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INTIMATE PARTNER VIOLENCE AND INCOME:
QUASI-EXPERIMENTAL EVIDENCE FROM THE EARNED INCOME TAX CREDIT

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Intimate Partner Violence and Income: Quasi-Experimental Evidence from the Earned Income Tax Credit
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ABSTRACT

We estimate the impact of an exogenous increase in income on the prevalence and counts of intimate partner violence (IPV). Using the 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 reduces 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 unmarried Black women. If increased income (rather than changes in employment) is the only channel by which the EITC decreases IPV, an additional $1,000 of after-tax income decreases the prevalence of physical or sexual violence of unmarried low-educated women by 9.73% and the counts of physical or sexual violence by 21%. We explore potential mechanisms behind these findings.

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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 (before age 18) and are most common when women are in their twenties and thirties (Aizer 2010). Black, low-income, 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 (Anderberg and Moroni 2020; 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). Hence, it is crucial to identify policies that can mitigate IPV.

While we know a lot about the factors (Stöckl *et al.* 2014), costs (CDC 2003; Sorenson 2003), and consequences (Ansara 2011; Beydoun *et al.* 2012) associated with IPV from a wide range of disciplines, including sociology, psychology, social work, public health, and medicine, the evidence on what factors causally mitigate or exacerbate IPV is scarce, especially in high-income countries.[3] The reason is twofold: (i) the lack of high-quality population-based IPV data; and (ii) the challenges associated with identifying causality. In this paper, we address both issues. We use repeated cross-sectional data from 1992 to 2000 from the National Crime Victimization Survey (NCVS), a nationally representative survey administered by the US Bureau of Justice Statistics, that collects self-reported information on rape or sexual assault, aggravated and simple assault, as well as victim-offender relationship. Our sample amounts to over 200,000 women.

To identify causality, we take advantage of 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

[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.
[3] There is a growing literature on causal factors of IPV in developing countries using experimental and quasi-experimental methods in low- and middle-income countries.

“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 until 1998, reaching between 18% and 40% of their earned income,[4] but it remained practically flat for the latter, at 6% to 8% of their earned income. Identification, in this case, is based on the differential relative increase in tax credits between the treatment and comparison groups. It is worth noting that while some families with no children receive the tax credit, the proportion who qualify is very small, and the dollar amount they receive is also small.

In this paper, we estimate the causal effect of the Earned Income Tax Credit (EITC) expansion on the prevalence and counts of IPV against women in the United States.[5] Theoretically, the relationship between the EITC and IPV is ambiguous. On the one hand, the feminist theory argues that IPV results from women’s economic dependence on their partner and/or weak bargaining power within the household. As their earned income increases with the EITC expansion, women 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 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. On the other hand, in contrast with the feminist theory, other theories suggest that IPV emerges when men feel threatened by their partner’s greater economic independence (‘male backlash’ theory in sociology) or potential exposure to other men (evolutionary theory), or because men want to extract monetary transfers from their partner (‘extractive’ theory).[6]

Changes to the EITC may also affect IPV through employment effects that reduce women’s exposure to violence (as they spend less time in the household), 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).[7] Using the Minnesota

[4] 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.

[5] To measure ‘prevalence’ we use a discrete variable for whether the respondent experienced IPV. To measure ‘counts’, we use the number of incidents of IPV experienced. In this manner, ‘prevalence’ may be interpreted as the extensive margin, while the ‘count’ is the intensive margin.

[6] For example, Hsu (2017) studies the impact of the timing of Temporary Assistance for Needy Families (TANF) welfare payments on reports of IPV, specifically intimidation and assault. She finds an increase in reports of male-on-female assault and intimidation shortly after receiving welfare payments, finding suggestive evidence that the man will use threats to secure a monetary transfer when the woman receives income.

[7] 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

Family Investment Program and a sub-study of the National Evaluation of Welfare-to-Work Strategies, Gibson-Davis *et al.* (2005) analyze the causal effect of increased maternal employment on domestic abuse among low-income single mothers. They estimate that an increase of one quarter of employment reduced the probability of reported incidences of domestic abuse by 6 percent to 8 percent. At the same time, others have found that increased shared time at home has been shown to increase IPV. For instance, Leslie and Wilson (2020) find an 8% increase in domestic violence calls following the imposition of stay-at-home orders 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) show that increased economic stress at both the household and regional levels leads to an increase in IPV. Since the EITC affects both employment and income, we cannot completely separate the impact of these two effects.

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 as well as unmarried women with less than a four-year college degree. To assess the validity of the pre-existing parallel trends assumption, we perform event-study analyses. Analyses 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.

We find suggestive evidence that the EITC expansion reduced both the prevalence and counts 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 unmarried Black women—groups that are both more likely to experience IPV (Catalano, 2007) and that have high rates of eligibility for the EITC (Jones, 2014; Hardy et al., 2022). Specifically, the counts 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 prevalence of sexual IPV

associated with increases in child maltreatment. In contrast, Anderberg *et al.* (2015) develop a model and find evidence in the UK of the opposite: that male (female) unemployment decreases (increases) IPV.

decreased by 0.1 percentage points (relative to the pre-OBRA-93 control means of 0.18). These three coefficients are statistically significant at the 5 percent level or lower.

We investigate the mechanisms through which EITC may affect IPV. The EITC may generate reductions in IPV through multiple avenues. Given our data, we explore how increased work may reduce women's exposure to violence by reducing time spent at home or through higher discretionary income, which can reduce IPV through: (1) promoting women's agency, (2) boosting women's household bargaining power, (3) channeling spending to lower family stress, or (4) a change in social networks associated with working. We find suggestive support for the increase in discretionary income and the benefits of promoting work.

We contribute to the ~~meager~~ literature on the impacts of income on IPV in rich countries. Our work complements Aizer's findings and generalizes them both at the country level and to IPV measures beyond those requiring hospitalization, offering a different policy context and giving external validity to her conclusions. Given that almost 20% of all tax filers and 44% of filers with children in the US received the EITC in 2014 and that we find sizable and significant reductions from EITC on the groups with the highest prevalence of IPV, our findings suggest that the EITC expansion may have had large effects on IPV.

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 prevalence and counts of IPV. Yet, there is a broad literature that uses a similar identification strategy to analyze the impact of EITC on maternal employment (Eissa and Liebman 1996; Meyer and Rosenbaum 2001; Eissa and Hoynes 2004; Nichols and Rothstein 2015)[8]; fertility (Baughman and Dickert-Conlin 2009) or family formation (Dickert-Conlin 2002; Ellwood 2000; Herbst 2011); maternal and infant health (Evans and Garthwaite 2014; Strully, Rehkopf, and Xuan 2010; Hoynes, Miller, and Simon 2015).[9] Our results are consistent with studies finding that the EITC expansion improved mothers' self-reported health and lowered their counts of the risky biomarkers. By focusing on IPV, our work offers potential channels to these earlier findings, whereas maternal health improvements could be the result of increased bargaining power and physical safety.

