.2095)	(0.0357)	(0.0015)	(0.0850)	(0.2030)	(0.0007)	(0.0014)
Physical or Sexual Assault	0.0074	0.0047	0.0025***	0.0063	0.0047	0.0016***	-0.0009
	(0.0856)	(0.0685)	(0.0007)	(0.0791)	(0.0685)	(0.0004)	(0.0007)
Physical or Sexual Count	0.0255	0.0132	0.0130***	0.0160	0.0148	0.0017	-0.0113**
	(0.5886)	(0.3685)	(0.0045)	(0.4221)	(0.4620)	(0.0024)	(0.0047)
Worked	0.5583	0.7338	-0.1727***	0.5858	0.7455	-0.1649***	0.0083*
	(0.4966)	(0.4420)	(0.0039)	(0.4926)	(0.4356)	(0.0026)	(0.0046)
Observations	53,767	21,948		11,7191	46,129		

Notes: Standard deviations in parentheses. Source: National Crime Victimization Survey (NCVS).   *** p<0.01, ** p<0.05, * p<0.1.

Table 4. Baseline Estimates, Women 16 to 40 years old with less than a four-year college degree (unless otherwise stated)

VARIABLES	(1) Physical Abuse Dummy	(2) Physical Abuse Count	(3) Sexual Abuse Dummy	(4) Sexual Abuse Count	(5) Physical or Sexual Abuse Dummy	(6) Physical or Sexual Abuse Count	(7) Worked
Panel A: All							
Post-OBRA-93 x Children >= 1	-0.001	-0.006	-0.001***	-0.005***	-0.001	-0.011**	0.021***
	(0.001)	(0.004)	(0.000)	(0.002)	(0.001)	(0.005)	(0.005)
Observations	239,035	239,035	239,035	239,035	239,035	239,035	236,854
Panel B: Unmarried women							
Post-OBRA-93 x Children >= 1	-0.002	-0.014**	-0.001**	-0.008***	-0.003**	-0.022***	0.043***
	(0.001)	(0.007)	(0.000)	(0.003)	(0.001)	(0.007)	(0.006)
Observations	123,954	123,954	123,954	123,954	123,954	123,954	122,761
Panel C:							
Placebo Test: 22 to 40 years old single women with at least a 4-Year College Degree							
Post-OBRA-93 x Children >= 1	-0.004	-0.003	-0.000	0.003	-0.004	0.000	0.026*
	(0.004)	(0.014)	(0.002)	(0.004)	(0.004)	(0.016)	(0.013)
Observations	30,294	30,294	30,294	30,294	30,294	30,294	30,024

Notes: Standard errors robust to heteroskedasticity are in parenthesis. *** p<0.01, ** p<0.05, * p<0.1. Each model controls for race indicators, age, educational attainment, year and month fixed effects, and the number of children.

Table 5. Subgroup analysis: Single women 16 to 40 years old with less than a four-year college degree (unless otherwise stated)

VARIABLES	(1) Physical Abuse Dummy	(2) Physical Abuse Count	(3) Sexual Abuse Dummy	(4) Sexual Abuse Count	(5) Physical or Sexual Abuse Dummy	(6) Physical or Sexual Abuse Count	(7) Worked
Panel A: White							
Post-OBRA-93 x Children >= 1	-0.001	-0.015*	-0.001	-0.009***	-0.002	-0.024**	0.035***
	(0.001)	(0.008)	(0.001)	(0.003)	(0.002)	(0.009)	(0.007)
Observations	93,856	93,856	93,856	93,856	93,856	93,856	93,013
Panel B: Non-White							
Post-OBRA-93 x Children >= 1	-0.003	-0.014	-0.002*	-0.005	-0.004*	-0.019*	0.049***
	(0.002)	(0.009)	(0.001)	(0.004)	(0.002)	(0.010)	(0.014)
Observations	30,098	30,098	30,098	30,098	30,098	30,098	29,748
Panel C: Black							
Post-OBRA-93 x Children >= 1	-0.005*	-0.015	-0.002*	-0.005	-0.005*	-0.020*	0.059***
	(0.003)	(0.010)	(0.001)	(0.004)	(0.003)	(0.012)	(0.016)
Observations	25,222	25,222	25,222	25,222	25,222	25,222	24,937
Panel D: HS or Less							
Post-OBRA-93 x Children >= 1	-0.003	-0.022**	-0.001**	-0.011***	-0.004**	-0.033***	0.049***
	(0.002)	(0.008)	(0.001)	(0.004)	(0.002)	(0.010)	(0.008)
Observations	82,438	82,438	82,438	82,438	82,438	82,438	81,601

Notes: Standard errors robust to heteroskedasticity are in parenthesis. *** p<0.01, ** p<0.05, * p<0.1. Each model controls for race indicators, age, educational attainment, year and month fixed effects, and the number of children.