[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 in response to the EITC. Kleven (forthcoming) argues that the extensive margin employment impacts of the EITC are over-stated and instead pick up the effect of welfare reform, which was occurring around the same time. In response, Schanzenbach and Strain (2021) argue that Kleven's approach to controlling for welfare reform absorbs much of the true identifying variation of the EITC. They are able to replicate Kleven's results but find that there are still sizable impacts of the 1993 EITC expansion on maternal employment after including controls for the business cycle, focusing on low education mothers, and measuring maternal labor supply over the year instead of the week preceding the interview. We address Kleven's concerns in the main text, sections II and V.

[9] These studies find that EITC income reduces the prevalence 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.

Finally, it is important to underscore the work of Moe *et al.* (2020), Edmonds *et al.* (2022) and Sims *et al.* (2024), which is closer to our analysis as they ask the same question: what is the causal impact of EITC on non-lethal or lethal IPV? In contrast with our identification strategy, which exploits the Federal 1994 EITC increase implemented through the OBRA-93 reform, these other studies exploit variation in state-level EITC generosity between 1999 and 2016 (Moe *et al.* 2020), between 1999 and 2013 (Edmonds *et al.* 2022)[10], and between 1999 and 2019 (Sims *et al.* 2024). Neither study finds a statistically significant effect of state EITC expansions on state-level rates of IPV per 1,000 habitants (Edmonds *et al.* 2022), nor IPV homicides (Moe *et al.* 2020), nor lethal IPV and non-IPV (Sims *et al.* 2024). This result contrasts with our findings and is likely due to the lack of granularity in their state-level outcomes, preventing them from identifying potential effects for specific individuals or subgroups. By analyzing individual-level data, our study allows us to focus on those population subgroups that are more likely to both receive EITC and have a higher risk of victimization. At the same time, as the above studies focus on state variation in the EITC generosity and since state generosity is only a percentage of the Federal EITC — ranging between 3% in Montana to 85% in California—, it is plausible that their lack of statistically significant impacts is further due to the overall smaller intensity of the state benefits relative to the Federal expansion under OBRA-93. In contrast, Spencer *et al.* (2020), which focuses primarily on the effects of TANF on IPV using the Fragile Families dataset, use a discrete state measure of EITC exposure--no EITC, refundable and non-refundable credits—and find that the refundable EITC lowers rates of IPV among women with less than a high school degree relative to a control group of high school graduates.

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

The earned income tax credit (EITC) provides in-work tax credits, based on family size and earned-income eligibility, deducted 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 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,

[10] Edmonds *et al.* (2022) also present an individual-level longitudinal data analysis using the Fragile Families and Child Well-being Study, and again find no association between maximum estimated federal and state EITC benefit for each person-wave based on the year prior to the interview and their concurrent risk of victimization. However, their analysis lacks a comparison group, which precludes netting out any confounding effects.

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 a linear probability model and using a difference-in-differences (DiD) approach, we estimate:

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

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 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 corresponding 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/ethnicity (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

[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.

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.[13] 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 OBRA-93 for women with children relative to those with 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.[14] 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 compared to the expansion of EITC for people without children. As we do not observe whether individuals received the EITC payments, our estimates are intention-to-treat estimates. At the end of Section IV, we scale our ITT estimates by first-stage effects on employment and income estimated using supplementary data in the Current Population Survey (CPS) as the NCVS has limited income data that is only provided in bins. Doing so provides us with treatment on the treated (TOT) estimates under the assumption that the mechanism is driven fully though income. Because the EITC targets low- to moderate-income working individuals and couples, we focus our analysis on women without a four-year college degree. It is estimated that 86 percent of EITC-eligible tax filers do not have a college degree (Murray and Kneebone, 2017). At the same time, 82 percent 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, though we present results for both groups, our main estimates are for those who are unmarried.[15]

While income data in the NCVS is binned (see details in Section III below), we can impute the midpoints of the bins to approximate in our sample the fraction of women without a college degree and unmarried women without a college degree who qualify for the EITC based on the 1995 EITC income limits (which vary by number of children). Approximately 50.3% of all mothers between the ages of 16-40 without a college degree and with children (our treated group) qualify for at least some amount of the EITC. This rate increases to 55% when we look at

[13]Treatment enters our design at the level of number of children (0, 1, 2+); however, this yields only three clusters, which is not enough to clusters to identify a cluster robust variance matrix.
[14] 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.
[15] We define as unmarried women those who are widowed, divorced, separated, or never married.

unmarried mothers. This contrasts with only 11.9% (or 15% for unmarried women) of the comparison group. For a more precise estimate of how the EITC impacts income for the treated group, we estimate changes in real income using the CPS as described in Section IV below.

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 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 dependent children, respectively. This richer specification checks if there are greater impacts on women with two or more children who experienced a larger differential increase in the tax credit.

One may also be concerned that the EITC expansion is confounded with other policy changes that differentially affect women with and without children. Therefore, 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.

Finally, Kleven (forthcoming) raises the concern that welfare reform is happening at this time and that strategies typically used to identify the effects of the 1993 EITC expansion may in fact be picking up differential changes in welfare benefits by family size. In the robustness section, we address Kleven's concerns by analyzing the extent to which our findings strictly increase with family size as AFDC benefits typically increased at a set schedule linearly with the number of children (details were often dependent on specific state programs). On the other hand, the EITC only increased from 1 to "2 or more" children. Evidence that our findings are not greater for families with "3 or more" children than those with 2 children supports our hypothesis that we are indeed capturing EITC as opposed to AFDC benefit changes. Some papers additionally address Kleven's concerns by including state-level controls for welfare reform: unfortunately, our data does not include state identifiers.

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 IPV and sexual assault (Kindermann et al., 1997).[16] 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 levels. For each household, every household member who is 12 years old or older 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 the 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 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. Crucially, there is information on the number of people

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

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, $(children \geq 1)_{iat}$. Because of strict data confidentiality reasons, the NCVS does not disclose information on individuals' state of residence.

While these survey data provide crucial information for our identification strategy, they may also be subject to underreporting IPV and measurement issues (Aizer 2010). To the extent that this underreporting is not correlated with our treatment—the difference between no versus one or more children before and after the time of OBRA's passage—, one should expect this to lead to less precise but unbiased estimates. However, the NCVS offers some significant advantages over administrative data for the study of family violence. In particular, this dataset allows us to examine the effects of tax credits on a broad range of family violence, including violence that does not generate an official report through police records and/or hospital administrative records, which tend to capture the most extreme cases.[17] Moreover, since our dataset is nationally representative, we are able to generate nationwide estimates of the effects of tax credits on family violence, as opposed to local or state effects. Finally, we can identify whether the perpetrator is the partner or ex-partner.

We focus our analysis on the effects of an EITC expansion on the prevalence and counts 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 prior to the survey; 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 prevalence (physical or 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 on women without a four-year college degree because the EITC expansion targeted low- to moderate-income working individuals and couples. We further restrict our sample to women

[17] Anderberg, Rainer and Siuda (2022) find that police-recorded domestic violence incidents cannot reliably inform us about the scale of the domestic violence problem, especially during crises like COVID-19.

between the ages of 16 and 40 because IPV is most common in this age range (Aizer 2010)[18], 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 whom 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 approximately higher annual income than women with no children ($30,705 versus $29,332 based on imputing the midpoint of the binned income variable provided in the NCVS).[19] Appendix Table A.1 presents similar pre-OBRA-93 descriptive statistics for unmarried women and shows that unmarried mothers tend to be more socio-economically vulnerable than unmarried women with no qualifying children. For example, they are more likely to be non-White (31 percent [20]) and teenagers (38 percent) than unmarried women with no qualifying children in the household (19 percent are non-White and 21 percent are teenagers). 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[21] relative to 58 percent among unmarried women with no children.

Before OBRA-93, the number of women who experienced any physical assault by a partner or ex-partner among 16- to 40-year-old women in the United States was 6.65 per 1,000 with an average total number of incidents of 21.4 per 1000 women. However, the prevalence and counts of sexual assault by a partner or ex-partner is considerably lower, at 0.95 women per 1,000 experiencing sexual assault, with an average total number of counts of 0.0042.[22]

[18] In our data, women between the ages of 16 and 40 report IPV 6 times more frequently than women over the age of 40.

[19] The NCVS only provides binned income ranging from $0-$5,000; up to $75,000. Exact bin sizes vary but are typically $2,500 at the lower end of the distribution, and $5,000 to $10,000 as income gets closer to $75,000. We used $90,000 as the mid-point income for those with higher incomes than $75,000. Using alternative income levels for the top income level group had no bearing on our findings.

[20] 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.

[21] $25,296 was the earned income threshold for people with two or more qualifying children to receive any EITC.

[22] 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 a prevalence 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, IPV increases with motherhood (Vatnar & Björkly, 2010; Bowen *et al*. 2005; Charles and Perreira 2007; Massenkoff and Rose 2023; Britto *et al.* 2024). For example, 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. This implies that, in the pre-OBRA-93, mothers were 53.5 percent 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 percent higher counts of physical abuse and almost six times more counts of sexual abuse than women with no qualifying children before OBRA-93.

Figure 2.A plots our key variable, counts of physical or sexual abuse, over time for the treated groups (non-college-educated women with children distinguished by whether they have one child or two or more children), and for the comparison group (non-college-educated women without children). The first vertical line indicates the first year the EITC was implemented; the second vertical line indicates the final year of the phase in of the EITC expansion (see Figure 1). Figure 2.A reveals a drop in counts of physical or sexual abuse for both treated groups, with the largest and most distinct decline for non-college-educated women with two or more children. Further the size of the decline for these mothers grows over time. For non-college-educated women with two children, the decline amounts to a 0.01 count in 1995 and close to 0.02 count by 1999. Importantly, the growing decline in physical or sexual abuse potentially reflects the phasing in of the EITC expansion shown in Figure 1. For non-college-educated women with one child, even though the 1995 dip is small, in subsequent years this group's counts of physical or sexual abuse continues to decline dropping by 0.01 count in 1997.

By the end of the sample period, the counts of physical or sexual abuse of the treated groups have converged to those of the comparison group (women with no children), which pre-reform were between one half and one third smaller than those of the treated groups. Figure 2.A also shows that counts of IPV for women in the control group remain fairly flat through 1998, the period in which the EITC finishes completely being phased in. If anything, there is potentially a small *increase* in IPV for the control group following the 1995 reform.

Figure 2.B presents the raw data for unmarried, college-educated women ages 22 to 40 by whether they have no children, one child, and two or more children. In this case, there is no dip in physical or sexual assaults for educated women with children. Instead, during the first two years after the EITC reform, assault counts *increase* for these women. These placebo results suggest that the decrease observed among the non-educated women with children is not driven by some other confounding effects that may have impacted the risk of victimization of women with children.

Furthermore, Figures 2.A and 2.B address concerns that our estimates below could be confounded with the Family and Medical Leave Act (FMLA) of 1993, which could have led to a drop in IPV by reducing reliance on abusive partners (for example, for childcare when a child is sick and the mother would otherwise be at work). However, the soonest the 1994 EITC expansion could have impacted women would be the following tax year when they would receive the credit, that is, in 1995. In contrast, the FMLA took place in 1993. Figure 2.A shows a small increase in assaults for non-college-educated women with children after the 1993 FMLA, suggesting that it is the EITC expansion driving the outcomes.

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. There was a relative decline of violence 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 mothers' pre-reform 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 prevalence and counts 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 counts 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 prevalence 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.

We turn next to potential employment effects of the EITC. It is well known that the EITC incentivizes employment at the extensive margin for unmarried mothers because it acts as a wage subsidy.[23] Hence, an expansion of the EITC will not deter working taxpayers who already worked

[23] 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.

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 mothers' 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.[24] 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 in when the timing of treatment should be assigned which we will explore in Section VI.

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.[25] 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.[26]

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 3.A 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), and unmarried women with less than a four-year degree (Panel B). 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

[24] 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.

[25] Since one estimate is significant at the 10 percent level out of 7 placebo regressions, this is consistent with what we would expect due to type one error.

[26] Before OBRA-93, the employment of highly educated mothers in our sample was 81.88 percent.

red vertical line indicates the first year of OBRA-93 EITC receipt. Hence, 1994 is the year prior to the first OBRA-93 payment receipt. The event study figures show no pre-existing trends in the three years of pre-OBRA-93, followed by a decrease in the prevalence of physical or sexual IPV corresponding with the increase in EITC benefits.

Next, in Figure 3.B, we perform placebo event-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 Figure 3.B, 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.