Table 6. By Parity: Single women 16 to 40 years old with less than a four-year college degree

VARIABLES	(1) Physical Abuse Dummy	(2) Physical Abuse Count	(3) Sexual Abuse Dummy	(4) Sexual Abuse Count	(5) Physical or Sexual Abuse Dummy	(6) Physical or Sexual Abuse Count	(7) Worked
Panel A: All							
Post-OBRA-93 x Children = 1	-0.003*	-0.009	-0.001	-0.007**	-0.003**	-0.017*	0.030***
	(0.002)	(0.008)	(0.001)	(0.003)	(0.002)	(0.009)	(0.008)
Post-OBRA-93 x Children 2+ x After	-0.001	-0.018**	-0.001**	-0.008**	-0.002	-0.026***	0.053***
	(0.001)	(0.008)	(0.001)	(0.003)	(0.002)	(0.009)	(0.007)
Observations	123,954	123,954	123,954	123,954	123,954	123,954	122,761
Panel B: White							
Post-OBRA-93 x Children = 1	-0.003	-0.008	-0.001*	-0.009**	-0.004**	-0.017	0.031***
	(0.002)	(0.010)	(0.001)	(0.004)	(0.002)	(0.011)	(0.009)
Post-OBRA-93 x Children 2+ x After	-0.000	-0.021*	-0.001	-0.008**	-0.001	-0.030**	0.038***
	(0.002)	(0.011)	(0.001)	(0.004)	(0.002)	(0.013)	(0.008)
Observations	93,856	93,856	93,856	93,856	93,856	93,856	93,013
Panel C: Black							
Post-OBRA-93 x Children = 1	-0.004	-0.015	-0.000	-0.002	-0.003	-0.016	0.031
	(0.003)	(0.014)	(0.001)	(0.003)	(0.003)	(0.015)	(0.019)
Post-OBRA-93 x Children 2+ x After	-0.005*	-0.015	-0.002*	-0.007	-0.006*	-0.022*	0.076***
	(0.003)	(0.011)	(0.001)	(0.006)	(0.003)	(0.013)	(0.017)
Observations	25,222	25,222	25,222	25,222	25,222	25,222	24,937

Notes: Standard errors robust to heteroskedasticity are in parenthesis. *** p<0.01, ** p<0.05, * p<0.1. Each model controls for race indicators, age, educational attainment, year and month fixed effects, and the number of children.

Table 7. Economic Impacts Unmarried Women 16-40 with Less than a College Degree

VARIABLES	(1) Physical Abuse Dummy	(2) Physical Abuse Count	(3) Sexual Abuse Dummy	(4) Sexual Abuse Count	(5) Physical or Sexual Abuse Dummy	(6) Physical or Sexual Abuse Count
Panel A: All						
Children 1+ vs 0						
Treatment Effect	-0.002	-0.014**	-0.001**	-0.008***	-0.003**	-0.022***
Increase in After Tax Income	$2,251	$2,251	$2,251	$2,251	$2,251	$2,251
ToT per $1000, % Impact	-7.22%	-15.86%	-24.68%	-48.68%	-9.73%	-21.06%
Mean	0.0123	0.0392	0.0018	0.0073	0.0137	0.0464

Notes: This table scales the coefficients of the OBRA93 expansion on IPV estimated in tables 4 and 5 by the estimted $ increase in aftertax income. Aftertax income includes predicted EITC eligibility imputed using taxsim. All amounts are inflation adjusted to be in 2010 dollars.

Table 8: The Impact of EICT on DV by the Day of the Time

VARIABLES	(1) Day	(2) Night
Post-OBRA-93 x Children >= 1	-0.002**	-0.002*
	(0.001)	(0.001)
Observations	123,954	123,954
R-squared	0.003	0.002

Notes: Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1 'Day' and 'Night' are defined as the hours between 6AM to 6PM and 6PM to 6AM, respectively. Each model controls for race indicators, age, educational attainment, year and month fixed effects, and the number of children.

Table 9: The Impact of EICT on DV by the Incident Quarter

VARIABLES	(1) DV= 1 & Quarter = 1	(2) DV= 1 & Quarter = 2	(3) DV= 1 & Quarter = 3	(4) DV= 1 & Quarter = 4
Post-OBRA-93 x Children >= 1	-0.000	-0.000	-0.002***	-0.001
	(0.001)	(0.001)	(0.001)	(0.001)
Observations	123,954	123,954	123,954	123,954
R-squared	0.002	0.002	0.003	0.003

Notes: Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1 Each model controls for race indicators, age, educational attainment, year and month fixed effects, and the number of children.