Figures 4.A through 4.C present the results from estimating the event study discussed above using equation (2) from Section II on unmarried women in our sample with less than a four-year degree looking specifically at physical abuse counts (Panel A) and sexual abuse counts (Panel B), with employment effects plotted in Panel C. These event studies correspond to our main findings in panel B of Table 4. They follow the same overall trend as the event study shown in Figure 3.A, but with larger standard errors and (in the case of 4.B) less stark treatment effects. The additional noise is a natural consequence of using the more disaggregated data on specific types of IPV. Figures 4.A and 4.B show that both physical and sexual abuse counts fall though there is a stronger reduction from the EITC on physical abuse counts, which is more prevalent in our data, than on sexual assault. Finally, consistent with Table 4, Figure 4.C indicates strong increases in employment among unmarried women with less than a four-year degree, particularly after the full EITC phase-in.

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 prevalence of physical or 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 represents a 40.9 percent decrease relative to the pre-OBRA-93 control group mean of 0.98 incidents per 100 women. Among unmarried Black women, the prevalence of physical or sexual IPV dropped by 48.5 percent (or 0.5 percentage points) with the EITC expansion, a decrease considerably larger than the non-statistically significant decrease observed among unmarried White women of 5.6 percent. Similarly, OBRA-93 reduced the counts of physical or sexual violence for both Black and White women, with the reduction being larger among Black women: 74.1 percent versus 57.6 percent. Both estimates 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 (an 11.9 percent or 5.9 percentage points increase) than White women (a 5.6 percent or 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 prevalence and average counts of physical or sexual IPV among this group decreased by 32 percent and 76.2 percent (0.4 and 3.3 percentage points); and their employment increased by 9.2 percent (4.9 percentage points). All three estimates are statistically significant at the 5 percent level or lower.

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 the 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 (8.9 percent increase versus a 5 percent increase relative to the pre-OBRA-93 control-group means). Interestingly, this parity difference is considerably larger among Black women than White women, 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 (a 15.4 percent increase relative to a pre-OBRA-93 control mean of 49.4 percent). 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 or 6.3 percent increase).

Moving to the differential impacts of the reform on IPV, we observe a higher reduction of both the prevalence and counts of sexual or 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: 58.3 versus (non-statistically significant) 29.1 percent decrease in prevalence and 81.5 versus (non-statistically significant) 59.3 percent in counts. 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 sexual or physical IPV counts among those with two children or more after OBRA-93 than those with only one child relative to their counterparts with no qualifying children: 71.9 versus (non-statistically significant)

40.8 percent. This effect is driven by a relatively higher reduction in the counts of physical IPV for those with higher parity. In contrast, OBRA-93 reduced sexual IPV counts 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), which is particularly true for Black women. This implies that the groups with the largest treatment in terms of expanded tax credits, generally, saw the largest declines in IPV. The stronger impacts for unmarried mothers with 2+ children relative to 1 are illustrated in Figure 5, where Panel A shows the event study analysis using women with 1 child (treated group) and those without children (comparison group) and Panel B shows the event study analysis using women with 2+ children versus those without. In both figures the parallel trends assumption holds. Yet, the reduction in violence is stronger among mothers with 2+ children consistent with their higher EITC receipt.

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).[27] We inflation adjust all income to be in 2010 dollars. Table 7 shows these results for the economic impact of our main specification comparing unmarried women with one or more children to those with no children.[28] 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 the 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

[27] Studies from independent researchers and the IRS find that take-up is relatively high at this time, ranging from 80 to 87 percent (Scholz, 1994; Internal Revenue Service, 2002). Note, the 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.

[28] 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.

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 of the potential extensive margin employment changes, which could independently impact IPV. However, our estimated effects still offer a useful scaling, particularly for comparing these results to the literature on income and IPV.

Our results imply that an additional $1,000 decreases the counts of physical violence by 15.86 percent for unmarried women. We see larger effects on sexual violence, with prevalence and counts decreasing by 24 percent and 48 percent, respectively. For effects on any type of violence, we find that the prevalence and counts falls by 9.73 percent and 21 percent, respectively. We can also scale our main results by changes in employment instead of income. This is shown in Appendix Table A.3. These results imply that a 10 percentage point increase in work among unmarried would reduce the prevalence and counts of physical or sexual abuse by 1.43 percent or 10.48 percent.

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 percent 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 percent decrease in the prevalence of IPV (with respect to the mean), and a 43 percent decrease in the counts of IPV among first- and second-generation immigrants in 28 European countries in 2012.

V. Robustness Checks

A recent paper by Kleven (forthcoming) raises the concern that the effects of the OBRA-93 expansion on low-income female labor supply may be confounded with the caseload reductions that accompanied the federal welfare reform act of 1996 and the numerous state welfare reforms implemented between 1992-1996. Since welfare payments were contingent on not working, reducing welfare payment amounts may have incentivized women to enter the workforce. Yet, both Meyer and Rosenbaum (2001) and Schanzenbach and Strain (2021) have directly modeled the effects of welfare reform and still found substantial impacts of the EITC on labor supply. Schanzenbach and Strain (2021) additionally omit all states from their analysis that ever had a welfare reform waiver and still find effects of the 1993 EITC on employment.[29]

[29] Because there were many state waivers during the 1994 to 1996 period, a complementary way to test for confounding variation from welfare waivers is to see if the estimates are sensitive to including welfare waivers by number of children year effects or time trends; however, there is a risk that such models absorb much of the true effects of the EITC. Further, our data set, the National Crime Victimization Survey (NCVS) lacks state identifiers. This makes it impossible to include a state level indicator for welfare waivers, or to omit states that ever had a waiver.

Nonetheless, to address Kleven's concerns, we test whether the effects on IPV increased linearly with family size. Specifically, AFDC benefits increase linearly with the number of children, while the EITC only increased from 1 to "2 or more" children. Therefore, if we are picking up AFDC effects, we should see greater impacts of our estimation strategy for families with 3+ children than the effects for families with only 2 children (both estimated using no child families as the control group). On the other hand, if the effects on families with 3+ children are similar to those with 2 children, then our results are unlikely to be driven by reductions in welfare income, because the change in the amount of money was larger for the larger families. This is indeed what we find in Appendix Table A.4, which shows similar magnitude of coefficients for both types.

Another concern is how to cluster our standard errors. Abadie *et al.* (2017) argue 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.5 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,[30] 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 (number of children by year) to less aggregated ones (number 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 little to no effect on the estimated treatment effect. We show this in Appendix Table A.6 by demonstrating robustness to model specification. The first row shows the coefficients when dropping all our control variables[31], the second row shows our baseline specification for comparison, and the third row additionally controls for the number of children groups (0, 1, 2+) interacted with the various demographic controls (age, education, race groups).