Table 10. Moving Estimates by Subsample

VARIABLES	(1) All	(2) unmarried	(3) unmarried white	(4) unmarried non-white	(5) unmarried Black	(6) unmarried HS or Less
Panel A: Months Since Moved						
Post-OBRA-93 x Children >= 1	-0.015	0.005	0.061	-0.196	0.043	0.031
	(0.065)	(0.087)	(0.099)	(0.183)	(0.096)	(0.163)
Observations	239,035	123,954	93,856	30,098	98,732	41,516
Panel B: Moved within 6 Months						
Post-OBRA-93 x Children >= 1	0.002	-0.001	-0.000	-0.006	-0.000	-0.003
	(0.002)	(0.003)	(0.003)	(0.006)	(0.003)	(0.005)
Observations	239,035	123,954	93,856	30,098	98,732	41,516
Panel C: Moved Past Year						
Post-OBRA-93 x Children >= 1	0.001	-0.001	0.001	-0.010	0.000	-0.001
	(0.002)	(0.003)	(0.004)	(0.007)	(0.004)	(0.006)
Observations	239,035	123,954	93,856	30,098	98,732	41,516

Notes: Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1. Each model controls for race indicators, age, educational attainment, year and month fixed effects, and the number of children.

Table 11: The Impact of EICT on DV by the Incident Month

VARIABLES	(1) DV = 1 & Month = 1	(2) DV = 1 & Month = 2	(3) DV = 1 & Month = 3	(4) DV = 1 & Month = 4	(5) DV = 1 & Month = 5	(6) DV = 1 & Month = 6	(7) DV = 1 & Month = 7	(8) DV = 1 & Month = 8	(9) DV = 1 & Month = 9	(10) DV = 1 & Month = 10	(11) DV = 1 & Month = 11	(12) DV = 1 & Month = 12
Post-OBRA-93 x Children >= 1	0.0002	-0.0000	-0.0002	-0.0001	0.0000	0.0000	-0.0004	-0.0006	-0.0008**	-0.0001	-0.0005	-0.0002
	(0.0003)	(0.0004)	(0.0005)	(0.0004)	(0.0003)	(0.0004)	(0.0004)	(0.0004)	(0.0003)	(0.0003)	(0.0004)	(0.0004)
Observations	123,954	123,954	123,954	123,954	123,954	123,954	123,954	123,954	123,954	123,954	123,954	123,954
R-squared	0.001	0.001	0.002	0.001	0.001	0.001	0.002	0.001	0.002	0.001	0.002	0.001

Notes: Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1 Each model controls for race indicators, age, educational attainment, year and month fixed effects, and the number of children.

Appendix Figure 1. Placebo Event Study Analysis of Physical and Sexual Intimate Partner Violence Counts among Women with at least a Four-Year College Degree

Panel A. All Women with >= 4-Year College Degree

Panel B. Unmarried Women with >= 4-Year College Degree

Notes: Standard errors robust to heteroskedasticity are in parenthesis. *** p<0.01, ** p<0.05, * p<0.1. All samples include age groups 16- to 40-year-old women with at least a four-year college degree. In our sample, the NCVS, year 0 corresponds to survey round 1995, when citizens started to receive the EITC payments for 1994, in which the OBRA-93 went into effect. Year 3 corresponds to survey round 1998, representing the tax year 1997, when the OBRA-93 was fully implemented. Event study coefficients were obtained from the estimates of equation (2). Each model controls for race indicators, age, educational attainment, year and month fixed effects, and the number of children.

Appendix Table A.1. Summary Statistics Before OBRA-93, Unmarried women 16 to 40 years old with less than a four-year college degree

Variable	Children >= 1	Children = 0	(1)-(2)
Other Race	0.0103	0.0089	0.0018
	(0.1010)	(0.0938)	(0.0011)
Black	0.2729	0.1478	0.1162***
	(0.4455)	(0.3549)	(0.0042)
Asian	0.0240	0.0305	-0.0055***
	(0.1531)	(0.1720)	(0.0017)
Hispanic	0.1380	0.0788	0.0574***
	(0.3449)	(0.2694)	(0.0034)
Ages 16 to 19	0.3790	0.2098	0.1617***
	(0.4851)	(0.4072)	(0.0048)
Ages 20 to 29	0.3285	0.5199	-0.1894***
	(0.4697)	(0.4996)	(0.0050)
Ages 30 to 39	0.2732	0.2410	0.0372***
	(0.4456)	(0.4277)	(0.0047)
1 if high school, 0 otherwise	0.4161	0.4019	0.0180***
	(0.4929)	(0.4903)	(0.0052)
1 if some college, 0 otherwise	0.2133	0.5105	-0.2942***
	(0.4097)	(0.4999)	(0.0047)
HH Income	25,955.8	26,583.4	-456.3725*
	(22,689.1)	(22,637.6)	(252.5490)
HH Income < 25,000	0.6022	0.5814	0.0216***
	(0.4895)	(0.4934)	(0.0054)
Observations	23,403	14,708	