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

[31] 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.

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.

Finally, if the EITC expansion increased fertility then our results could be biased if effects on fertility or entry into marriage change the composition of single mothers. However, Baughman and Dickert-Conlin (2009) show little to no impact of the EITC on fertility, with only a very small decrease in fertility among white women. The implied elasticity of the EITC on fertility from their estimates is -0.009, an “economically insignificant” effect according to the authors. Likewise, Hoynes, Miller, and Simon (2013) find no significant impact of EITC on fertility using data from the vital statistics natality records.

In terms of marriage, Herbst (2011) finds a small decrease in marriage following an increase in the EITC, with an extra $1000 of EITC income decreasing marriages by 5% per year relative to a mean of 0.056, or 2.8 fewer marriages per year per 1000 unmarried women (0.05*0.056*1000=2.8). Given that we look at 5 years of post-period: this could reflect as many as 15 more unmarried women per year in our sample. If, in the limiting case these women experience no domestic violence, then we would see an upper bound compositional decrease in domestic violence of -0.02 per 1000 unmarried women.[32] A small compositional impact relative to our estimated treatment effect of -9.73% for $1000 of EITC income or -1.3 fewer unmarried women experiencing domestic violence per 1000 (0.0973*0.137*1000=-1.3).

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 and employment, there could, in fact, be several mechanisms by which the OBRA-93 expansion of the EITC decreased IPV, or even a combination of multiple mechanisms. Here we investigate these possibilities more carefully.

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 directly provide the resources a mother needs to escape an abusive relationship. Figure 6 takes an initial descriptive

[32] From table 7 the sample mean of having any physical or sexual abuse for unmarried women is .0137. Consider adding 15 marginal unmarried women per every 1000 in our sample, as the results from Herbst (2011) suggest, none of whom experience domestic violence. Doing so would change the sample mean to approximately 0.0135 (13.7/1015), a compositional decrease of -.0002 or -0.2 fewer unmarried women experiencing domestic violence per 1000. We would like to thank an anonymous referee for pointing this out for us.

approach to answer this question by estimating equation 1 on IPV, employment, and after-tax income across different subgroups.

The top panel of Figure 6 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 6 does the same analysis but instead with predicted after-tax income (estimated from the 1992-2000 March CPS using taxsim) on the X-axis. In essence, we use variation by subgroup to test the relative degree to which employment/income correlates with IPV impacts. We see a relatively strong correlation with employment: subgroups experiencing the largest increases in employment also experience the largest declines in IPV (ex: Blacks, unmarried mothers, and those with a high school education or less). The bottom panel of Figure 6 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 suggest that the impact 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. Therefore, we caution against over interpreting these results.

The above analysis in Figure 6 is mainly descriptive. To further explore the 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 lower the amount of time spent with the perpetrator of IPV due to reducing time spent at home.[33] 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.[34] 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, suggesting that reduced exposure to a partner from changes in time use is unlikely an important mechanism, an alternative interpretation is that we find no differential impact between day and time not because exposure does not change but because of low-income workers' frequent non-standard hours.

[33] Aizer (2010) uses a similar strategy to examine exposure effects on IPV, by using weekend versus weekday incidents.

[34] 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.

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 percent for unmarried women, with larger relative effects on sexual violence and for unmarried Black women. We test mechanisms related to work or higher after-tax income. 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 abuse in the US. Likewise, our findings contradict the "evolutionary" and "male backlash" theories, which posit that when women gain more resources and self-sufficiency, men compensate with violence 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 the EITC expansion was 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).[35] Taking our treatment on the treated estimates from Table 7: $1,000 spending per recipient of the EITC decreases physical or sexual abuse by 9.37 percent, implying about 41,615 fewer women experiencing IPV after the expansion.[36] This in turn generates a gross benefit of roughly 4.3 billion in 2003 dollars.[37] 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 percent decline in the total counts. 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, our results imply large monetary benefits to society from reducing IPV through investing in women's economic self-sufficiency.

[35] Using weights, 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 IPV prevalence. 41,500,000*.0107 = 444,133 women. This is potentially a lower-bound estimate if IPV is underreported in the NCVS.
[36] 444,133*0.0937 = 41,615.
[37] 41,615 women multiplied by the CDC cost of $103,767 is approximately 4.3 billion dollars.

Given that there are multiple mechanisms by which the EITC could decrease IPV, what broader policy implications can be drawn from these results? One way to explore this question is to look at how these different mechanisms may be relevant to either recent proposals for reforming the Child Tax Credit or alternative policies, such as food stamps. There are four main current proposals to reform the child tax credit in Congress: 1) benefits will be indexed to inflation, 2) the maximum refundable portion of the credit will increase from $1,600 to $1,800, 3) there will be a steeper phase in rate (based on number of children), so lower-income families reach the maximum refundable amount more quickly, and 4) a "look back" provision is added so families can use their previous years of income to qualify for the credit. The "look back" provision is the most innovative (and controversial) as it potentially allows families with no current tax year income to qualify for the credit. This could provide an important safety-net to families with a temporary job loss but also could potentially disincentivize labor supply.

We think about the effect of each of these provisions in turn in light of our findings. The absolute dollar increase in the child tax credit is relatively small; however, the effects on IPV could still be moderately sized. For example, by our estimates a $200 increase (provision 2) would at most decrease IPV by 3.16 percent, assuming that the mechanism operates primarily through an income transfer. However, increasing the phase in rate of the tax credit (provision 3) is likely to incentivize employment because the returns to work (relative to not working) accumulate faster As we cannot isolate the exact mechanism (work, income, or a complementarity between the two) of the EITC on IPV our results speak to most directly to policies that both incentivize work and increase income. Thus, Provisions 2) and 3) of the proposed child tax credit reform may complement each other.

On the other hand, enthusiasm for the proposed tax credit reform should be cushioned by the uncertainty of the impacts of proposed provision 4 (the "look back" provision). Little is known about whether having the work incentives only apply *every other* tax year will continue to encourage labor market participation as the EITC does; or if income effects from years where participation is not required could even discourage work. This is an area where more policy research is needed.

The effect of other policies could be viewed through a similar lens. For example, evidence suggests that food stamps act largely as an income transfer (Hoynes and Schazenbach 2009); implying similar results *if* the primary mechanism is income. Income effects from food stamps may cause small work disincentives (Hoynes and Schazenbach 2012; East 2018), though more recently states have enacted work requirements for food stamps. However, there is little evidence that work requirements on food stamps increase labor supply with recent studies finding no effect on work or, at most, small effects (Gray *et al.* 2023; Stacy *et al*. 2018). Jointly, this implies that food stamps could help decrease IPV particularly if food stamp policies could be combined with similar work incentives as work-based tax credits.