Notes: Standard deviations in parentheses. Source: National Crime Victimization Survey (NCVS). ***
p<0.01, ** p<0.05, * p<0.1. Each model controls for race indicators, age, educational attainment, year and month fixed effects, and the number of children.
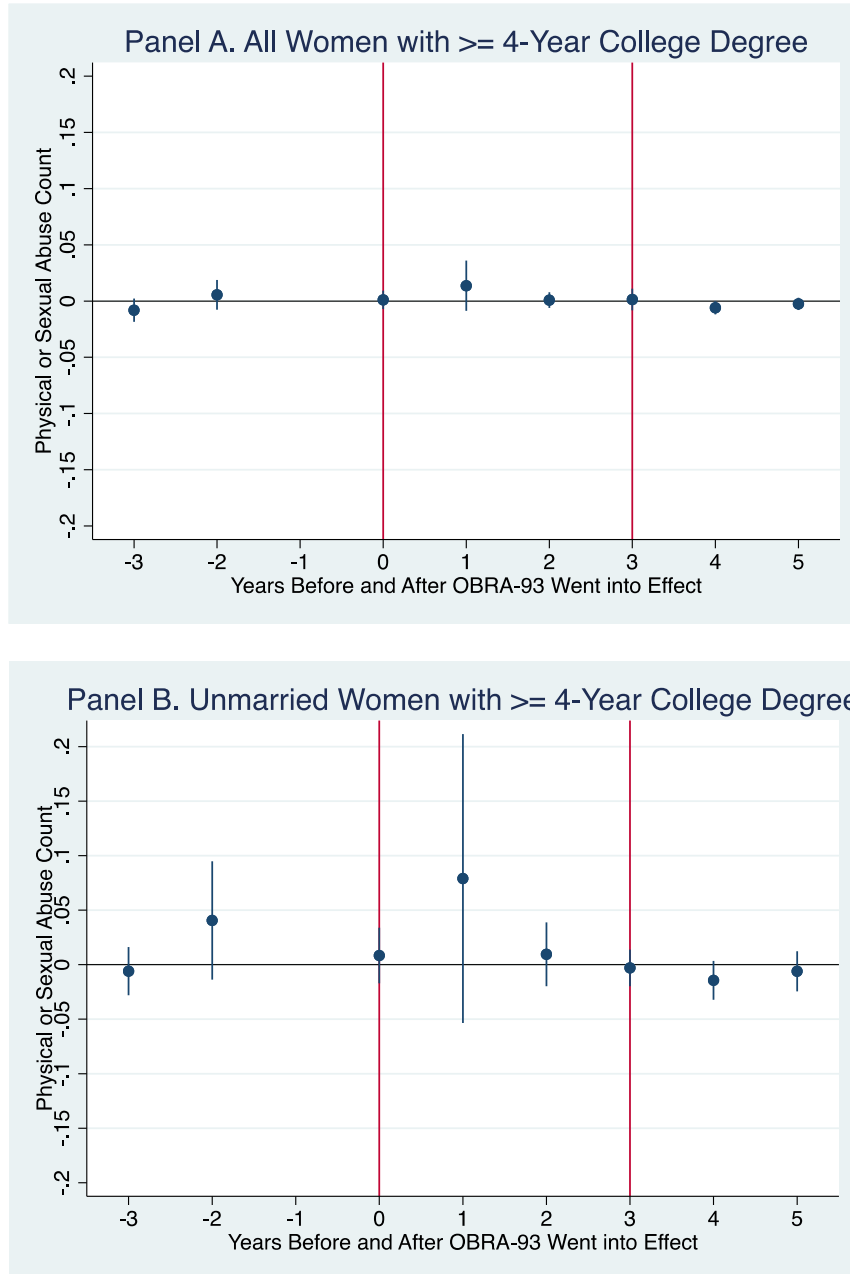
Appendix Table A.2. Outcome Variables, Unmarried women 16 to 40 years old with less than a four-year college degree

Variable	Before OBRA-93 Children >= 1	Before OBRA-93 Children = 0	Before OBRA-93 (1)-(2)	After OBRA-93 Children >= 1	After OBRA-93 Children = 0	After OBRA-93 (4)-(5)	DID (6)-(3)
Physical Assault Dummy	0.0123	0.0055	0.0071***	0.0103	0.0052	0.0055***	-0.0016
	(0.1104)	(0.0740)	(0.0011)	(0.1008)	(0.0722)	(0.0007)	(0.0012)
Physical Assault Count	0.0393	0.0141	0.0284***	0.0265	0.0149	0.0135***	-0.0150**
	(0.6959)	(0.3542)	(0.0066)	(0.5602)	(0.4698)	(0.0039)	(0.0073)
Sexual Assault Dummy	0.0018	0.0006	0.0010***	0.0010	0.0009	0.0003	-0.0007*
	(0.0425)	(0.0253)	(0.0004)	(0.0322)	(0.0301)	(0.0002)	(0.0004)
Sexual Assault Count	0.0073	0.0009	0.0064**	0.0022	0.0036	-0.0005	-0.0069***
	(0.2767)	(0.0430)	(0.0025)	(0.1179)	(0.2329)	(0.0012)	(0.0024)
Physical or Sexual Assault	0.0137	0.0061	0.0078***	0.0110	0.0060	0.0056***	-0.0023*
	(0.1163)	(0.0778)	(0.0011)	(0.1045)	(0.0774)	(0.0007)	(0.0013)
Physical or Sexual Count	0.0465	0.0150	0.0348***	0.0287	0.0184	0.0130***	-0.0219***
	(0.8074)	(0.3593)	(0.0076)	(0.5774)	(0.5253)	(0.0041)	(0.0080)
Worked	0.5066	0.7191	-0.2062***	0.5664	0.7356	-0.1711***	0.0362***
	(0.5000)	(0.4494)	(0.0051)	(0.4956)	(0.4410)	(0.0034)	(0.0060)
Observations	23,403	14,708		54,173	31,670		

Notes: Standard deviations in parentheses. Source: National Crime Victimization Survey (NCVS). *** p<0.01, ** p<0.05, * p<0.1. Each model controls for race indicators, age, educational attainment, year and month fixed effects, and the number of children.

Appendix Table A.3. Robustness to Clustering at Different Levels, Ages 16 to 40 Unmarried Sample

VARIABLES	(1) Physical Abuse Dummy	(2) Physical Abuse Count	(3) Sexual Abuse Dummy	(4) Sexual Abuse Count	(5) Physical or Sexual Abuse Dummy	(6) Physical or Sexual Abuse Count	(7) Worked
Baseline, heterogeneity robust SEs (not clustered)							
Parity1+ x After	-0.002	-0.014**	-0.001**	-0.008***	-0.003**	-0.022***	0.043***
	(0.001)	(0.007)	(0.000)	(0.003)	(0.001)	(0.007)	(0.006)
Clustered at # of Children-by-Year							
Parity1+ x After	-0.002	-0.014***	-0.001***	-0.008***	-0.003**	-0.022***	0.043***
	(0.001)	(0.003)	(0.000)	(0.002)	(0.001)	(0.003)	(0.009)
Clustered at # of Children-by-Year Quarter							
Parity1+ x After	-0.002	-0.014**	-0.001**	-0.008***	-0.003**	-0.022***	0.043***
	(0.001)	(0.006)	(0.000)	(0.003)	(0.001)	(0.007)	(0.007)
Clustered at # of Children-by-Year Month							
Parity1+ x After	-0.002	-0.014**	-0.001**	-0.008***	-0.003**	-0.022***	0.043***
	(0.001)	(0.006)	(0.000)	(0.003)	(0.001)	(0.007)	(0.007)
Clustered at # of Children-by-Race							
Parity1+ x After	-0.002*	-0.014***	-0.001***	-0.008***	-0.003**	-0.022***	0.043**
	(0.001)	(0.004)	(0.000)	(0.001)	(0.001)	(0.003)	(0.017)
Clustered at # of Children -by-Race-Year							
Parity1+ x After	-0.002	-0.014***	-0.001***	-0.008***	-0.003**	-0.022***	0.043***
	(0.001)	(0.005)	(0.000)	(0.002)	(0.001)	(0.005)	(0.010)
Observations	123,954	123,954	123,954	123,954	123,954	123,954	122,761

Notes: Standard errors are in parenthesis. *** p<0.01, ** p<0.05, * p<0.1. Each model controls for race indicators, age, educational attainment, year and month fixed effects, and the number of children.

Appendix Table A.4. Robustness to Different Controls, Ages 16 to 40 Unmarried Sample

VARIABLES	(1) Physical Abuse Dummy	(2) Physical Abuse Count	(3) Sexual Abuse Dummy	(4) Sexual Abuse Count	(5) Physical or Sexual Abuse Dummy	(6) Physical or Sexual Abuse Count	(7) Worked
Limited Control Variables							
Parity1+ x After	-0.002	-0.014**	-0.001**	-0.008***	-0.003**	-0.021***	0.044***
	(0.001)	(0.007)	(0.000)	(0.003)	(0.001)	(0.007)	(0.006)
Full Control Variables							
Parity1+ x After	-0.002	-0.014**	-0.001**	-0.008***	-0.003**	-0.022***	0.043***
	(0.001)	(0.007)	(0.000)	(0.003)	(0.001)	(0.007)	(0.006)
Full Controls							
Parity1+ x After	-0.002	-0.020**	-0.001**	-0.011***	-0.003*	-0.031***	0.046***
	(0.002)	(0.008)	(0.001)	(0.004)	(0.002)	(0.010)	(0.008)
Observations	123,954	123,954	123,954	123,954	123,954	123,954	122,761

Notes: Standard errors robust to heteroskedasticity are in parenthesis. *** p<0.01, ** p<0.05, * p<0.1. Limited controls include year dummies number of children indicators. Full controls include race indicators, age, educational attainment, year dummies, month fixed effects, and number of children indicators.