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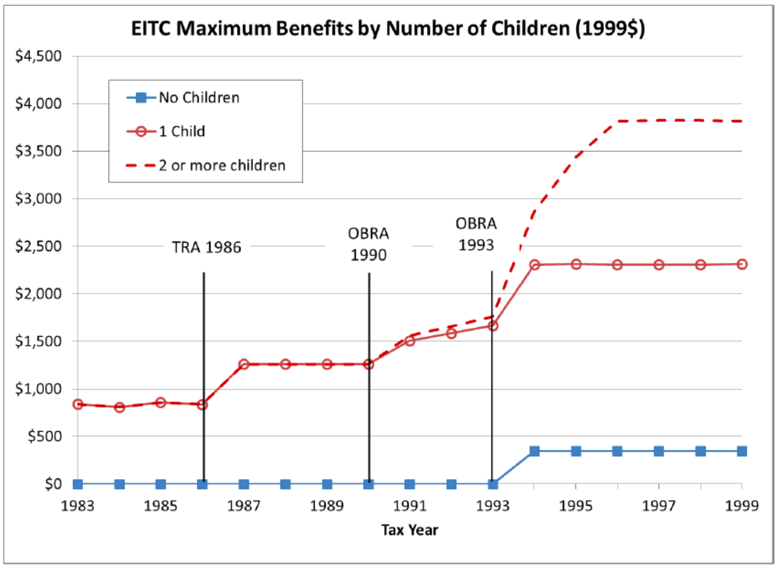
Figure 1. Maximum credit for federal EITC by tax year and number of qualifying children



Source: Reprinted from Hoynes, Miller, and Simon (2015).

Figure 2.A. Physical or sexual abuse counts before and after EITC, women with less than a 4-year college degree, ages 16 to 40 years old

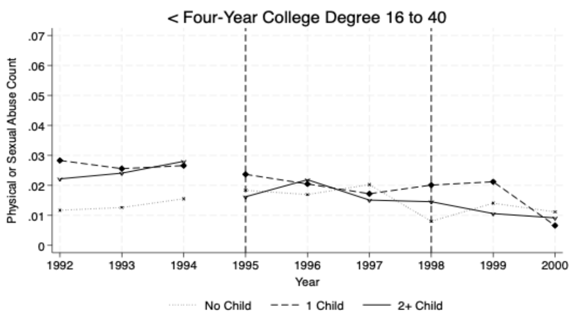
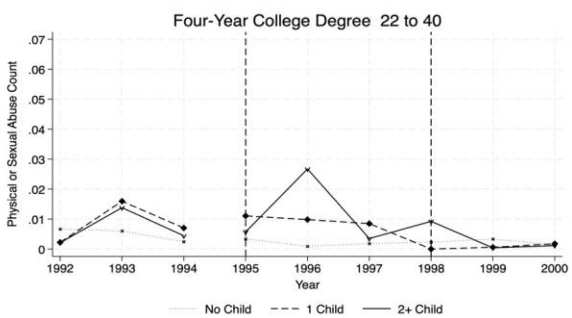


Figure 2.B. Physical or sexual abuse counts before and after EITC for 4-year college-educated unmarried women, ages 22 to 40 years old



Notes: Figure 2.A is comprised of women with less than a four-year college degree and are 16- to 40-years old. Figure 2.B is comprised of women who have at least a four-year college degree, are unmarried, and are 22 to 40 years old. For both figures, year is on the X-axis and annual raw mean counts of physical or sexual abuse are on the Y-axis. The first vertical line represents the first-year taxpayers began to receive additional refunds from the OBRA 1993 legislation. The second vertical line represents the year the EITC was completely implemented. We show series for mothers with no children (the dotted line), one child (the dashed line) and 2 or more children (the solid line).

Figure 3.A. Event study analysis of physical or sexual IPV counts among women with less than a four-year college degree

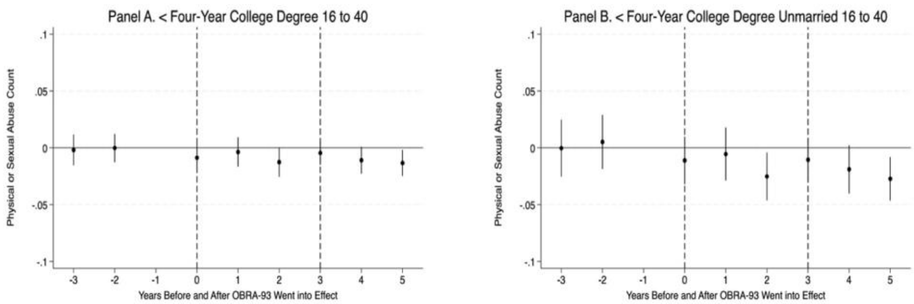
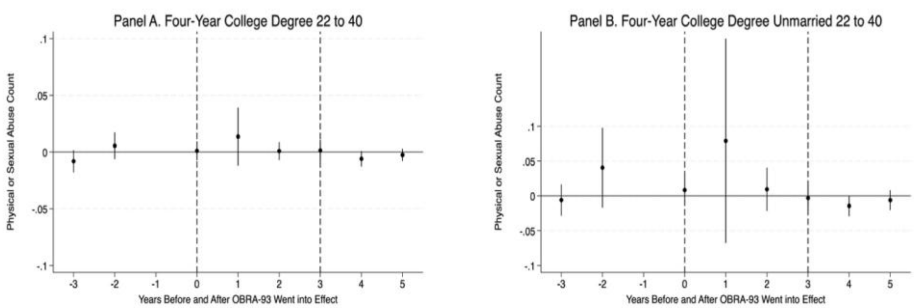
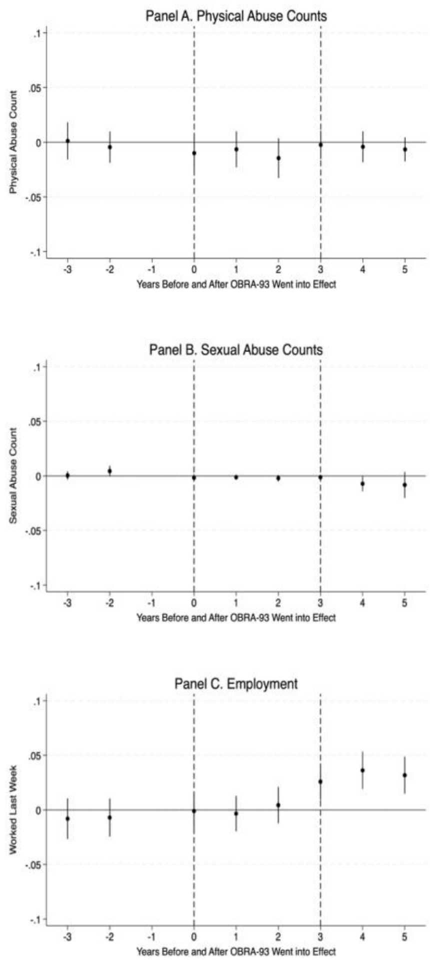