Appendix Table A.5. Baseline Characteristics (Before OBRA-93 by Different Sample

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Age Range	*16 to 40*	*16 to 40*	*16 to 40*	*16 to 40*	*16 to 40*	*16 to 40*	*16 to 40*	*16 to 40*	*16 to 40*	*16 to 40*	*22 to 40*	*22 to 40*
Marital Status	*Unmarried*	*Unmarried*	*Unmarried*	*Unmarried*	*Unmarried*	*Unmarried*	*Unmarried*	*Unmarried*	*Unmarried*	*Unmarried*	*Unmarried*	*Unmarried*
Education	*<= Some College*	*<= Some College*	*<= Some College*	*<= Some College*	*<= Some College*	*<= Some College*	*<= Some College*	*<= Some College*	*<= HS*	*<= HS*	*4 Year College*	*4 Year College*
Race	*White*	*White*	*Non-White*	*Non-White*	*Black*	*Black*	*Hispanic*	*Hispanic*	*All*	*All*	*All*	*All*
Children	*Yes*	*No*	*Yes*	*No*	*Yes*	*No*	*Yes*	*No*	*Yes*	*No*	*Yes*	*No*
Physical Assault Dummy	0.0130	0.0058	0.0108	0.0043	0.0116	0.0037	0.0073	0.0056	0.0122	0.0068	0.0127	0.0026
	(0.1134)	(0.0758)	(0.1033)	(0.0655)	(0.1072)	(0.0606)	(0.0849)	(0.0747)	(0.1097)	(0.0823)	(0.1119)	(0.0513)
Physical Assault Count	0.0462	0.0152	0.0236	0.0095	0.0250	0.0103	0.0203	0.0195	0.0414	0.0155	0.0287	0.0059
	(0.7832)	(0.3805)	(0.4389)	(0.2041)	(0.4526)	(0.2259)	(0.4672)	(0.3318)	(0.7546)	(0.2608)	(0.4231)	(0.1540)
Sexual Assault Dummy	0.0018	0.0007	0.0019	0.0002	0.0018	0.0003	0.0008	--	0.0018	0.0008	0.0019	0.0007
	(0.0419)	(0.0272)	(0.0438)	(0.0146)	(0.0419)	(0.0165)	(0.0286)	--	(0.0420)	(0.0285)	(0.0435)	(0.0257)
Sexual Assault Count	0.0077	0.0010	0.0063	0.0004	0.0062	0.0005	0.0078	--	0.0076	0.0013	0.0019	0.0012
	(0.3110)	(0.0456)	(0.1766)	(0.0292)	(0.1793)	(0.0329)	(0.3451)	--	(0.2891)	(0.0550)	(0.0435)	(0.0665)
Physical or Sexual Assault	0.0146	0.0065	0.0116	0.0045	0.0122	0.0040	0.0079	0.0056	0.0135	0.0075	0.0146	0.0033
	(0.1200)	(0.0801)	(0.1073)	(0.0671)	(0.1097)	(0.0628)	(0.0884)	(0.0747)	(0.1153)	(0.0864)	(0.1198)	(0.0573)
Physical or Sexual Count	0.0539	0.0162	0.0299	0.0099	0.0312	0.0108	0.0281	0.0195	0.0490	0.0168	0.0306	0.0071
	(0.9108)	(0.3860)	(0.5008)	(0.2062)	(0.5170)	(0.2282)	(0.5862)	(0.3318)	(0.8758)	(0.2734)	(0.4252)	(0.1677)
Worked	0.5392	0.7466	0.4332	0.5991	0.4293	0.6198	0.4099	0.7113	0.4541	0.6997	0.8188	0.8738
	(0.4985)	(0.4350)	(0.4956)	(0.4902)	(0.4950)	(0.4856)	(0.4919)	(0.4534)	(0.4979)	(0.4584)	(0.3853)	(0.3321)
Other Race	--	--	0.0336	0.0474	--	--	0.0019	0.0019	0.0103	0.0112	0.0051	0.0032
	--	--	(0.1801)	(0.2125)	--	--	(0.0437)	(0.0435)	(0.1008)	(0.1054)	(0.0711)	(0.0567)
Black	--	--	0.8882	0.7895	--	--	0.0281	0.0202	0.2755	0.1746	0.2202	0.0915
	--	--	(0.3151)	(0.4077)	--	--	(0.1654)	(0.1406)	(0.4468)	(0.3796)	(0.4145)	(0.2883)
Asian	--	--	0.0782	0.1631	--	--	0.0050	0.0016	0.0225	0.0199	0.0395	0.0496
	--	--	(0.2685)	(0.3695)	--	--	(0.0705)	(0.0403)	(0.1484)	(0.1397)	(0.1949)	(0.2171)
Hispanic	0.1922	0.0946	0.0157	0.0100	0.0142	0.0108	--	--	0.1467	0.0951	0.0771	0.0383
	(0.3940)	(0.2927)	(0.1245)	(0.0994)	(0.1184)	(0.1033)	--	--	(0.3538)	(0.2934)	(0.2668)	(0.1919)
Ages 16 to 19	0.4267	0.2155	0.2713	0.1850	0.2496	0.1705	0.3514	0.1737	0.4460	0.2019	0.0000	0.0000
	(0.4946)	(0.4112)	(0.4447)	(0.3884)	(0.4328)	(0.3761)	(0.4775)	(0.3790)	(0.4971)	(0.4015)	(0.0000)	(0.0000)
Ages 20 to 29	0.3025	0.5218	0.3872	0.5116	0.3947	0.4786	0.3673	0.5491	0.2933	0.4719	0.3397	0.6216
	(0.4594)	(0.4995)	(0.4871)	(0.5000)	(0.4888)	(0.4997)	(0.4821)	(0.4978)	(0.4553)	(0.4992)	(0.4738)	(0.4850)
Ages 30 to 39	0.2526	0.2363	0.3198	0.2613	0.3331	0.3008	0.2672	0.2499	0.2452	0.2886	0.6001	0.3491
	(0.4345)	(0.4248)	(0.4664)	(0.4394)	(0.4714)	(0.4587)	(0.4426)	(0.4331)	(0.4302)	(0.4531)	(0.4900)	(0.4767)
High School	0.3926	0.3970	0.4692	0.4231	0.4821	0.4659	0.3319	0.4009	0.5290	0.8211	--	--
	(0.4883)	(0.4893)	(0.4991)	(0.4942)	(0.4997)	(0.4990)	(0.4710)	(0.4903)	(0.4992)	(0.3833)	--	--
1 if some college, 0 otherwise	0.2145	0.5217	0.2108	0.4621	0.2060	0.4219	0.1635	0.4100	0.0000	0.0000	--	--
	(0.4105)	(0.4995)	(0.4079)	(0.4987)	(0.4044)	(0.4940)	(0.3699)	(0.4920)	(0.0000)	(0.0000)	--	--
HH Income	29987.7	28163.2	16868.3	19572.8	15276.5	18383.1	19961.4	24048.4	25289.5	26190.7	35113.9	37498.4
	(23694.2)	(23230.4)	(17045.3)	(18213.9)	(15270.2)	(16341.9)	(17945.7)	(20247.4)	(22595.7)	(20846.1)	(23700.7)	(24622.8)
HH Income < 25,000	0.5212	0.5473	0.7848	0.7324	0.8186	0.7518	0.7221	0.6397	0.6155	0.5926	0.3963	0.3593
	(0.4996)	(0.4978)	(0.4110)	(0.4428)	(0.3854)	(0.4321)	(0.4481)	(0.4803)	(0.4865)	(0.4914)	(0.4893)	(0.4798)
Observations	16739	12174	6664	2534	5831	1955	3219	1186	18326	7190	1514	6830

Standard deviations in parentheses

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Appendix Table A.6. Outcome means for different samples (Table 4).

	(1)	(2)	(3)	(4)
Age Range	*16 to 40*	*16 to 40*	*16 to 40*	*16 to 40*
Marital Status	*All*	*All*	*Unmarried*	*Unmarried*
Education	*<= Some College*	*<= Some College*	*<= Some College*	*<= Some College*
Race	*All*	*All*	*All*	*All*
Children	*Yes*	*No*	*Yes*	*No*
Physical Assault Dummy	0.0066	0.0043	0.0123	0.0055
	(0.0813)	(0.0655)	(0.1104)	(0.0740)
Physical Assault Count	0.0214	0.0126	0.0393	0.0141
	(0.5071)	(0.3651)	(0.6959)	(0.3542)
Sexual Assault Dummy	0.0010	0.0004	0.0018	0.0006
	(0.0309)	(0.0210)	(0.0425)	(0.0253)
Sexual Assault Count	0.0041	0.0006	0.0073	0.0009
	(0.2095)	(0.0357)	(0.2767)	(0.0430)
Physical or Sexual Assault	0.0074	0.0047	0.0137	0.0061
	(0.0856)	(0.0685)	(0.1163)	(0.0778)
Physical or Sexual Count	0.0255	0.0132	0.0465	0.0150
	(0.5886)	(0.3685)	(0.8074)	(0.3593)
Worked	0.5583	0.7338	0.5066	0.7191
	(0.4966)	(0.4420)	(0.5000)	(0.4494)
Observations	53,767	21,948	23403	14708

Notes: Standard deviations in parentheses.

Appendix Table A.7. Coefficient Estimates on Control Variables in Panels A & B of Table 4

	(1) Physical Abuse	(2) Physical Abuse	(3) Sexual Abuse	(4) Sexual Abuse	(5) Physical or Sexual	(6) Physical or Sexual	(7) Worked