Figure 3.B. Placebo event study analysis of physical or sexual intimate partner violence counts among women with at least a four-year college degree



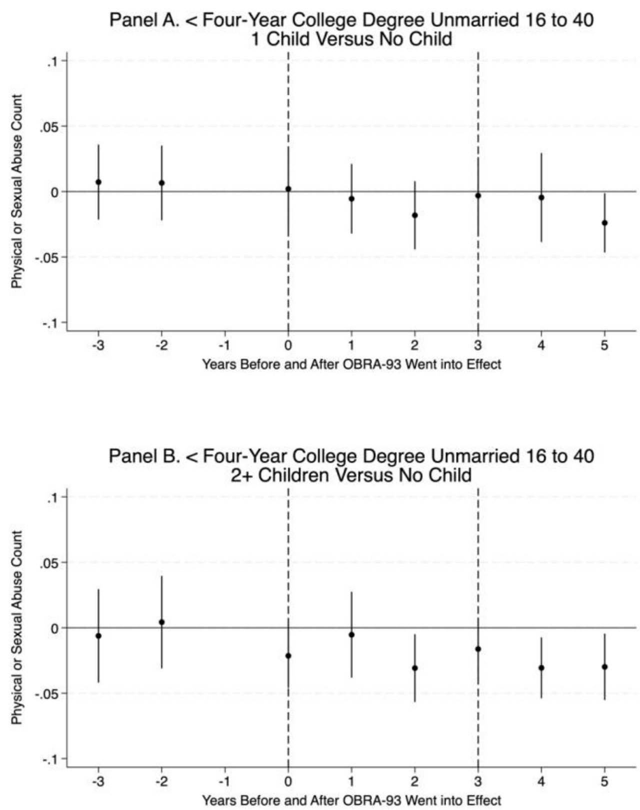
Notes: Standard errors are robust to heteroskedasticity. In Figure 3.A, women have less than a four-year college degree and are 16- to 40-years old. In Figure 3.B, women have at least a four-year college degree and are 22 to 40 years old. In our sample, the NCVS, year 0 corresponds to survey round 1995, 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 4. Event study analysis of physical abuse counts, sexual abuse counts and employment, women with less than a 4-year college degree, ages 16 to 40 years old



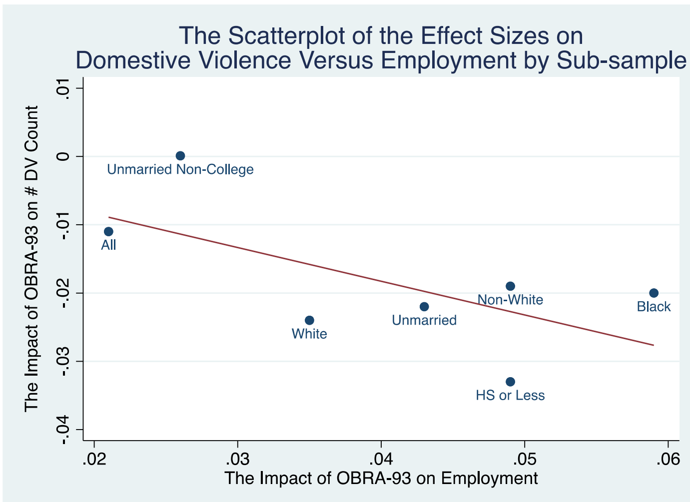
Notes: Standard errors are robust to heteroskedasticity. All samples include age groups 16- to 40-year-old women with less than a four-year college degree. Event study coefficients were obtained from the estimates of equation (2). Panel A shows results from the outcome being counts of physical abuse reported by the woman. Panel B show results from the outcome being counts of sexual abuse reported by the woman. Panel C shows results from the outcome being reportedly employed last week. 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. Each model controls for race indicators, age, educational attainment, year and month fixed effects, and the number of children.

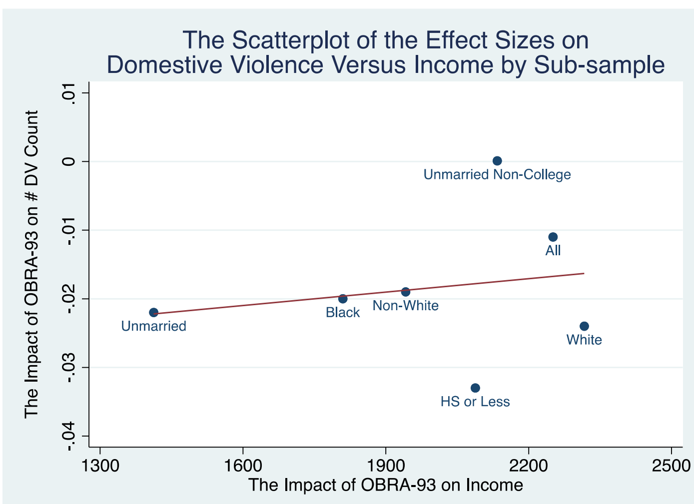
Figure 5. Event study analysis of physical or sexual IPV counts among unmarried women with less than a four-year college degree, ages 16 to 40



Notes: Standard errors are robust to heteroskedasticity. All samples include age groups 16- to 40-year-old women with less than a four-year college degree. In Panel A, women either have 1 child or no children. In Panel B, women have either have 2+ children or no children. 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 6. Treatment effects on employment, income, and IPV across subgroups





Notes: The X-axis on the top panel of Figure 6 shows the impact of the OBRA-93 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 IPV (from the NCVS). Each point represents a different subgroup whose effects (on work, income, and IPV) 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: Data from the National Crime Victimization Survey (NCVS). Standard deviations in parentheses.
The NCVS only provides binned income ranging from $0-$5,000; up to $75,000. Exact bin sizes vary but are typically $2,500 at the lower end of the distribution, and $5,000 to $10,000 as income gets closer to $75,000.
*** 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

	Pre-OBRA-93			Post-OBRA-93			Post-Pre
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Variable	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: Data from the National Crime Victimization Survey (NCVS). Standard deviations in parentheses.
*** 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 unmarried 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. Each model controls for race indicators, age, educational attainment, year and month fixed effects, and the number of children.
*** p<0.01, ** p<0.05, * p<0.1.

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
Control Mean	0.0107	0.0361	0.00135	0.00560	0.0119	0.0417	0.630
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
Control Mean	0.00924	0.0220	0.00109	0.00326	0.00978	0.0252	0.491
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
Control Mean	0.00989	0.0238	0.00103	0.00321	0.0103	0.0270	0.494
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
Control Mean	0.0115	0.0375	0.00137	0.00584	0.0125	0.0433	0.533

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

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	-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
Control Mean	0.0104	0.0327	0.00129	0.00504	0.0114	0.0377	0.597
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	-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
Control Mean	0.0107	0.0361	0.00135	0.00560	0.0119	0.0417	0.630
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	-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
Control Mean	0.00989	0.0238	0.00103	0.00321	0.0103	0.0270	0.494

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

Table 7. Economic impacts on unmarried women 16 to 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
Children 1+ vs. 0 children						
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 estimated $ increase in after-tax income. After-tax income includes predicted EITC eligibility imputed using taxsim. All amounts are inflation adjusted to be in 2010 dollars.

Table 8: The impact of EITC on IPV 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
Control Mean	0.0068	0.0039

Notes: '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. Robust standard errors in parentheses.
*** p<0.01, ** p<0.05, * p<0.1

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

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

Appendix Table A.3. Employment impacts on 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
Children 1+ vs. 0 children						
Treatment Effect	-0.002	-0.014**	-0.001**	-0.008***	-0.003**	-0.022***
Increase in Employment	0.021	0.021	0.021	0.021	0.021	0.021
ToT for 10 p.p. Increase in Work	-9.5%	-6.67%	-4.76%	-3.81%	-1.43%	-10.48%

Notes: This table scales the coefficients of the OBRA-93 expansion on IPV estimated in tables 4 and 5 by the estimated change in employment.

Appendix Table A.4. By parity: Unmarried 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**	-0.016*	0.030***
	(0.002)	(0.008)	(0.001)	(0.003)	(0.002)	(0.009)	(0.008)
Post-OBRA-93 x Children = 2	-0.001	-0.021**	-0.001**	-0.009***	-0.002	-0.030***	0.075***
	(0.002)	(0.008)	(0.001)	(0.003)	(0.002)	(0.010)	(0.008)
Post-OBRA-93 x Children >= 3	-0.001	-0.012	-0.001	-0.008**	-0.002	-0.020*	0.025***
	(0.002)	(0.009)	(0.001)	(0.003)	(0.002)	(0.010)	(0.008)
Observations	123,954	123,954	123,954	123,954	123,954	123,954	122,761
Control Mean	0.0104	0.0327	0.00129	0.00504	0.0114	0.0377	0.597
Panel B: White							
Post-OBRA-93 x Children = 1	-0.003	-0.007	-0.001*	-0.009**	-0.004**	-0.016	0.030***
	(0.002)	(0.010)	(0.001)	(0.004)	(0.002)	(0.011)	(0.009)
Post-OBRA-93 x Children = 2	-0.000	-0.025**	-0.001	-0.008**	-0.001	-0.033**	0.057***
	(0.002)	(0.012)	(0.001)	(0.004)	(0.002)	(0.013)	(0.009)
Post-OBRA-93 x Children >= 3	0.000	-0.014	-0.000	-0.008**	-0.000	-0.023	0.009
	(0.002)	(0.013)	(0.001)	(0.004)	(0.002)	(0.014)	(0.009)
Observations	93,856	93,856	93,856	93,856	93,856	93,856	93,013
Control Mean	0.0107	0.0361	0.00135	0.00560	0.0119	0.0417	0.630
Panel C: Black							
Post-OBRA-93 x Children = 1	-0.004	-0.015	-0.000	-0.002	-0.003	-0.016	0.030
	(0.003)	(0.014)	(0.001)	(0.003)	(0.003)	(0.015)	(0.019)
Post-OBRA-93 x Children = 2	-0.005*	-0.017	-0.003**	-0.010	-0.006*	-0.026**	0.107***
	(0.003)	(0.010)	(0.001)	(0.006)	(0.003)	(0.013)	(0.018)
Post-OBRA-93 x Children >= 3	-0.006*	-0.013	-0.002	-0.005	-0.006*	-0.017	0.047***
	(0.003)	(0.011)	(0.001)	(0.007)	(0.003)	(0.014)	(0.018)
Observations	25,222	25,222	25,222	25,222	25,222	25,222	24,937
Control Mean	0.00989	0.0238	0.00103	0.00321	0.0103	0.0270	0.494

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

Appendix Table A.5. 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)							
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)
Clustered at # of Children-by-Year							
Post-OBRA-93 x Children >= 1	-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							
Post-OBRA-93 x Children >= 1	-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							
Post-OBRA-93 x Children >= 1	-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							
Post-OBRA-93 x Children >= 1	-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							
Post-OBRA-93 x Children >= 1	-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. Each model controls for race indicators, age, educational attainment, year and month fixed effects, and the number of children.
*** p<0.01, ** p<0.05, * p<0.1.

Appendix Table A.6. 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							
Post-OBRA-93 x Children >= 1	-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)
Baseline Model							
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)
Additional Controls Variables							
Post-OBRA-93 x Children >= 1	-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. Limited controls include year dummies number of children indicators. The Baseline Model is the one we have used as our preferred specification throughout the text: it includes race indicators, age, educational attainment, year dummies, month-fixed effects, and the number of children indicators. Additional Control Variables add to the baseline specification the number of children groups (0, 1, 2+) interacted with the various demographic controls (age, education, race groups).
*** p<0.01, ** p<0.05, * p<0.1.

Appendix Table A.7. Baseline characteristics (before OBRA-93) for different population subgroups

	(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	16,739	12,174	6,664	2,534	5,831	1,955	3,219	1,186	18,326	7,190	1,514	6,830

Notes: Standard deviations in parentheses

Appendix Table A.8. Outcome means for different population subgroups

	(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	23,403	14,708

Notes: Standard deviations in parentheses.

Appendix Table A.9. Coefficient estimates on control variables in Panels A & B of Table 4

	(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)
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. Each model also controls for year and month-fixed effects.
*** p<0.01, ** p<0.05, * p<0.1.