	Dummy	Count	Dummy	Count	Abuse Dummy	Abuse Count	
Panel A: All							
Post-OBRA-93 x Children >= 1	-0.001	-0.006	-0.001***	-0.005***	-0.001	-0.011**	0.021***
	(0.001)	(0.004)	(0.000)	(0.002)	(0.001)	(0.005)	(0.005)
Other Race	0.006**	0.014	0.000	0.000	0.006**	0.014	-0.100***
	(0.003)	(0.011)	(0.001)	(0.003)	(0.003)	(0.011)	(0.012)
Black	-0.001**	-0.011***	-0.000	-0.001	-0.002***	-0.012***	-0.057***
	(0.001)	(0.002)	(0.000)	(0.001)	(0.001)	(0.003)	(0.003)
Asian	-0.005***	-0.016***	-0.000	-0.002***	-0.005***	-0.018***	-0.116***
	(0.000)	(0.002)	(0.000)	(0.000)	(0.001)	(0.002)	(0.006)
Hispanic	-0.003***	-0.010***	-0.000***	-0.001	-0.003***	-0.011***	-0.062***
	(0.000)	(0.002)	(0.000)	(0.001)	(0.000)	(0.002)	(0.003)
Ages 20 to 29	0.006***	0.016***	0.000	0.002*	0.006***	0.017***	0.076***
	(0.001)	(0.003)	(0.000)	(0.001)	(0.001)	(0.003)	(0.003)
Ages 30 to 39	0.004***	0.009***	0.000	0.002*	0.004***	0.010***	0.141***
	(0.001)	(0.002)	(0.000)	(0.001)	(0.001)	(0.003)	(0.003)
1 if high school, 0 otherwise	0.000	-0.003	0.000	-0.000	0.000	-0.003	0.211***
	(0.001)	(0.003)	(0.000)	(0.001)	(0.001)	(0.003)	(0.003)
1 if some college, 0 otherwise	-0.001**	-0.007***	0.000	-0.002	-0.001**	-0.009***	0.256***
	(0.001)	(0.003)	(0.000)	(0.001)	(0.001)	(0.003)	(0.003)
Married	-0.009***	-0.025***	-0.001***	-0.004***	-0.010***	-0.029***	-0.026***
	(0.000)	(0.002)	(0.000)	(0.001)	(0.000)	(0.002)	(0.002)
Children = 1	0.004***	0.016***	0.001**	0.003***	0.005***	0.019***	-0.086***
	(0.001)	(0.004)	(0.000)	(0.001)	(0.001)	(0.004)	(0.004)
Children 2+	0.005***	0.016***	0.001***	0.005***	0.006***	0.020***	-0.155***
	(0.001)	(0.004)	(0.000)	(0.001)	(0.001)	(0.004)	(0.004)
Observations	239,035	239,035	239,035	239,035	239,035	239,035	236,854
Panel B: Unmarried women							
Post-OBRA-93 x Children >= 1	-0.002	-0.014**	-0.001**	-0.008***	-0.003**	-0.022***	0.043***
	(0.001)	(0.007)	(0.000)	(0.003)	(0.001)	(0.007)	(0.006)
Other Race	0.007*	0.020	-0.000	-0.000	0.007	0.020	-0.125***
	(0.004)	(0.017)	(0.001)	(0.004)	(0.004)	(0.017)	(0.016)
Black	-0.003***	-0.019***	-0.001**	-0.002	-0.004***	-0.021***	-0.111***
	(0.001)	(0.004)	(0.000)	(0.001)	(0.001)	(0.004)	(0.004)
Asian	-0.008***	-0.027***	-0.001	-0.003***	-0.008***	-0.030***	-0.157***
	(0.001)	(0.002)	(0.001)	(0.001)	(0.001)	(0.003)	(0.009)
Hispanic	-0.004***	-0.017***	-0.001***	-0.003*	-0.005***	-0.020***	-0.076***
	(0.001)	(0.004)	(0.000)	(0.001)	(0.001)	(0.004)	(0.004)
Ages 20 to 29	0.008***	0.022***	0.000	0.002	0.008***	0.024***	0.112***
	(0.001)	(0.004)	(0.000)	(0.001)	(0.001)	(0.004)	(0.004)
Ages 30 to 39	0.005***	0.012***	0.000	0.002	0.005***	0.014***	0.173***
	(0.001)	(0.004)	(0.000)	(0.001)	(0.001)	(0.004)	(0.004)
High School	0.002**	-0.001	0.000*	0.001	0.002***	-0.000	0.218***
	(0.001)	(0.004)	(0.000)	(0.001)	(0.001)	(0.005)	(0.004)
1 if high school, 0 otherwise	-0.001	-0.008*	0.000	-0.001	-0.001	-0.010**	0.266***
	(0.001)	(0.004)	(0.000)	(0.002)	(0.001)	(0.005)	(0.004)
Children = 1	0.007***	0.029***	0.001**	0.005***	0.008***	0.034***	-0.071***
	(0.001)	(0.006)	(0.000)	(0.002)	(0.001)	(0.006)	(0.006)
Children 2+	0.009***	0.030***	0.002***	0.008***	0.011***	0.038***	-0.129***
	(0.001)	(0.007)	(0.000)	(0.002)	(0.001)	(0.007)	(0.006)
Observations	123,954	123,954	123,954	123,954	123,954	123,954	122,761

Notes: Standard errors robust to heteroskedasticity are in parenthesis. *** p<0.01, ** p<0.05, * p<0.1.
Each model also controls for year and month fixed effects.